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Risky Choice With Heuristics: Reply to Birnbaum (2008), Johnson, Schulte-Mecklenbeck, and Willemsen (2008), and Rieger and Wang (2008)

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E. Brandstätter, G. Gigerenzer, and R. Hertwig (2006) showed that the priority heuristic matches or outperforms modifications of expected utility theory in predicting choice in 4 diverse problem sets. M. H. Birnbaum (2008) argued that sets exist in which the opposite is true. The authors agree—but stress that all choice strategies have regions of good and bad performance. The accuracy of various strategies systematically depends on choice difficulty, which the authors consider a triggering variable underlying strategy selection. Agreeing with E. J. Johnson, M. Schulte-Mecklenbeck, and M. C. Willemsen (2008) that process (not "as-if") models need to be formulated, the authors show how quantitative predictions can be derived and test them. Finally, they demonstrate that many of Birnbaum's and M. O. Rieger and M. Wang's (2008) case studies championing their preferred models involved biased tests in which the priority heuristic predicted data, whereas the parameterized models were fitted to the same data. The authors propose an adaptive toolbox approach of risky choice, according to which people first seek a no-conflict solution before resorting to conflict-resolving strategies such as the priority heuristic.

Keywords: risky choice, heuristics, decision making, choice process, adaptive toolbox

In Brandstätter, Gigerenzer, and Hertwig (2006), we reported findings that, to the best of our knowledge, were novel. First, we showed that a simple sequential cognitive process, the priority heuristic, implies several classic violations of expected utility theory that had previously been accounted for by adding complex nonlinear transformations of utilities and probabilities on top of the expected utility framework. Second, across four different data sets with a total of 260 problems, the priority heuristic predicted the majority choice better than each of three modifications of expected utility theory did. Third, with the exception of tallying, none of 10 well-known heuristics (such as minimax and equal-weight) predicted the majority choice much better than at chance level.

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These counterintuitive findings have stirred up a debate that addresses fundamental issues of how to model decision making under risk. Does the mind rely on a single utility calculus, or is it designed in a modular way, embodying an adaptive toolbox of heuristics? Should we model cognitive processes, or predict observable choices only? We are grateful to the authors of the three comments for their thoughtful remarks and the challenges they set out for us. In what follows, we address three major points of their criticism: (a) that we were selective in our choice of data against which we tested the priority heuristic and that data sets exist in which the priority heuristic predicts behavior less accurately than modifications of expected utility theory do; (b) that when parameters are allowed to be fitted to choices, modifications of expected utility are better than the priority heuristic at accounting for choice and are therefore superior; and (c) that the process predicted by the priority heuristic is supported by some of the Mouselab data but conflicts in particular with the frequent transition between outcomes and their probabilities.

First, with respect to selectivity, we are not aware of any other investigation of risky choice that tested so many competing models against such a diverse set of problems, none of which were designed by us. We acknowledge the existence of data sets in which the priority heuristic does not fare well and point out that all choice models have regions of good and bad performance—a fact that, in our view, supports an adaptive toolbox approach to risky choice. Second, we question the value of biased tests in which one model predicts data, whereas competing ones are allowed to be fitted to the same data. Third, we show how one can derive quantitative predictions of transition probabilities for competing models and then test them.

Multiple Strategies or One

In their comments, Birnbaum (2008) and Rieger and Wang (2008) championed a single model for all choices reviewed. Rieger and Wang called cumulative prospect theory the most established model and seemed to buy its validity lock, stock, and barrel. At the same time, Birnbaum—who has collected counterexamples to cumulative prospect theory (e.g., Birnbaum & Naverrete, 1998)—presented the transfer-of-attention-exchange model as the single best theory. In contrast, Johnson, Schulte-Mecklenbeck, and Willemsen (2008) focused on process models, a focus that has also been prominent in research on the "adaptive decision maker," whereby people are assumed to select among multiple strategies, depending on the task at hand (e.g., Johnson & Payne, 1985; Payne, Bettman, & Johnson, 1993). The three sets of comments reflect the heterogeneity of opinions in research on risky choice.

Each Model Has Its Regions of Good and Bad Performance

Birnbaum (2008) and Rieger and Wang (2008) demonstrated that problem sets exist that the priority heuristic cannot predict well. Although correct, the same finding can be demonstrated for every model of risky choice, not just the priority heuristic. To appreciate how prediction performance systematically changes across tasks, we analyzed Erev, Roth, Slonim, and Barron's (2002) 100 randomly drawn problems consisting of two-outcome gambles. Specifically, we investigated how well various strategies can predict the majority choice as a function of the ratio between the gambles' expected values. This ratio can be understood as a proxy for the subjective difficulty of the problem, with ratios from 1 to 2 representing "difficult problems" and ratios larger than 2 representing increasingly "easy problems." This distinction also becomes psychologically manifest in faster responses and higher interindividual consistency for easy problems relative to difficult ones (see Busemeyer & Townsend, 1993; Mosteller & Nogee, 1951). In the Erev et al. problems, for instance, the proportion of people making the same choice increased from 79% to 88% when the median ratios between expected values increased from 1.2 in the first quartile to 6.7 in the fourth quartile, respectively. Similarly, for the set of problems by Mellers, Chang, Birnbaum, and Ordóñez (1992), this proportion increased from 73% to 95% when the average ratios between expected values increased from 1 to 5.8 (Brandstätter et al., 2006, Footnote 8).

