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Author's Response

Clarifying Problems in Behavioural Control: Interface, Lateness and Consciousness

PHILIP J. CORR*
University of East Anglia, UK

Abstract

The target paper highlights a number of unresolved issues that, I believe, continue to impede the construction of a viable model of behavioural control in personality psychology; namely, (a) the relationship between controlled and automatic processing (the 'interface' problem') and (b) the time it takes for controlled processes, including consciousness, to be generated (the 'lateness' problem). The diversity of views expressed in the commentatories indicates that these are, indeed, real and unresolved problems. This response is structured around the following key questions. (1) How long-term goal planning interfaces with the automatic machinery of behaviour? (2) The extent of the impact of the 'lateness' of controlled (including conscious) processes for building models of behavioural control? (3) How best to characterise the personality traits associated with the FFFS, BIS and BAS? (4) How does the BIS control mismatch detection, the generation of error signals, and response inhibition and switching? (5) Is consciousness really a necessary explanatory construct in models of behavioural control? (6) Might neural 'crosstalk' of encapsulated action-goal response systems point to the functional significance of consciousness? (7) What are the implications of issues raised in the target paper for lexical and social-cognitive approaches to personality? I conclude by re-iterating the importance of the problems of 'lateness' and 'interface' for the construction of a viable model of behavioural control sufficient for the fostering of theoretical integration within personality psychology as well as affording the building of conceptual bridges with general psychology. Copyright © 2010 John Wiley & Sons, Ltd.

INTRODUCTION

Critiques of my target paper attest to the intellectual vitality and rigour, as well as the dispositional good nature, of the commentators. They highlight the principal strengths and weaknesses of the proposed model of behavioural control and, in consequence, have helped to clarify its purpose, technical specifications and implications for personality psychology. However, it is evident from residual disagreement among commentators that there remain

*Correspondence to: Philip J. Corr, University of East Anglia, UK. E-mail: p.corr@uea.ac.uk

critical issues still to be adequately acknowledged and addressed. In the spirit of progressive science, the sign-posting of these issues is a valuable step in identifying areas in need of further sustained attention.

This preamble would be incomplete without the expression of gratitude to the editor, Marco Perugini, and the two anonymous reviewers, for their most detailed engagement with, and critique of, the draft proposal for this target paper—they must also be thanked for their forbearance with some of its more recondite features. The final version of the paper owes much to their patient guidance and advice. I especially appreciated the fact that they were tolerate of my preferred theoretical perspective and appreciated the potential of the challenges it poses for personality psychology—whether their kindly tolerance was justified must be left, in the first instance, to readers of the *European Journal of Personality* but, ultimately, to the outcome of future scientific research. Irrespective of the final fate of the proposed model of behavioural control, if the target paper stimulates new and fruitful lines of thinking and experiment in personality psychology then its principal aim would have been met.

The possibility that the proposed model is targeting relevant issues in a potentially useful manner receives good support. **DeYoung** notes that the exploration of the interaction of controlled and automatic processes may open up new avenues to investigate additional personality traits; and the proposed model is useful for developing theories of the biological systems underlying traits, even those not directly related to the BIS, BAS and FFFS on which the model is based. **Revelle, Wilt and Condon** endorse the view that Reinforcement Sensitivity Theory (RST) is a powerful beginning of a general theory of behavioural control. **Poropat** echoes this sentiment by stating that theoretical explanations of personality rarely encompass so broad a range as the perspective afforded by RST, which attempts to integrate brain physiology, cognitive psychology and consciousness with personality structure and processes. **Morsella and Hubbard** favour the focus in the target paper on the inter-goal dynamics of primary systems that strive for parsimony and avoid homuncular-like 'supervisory' systems. **Hoffman** lauds the paper for presenting a framework that serves to integrate consciousness research, dual-system thinking, and self-regulation with personality psychology; and, in doing so, it 'raises many fascinating issues' and 'bridges cognitive (i.e. processing-oriented) and personality (i.e. construct-oriented) approaches in a number of innovative and unique ways'. This commentator goes on to say that the target paper deserves 'credit for carrying this idea of control as a re-programming of automatic processing parameters deeply into the field of personality'. He rather nicely captures the main theme with his statement, 'It is a valuable attempt to bridge areas of psychology that are typically viewed in isolation from each other by relating the distinction between automatic and controlled processing and the related issues of consciousness and volition to concepts of personality', noting also that 'the relevance of these models for personality psychology has mostly been overlooked'. **Matthews**, too, finds much to agree with, and believes that the target paper 'provides an invaluable contribution to personality theory through its discrimination of multiple levels of control that may support stable individual differences in behavior and subjective experience'; and, whether or not one agrees with its conclusions, it 'pinpointed key issues of debate whose resolution will shape future personality theory'. I appreciate these observations on what the target paper aims to achieve.

