spatial strategy users must be branded as irrational, this can only be because they lack the necessary resources to discover the more efficient method. This conceptualisation of irrationality as a lack of creativity may be unappealing to some, but for this example the categorisation of these people as irrational is artificial and driven only by the perceived need to address the rationality question.

Overall, S&W make many valid points. We agree that individual differences are an important aspect of human cognition. But to use them merely to resolve the rationality debate is problematic and neglects their full potential. Issues of how people reason, and how these processes change and develop with experience can be better answered by not being side-tracked in this way.

Authors' Response

The rationality debate as a progressive research program

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Abstract: We did not, as Brakel & Shevrin imply, intend to classify either System 1 or System 2 as rational or irrational. Instrumental rationality is assessed at the organismic level, not at the subpersonal level. Thus, neither System 1 nor System 2 are themselves inherently rational or irrational. Also, that genetic fitness and instrumental rationality are not to be equated was a major theme in our target article. We disagree with Bringsjord & Yang's point that the tasks used in the heuristics and biases literature are easy. Bringsjord & Yang too readily conflate the *ability* to utilize a principle of rational choice with the *disposition* to do so. Thus, they undervalue tasks in the cognitive science literature that compellingly reveal difficulties with the latter. We agree with Newton & Roberts that models at the algorithmic level of analysis are crucial, but we disagree with their implication that attention to issues of rationality at the intentional level of analysis impedes work at the algorithmic level of analysis.

We found much that is congenial to our way of thinking in these three commentaries. For example, we welcome **Brakel & Shevrin**'s points about Freud and dual-process theorizing. Such theorizing of course predates Freud as well, going back at least to Plato. As Plato writes in *The Republic*,

> we may call that part of the soul whereby it reflects, rational; and the other, with which it feels hunger and thirst and is distracted by sexual passion and all the other desires, we will call irrational appetite, associated with pleasure in the replenishment of certain wants. (Cornford 1945, p. 137)

While we welcome Brakel & Shevrin's addendum, we take it as understood that the purpose of our paper was not the historical exegesis of dual-process notions. Some historian really does need to do a treatise tracing dual process ideas from Plato, through Freud, to the cognitive revolution (e.g., Evans & Wason 1976; Shiffrin & Schneider 1977), but this was not our purpose. Our argument depended only upon common assumptions of these theories and not on nuanced differences or historical relationships.

There is much in **Brakel & Shevrin**'s characterization of System 1 and System 2 that we agree with. For example, we agree (as do most of the dual-process theorists that we cite in the target article) that System 1 processing is not supplanted by System 2 processing with development, but rather, that both types of processing continue to operate in parallel. However, there are some points of misinterpretation as well. Brakel & Shevrin seem to imply that we are labelling *systems* as rational or irrational, but this is not the case. Instrumental rationality (what was termed normative rationality in our target article) is assessed at the organismic level, not at the subpersonal level. Neither System 1 nor System 2 are themselves inherently rational or irrational.

We focused in the target article and elsewhere (e.g., Stanovich & West 2003) on situations where System 1 functioning served to disrupt the pursuit of instrumental rationality (if not overridden by System 2 processes). But we were also clear to note in the target article that "It must be stressed though that in the vast majority of mundane situations, the evolutionary rationality embodied in System 1 processes will *also* serve the goals of normative rationality" (Stanovich & West 2000, p. 661); and in our Authors' Response we repeated that "we made it clear in the target article that in most cases the goals of Systems 1 and 2 will coincide and that System 1 processes will often *also* serve the goal of normative rationality" (p. 708). So, System 1 serves the organism most of the time by facilitating instrumental rationality, but sometimes disrupts the pursuit of instrumental rationality and must be overridden by System 2.

Thus, System 1 is not appropriately characterized *itself* as being either inherently rational or irrational – a point we feel we made clear in the original target article. Furthermore, the same is true of System 2. It can instantiate rules of rational thought which facilitate maximal goal satisfaction (our emphasis in the target article), but it can also instantiate ideas and rules (memes, in the view of Dennett 1991 and Blackmore 1999) that impede the organism's pursuit of instrumental rationality – a theme we did not emphasize in the target article, but have stressed in subsequent publications (Stanovich 2004; Stanovich & West 2003). Thus, System 2 likewise should not be characterized as inherently rational or irrational, since it too is a subpersonal entity.