Figure 1 shows that the accuracy of the strategies systematically depends on the ratio between the gambles' expected values. More specifically, the pattern of accuracy suggests two classes of strategies: The modifications of expected utility theory—security-potential/aspiration theory (Lopes & Oden, 1999), cumulative

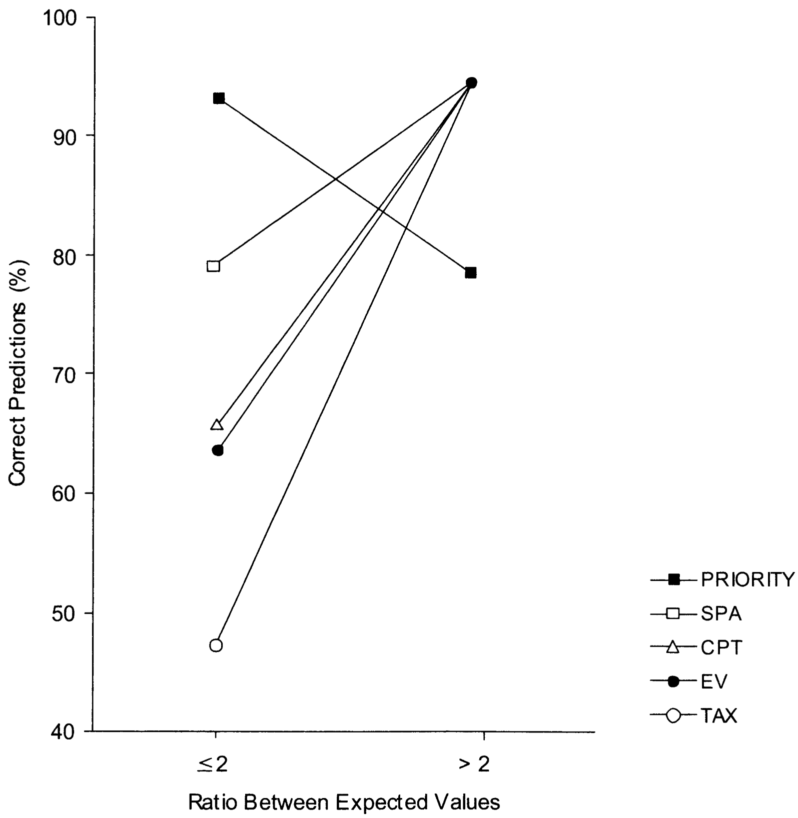

Figure 1. Correct predictions dependent on the ratio between expected values for the set of problems in Erev et al. (2002). For parameter estimates, see Footnote 8 in Brandstätter et al. (2006). PRIORITY = priority heuristic; SPA = security-potential/aspiration theory; CPT = cumulative prospect theory; EV = expected value theory; TAX = transfer-of-attention-exchange model.

prospect theory (Tversky & Kahneman, 1992), and the transfer-of-attention-exchange model (Birnbaum & Chavez, 1997)—all better predict the majority choice for easy problems than for difficult ones. So do almost all of the heuristics investigated by Thorngate (1980), whose values we did not plot in Figure 1 for the sake of clarity. In contrast, the priority heuristic shows the reverse pattern: It predicts the majority choice better for difficult problems than for easy ones. This result indicates that all models have regions of good and bad performance and, specifically, that high difficulty is a triggering condition for the priority heuristic.

This contingency between strategy accuracy and choice difficulty holds beyond the Erev et al. data set. As we have shown in Brandstätter et al. (2006, Figure 8, p. 426), it also occurs in Mellers et al.'s (1992) set of 450 problems. Birnbaum criticized the priority heuristic for correctly predicting only 327 of the 450 choices (27% errors) and for its highly systematic errors. He was correct that there is a systematic pattern in errors. But as Figure 1 shows, a systematic pattern in errors exists across all strategies. Another observation in Figure 1, also made in the Mellers et al. problems (Figure 8 of Brandstätter et al., 2006), is that when choice is easy, good old-fashioned expected value theory predicts behavior as well as any of the parameterized modifications of expected utility theory do. This poses a challenge to the latter. To illustrate, in every single problem that Birnbaum presented as a counterexample to the priority heuristic in which the ratio between expected values exceeds 2, the majority choice is predicted by expected value theory.

To summarize, if different kinds of problems trigger different strategies—a view also espoused by Johnson et al. (2008)—then regions of good and bad performance are inevitable. By forgoing the assumption that people rely on one single calculus and presuming instead that they use two or more strategies, the research question becomes, What strategies (in the plural) do people use, and what situations trigger one over the other? In the next section, we propose that people first seek a no-conflict solution; only if that fails do they rely on heuristics that can resolve conflicts, such as the priority heuristic.

Looking for a No-Conflict Solution First

We tend to know in an instant when one gamble is obviously the better one. Little is known about how to model this fast formation of an intuitive impression. Although we used the ratio between the gambles' expected values as a proxy for perceived difficulty in our analysis, we do not assume that people calculate expected values (utilities), a concern that Birnbaum expressed. Here are two first hypotheses about how the intuitive impression materializes that one gamble is clearly superior to another. First, dominated gambles are often recognized as such. For instance, Birnbaum (p. 254) reported that 92% of participants chose Gamble A ($100, .20; $96, .30; $4, .50) over Gamble B ($100, .20; $12, .30; $4, .50).

Most people can see that there is no conflict between A and B, because only one outcome differs. No further computations are necessary, neither those described by modifications of expected utility nor those described by models of heuristics. Second, if dominance is absent or not recognized, other characteristics of the gambles can prevent conflict. We propose a similarity heuristic, based on Rubinstein's (1988) and Leland's (1994) similarity model, in which we define similarity quantitatively, using the same definition of aspiration level as for the priority heuristic (Brandstätter et al., 2006, pp. 412–413):

> If in both gambles, all corresponding outcomes and probabilities are similar, that is, all differences are zero or below the aspiration level, but one or more differences far exceed the aspiration level and favor the same gamble, then choose this gamble.