Before having much of a chance to bask in the warm glow of these comments, I was made aware that they were merely preludes to the exercise of incisive critique. For example, although the ambitious attempt ('brave' to some; **Matthews, Wilt & Condon**) to

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integrate broader ideas associated with reflective processing, conscious awareness and behavioural control, based on a neurocognitive research is noted by many commentators, **Poropat** believes that, if anything, the arguments were not bold enough, especially in relation to the limitations on self-awareness inherent in the model and to the consideration of the model's implications for a broader understanding of personality. Indeed, he contends that some of the more intriguing possibilities of the model are barely hinted at, such as its implications for the value of self-reports in personality research, or as a mechanism that may be able to integrate social influence with psychophysiology. However, for other commentators, the target paper goes too far along the *terra incognita* of some areas (e.g. consciousness; **Matthews**). As theoretical disagreement is the fuel of scientific debate and progress, I very much welcome the opportunity to reply to these different points of view.

Now, before addressing the specific points raised in the commentaries, it may be useful to stand back to view the larger landscape of personality psychology, how it fits with general psychology, and the broader implications of the issues considered in the target paper.

THE NEED FOR INTEGRATION MODELS IN PERSONALITY PSYCHOLOGY

The principal impetus for the target paper was a dissatisfaction with the prevailing orthodoxy that, in some way, 'personality psychology' stands apart from mainstream experimental and cognitive psychology; and the model of behavioural control was developed to provide theoretical guide-ropes for the construction of more specific (and viable) models in personality psychology in order to foster the integration of theoretical perspectives *within* personality psychology as well as offering a more fluent *lingua franca* between personality psychology and general psychology. The need for the unification of personality psychology with general psychology is hardly a new topic (Cronbach, 1957; Eysenck, 1997), but it remains unmet.

Although it would be tempting to assign responsibility for this lack of progress on the failure of experimental/cognitive psychology to acknowledge the relevance of individual differences research, this opinion would not be entirely consistent with reality, because, as noted by Revelle and Oehlberg (2008, p. 1390),

> The unfortunate conclusion from this brief review of publication practices [in personality research] is that the use of experimental techniques is underemployed in current research. This suggests that the desired unification of the correlational/observational with the experimental disciplines called for by Cronbach and Eysenck has not yet occurred.

In addition to the relative underemployment of experimental methodologies, theoretical divisions are also widespread, as noted by Corr and Matthews (2009, p. xx),

> . . .the study of personality has often been contentious and riven by fundamental disputes among researchers. A persistent issue is the nature of personality itself: what issues are central to investigating personality, and which properly belong to other sub-disciplines of psychology? At times, it has seemed as though different schools of 'personality' research have been addressing entirely different topics. Until quite recently, there was little communication between biologically and socially oriented researchers, for example. Debates in the field tended to devolve into rigid dichotomies, forcing researchers into one camp or another.

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The absence of adequate integration models may be one important reason for this state of affairs. If the target paper contributes to this debate, perhaps by suggesting conceptual bridges between, largely isolated, islands of knowledge, then some good may have been done. However, it was admitted that the proposed model of behavioural control is little more than a prolegomenon to the development of a fully-adequate model—clarifying key foundational issues is a necessary start.

STRUCTURE OF RESPONSE

Reflecting the issues raised in the commentatories, this Response is structured thus: (1) controlled-reflective processing in relation to long-term goals, and the implications of the 'interface' problem, *viz*., how controlled-reflective processes engage with the neural machinery of immediate behavioural control (**Hoffman; Revelle, Wilt & Condon**); (2) the extent and implications of the 'lateness' of controlled processes *viz*., that phenomenally such processing seems to 'control' behaviour, but experimentally it can be shown to postdate the behaviour it *re*presents (**Hoffman; Matthews; Poropat; Revelle, Wilt & Condon**); (3) the characterisation of personality traits associated with the FFFS, BIS and BAS (**DeYoung; Matthews**); (4) the role of the BIS in mismatch detection, the generation of error signals, and response inhibition and switching (**Hoffman; Morsella & Hubbard; Pickering**); (5) the causal status of consciousness in cognitive models and personality psychology (**Matthews; Morsella & Hubbard; Poropat; Revelle, Wilt & Condon**); (6) the neural 'crosstalk' between encapsulated systems that have opposing action-goal inclinations as a plausible functional model of consciousness (**Morsella & Hubbard**); and (7) the implications of the model for lexical theories of personality and social-cognitive influences on personality development (**DeYoung; Poropat**).

