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Theory and Metatheory in the Study of Dual Processing: Reply to Comments

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Abstract
In this article, we respond to the four comments on our target article. Some of the commentators suggest that we have formulated our proposals in a way that renders our account of dual-process theory untestable and less interesting than the broad theory that has been critiqued in recent literature. Our response is that there is a confusion of levels. Falsifiable predictions occur not at the level of paradigm or metatheory—where this debate is taking place—but rather in the instantiation of such a broad framework in task level models. Our proposal that many dual-processing characteristics are only correlated features does not weaken the testability of task-level dual-processing accounts. We also respond to arguments that types of processing are not qualitatively distinct and discuss specific evidence disputed by the commentators. Finally, we welcome the constructive comments of one commentator who provides strong arguments for the reality of the dual-process distinction.

Keywords
dual processes, reasoning, decision making, individual differences, default-interventionist

Our target article (Evans & Stanovich, 2013, this issue) offered a broad response to various published critiques of dual-process and dual-system theories. There are many such theories of different origin making different assumptions, so we believe that there is no single or agreed generic theory of the kind that critics are apparently attacking. However, we do believe it proper that the current debate should be engaged at a level of metatheory. Broad frameworks, like dual-process theory, have a very important role to play in psychology, and there are numerous examples of research programs organized within and around such frameworks (e.g. cognitive dissonance theory, attribution theory, social learning theory, mental model theory, attachment theory, or operant learning theory). What we can expect at this level is general principles, coherence, plausibility, and the potential to generate more specific models and the experiments to test them. Such metatheories tend to survive as long as they continue to stimulate new research and accumulate enough supportive evidence. It must be understood, however, that such frameworks cannot be falsified by the failure of any specific instantiation or experimental finding. Only specific models tailored to the tasks can be refuted in that way.

The criticisms of dual-process and dual-system theories to which we were responding are aimed at a program that is only paradigmatic in a very loose sense and in no way constitutes a well-formed metatheory. There is no canonical dual-process theory analogous to Johnson-Laird's (2006) mental model theory or to Baddeley's (2007) working memory theory. The theory that critics are attacking is in fact a construction: an abstraction of salient features from many different dual-processing proposals, that we term the *received view*. Our purpose was to clarify our own concerns about the many problems with dual-processing accounts and to put forward principles of our own high level account of a dual-processing framework that we believe to be coherent, plausible, broadly supported by evidence, and capable of serving as a basis for more specific models that could be experimentally tested. To this end, we outlined principles based on a default-interventionist (DI) architecture and a set of key defining features distinguishing Type 1 from Type 2

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processing that we regard as a metatheory of dual processing and are happy to defend as such.

The confusion of levels is particularly evident in Keren's comments (2013, this issue) and in his previous critique (Keren & Schul, 2009). He cannot expect, as he apparently does, even a well formed metatheory to be highly testable at the level of meta-theory itself. On this basis, he could attack all high level theories in the history of psychology (such as those mentioned above) with equal effect. For example, he complains that in describing some features as "only" typical correlates, we have made the theory untestable. This complaint fails to acknowledge what we thought we had made clear in our target article. Because it bears on the concerns of other commentators, including Kruglanski (2013, this issue), we deal first with the issue of correlated features.

Why Some Features Are Imperfectly Correlated: The Example of Normative Responding

Like Keren, Kruglanski clearly believes that we have weakened the theory (by abandoning the assumption of strong feature alignment), reduced its interest, and made it less easy to test. In commenting on the fact that we proposed normative correctness as only a correlated feature of Type 2 processing, he states that ". . . this proposal is vague, and impossible to operationalize: It is unclear what size of correlation is implied and across what kinds of instances such correlations are to be computed" (p. 243). Like Keren, he is mixing levels with this comment. It is not unclear at all in particular applications; it simply cannot be specified as a necessary feature at the metatheoretical level. For example, Stanovich and West (2008) present a specific model showing why correlations between normative responding and cognitive ability are to be expected in belief bias syllogisms paradigms but not in between-subjects studies of anchoring effects. What Keren and Kruglanski both overlook is that there are good theoretical reasons for some features to be correlated, but imperfectly correlated. It is only at the level of the metatheory that this supposedly "vague" claim is made. Particular dual-processing accounts of particular tasks predict these features to be present or else specify conditions under which they will be absent.

