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.

Received April 24, 2007
Revision received September 24, 2007
Accepted September 26, 2007 ■

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

Eduard Brandstätter
Johannes Kepler University of Linz

Gerd Gigerenzer
Max Planck Institute for Human Development

Ralph Hertwig
University of Basel

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.

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[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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