'INTERFACE': REFLECTIVE PROCESSING AND LONG-TERM GOALS

Revelle, Wilt and Condon note that three levels of information processing (reactive, routine and reflective) provide a useful framework for the conceptualisation of the different levels of behavioural control, especially when personality is viewed as the 'coherent patterning over time and space of affect, behavioural, cognition and desires'. Almost inevitably, this definition of personality demands a multi-level approach and therefore the potential problem of 'interface' raises its head. It is noteworthy that, in contrast to **Matthews**, these commentators have few qualms in liberally invoking conscious awareness in the causal chain of events, a point I return to below.

I wholeheartedly agree with **Revelle, Wilt and Condon** that there is considerable room for expansion of the proposed model to capture higher-level reflective processes, including long-term goal setting. They, in my view, rightly claim that most of the analysis contained in the target paper concentrates on the reactive and routine levels—indeed, one might go further and claim that the 'business end' of model is, almost, entirely at this non-reflective level. This focus might be seen as a serious limitation of the model. As I will attempt to show, embracing higher-level controlled processes, and consciousness in particular, comes with its own theoretical price tag. Specifically, greater focus on controlled processes has to face the challenges of the issues of 'interface' and 'lateness', the implications of which are rather glossed over in the commentary by **Revelle, Wilt and Condon**. As they imply, it would, indeed, be downright foolish to hold that long-term goals do not have any causal efficacy (e.g. this target paper has been in preparation for several years, and the cognitive

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and motoric processes now being used to write these words can be traced to these causal antecedents); however, consideration of the proposed model highlights several problems that must be confronted when assuming a temporally spaced, reflective, perspective.

The proposed model already contains a strong temporal perspective—as **Hoffman** states, ‘I strongly appreciated the dynamical perspective the author advocates, as I am myself convinced that issues such as control and self-regulation can be fully understood only if the interplay of automatic and controlled processes are modelled over time’. However, this dynamical nature was not over a long enough time span for **Revelle, Wilt and Condon**. In terms of the specification of the model, the difference between hundreds of milliseconds and minutes/days/weeks/years is, largely, irrelevant, at least in terms of the ‘lateness’ problem: All high-level controlled processes are ‘late’. What is relevant is the processing efficiency of reflective processes; and, crucially, the extent to which this reflection engages automatic reactive and routine levels of processing, that is the ‘interface’ problem.

To illustrate some of the issues involved, consider the example of the graduate student, John, who gets up in the morning in reaction to an alarm clock, brushes his teeth in a routine mode, washes, and has breakfast while reflecting on the day ahead. During these activities, John might be reflecting on his academic work, perhaps thinking of the different ways he could design a new scientific study worthy of consideration by the editor of *Nature*. He may also be reflecting upon advice he received from his doctoral supervisor regarding how best to present at scientific conferences. **Revelle, Wilt and Condon** are surely correct to say that, to some extent and somehow, these mental activities involve non-reactive and non-routine reflective processes (although, there would be a considerable degree of reactive and routine processing also going on). They assume that these controlled-reflective processes serve a steering function for future behaviour. The proposed model makes this assumption too, at least under optimal operating conditions. However, importantly, the model assumes that the *interface* of controlled-reflective and automatic processes can, and often does, not function effectively. This failure can be the result of a number of factors: (a) inadequate controlled processing; (b) powerful pre-potent forces that are difficult to inhibit; (c) controlled processes being inundated with error signals from an overactive BIS; or (d) the ‘transmission’ (i.e. interface) from normally functioning controlled and automatic processing being disengaged. Such problems are seen widely across the clinical, health and personality landscape (examples were provided in the target paper). Returning to the example of John, as many academics know all too well, mental rehearsal for a successful conference presentation does not always lead to flawless performance on the day (pre-potent excitement, nerves, distraction, etc. all serve to ensure that actual performance departs from planned performance). For most conference speakers, these are minor departures from the rehearsed behavioural repertoire, but it should be remembered how many people never agree to give a presentation in the first place because they cannot control their pre-potent ‘nerves’?