Consider the example that Keren pinpoints. He notes that we cited evidence of decreased logical accuracy (as well as increased belief bias) under time pressure as evidence for the dual-processing account, and then comments that "[c]learly, a correlative attribute cannot be used as a marker to distinguish models" (p. 259). It is important to understand why this criticism is wrong. At the metatheoretical level, normative responding may only be a common correlate of Type 2 processing, but these authors (Evans & Curtis-Holmes, 2005) were testing a particular instantiation of the theory. In the context where participants are given a novel deductive reasoning task and specifically instructed to draw logically necessary conclusions, the dual-process account quite specifically attributes logical solutions to Type 2 processing. Not because Type 2 processing is necessarily normative, but because there is no Type 1 process that can cue the correct answer on this task in the absence of experiential learning. So the theory does quite specifically claim, in this context, that Type 2 reasoning is necessary for correct answers.

The framework however, does not claim that Type 1 processing is responsible for biases and that Type 2 processing is responsible for normatively correct responding regardless of context. This claim would clearly be wrong. With relevant experience, Type 1 processing can lead to correct answers on some tasks. There are also numerous reasons why Type 2 reasoning can lead to wrong answers, including shallow processing, lack of cognitive capacity, and absence of necessary mindware.[1] In fact, both of us have previously elaborated, in very great detail, the conditions under which Type 2 reasoning will fail to solve problems (Evans, 2007; Stanovich, 2011). Examples are provided in the Appendix. What seems to confuse our commentators is that quite often we cite as evidence for dual processing that normative solutions are more often associated with those of higher cognitive ability and more easily disrupted by concurrent working memory loads. We do so because we are commenting on a specific class of experiments where untrained participants are given novel and difficult problems that have no heuristic or experiential cues that can lead them to the right answer. Such situations are contrived more often than not by researchers in the fields of reasoning and judgment. In such contexts, Type 2 processing is necessary (but not sufficient) for normative success such that the latter can be used as an indication of its involvement. However, when the conditions for application of Type 2 reasoning are not met, the correlation will be low or absent (see the Appendix). As an example, when participants are instructed to reason pragmatically rather than deductively, high ability participants show no advantage in suppressing belief biases and achieving higher logical success (Evans, Handley, Neilens, Bacon, & Over, 2010). Similarly, cognitive ability is much more highly correlated with normative responding on abstract than deontic selection tasks (Stanovich, 1999) because the latter provide pragmatic cues to the solution that minimize demands on Type 2 processing. In both examples, dual-process theories were employed to predict both when the association with cognitive ability would be strong and when it would be weak.

Quantitative Versus Qualitative Differences

We explained in our target article that dual type theories posit qualitatively distinct Type 1 and 2 processes but that they are often confused with dual mode theories that propose cognitive styles of Type 2 thinking. The latter, but not the former, can be seen as lying on a continuum. Evidently, we have not convinced all our commentators on this point which comes up in several different places.

Kruglanski seems to think that because Type 1 and 2 processing are qualitatively distinct, the former must act in an all-or-none manner. He points to evidence that there are degrees of associative strength and that some intuitive answers come to mind more immediately than others. We quite agree that this is a feature of Type 1 processes. In fact, it is an aspect that a specific dual-processing theorist has built in as a central feature. A general issue for the DI approach is to specify when intervention on default Type 1 processing by more effortful Type 2 processing is likely to occur. Valerie Thompson and colleagues have run a series of studies (e.g. Thompson, Prowse Turner, & Pennycook, 2011) showing that fluency of an intuitive answer, operationalized as the time it takes to come to mind, results in varying degrees of *feelings of rightness* (FOR), which in turn mediate efforts at Type 2 intervention measured by rethinking time and answer changes when a second attempt is allowed. So the variable fluency of Type 1 intuitions, far from precluding the operation of separate Type 2 processes, is actually a known predictor of the presence of the latter.