For illustration, consider one of four problems Birnbaum and Navarrete (1998) used to test stochastic dominance: The choice was between Gamble A ($12, .05; $14, .05; $96, .90) and Gamble B ($12, .10; $90, .05; $96, .85).

Most respondents, 73%, chose the second gamble. All differences are zero or below the aspiration level, and they are eclipsed by the difference between $14 versus $90. Such a similarity check, like a dominance check, can be seen as an attempt to find a no-conflict solution. This similarity heuristic predicts the majority choice in all four of Birnbaum and Navarrete's (1998) stochastic dominance problems.

These are initial testable ideas about what constitutes a no-conflict solution and what makes people forgo using the priority heuristic or other conflict-resolution strategies. These ideas map directly onto Rubinstein's (1988) three-step-model, according to which people first check dominance and stop if it is present, otherwise check whether the similarity heuristic can be applied, and if it cannot, proceed to a third step that Rubinstein left unspecified. The priority heuristic elaborates on Rubinstein's framework by specifying his Step 3.

Dominance and similarity are not the only no-conflict solutions. Consider the Birnbaum and Navarrete (1998) set of gambles in which, as Birnbaum (2008) pointed out, the priority heuristic does not fare well. In fact, there is a subset of 54 choices between three-outcome gambles in which the priority heuristic performs at chance level. In 17 of these, there was little consistency among individuals (the majority choice ranged between 45% and 55%).[1] To check which majority choices are robust, we conducted an experiment using the same 54 problems (with 80 participants) and replicated Birnbaum and Navarrete's majority choice in only 38 of them. Seventeen of these 38 problems have a very specific structure: one branch (outcome-probability combination) is exactly the same in both gambles, and the probabilities of the other two branches are all identical. In his comment, Birnbaum presented four problems of this kind, one being a choice of Gamble A ($110, .50; $96, .25; $12, .25) or Gamble B ($110, .50; $34, .25; $30, .25).

As Birnbaum correctly pointed out, the priority heuristic cannot predict the majority choice of Gamble A, whereas his transfer-of-attention-exchange model can (if a set of prior parameters is used). We conjecture that participants are unlikely to go through the fairly sophisticated computations of the transfer-of-attention-

[1] In addition, choice between two monetary gambles is known to be only moderately reliable if the expected values are close: When people were presented with the same problem twice, the average individual consistency was only 66% (with 50% for random choice), regardless of whether the interval was 1 week or 90 min (Pachur, Hertwig, Brandstätter, & Gigerenzer, 2007; see also Hey, 2001). This modest consistency puts a natural limit on what can be explained in individual behavior; "explaining" beyond this limit means that a theory fits noise by means of adjustable parameters. For this reason, we turned to the more stable majority choice when testing all models.

exchange model, which among other processes involve nonlinear transformations of probabilities similar to those assumed in prospect theory. Rather, the specific structure of this problem may give rise to another no-conflict solution. We conjecture that respondents begin by canceling the identical outcome-probability branch (see also Kahneman & Tversky, 1979)—an operation for which there is direct evidence in the literature (see Raynard's [1995] process tracing study). In the example, the identical branch ($110, .50) is cancelled. The remaining four probabilities are all equal (.25) and therefore uninformative for the choice. This "all probabilities are equal" property of the gambles represents, so we hypothesize, the triggering condition for the *toting-up heuristic*:

> If all probabilities are the same, then add up the outcomes of each gamble and select the gamble with the higher sum

In the example, the sum total of A ($108) being larger than B's ($64) predicts a preference for A, consistent with Birnbaum's (2008) finding. Can this heuristic account for the other problems with the same structure? In 2 of the 17 problems, the sum is a tie; among the remaining 15 problems, the heuristic correctly explains the majority choice in 14 of them. Among the 4 problems with this structure that Birnbaum presented in his comment, the first and third could not be replicated; that is, we found a majority choice in the opposite direction, whereas the other two can be explained by cancellation and the toting-up heuristic.

In summary, we suggest that each strategy has regions of good and bad performance (see also Day & Loomes, 2007). From this, we conclude that the mind relies on multiple strategies. There is empirical evidence for such an adaptive toolbox of heuristics, and their task-contingent use in inference (e.g., Bergert & Nosofsky, 2007; Bröder & Gaissmaier, 2007; Bröder & Schiffer, 2003; Gigerenzer, Todd, & the ABC Research Group, 1999; Gigerenzer & Selten, 2001; Rieskamp & Otto, 2006) and choice under certainty (e.g., Denstadli & Lines, 2007; Ford, Schmitt, Schlechtman, Hults, & Doherty, 1989; Payne et al., 1993; Tversky, 1972; Yee, Dahan, Hauser, & Orlin, 2007). We propose a corresponding view for risky choice in which people first look for a no-conflict solution before applying a conflict-resolution strategy.

Fit or Predict

One key issue in research on risky choice is post-hoc data fitting, a problem that also applies to heuristics. Recall the set of 38 three-outcome problems by Birnbaum and Navarrete (1998), 17 of which had the specific structure described above. What about the other 21 problems? These also have an unusual structure: One outcome-probability branch is always identical, and the two problems have the same probability distribution. An example is Gamble A ($110, .60; $34, .30; $30, .10) or Gamble B ($110, .60; $96, .30; $12, .10).