Brakel & Shevrin seem to have been confused by our use of the term evolutionary rationality, but here the fault might be ours. Our use of the term in the target article was perhaps too clever by half. The term was coined as an indirect tweak at the evolutionary psychologists who conflate behavior serving genetic fitness with behavior that is instrumentally rational (a major theme in our book-length treatments of these issues; Stanovich 1999; 2004). The terms evolutionary rationality (behavior serving genetic fitness) and normative rationality (instrumental rationality) were meant to separate these two. For example, Over (2000), in his critique of work on fast and frugal heuristics (e.g., Todd & Gigerenzer 2000), makes use of our distinction in exactly the way we intended. Nevertheless, we acknowledge that the term evolutionary rationality may have invited people to conflate just the distinction that we wished to emphasize (as Brakel & Shevrin seem to have done). Thus, in a new book by one of us (Stanovich 2004) – which is largely devoted to working out the implications of mismatches between behavior serving the interests of replicators in the environment of evolutionary adaptation and current instrumental rationality for the organism – the term

is omitted in favor of stating exactly what it is, fitness at the level of the gene.

Brakel & Shevrin's characterization of rationality seems to be overly tied to a conception that emphasizes conscious reasoning according to logical rules. Our conception of rational choice and thought is informed by the much more general conception of rationality in modern cognitive science and decision theory. This difference is apparent in Brakel & Shevrin's statement that birds cannot be rational or irrational. Decision scientists disagree. Much work has been done on whether animals (some as simple as bees) satisfy the strictures of axiomatic utility theory (see Kagel 1987; Real 1991; Shafir et al. 2002). Most cognitive scientists would agree with Millar (2001) that higher-order representation is necessary for something to be a rational *agent,* but it is not necessary for something to be called a rational animal.

As Meliorists, we share **Bringsjord & Yang**'s concern for emphasizing the effects of education on reasoning. We disagree, however, that the problems studied by researchers in the rationality debate are easy, and by implication trivial. We do not share their definition of what is an easy reasoning problem (in Bringsjord & Yang's view, a problem is easy "if there is a simple, easily taught algorithm which, when followed, guarantees a solution to the problem"). Many principles of rational thought can be acquired, but without the dispositions and/or skills necessary to appreciate the applicability of the principles. Many statistics instructors experience frustration with students who learn principles such as the law of large numbers or regression to the mean but cannot think to apply these principles in situations where they are applicable. This is why our own research group and many others (e.g., Klaczynski et al. 1997; Newstead et al. 2002; Perkins 1995; Sa et al. 1999; Schommer 1990;Sinatra & Pintrich 2003; Sternberg 1997) have focused not only on the principles themselves, but also on the cognitive dispositions that facilitate their actual *use* in real contexts. The common distinction in the critical thinking literature between abilities and dispositions is important.

Thus, we do not agree that, just because the principle *behind* a task in the heuristics and biases literature is easily taught, the problem itself is easy, and that it is not relevant to functioning in the modern world. Many of the axioms of rational choice (e.g., transitivity, the sure-thing principle, independence of irrelevant alternatives) are quite easy to apply and teach, but the decision theory literature is littered with dozens of studies showing that the ability to appropriately apply the (admittedly very simple) principles can be a difficult, though important, skill to acquire. As Shafir et al. (1993) note,

> it has been repeatedly observed that the axioms of rational choice which are often violated in non-transparent situations are generally satisfied when their application is transparent. . . . These results suggest that the axioms of rational choice act as compelling arguments, or reasons, for making a particular decision when their applicability has been detected, not as universal laws that constrain people's choice. (p. 34)

Difficulty in seeing the applicability of very simple choice axioms in real-life tasks has been amply demonstrated in the decision theory literature. This is why the rational thinking skills involved should in no way be characterized as simple or trivial (even *though* education in the skills can improve them). Because of the failure to apply some basic choice axioms, people choose less effective medical treatments; people fail to accurately assess risks in their environment; information is misused in legal proceedings; parents fail to vaccinate their children; billions of dollars are wasted on quack medical remedies; and costly financial misjudgments are made (e.g., Baron 1998; Bazerman et al. 2001; Belsky & Gilovich 1999; Dawes 2001; Kahneman & Tversky 2000; Margolis 1996; Russo & Schoemaker 2002).