Kruglanski also returns to the issue of rule-based processing. There clearly has been no meeting of minds with regard to our comments about Kruglanski and Gigerenzer's (2011) unimodel approach to intuition and reflection in decision making. Our point was that because Type 1 and 2 processes are both computable, each can be described using rules, but they can still be qualitatively distinct. He simply asserts that "if rule application is the mechanism involved in both . . . Type 1 and 2 processing, then the claim for distinct cognitive mechanisms for these two cases is a non sequitur" (p. 243). We confess to being baffled by this remark. For example, we can easily show that pattern recognition in the visual system is rule-based (in the sense that it can be computationally modeled), as is the decoding of grammatical structure in the language system. Are we therefore to conclude that these two very different cognitive activities, each with its own specialized region in the brain, arise from the same cognitive system?

Osman, Kruglanski, and Keren all note that the correlation between cognitive ability (or working memory capacity) and normative success shows a graded level of Type 2 activity, thus implying a continuum between Type 1 and 2 processes. We do not agree. The proposal that the two types of processing are qualitatively distinct is an intraindividual claim. A given individual may use a mixture of the two kinds of processing, in effect, to control responding, but that mixture differs across individuals. The extent to which an individual (a) relies on Type 1 processing, (b) invokes Type 2 processing that only confirms the intuition, or (c) intervenes with Type 2 processing that leads to a different answer are all a matter of degree when considered across individuals. As much individual differences research has now shown (see Stanovich, 2011), individuals differ in both their disposition to intervene with more careful reasoning and their cognitive ability to find a better answer when they do so. This will inevitably lead to correlations across individuals in the extent of known cognitive biases and/or normatively correct answers. Also, although there may be other interpretations of the individual differences findings, we use these in conjunction with findings from experimental and neuroscience methods to build our broader case.

In contrast with the other commentators, Thompson (2013, this issue) makes a strong case for the qualitative distinction between Type 1 and 2 processing: the former being autonomous with mandatory outputs, and the latter representing a more flexible, controlled form of cognition. Thompson drives straight to the heart of what is theoretically, and indeed philosophically, interesting about dual-process theory. For example, she notes:

> It does not really matter if, as Keren and Schul (2009) argued, some processes that are usually autonomous can, in some circumstances, be subject to top-down control. It does matter that, for a given person confronted with a given situation, a process is initiated without any intention on their part. (p. 253)

Thompson sees the crucial point that the representation triggered by an autonomous process becomes an obligatory part of the initial problem representation, and she rightly urges us to remember our basic cognitive principles—that this initial representation will determine much of subsequent thought.

Thompson clearly agrees with us that the autonomous nature of Type 1 processes make them qualitatively different from Type 2 processes:

> [T]he fact that some processes can be executed without much drain on that capacity, and others may not be executed for want of it, has profound implications for the outcome of our decision making. (p. 254)

> [T]he assumption of the autonomy of Type 1 processes . . . [has] important implications for further theory development. (p. 256)

We would add that we see a firm biological and evolutionary basis for the development of the two kinds of processing (Chein & Schneider, 2012; Schneider & Chein, 2003), which is also present in higher animals—albeit with a much more rudimentary form of controlled processing (see Toates, 2006). It is efficient and adaptive to have processes that operate in a fixed manner and can be instantiated as low effort and special purpose devices by a combination of evolution and learning. However, the ability to solve novel problems (which in humans developed a very special form of intelligence) and those in situations with multiple demands is equally important (Duncan, 2010).

Questioning the Evidence

Osman seems happy that the version of dual-process theory proposed in our article is a testable framework, but she points to specific results in the literature that she feels may falsify this account. Kruglanksi and Keren both question whether our framework is testable at all, but they dispute some of the evidence anyway. Thompson is broadly supportive but raises the issue that Type 2 reasoning does not necessarily override or change Type 1 intuitions when it is applied. We agree with her point, which is completely consistent with our own DI accounts of dual processing (see Appendix).