A conflict-resolving heuristic exists that accounts for 19 of the 21 majority choices after cancellation. Yet it would be inappropriate to propose it as an explanation (rather than a hypothesis to be tested in the future), because we discovered it by testing several of Thorngate's (1980) heuristics until finding one that almost perfectly fit. And, most important, we do not yet understand its triggering conditions. This heuristic is known as the *most-likely heuristic* (see Brandstätter et al., 2006, p. 417):

> Determine the most likely outcome of each gamble and their respective payoffs. Then select the gamble with the highest, most likely payoff.

Explaining existing data by this or other heuristics without specifying their triggering conditions would be tantamount to fitting data post hoc. In contrast, identifying a triggering condition—such as identical probabilities for the toting-up heuristic—enables predictions to be made about the future use of heuristics. To this end, Bettman, Johnson, Luce, & Payne's (1993) analysis of the most-likely heuristic (the first step of their lexicographic heuristic) indicates that high variance in probabilities and positive correlation between attributes might be candidates for triggering conditions.

Before continuing the discussion of the problem of fitting, let us first be clear about terminology. Birnbaum (2008) referred to the Brandstätter et al. (2006) tests of 14 strategies as "contests of fit" (p. 254). It was, however, a contest of prediction. We first derived the priority heuristic using logical constraints, empirical findings (e.g., outcome matters more than probability), and psychological concepts embodying bounded rationality, such as aspiration levels (see Brandstätter et al., 2006, pp. 411–413). We then made predictions for the data sets. Differentiating between fitting and prediction is crucial for the evaluation of models of risky choice. If a model has adjustable parameters—cumulative prospect theory, for instance, has five—and their values are adjusted retrospectively to the data to be explained in order to maximize a measure of goodness of fit, this is called data fitting. In contrast, if the parameters of a model are fixed or measured independently of the data to be explained, this is called prediction. For a model with fixed parameters, such as the priority heuristic, the parameters cannot be fitted anew for each experiment. For models with adjustable parameters, fitting performance is generally higher than prediction performance (Roberts & Pashler, 2000).

When Birnbaum (2008) and Rieger and Wang (2008) claimed superior performance of their preferred models, they frequently compared the *fitting* performance of their model with the *predictive* accuracy of the priority heuristic. Here are several illustrations. In Brandstätter et al. (2006), we reported that cumulative prospect theory and the transfer-of-attention-exchange model *predicted* 64% (averaged across three sets of parameters) and 71% of the majority response, respectively, in the Kahneman and Tversky (1979) problems. Birnbaum defended his model by stating that cumulative prospect theory and the transfer-of-attention-exchange model fit the Kahneman and Tversky (1979) data set perfectly when parameters are estimated from the data. Fitting parameters to the same data that one aims to explain, as Birnbaum proposed, is an inadequate test of models: The more adjustable parameters one adds to a model, the better its fit will be (e.g., Roberts & Pashler, 2000). An adequate test is based on prediction.

Data fitting—using whatever parameter values fit best—is likely to result in a wide range of parameter values. Birnbaum's comment gives testimony to this range. On the one hand, Birnbaum often refrained from fitting and used a set of prior parameter values, such as $\beta = 1$ (the exponent of the utility function). But when he reported how much better his model fits data in comparison with the priority heuristic, he also used values as low as 0.31 for his β parameter. In his Figure 1, he even used β values between .06 and .68. What do these values mean psychologically? For instance, $\beta = 1$ means that an objective amount of $150 is transformed to a utility of 150, whereas a value of 0.31 corresponds to a utility of 4.7. This illustrates the enormous flexibility that parameter fitting gives a researcher in "explaining" data.

In Brandstätter et al. (2006), we also reported that cumulative prospect theory predicted 79% of the inferred majority responses (averaged across two sets of parameters), based on the Tversky and Kahneman (1992) problems. The priority heuristic predicted 89% correctly. Rieger and Wang (2008), performing a different test, reported superior fit for cumulative prospect theory in their Figure 2 (p. 277). Again, this result involving certainty equivalents was obtained from parameter values for cumulative prospect theory that were derived from the very same data set. Similarly, Birnbaum claimed superiority of cumulative prospect theory and the transfer-of-attention-exchange model by once again using the parameter values fitted to the same data set that was to be explained. In each of these three cases, the tests were biased because the models with adjustable parameters were "tested" by fitting data, whereas the priority heuristic was tested by making predictions.

The issue of data fitting appears not only in commentators' reanalysis of our analyses but also in their presentation of counterexamples. Rieger and Wang (2008) constructed a set of 99 choices between a gain of 200 with probability p and a gain of 100 for sure, where p varies from .01 to .99. They asserted that cumulative prospect theory predicts that the lottery is preferred as long as the probability of winning is larger than 71% if the value and weighting functions are based on the parameters measured by Tversky and Kahneman (1992). But what if one of the many other available sets of parameter estimates were used? If one used the values from Lopes and Oden (1999), Erev et al. (2002), or, for instance, Bernstein, Champan, Christensen, and Elstein (1997), cumulative prospect theory would predict that the lottery is preferred as long as the probability of winning is greater than 80%, 88%, or 96%, respectively. In other words, the range in which competing models should be tested is not actually between 50% and 71%, as Rieger and Wang depicted in their Figure 1 (p. 276), but can be as large as between 50% and 96%, depending on the set of parameter estimates used.

In summary, there are two ways in which a theory can fail. It can be empirically wrong, or it can be difficult to be proven wrong, due to the flexibility of its parameters. The priority heuristic does not use adjustable parameters and therefore can be easily shown to be wrong in certain situations, whereas the modifications of expected utility theory typically rely on several adjustable parameters, equipping them with enormous flexibility (although, as Birnbaum, 2008, pointed out, there can be critical properties like intransitivity that no set of parameters can fit). As shown in Brandstätter et al. (2006, Table 5), the overlap in predictions between modifications of expected utility theory is less driven by the psychological concepts embodied by a particular theory than by whether they are fitted to the same set of problems.