As we indicated in the Authors' Response to other commentators who raised the issue of the importance of a process analysis of the tasks used in cognitive science (Stanovich & West 2000), we agree with **Newton & Roberts** that a fully explicated model at the algorithmic level of analysis is a crucial part of most cognitive science endeavors. We reiterate that we have worked at just such a level of analysis in another task domain of cognitive psychology for over two decades (Stanovich 2000; West & Stanovich 1978; 1986). We disagree, however, with the implication (in phrases like "there is little to gain by addressing the rationality question") in the Newton & Roberts commentary that it is a zero-sum game – that a focus at the intentional level of analysis precludes work at the algorithmic level. Our Authors' Response pointed to the venerable tradition in cognitive science (Anderson 1990; 1991; Levelt 1995; Marr 1982; Newell 1982; Oaksford & Chater 1995) which supports the notion that there can be synergistic interplay between levels. Indeed, one could view the interdisciplinary field of cognitive science as reflecting an attempt to integrate sciences focused on the algorithmic level of analysis (e.g., psychology) with sciences focused on the intentional level (e.g., anthropology, economics). Thus, although we wholeheartedly agree that individual difference analyses at the algorithmic level of analysis – of the type that Newton & Roberts are conducting in their ongoing research program – are of immense importance, we disagree with their denigration of individual difference analyses at the intentional level.

In fact, the generic dual-process models that we discuss in the target article represent the beginnings of an algorithmic understanding of the source of irrational responding. Other investigators have been refining the specifics of this generic process explanation (e.g., Evans 2002; Sloman 2002; Sloman & Rips 1998; Slovic et al. 2002) and some neurophysiological work on it has also appeared (Goel & Dolan 2003). Kahneman and Frederick (2002) describe dual-process explanations of many effects in the heuristics and biases literature that never would have become objects of attention except for the intentional-level focus on the goal-thwarting properties of the typical response on the task – that is, never would have been objects of attention *except* for the rationality debate.

We also disagree with how they frame their discussion of the compass point task. For reasons that are related to our earlier remarks on the **Brakel & Shevrin** commentary, we do not think the question of whether a certain (internal) strategy is rational or irrational is well formed. We do not believe the term rationality applies to subpersonal entities. Rationality concerns the actions of an entity in its environment that serve its goals. One could, of course, extrapolate the notion of environment to include the interior of the brain itself, and then talk of a submodule that chose strategies rationally or not. This move creates two problems. First, what are the goals of this subpersonal entity – what are its interests that its rationality is trying to serve? This is unclear in the case of a subpersonal entity. Second, such a

move regresses all the way down. We would need to talk of a neuron firing being either rational or irrational ("turtles all the way down!"). It was a version of this mistake that we invited by our use of the term evolutionary rationality. It was, of course, not means-ends rationality we had in mind for a gene, but the optimization of its fitness in a biological sense.

The task in question is not a good example of any of the points relevant to our target article. Unlike many tasks in the heuristics and biases literature, the normative response in the compass task is not in dispute. A correlational analysis of the type we applied to the former would reveal a fairly mundane result. Fewer errors on the compass task would be made by individuals utilizing the cancelling strategy and, as **Newton & Roberts** note, they would be subjects of higher ability. This would yield a correlation between ability and the normative response – a correlation utterly expected on tasks for which there is no dispute about the normative response. Asking whether the spatial strategy is rational or not is a category mistake. The spatial strategy is less efficient, and thus subpar performance on the task due to the use of the spatial strategy represents a computational limitation in our taxonomy, albeit of a somewhat different type than that discussed in our target article. However, on page 239 of our book-length treatment (Stanovich 1999) we discuss different types of computational limitations that would encompass instances more similar to that occurring in this example.

The research program sketched by **Newton & Roberts** seems indeed an extremely useful one, but likewise, our use of the rationality debate to discuss a mix of individual differences at the algorithmic and intentional level has born fruit in the study of belief bias (Stanovich & West 1997), schizophrenia (Oaksford & Sellen 2000), disjunctive reasoning (Toplak & Stanovich 2002), developmental trends (Klaczynski 2001; Kokis et al. 2002), conceptual change (Southerland & Sinatra 2003), and discontuities between intelligence and rational behavior (Sternberg 2002). Many other researchers (e.g., Elio 2002; Evans & Over 1996; Kuhberger 2002; Over 2002), like us, see the rationality debate as a progressive research program.

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[Note: The letter "r" appearing before authors' initials refers to response article references]

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