Long-term plans may well be likened to the manoeuvring of an oil tanker which can take, at least, 1 hour (**Revelle, Wilt & Condon**). This is an excellent example, although perhaps not for the reasons it was chosen by these commentators. The captain on the bridge of an oil tanker can issue as many instructions as (s)he wishes, but if nobody is listening in the engine room, or if the steering mechanism is faulty, then controlled-reflective processes of the captain will be ineffectual. Usually oil tankers do steer correctly (but sometimes they do not); and usually controlled and automatic processes work in harmony (but sometimes they do not). It is the latter cases that prove the rule of the proposed model (in the same way that clinical data help to dissect neuropsychological systems; e.g. the ventral and dorsal visual streams of processing).

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In the real world, oil tankers run aground and aeroplanes crash; students do not always manage to put into practice their well-rehearsed (or, at least, well-intentioned plans); concerned parents do not always save enough for their children's education, despite their 'best will in the world'; and, couples do not always remain married despite their sworn intention to remain together 'till death do us apart'. But often they do. All-too-frequently, immediate pre-potent factors override Panglossian reflective long-term goals. Such failures may be thought to be little more than a temporary blip in an otherwise ordinary 'steering' by long-term goals, but this 'interface' failure may well have long-term consequences, as seen in the well-known study of Mischel, Shoda and Rodriguez (1989) which showed that a failure to delay gratification at the age of 4 years of age predicted, more than 10 years later, poorer outcomes in terms of cognitive and social competence, scholastic achievement, and problems with coping with frustration and stress.

The proposed model predicts that the longer the term of the plan the less likely it is to be fulfilled—time allows for too many competing pre-potent responses to intervene. According to the model, the trick is to transform, as soon as possible, long-term goals into a series of immediate automatically-processed routines; but this can happen only with an effective transmission between controlled and automatic processes (Carver, Johnson, & Joormann, 2008). The fact that the controlled-automatic interface appears seamless is illusory, just as visual perception seems unitary yet we know from clinical cases (e.g. blindsight) that there are two quite distinct visual processing systems (Goodale & Milner, 2006).

I do not want to remain being 'particularly silent with respect to goals' over the long time frame—although I would note that, for good reasons, the proposed model is not especially interested in 'long-term goals', but instead in how controlled processing (which includes goal planning) interfaces with automatic processes *at any moment in time*. Some clarification would be helpful though. According to the perspective afforded by the proposed model, long-term goals collapse to a series of short-term ones (here-and-now goals; e.g. I *still* want to save for my children's education). Indeed, I would go further to state that unless these *here-and-now* goals get translated and transmitted to automatic processes (although they may still have presentation in consciously-accessible controlled processes) then they are quickly forgotten. Long-terms goals may be seen as goals that have a long temporal duration and, at the moment of reflection, are routinely processed. In any case, for any goal to influence actual behaviour, the underlying controlled processing must engage the automatic behavioural machinery. We see this phenomenon in people who, successfully, 'decide' to stop smoking: They do not have to think (reflective) about it; they just do not smoke (reactive/routine behaviour). As many students know all too well, *thinking* about doing a piece of work is one of the best ways of not starting it. Professional writers do not typically sit around waiting for their muse; rather they are disciplined and go to their desk at a pre-determined time, if they feel like it or not—they know that the activation of reactive and routine processes will pump-prime their controlled-reflective processes (according to the model, perhaps by the generation of mismatch signals).

'LATENESS': HOW MUCH OF A PROBLEM?

Hoffman agrees with the model that 'the notion of controlled processing influencing future automatic processing fits nicely with cognitive research showing how the formation of conscious intentions, like water flowing down the cascades of a fountain, adjust parameters in a whole range of automatic sub-systems involved in, for instance, attention allocation,

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stimulus encoding, response selection and response execution'. However, one of the aspects of the model that 'haunted' his consciousness (his pun) is the conclusion that the lateness of conscious awareness precludes any immediate control of automatic processing. He asks the germane question: 'Given Libet is right, does the time it takes for controlled processing (and conscious control in particular) to 'build up' really preclude the on-line control of behavior?' On the one hand, he agrees that conscious control, 'almost by definition' has to follow the to-be-controlled response tendency that has been automatically activated, yet, on the other hand, he proposes that, in real life, 'translating pre-potent action tendencies into actual behavior may often afford considerable time and opportunity as well (e.g. going to the refrigerator, unwrapping the chocolate, putting it into one's mouth, chewing and swallowing it), unless we confine ourselves on reflex-like behaviors such as removing a hand from a hot stove'.