Neuroscience

The commentators raise doubts about evidence from neuroscience by referring to several critiques and the difficulties that lie in identifying distinct neural systems with separable cognitive systems. We understand these concerns and do not regard neuroscience as the ultimate arbiter in this debate. However, what these comments overlook is the strong *a priori* nature of the dual-process predictions in several of the studies that we cited in our target article. When the theory predicts that the two processes are in conflict, a region of the brain associated with conflict detection becomes activated. When the theory predicts that a correct answer is achieved on such a problem by the intervention of reasoning, a region of the frontal cortex associated with executive control is activated, which does not occur when the participant stays with belief and so on. Similar patterns characterize the literature on intertemporal choice (Essex, Clinton, Wonderley, & Zald, 2012; McClure, Laibson, Loewenstein, & Cohen, 2004; Wittmann, Leland, & Paulus, 2007) and moral dilemmas (Greene, Nystrom, Engell, Darley, & Cohen, 2004). Such studies are persuasive when considered in combination with those using experimental and psychometric methods. We believe that we built a very strong empirical case for dual processing in our target article and will not repeat it here.

Individual differences

Keren and Kruglanski provide no rebuttal at all to the voluminous individual difference results that support dual-process theory (Stanovich, 2011). Keren mentions only one researcher (Kareev) and cites two papers concerning the simulation of an experimental manipulation—not empirical relationships with measured individual difference variables like intelligence or need for cognition. This citation of a single researcher discussing a single paradigm contrasts markedly with Table 8 of Stanovich and West (2008), which lists the relations between cognitive ability and 28 different cognitive biases and links to an accompanying model that explains when correlations are expected and when they are not expected. Kruglanski, on the other hand, wrongly implies that the observed correlations between normative responding and cognitive ability are due to differential learning. However, from our earliest studies, when such correlations were obtained, they have always survived statistical control for learning experiences (e.g., number of logic, statistics, and critical thinking courses). Contrary to what Keren implies, Stanovich and West have always acknowledged the potential situational dependency of relationships between normative responding and cognitive ability and have reported cases of negative correlation from the beginning of our research program (see Stanovich, 1999).

Experiments

In commenting on experimental evidence, Osman focuses not on any of the studies that the two of us have published but rather on those of another researcher: Wim de Neys. We consider a particular example that Osman cites (De Neys, 2006). She claims that the deontic Wason selection task (in which familiar content cues correct choices) should be solved by Type 1 processes whereas the abstract task requires Type 2 processing. Notwithstanding several findings in De Neys' study that supported dual-process predictions, she focuses on one that seems problematic: concurrent working memory load impaired performance on the deontic as well as the abstract selection task. It is true that a dual-process account does predict that difficult Type 2 reasoning is required for the abstract task, and correspondingly, it has been shown that those able to solve it have high general intelligence (Newstead, Handley, Harley, Wright, & Farelly, 2004; Stanovich & West, 1998). However, the same studies also show some degree of correlation of intelligence with performance on the deontic task (depending on the spread of ability in the sample): a finding that corresponds to the one that Osman highlights in De Neys' study. Finally, the treatment of the De Neys (2006) evidence on the deontic selection task is

somewhat selective in the Osman commentary. Two different types of cognitive load were used by De Neys (2006), and the one used in Experiment 4 resulted in no significant decrease in deontic performance, just as predicted by dual-process theory. The other type of cognitive load did display a significant effect on deontic performance in Experiment 2, but even here, under load, correct responding dominated the matching response by 73.8% to 9.5%, a result suggesting the incredible robustness of the deontic structure in supporting performance (a robustness supporting the hypothesis of qualitative distinction between indicative and deontic responding, see Stanovich & West, 1998).

Regarding Osman's comments about the time taken on heuristics and biases tasks, we must make it clear that there is no laboratory word problem that could be solved by only the application of Type 1 processes. Participants must respond to experimental instructions and justify their choices, thus requiring Type 2 processes. In the case of the abstract selection task, we know that participants focus their attention on matching cards but also that they spend some time and effort in reasoning to try to justify these choices. This is indicated by studies of both verbal protocols and card inspection times (Ball, Lucas, Miles, & Gale, 2003; Evans, 1996; Evans & Wason, 1976; Lucas & Ball, 2005). This Type 2 reasoning mostly leads to justification of the intuitive response but in some cases can lead to a change of decision (Evans & Ball, 2010). On the deontic version, there is also evidence that people focus quickly on the (typically correct) cards they are likely to end up choosing (Evans, 1996). So we agree it is a little surprising that some cognitive capacity is still required to get this problem right. A possible explanation is that even when intuitive responses are quickly prompted, we know that participants take some time attempting to justify them. Should they fail to do so, the answer may be withheld. This proposal is supported by a study in which imposition of a fast time limit led to withholding of conditional inferences of all kinds, both valid and invalid (Evans, Handley, & Bacon, 2009). Osman also fails to draw attention to a finding from De Neys's (2006) Experiment 1 that is strongly supportive of the dual-process account of indicative/deontic differences—that correct responding on the deontic version is substantially faster than correct responding on the indicative version.