Our and Others' Selection of Problems

Birnbaum (2008) contended that we "were somewhat selective in the data" (p. 253). This conjecture strikes us as unfair. We are not aware of another investigation that tested so many competing models against such a diverse set of problems, with every problem constructed and tested by others. Rieger and Wang (2008) admonished us that a theory (i.e., the priority heuristic) that differs completely from expected utility theory should not be solely tested on experimental data that were obtained to find "subtle differences" (p. 276) between people's choices and the predictions of expected utility theory. This criticism is unfounded. We tested the priority heuristic and competing models not only on problems designed to explore "subtle differences" but also on a set of 100 randomly selected problems (Erev et al., 2002). Even in this set, the priority heuristic predicted 85% correctly, whereas cumulative prospect theory predicted 82%, averaged across the two sets of parameter estimates used (Brandstätter et al., 2006).

Rieger and Wang (2008) began their comment with three problems that they presented as counterexamples to the priority heuristic. All lie outside its specified range of application. In Brandstätter et al. (2006), we explicitly specified that if one gamble transparently dominates the other (absolutely or stochastically), people will choose the dominant gamble rather than going through the steps of the priority heuristic. Likewise, we suggested that if choice is manifestly easy (for which we used a ratio between expected values larger than 2 as a proxy), they will not go through the steps of the heuristic. In Rieger and Wang's Examples 1 and 3 (p. 274 and p. 275, respectively), one alternative dominates the other, and in Example 2 (p. 275), the expected values differ by a factor of about 5. In each of these examples, choice is easy; neither the priority heuristic nor cumulative prospect theory nor the transfer-of-attention-exchange model is needed. Selected counterexamples are no doubt extremely useful for learning about the limits of various theories. Since each theory of risky choice has counterexamples, however, theories need to be evaluated in terms of how they predict (not fit) people's choice in problems that have been ideally designed to test a wide range of theories (e.g., Figure 1).

Evidence for Cognitive Processes

Models of risky choice belong to one of two classes: those that aim at predicting overt choices and those that aim at predicting choices while modeling and predicting the processes that produce them. The first are often called *as-if models* and the second *process models*. It is not always clear to which class a model belongs. Our commentators disagreed among themselves whether their preferred theories aim to capture actual processes or whether they are as-if models. Birnbaum (2008) postulated that people might use the transfer-of-attention-exchange model for computations and make a decision, suggesting a process interpretation of the model. Rieger and Wang (2008), in contrast, asserted that nobody assumes that people's brains are equipped with a cumulative-prospect-theory calculator, implying an as-if interpretation of cumulative prospect theory. Johnson et al. (2008), in turn, emphasized that research will progress precisely by moving beyond as-if models. We could not agree more.

Two tests of the processes underlying the priority heuristic now exist, each involving a different measurement, namely, of response time and information acquisition. The first test was reported in Brandstätter et al. (2006). For each problem, the stopping rule of the priority heuristic predicts how many reasons will enter the decision, which in turn implies response time differences. Participants' response times were shorter for those problems in which the heuristic predicted that only one rather than three reasons entered the decision. Neither cumulative prospect theory nor the transfer-of-attention-exchange model can predict these response time differences because they, unlike the priority heuristic, assume that the examined response times within a set of gambles are not contingent on the properties of the gambles.

We appreciate the study by Johnson et al., which provided another process test using a different process tracing tool, Mouselab. They reported 28 tests of ordinal process predictions. If one uses their $p \leq .05$ criterion, 11 were in the direction predicted by the priority heuristic, whereas 3 were in the opposite direction and 14 were not significant (see their Tables 1 and 2; p. 268 and p. 269, respectively).

The priority heuristic, however, enables not just ordinal predictions but also precise quantitative ones. In the following, we show how such a prediction can be derived for the one striking discrepancy between predictions and data reported by Johnson et al. (2008), the *probability-payoff hypothesis*. Let us stress that the following analysis does not invalidate their results, but illustrates how one can derive quantitative process predictions for competing models. According to Johnson et al., the priority heuristic predicts that transitions between an outcome (payoff) and its probability should be relatively infrequent or rare. How rare is rare? To derive quantitative predictions, we make the following simplifying assumptions: First, independent of their choice strategies, people initially read each single piece of information once, first for Gamble A and then for Gamble B.[2] Second, once the reading phase is terminated, the choice phase begins in which only those pieces of information that a given strategy deems relevant are looked up a second time. Note that repeated acquisitions of information, rather than the assumed single acquisition, make no difference in the predicted transitions, as long as these are randomly distributed over the reading and choice phases.

For illustration, consider the choice between Gamble A ($3,000, .75; $4,000, .25) and Gamble B ($2,800, .80; $5,000, .20) that Johnson et al. (2008) used (see their Figure 1, p. 265). Using their notation, let us assume that each cell is numbered, from 1 (W_a^1) to 4 (P_a^2), and 5 (W_b^1) to 8 (P_b^2). Reading all eight pieces of information results in four *outcome-probability transitions* (a transition between an outcome and its probability; 1-2, 3-4, 5-6, and 7-8), two other *within-gamble transitions* (a transition between a probability and the other outcome; 2-3, 6-7), and one *within-reason transition* (a between-gambles transition within one reason; 4-8). The latter is based on the assumption that people minimize distance in reading. Note that our analysis assumes that the transitions in the reading process are identical in the vertical and horizontal presentations of the gambles.