Hoffman's statement is very much along the lines of the oil tanker example of **Revelle, Wilt and Condon**, although over a shorter period of time. I agree with him, but with an important caveat, namely that it is true only when there is an adequate transmission between controlled and automatic processes: This is the 'interface' problem. **Hoffman** seems to recognise this problem when he says, 'This is not to deny that people may often lose the 'horse race' between pre-potent action tendencies and controlled processing'. **Hoffman** and I probably disagree on the extent to which there is a 'lateness' problem and/or the extent to which the problem seeps into everyday behaviour.

Matthews too takes up the issue of 'lateness', in making the plausible claim that this problem 'may be no more than a recognition that processes operate over different time scales, and with a granularity that is finer than "fast" versus "slow"'. He goes on to quote evidence from, for example, priming studies, which suggest a pivotal role for controlled processing in modifying lower-level processing without any significant degree of lateness. I agree that the example of rapid motor control, such as swinging a table tennis bat or steering a vehicle, are under rapidly-operating feedback control—and such control need not be conscious. I also agree that if we view behaviour as reflecting a continuous perception-action cycle, top-down processes, including controlled attention, expectancy and preparedness, play a fundamental role in directing action. However, given the statement that the 'lateness' problem may relate to slower and deliberate controlled processing, I remain convinced that there is a problem. As I stated in the target paper,

> Cognition need not involve high-level controlled processes or conscious awareness, but then these forms of 'cognition' (e.g. priming) do not differ in fundamental respects from automatic-reflexive behaviour (they may still be relatively complex, e.g. language comprehension)—in this way, pre/non-conscious cognition does not pose a problem for the model (but such cognition would need to be stripped of any 'late' components).

In terms of the three-levels model of **Revelle, Wilt and Condon**, these priming effects operate at the reflective/routine levels of control and are, therefore, already incorporated in the model. Perhaps the very word, 'priming', belies its status: To prime something, there must have been prior exposure of stimuli which, within the terms of the proposed model, are prior adjustments to the weights of the automatic system. Evidence to show that simultaneous and instantaneous control of the automatic system by truly controlled processes would falsify all claims in the target paper related to the 'lateness' problem.

Poropat appreciates the wider implications of the 'lateness' issue, especially the illusory nature of the apparent free will to act—it is 'just like any other process of perception, in that it takes time to consciously perceive an event even if the event (i.e. act initiation) is

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internal to the observer's brain'. Given the apparent lateness of the contents of consciousness, **Poropat** makes the salient observation that when asking people to report on their propensities to act, we are asking them to observe their behaviour in a manner that is not too dissimilar from the manner in which people observe the behaviour of others. So, even when rating myself, *I* (the observer) am effectively rating *me* (the actor) as if *I* was an *other*-rater. He argues that people may also be unaware of their wants or emotions, further limiting the ability of self-raters to assess their own personality. Given that there would be little or no 'personality psychology' without self-reflective measurement, **Poropat** touches on a fundamental issue. He then infers, also correctly in my view, that the proposed model adds strength to recent calls for greater emphasis on behavioural observation (Furr, 2009; also see Corr, 2009), especially as motor outputs are the means by which the brain contributes to survival and reproduction (see below).

CHARACTERISATION OF PERSONALITY TRAITS

In a far-reaching commentary, **DeYoung** draws our attention to a number of germane issues, involving the characterisation of the traits linked to RST systems, specifically: (a) the distinction between fear and anxiety; (b) labelling of the FFFS; (c) the nature of the BAS; and (d) the role of the five-factor model in controlled processes.

Fear versus anxiety

DeYoung is correct to note the need to clarify the states and traits associated with the FFFS—especially as it has played a relatively minor role in the historical development of RST—and how these states/traits differ from those related to the better specified (or, at least, better known) BIS. First, is the concern over the use of 'fear' as an appropriate label for the FFFS; and, secondly, is the doubt that the emotional state of 'fear-proneness, timidity and avoidance', as stated in the target paper, adequately characterises the traits associated with FFFS sensitivity/activation. These are well made points and may be seen to receive support from Gray's (1987) labelling of the FFS (the 'fight-flight' system) and Gray and McNaughton's (2000) adherence to behavioural labels for the revised FFFS ('fight-flight-freeze' system). There is also support for the observation that most stimuli 'feared' by human beings occur in situations containing potential reward, or at least in situations involving some degree of conflict. We might, therefore, want to give this language usage a certain degree of priority in our models, especially if we desire to develop theories that can be used to explain everyday behaviours in an accessible form (**Poropat**)—or, at least, not in a form that contradicts common language usage. I am favourably disposed to this position. However, there are sound theoretical and empirical reasons for wanting to keep 'fear' and 'anxiety' separate terms.