Here again we see the issues of levels. When we get to theories of specific tasks, like the Wason selection task, we may find that although a number of initial predictions succeed, some may fail. This mix is entirely normal with any research program. We do not then reject the entire dual-process framework in one fell Popperian swoop,[2] but rather seek to modify our task level account. This is how psychology works in general, particularly in the case of broad metatheories. If such metatheories were dismissed on the finding of any empirical difficulty in task level accounts, then programs like Johnson-Laird's mental models or Baddeley's working memory would have been abandoned years ago. Of course, those with strong opinions against such a framework will focus on difficult findings to the exclusion of many supportive ones. That is why debate is an important aspect of our science, and why journals like this one serve a very useful function.

DI and Interaction of Type 1 and 2 Processes

Osman and Thompson raises some specific questions about how Type 1 and 2 processes interact in decision making, and Kruglanski asks whether our DI account is really all that different from his own unimodel. In response, we have presented some detail of our recent DI models in Figures 1 and 2 in the Appendix. Note that the specifics of the DI model shown in Figure 2 are relevant to several points in the commentaries. The model is entirely compatible with Thompson's observation that intervention by Type 2 thinking does not necessarily override a Type 1 response in addition, it includes her FOR as a key factor in determining effort to check out the initial intuition with deeper processing. Clearly, the model predicts that when FOR is low, participants are less likely to be satisfied with their intuition, more likely to check it out with further reasoning, and more likely to change the initial answer just as her research has shown (Thompson et al., 2011).

With regard to Kruglanski's unimodel, we agree that it may take some effort to separate it from the DI model in terms of predictions that are generated about performance on particular tasks. But the DI model is quite clearly conceptually different in proposing that a qualitatively different kind of processing underlies A2 than A1 as opposed to simply searching for a set of rules on a continuum of accessibility and processing effort. Our case for this point remains with the kinds of evidence discussed in our target article based on working memory loads, selective individual differences, neural imaging studies and so forth. We have little optimism that the differences between the unimodel and the DI account can be settled simply on the grounds of performance differences, as both theories have evidently been adapted to what is known to affect performance.

Conclusions

The dual-process notion is so basic to the cognitive and social psychology of the last 30 years that the vehemence of the opposition to the concept by some authors

is puzzling to us. For example, Kahneman (2011) has organized the discussion of his epic 40-year research program around the insight that fast processing requiring little resources must combine with another kind of processing that is slow, effortful, and resource intensive. It is adaptive and efficient that our minds be organized in this way for the kinds of reasons that Thompson gives in her commentary and which we and many other authors have set out in previous publications. We note also that dual-process interpretations of cognitive biases are also displaying their fruitfulness by helping to structure such applied/clinical fields as the study of problem gambling, ADHD, addiction, and psychopathology (Evans & Coventry, 2006; Toplak, Sorge, Benoit, West, & Stanovich, 2010; Wiers, Gladwin, Hofmann, Salemink, & Ridderinkof, 2013).

The key point in this short reply is that the word *theory* is applied in a most ambiguous way in psychology. It can mean anything from a broad set of proposals of a particular kind, more accurately termed a *metatheory*, to a specific model of task level behavior. Metatheories are not directly testable, but they are interesting and important as the history of psychology shows beyond doubt. However, when critics refer to the dual-process theory at the program level, they somehow expect it to have the directly testable properties of a task-level account. In our target article, we presented what we regarded as well-formed metatheory of dual processing, but we cannot take responsibility for some generic "received" version, proposed by not a single author, that the critics would like to attack. In this response, we have shown why task-level dual-processing accounts do not suffer from the vagueness and lack of testability that is alleged of the broader program. We have also emphasized the reasons why a qualitative distinction between types of processing makes theoretical sense and is supported by much evidence. Finally, we have provided some more specifics about the way in which the DI system for reasoning and decision making actually works.