In the choice phase, transition probabilities depend on the strategy used. Specifically, if the priority heuristic examines r reasons (r = 1, 2, 3, or 4) in a given pair of gambles, there will be r within-reason transitions and $r - 1$ within-gamble transitions, that is, a total of $2r - 1$ transitions. Among the $r - 1$ within-gamble transitions, zero ($r = 1$), one ($r = 2$), one ($r = 3$), and two ($r = 4$) will be transitions from an outcome to its probability (independent of direction). That is, depending on the number of reasons examined, 0%, 33%, 20%, and 29% of all transitions will be from an outcome to its probability in the choice phase (for details, see Appendix). These quantitative predictions specify what Johnson et al. (2008) called a "relatively infrequent" (p. 265) transition from an outcome to its probability.

Neo-Bernoullian models that always use all pieces of information—such as expected utility theory, prospect theory, transfer-attention-exchange model, and similar modifications—will make $2m$ outcome-probability transitions (m = number of outcomes), $2(m - 1)$ other within-gamble transitions, and 1 within-reason transition. Given that these models employ no stopping rule but use all information, the analysis is based on the number of outcomes (m) rather than on the number of reasons (r) investigated before examination is stopped. Thus, they predict the proportion of transitions from an outcome to its probability to be $2m/(4m - 1)$. In two-outcome and five-outcome gambles, 57% and 53% of transitions will be outcome-probability transitions (for details, see Table 1).

Table 1
Predicted and Obtained Transition Percentages for Reading and Choice Phase Combined

	Two-outcome gambles			Five-outcome gambles	
Variable	$r = 1$	$r = 2$	$r = 3$	$r = 1$	$r = 3$
Outcome-probability transitions					
Predictions					
Priority heuristic	50	50	42	50	46
EU and modifications	57	57	57	53	53
Results					
Johnson et al.	51	—	49	33	33
Brandstätter et al.	—	36	—	—	—
Other within-gamble transitions					
Predictions					
Priority heuristic	25	20	25	40	38
EU and modifications	29	29	29	42	42
Results					
Johnson et al.	17	—	19	*na*	*na*
Brandstätter et al.	—	22	—	—	—
Within-reason transitions					
Predictions					
Priority heuristic	25	30	33	10	17
EU and modifications	14	14	14	5	5
Results					
Johnson et al.	9	—	13	*na*	*na*
Brandstätter et al.	—	15	—	—	—

Note. Outcome-probability transitions refer to transitions between an outcome and its corresponding probability (in both directions). Other within-gamble transitions refer to transitions within a gamble that are not outcome-probability transitions. Within-reason transitions refer to transitions between two corresponding pieces of information (e.g., between the minimum outcomes of both gambles or between their probabilities). A dash indicates that the condition was not studied in an experiment. EU = expected utility theory; *na* = data were not available.

[2] The assumption that *all* pieces of information are looked up once is implied by the search for a no-conflict solution, and it also allows the reader to determine the maximum and minimum outcomes and their probabilities, information that the priority heuristic needs.

It is not easy to determine when reading is over and choice begins. Johnson et al. (2008) used the plausible criterion of including in the process of reading all acquisitions that are made before all outcomes have been examined at least once. In our quantitative analysis, we similarly defined reading as looking up all information once. However, reading may involve repeated examination of one or several pieces of information (in particular in gambles with more than two outcomes). To avoid the thorny issue of distinguishing reading from choice, we collapsed transitions across both phases, and then predicted the proportions of three types of transitions. Table 1 provides the predicted proportions for two- and five-outcome gambles, listed separately according to the types of transitions and strategies.

We tested these quantitative predictions against the data collected by Johnson et al. (2008) and our own Mouselab data. In our study (using Gambles Experimental Software, Czienskowski, 2006), 20 respondents were tested on 20 of Erev et al.'s (2002) randomly generated problems, which consisted of choices between two simple gambles of the form (x, p; 0, 1 − p). In this gamble set, the priority heuristic always stopped after the second reason. Table 1 reports the predicted and observed transitions in both studies. Let us emphasize two results: First, the differences between the predicted transitions for expected utility models and the priority heuristic are consistently not as large as one may intuitively expect. Second, when one compares the predictions of the priority heuristic with those of expected utility theory and its modifications, the results are mixed. Consider first the outcome-probability transitions—the kind of transition for which Johnson et al. found a striking discrepancy between predictions derived from the priority heuristic and data. For choices between two-outcome gambles, the observed proportions (51%, 36%, and 49%) are in fact closer to those predicted by the priority heuristic (50%, 50%, and 42%) than to those predicted by expected utility theory and its modifications (57%). For five-outcome gambles, predictions and observed data are more divergent, yet in comparative terms, the priority heuristic's predictions are again slightly better. The other within-gamble transitions are also better predicted by the priority heuristic, whereas within-reason transitions are better predicted by expected utility theory and its modifications.

To summarize: At this point, process evidence in favor of the priority heuristic stems from a response time analysis (Brandstätter et al., 2006). In addition, there is some—admittedly mixed—evidence from Johnson et al.'s (2008) and our own Mouselab tests. Clearly, more process tests are needed. Moreover, the predictions regarding transition probabilities are very sensitive to the assumptions made about the reading phase. We used two simple assumptions: The information is read first for Gamble A and then for Gamble B, and the transition from A to B minimizes distance in reading. In running such tests, we believe two considerations are important. First, because no single best process-tracking tool is known today, multiple methods should be used that ideally yield converging evidence. Second, as in choices, there is no perfect model in process predictions, and one therefore needs to go beyond null-hypothesis testing and test quantitative predictions of competing models to find out which one is better.