Appendix

Detailed examples of default-interventionist (DI) models

To illustrate the DI approach in more detail, we describe two related examples from our own recent publications. First, Stanovich and West (2008) presented a model showing how individual differences in performance on reasoning and decision tasks arise, reproduced here as Figure 1. Note that this is a descriptive model, albeit one that makes direct reference to aspects of cognitive processing.

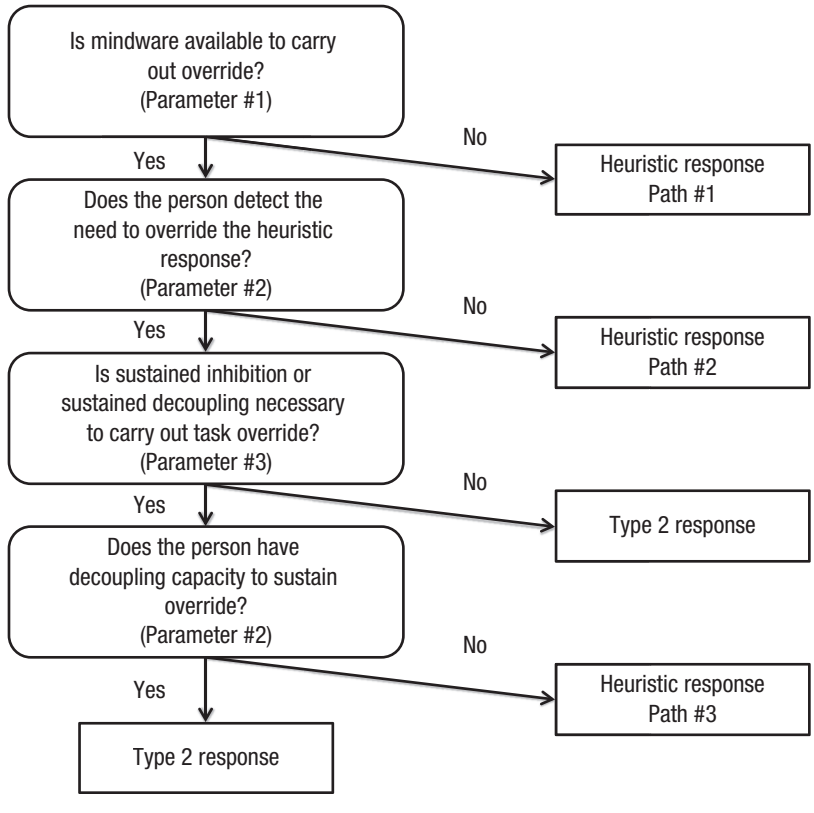

Fig. 1. Individual differences model reproduced from Stanovich and West (2008).

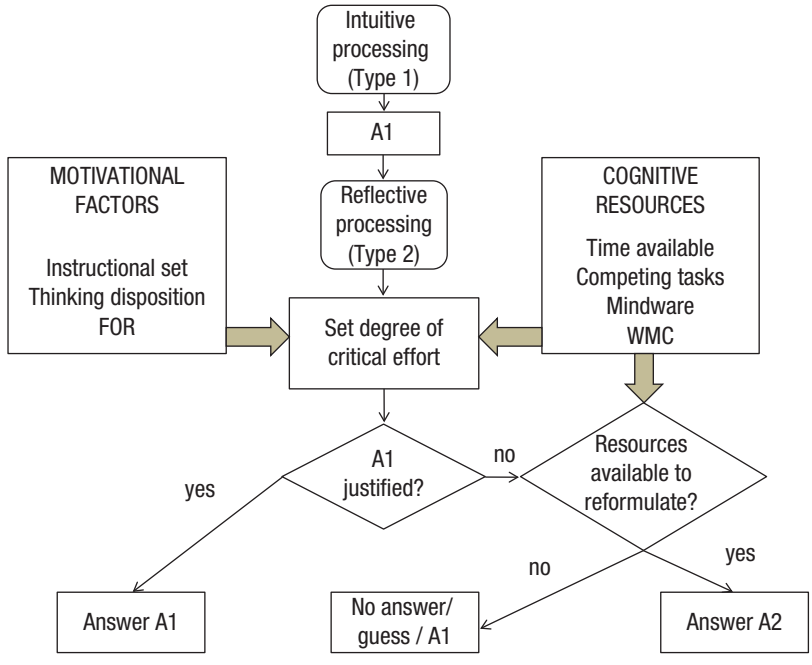

Fig. 2. Default-interventionist model reproduced from Evans (2011). Note that A1 refers to an initial response generated intuitively, and A2 refers to an alternative answer that may be substituted as a result of reflective processing.