Toward an Adaptive Toolbox Model of Risky Choice

In Brandstätter et al. (2006), we specified only one heuristic, the priority heuristic. In developing and testing it, our goal was to demonstrate that there is a viable alternative to neo-Bernoullian theories. Specifying one heuristic was not meant to imply that the priority heuristic is used across the board. Rather, our view has been inspired by the notion of the "adaptive decision maker" in research on choice (Payne et al., 1993) and by our own research on inference: People adaptively select between several strategies in the adaptive toolbox (Gigerenzer & Selten, 2001; Gigerenzer et al., 1999). Let us very briefly sketch out such an adaptive toolbox view of risky choice—at this point largely in terms of hypothetical statements—thereby also pointing to how future research can proceed in further opening the toolbox.

We propose that risky choice proceeds in two steps: First, people seek a no-conflict solution. Second, if this attempt fails, they resort to heuristics that resolve conflicts, such as the priority heuristic. A no-conflict solution may materialize in terms of (a) detecting dominance; (b) successfully applying the similarity heuristic (see Rubinstein, 1988); and (c) noticing that all probabilities are equal and therefore that summing across outcomes suffices, thus applying the toting-up heuristic. To the end of detecting such no-conflict solutions, we—like Kahneman and Tversky and others (Montgomery, 1983; Raynard, 1995)—assume that initial cognitive operations such as cancellation and combination can occur that simplify the choice problem. Note that the first no-conflict step is not at odds with the frugality of the priority heuristic or any other heuristic, as Birnbaum (2008) implied. We defined frugality as the proportion of pieces of information that a model ignores when making a decision (Brandstätter et al., 2006). Frugality is a property of a strategy and, as such, is orthogonal to the processes that precede its application.

In the absence of finding a no-conflict solution, people experience conflict, for instance, between high returns with low probabilities and low returns with high probabilities. Expected utility theory and its modifications master this conflict by making trade-offs in the sense of weighting and summing information. We proposed an alternative, the priority heuristic, which forgoes tradeoffs and instead chooses by examining reasons sequentially and terminating examination if a difference on one reason is considered large enough. The priority heuristic is certainly not the only conflict-resolution heuristic, but it is a first attempt to model the final step in Rubinstein's (1988) three-step model, according to which people first check for dominance and similarity and, if these checks are unsuccessful, resort to other processes.

We believe that no single strategy can account for the rich pattern of human risky choice and agree with Day and Loomes (2007) that some simple heuristics may be the most plausible explanation. Progress will be in identifying how people achieve no-conflict solutions as well as—if conflicts remain—the heuristics used to resolve them. Of equal importance is specifying precisely the heuristics' triggering conditions in terms of the properties of the gambles, people's risk dispositions, and circumstances such as time pressure. Transparency will be a key virtue in this endeavor. Transparent models of heuristics are better able to help us recognize the triggering conditions than are complex models with multiple adjustable parameters, whose predictions depend on the specific parameter set chosen among the millions of possible combinations. We are grateful to our commentators for sharing their thoughts about the priority heuristic and invite everyone interested to join us in further opening the adaptive toolbox of risky choice strategies.

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Appendix

Derivation of Predicted Transitions for Two-Outcome Gambles

		Choice phase				Choice and reading phase							
		Priority heuristic				Priority heuristic						EU	
						r = 1		r = 2		r = 3			
Types of transitions	Reading phase	r = 1	r = 2	r = 3	EU	No.	%	No.	%	No.	%	No.	%
Outcome-probability	4	0	1	1	4	4	50	5	50	5	42	8	57
Other within-gamble	2	0	0	1	2	2	25	2	20	3	25	4	29
Within reason	1	1	2	3	1	2	25	3	30	4	33	2	14
Total number of transitions	7	1	3	5	7	8		10		12		14	

Note. Example: There are seven transitions (eight pieces of information) in the reading phase, four of which are outcome-probability transitions (see text). In the choice phase, for $r = 1$, the priority heuristic predicts one further transition, a within-reason transition. Thus, across the reading and choice phases, there are a total of eight transitions, four of which are outcome-probability transitions. EU = expected utility theory and its modifications. r = number of reasons used by the priority heuristic. The predictions for five-outcome gambles were derived similarly.

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Postscript: Rejoinder to Johnson et al. (2008) and Birnbaum (2008)

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In their postscript, Johnson et al. (2008) emphasized that models of heuristics and their adaptive use will advance research on risky choice. We agree wholeheartedly. Yet they had two empirical concerns. First, they argued that although only 3 of 28 tests were significant in the opposite direction of what the priority heuristic predicts, one of them, the test for outcome-probability (probability-payoff) transitions, was more important than the others. Once again, we agree. They then provided Table 1 with three classic studies, which they interpreted as evidence for predominantly outcome-probability (or more generally, gamble-wise) transitions relative to within-reasons transitions, the latter being indicative of lexicographic processes such as the priority heuristic. They told us to listen to what the data are saying, which we have. However, neither the authors of these studies nor we view them as clear evidence for predominantly gamble-wise processing. First, Payne and Braunstein (1978) reported that "a majority of subjects processed information about the gambles in ways inconsistent with compensatory models of risky decision making" (p. 554). This evidence contradicts expected utility theory and its modifications as process models, but is consistent with noncompensatory heuristics. Second, Rosen and Rosenkoetter (1978) studied 6 participants and classified 2 as employing reason-wise and 4 as employing gamble-wise processing. Third, Russo and Dosher (1983) observed reason-wise processing in "roughly half of the eye-fixation patterns but [in] over two thirds of the coded verbal reports" (p. 690). We find it interesting that these studies indicate that the process tracking methods differ systematically: Verbal protocols show the most evidence in favor of reason-wise processing and Mouselab the least evidence, whereas the results for eye tracking are in-between (see also Lohse & Johnson, 1996). The bottom line is that contrary to Johnson et al.'s interpretation, these classic studies show that reason-wise processes as postulated by the priority heuristic have been frequently observed.

Moreover, the ratios in Table 1 of Johnson et al. (2008) should be interpreted with care. A ratio larger than 1 was taken to support models that look up information gamble-wise and a ratio smaller than 1 as support for reason-wise processing. Yet for a two-outcome gamble, gamble-wise processing predicts a ratio of 4 (four outcome-probability transitions and one within-reason transition), whereas reason-wise processing results in a ratio of 0.5 (two outcome-probability transitions and four within-reason transitions, assuming that all information is examined). Thus, the predicted ratios are 4 versus 0.5 and are not symmetrically distributed around 1. Therefore, if half of the participants in a study use a gamble-wise strategy and the other half use a reason-wise strategy, the mean ratio will be 2.25 (rather than 1). This shows that ratios below 2.25 are in fact consistent with the predominance of reason-wise strategies.[1] As this case illustrates, deriving quantitative predictions from competing process models is more transparent than using aggregate indices. Finally, let us emphasize that we

[1] If not all of the information is examined, values can be calculated similarly. For instance, if the priority heuristic stops after two reasons (r = 2), there is one outcome-probability transition and two reason-wise transitions, resulting also in a ratio of 0.5.

(*Appendix continues*)

did not ignore our distinction between reading phase and choice phase. In our quantitative analysis, we derived the combined predictions for both phases because it is difficult to discern from the Mouselab data when reading ends and choice begins.

Whereas Johnson et al. (2008) may have disagreed with us on specific issues, we shared consensus on the key questions. Which heuristics do people use? Which task conditions trigger one heuristic over another? Birnbaum (2008), in contrast, did not even pose these questions. The perspective of a single calculus of choice (the transfer-of-attention-exchange model) that, in our view, underlay his comment and postscript left him mystified by the most elementary consequences of an adaptive toolbox view. For instance, he criticized the similarity heuristic because it (a) "contradicts the priority heuristic" by paying attention to different pieces of information and (b) does not account for violations of stochastic dominance other than those "it was devised to fit" (p. 261). With respect to (a), it should be evident that two heuristics, like two bodily organs, must function differently in order to solve different problems. Argument (b) and its variants were repeatedly used by Birnbaum to criticize a heuristic if it does not account for all choices. But an adaptive toolbox view, as well as the concept of the adaptive decision maker (Payne, Bettman, & Johnson, 1993), postulates specialized tools of limited range. Moreover, we did not devise the similarity heuristic to fit some of his problems. Rather, it was designed by Rubinstein (1988) and extended and tested by others (e.g., Leland, 1994). By ignoring this existing work, Birnbaum seemed to create the impression that we invented the heuristic in hindsight. Our position is that people first look for a no-conflict solution (and here the similarity heuristic has its place) and only if that fails, do they use a conflict-resolution heuristic such as the priority heuristic. This—and not the "[expected value] plus priority heuristic model" (p. 260) that Birnbaum described—is our position.

In our view, Birnbaum has not presented a balanced view of the evidence. Each theory of risky choice has its limits, including his favored transfer-of-attention-exchange model, as demonstrated in our Figure 1. We wish we had been more successful in communicating the difference between parameter fitting and prediction with fixed parameters. We can only reiterate in condensed form what Pitt, Myung, and Zhang (2002) and Roberts and Pashler (2000) elaborated in more detail: Fitting free parameters to data alone is an inadequate test of a model. It is unfortunate that Birnbaum did not seem to take this distinction seriously and, in our view, misrepresented it in his postscript. As has been said, the term *prediction* does not necessarily refer to data in the future—although one cannot fit the future—but to tests that use fixed rather than adjustable parameters. The real issue is what kind of theories we want to build: those that, in statistical terms, err on the side of *variance* or those that err on the side of *bias*. Models with many free parameters can reduce bias but suffer from variance (the symptom is known as overfitting), whereas the priority heuristic, which has only fixed parameters, errs on the side of bias. The balance between bias and variance is known as the *bias–variance dilemma* (Geman, Bienenstock, & Doursat, 1992). One solution for this dilemma is an adaptive toolbox perspective with heuristics that have no free parameters, but where the bias of each single one can be compensated for by the other heuristics available.

To conclude, there is strong evidence that humans (e.g., Bergert & Nosofsky, 2007; Bröder & Schiffer, 2006; Payne et al., 1993) and animals (Hutchinson & Gigerenzer, 2005) rely on heuristics in inference and choice under certainty (e.g., Russo & Dosher, 1983). We see no good reason why choice under risk should be an exception. The challenge for the adaptive toolbox theory is to specify computational models of heuristics and their triggering conditions to predict in what situation which heuristic is used. This is the task ahead. The challenge for proponents of single-calculus models of choice is to provide triggering conditions for the specific parameter combinations used in different situations. This has rarely been attempted.[2] Given the nature of single-calculus models, this challenge is even harder to meet. Whereas we consider a handful of heuristics for risky choice, models such as the transfer-of-attention-exchange model and cumulative prospect theory allow for zillions of combinations of parameter values.

Time will show which class of models will ultimately capture the true nature of risky choice. For the present, competition is the engine for progress. The priority heuristic conceptualizes choice in psychological terms different from those of the prevailing neo-Bernoullian theories. This unorthodox perspective will promote the competition by challenging the traditional way of thinking about choice.

[2] One exception is Birnbaum's hypothesis that monetary outcomes smaller or larger than $150 would trigger utility functions that are linear and negatively accelerated, respectively.

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