The question addressed in the first stage of the model is whether the mindware is available for a given task to carry out an override (whether the procedures and declarative knowledge are available to substitute an analytic response for a heuristic one). If the relevant mindware is not available, then the person must respond heuristically. If the relevant mindware is in fact available, then the next question that becomes operative is whether or not the person detects the need to override the heuristic response. Even if the relevant mindware is present, if the participant does not detect any reason to override the heuristic response, then it will be emitted (this is Path #2 to a heuristic response, as labeled in Fig. 1). Many heuristics and biases tasks lead people down this path. People do not detect the need to override the response that comes naturally (Kahneman, 2011) even though, in retrospect, they would endorse the norm that the heuristic response violated.

The next branching point in Figure 1 concerns the task rather than the participant. If the relevant mindware is present, and if the need for override has been noted, the question then becomes whether or not the task requires sustained inhibition (cognitive decoupling) in order to carry out the override of the heuristic response. If not (or if the capacity required is low—this of course may not be an all or nothing issue), then the Type 2 response will be substituted for the heuristic response. In contrast, if the task requires sustained decoupling in order to carry out override, then we must ask whether the participant has the cognitive capacity that will be necessary. If so, then the Type 2 response will be given. If not, then the heuristic response will be given (Path #3 to the heuristic response in Fig. 1), despite the availability of the relevant mindware and the recognition of the need to use it. In order for cognitive ability to associate with a bias, there must be differences correlated with cognitive ability at some of the choice points in the framework—that is, in some of the person parameters that branch toward or away from heuristic paths. The remainder of the Stanovich and West (2008) model concerns this issue but is beyond our scope here.

Evans (2011) recently presented a cognitive model (reproduced in Figure 2) of how DI may work. The model conforms with Kahneman's assumption (2011; Kahneman & Frederick, 2002) that there is always some degree of Type 2 processing, even if it is minimal. This model is also strongly influenced by the writings of Thompson (e.g. Thompson, Prowse Turner, & Pennycook, 2011), who distinguished between initial answers (A1 in the model) that are given by fast intuitive processes and subsequent answers that are produced after a period of reflection and that may differ from A1 (these are labeled A2 in the diagram). A critical feature of this model is the degree of effort that is expended in checking A1 before

any attempt to rethink the answer is provided by Type 2 processing. The model shows a number of motivational and cognitive factors that may determine this. The next critical point is whether the participant accepts the default A1 by some shallow or deeper process of justification or whether they see the need to rethink it. Should rethinking occur, it is again constrained by cognitive resources and may eventually lead to a new answer (A2) or reversion to the original (A1).

The processing model in Figure 2 maps very directly on to the individual differences model in Figure 1. What Stanovich calls detection of the need for override corresponds to the decision as to whether A1 is justified in Evans's model and is affected by the same cognitive and motivational constraints. Similarly, when rethinking does occur in Evans's model, it is constrained by cognitive capacity and may still lead to an answer based on Type 1 (A1) rather than Type 2 (A2) processing. Similarly, the Stanovich model shows that according to the capacity of the individual attempts to intervene may lead to a heuristic (Type 1) or analytic (Type 2) response. So we can show that even at this more specific task level theorizing, our theories are again fully compatible.

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Notes

1. Mindware is knowledge of the rules or procedures required to solve the problem—normally provided by education.
2. In practice, psychology seems to operate in accord with a Bayesian philosophy of science (Howson & Urbach, 2006) in which belief in theories depends upon the accumulation of evidence in favor or against them. But this is true in other sciences also, like biology, in which empirical falsification most often leads to questioning of the experiments or to revision rather than abandonment of the theories (Fugelsang, Stein, Green, & Dunbar, 2004).

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