First, turning to theoretical reasons, there is a danger of semantic confusion, because 'social fear' in RST terms is BIS-related anxiety because *ex hypothesi* it entails conflict. I think we must be wary of how people use words, such of 'fear' and 'anxiety', to describe their states (e.g. they do not always use them consistently), and cognizant of **Poropat's** point about the verticality of self-reflections—although, at the same time, we might also want to admonish theorists who use such easily confusable terms. This problem is compounded by the fact that, in RST, it is to be expected that fear and anxiety are often co-activated and, related to this point, as they have different functional significance (FFFS-fear to avoid/escape and BIS-anxiety to approach under conditions of risk), they are often in

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conflict—which according to the proposed model and consistent with other work (**Morsella & Hubbard**), would lead to the triggering of consciousness awareness, the contents of which are accessible to self-reflection. Thus, what is experienced in consciousness may well be the conflation of coactivated fear and anxiety states, and may not reflect the separate underlying causes of dynamic systems (see **Poropat**). This possibility is suggested by an RST postulate, namely that the primary activation of the BIS (e.g. by approach-avoidance conflict) activates the FFFS, which provides the negative emotional fuel underlying anxiety. In this way, BIS activation can feel like fear, especially when there is behavioural dithering, reflecting the rapid switching of FFFS-avoidance/escape and BIS-cautious approach. In addition, primary activation of the FFFS can be of such intensity as to activate the BIS (due to the elicitation of conflict).

Therefore, in phenomenological terms, it is difficult to distinguish these two emotional states; indeed, it may be impossible, especially with BIS-FFFS behavioural switching likely in aversive conflict situations. For these reasons, subjective experience of a conflated fear-anxiety state may tell us very little about the underlying causal mechanisms, and it is this level of analysis that personality psychology must address (**Revelle, Wilt & Condon**). Having arrived at, what may seem to many readers, this dismal conclusion, it would still be a useful exercise to try to dissociate these subjective states from objective performance (**Morsella & Hubbard**; see Morsella, Wilson, Berger, Honhongva, Gazzaley, & Bargh, 2009) in order to determine the correspondence between the activation of underlying separate systems (as shown in objective performance) and consciously-accessible states of emotion.

If, indeed, FFFS-fear and BIS-anxiety states are often co-activated then, at least at the conscious level, the everyday language usage of 'fear' to mean RST-defined 'anxiety' is understandable. This possibility raises the related issue of how the proposed model relates to the lexical approach to personality (**Poropat**), as well as to the major issue of the measurement of personality. For example, does the linguistic conflation of 'fear' and 'anxiety' account for both types of items appearing in the BIS scales of the Carver and White's (1994) BIS/BAS scales; or can these items be differentiated at the statistical level. A related issue concerns the statistical recovery of RST-like 'fear' and 'anxiety' items from the five-factor model, as well as other well-established personality instruments. It is to these issues that I now turn.

Is it possible to differentiate fear and anxiety?

Even assuming that there exist different neural processes underlying FFFS-fear (avoidance/escape) and BIS-anxiety (cautious approach) *states*—it needs to be remembered that RST is basically a *state* theory of immediate motivation, emotion and behaviour—is it possible to differentiate these processes at the level of trait personality questionnaires? According to RST, states and traits are quite different; and the existence of the former does not necessarily imply that they would be represented in an isomorphic manner in trait structures.

The Carver and White's (1994) BIS/BAS scales remain the most widely used instrument to measure RST constructs. I suspect that their popularity is due, to some degree, to their predictive success by virtue of their conflation of fear and anxiety—most experimental set-ups designed to test RST have not adequately distinguished, in operational terms, conditions that favour either FFFS-fear or BIS-anxiety processes and are, therefore, engender mixed negative emotion states. Turning to a more compelling argument, there are several reasons for wanting to keep separate fear and anxiety states and associated traits.

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Close inspection of the BIS scale shows that it, does indeed, decompose into FFFS (fear) and BIS (anxiety) components, as predicted by revised RST (Corr & McNaughton, 2008) and empirically confirmed by different research groups (Beck, Smits, Claes, Vandereychen, & Bijttebier, 2009; Heym, Ferguson, & Lawrence, 2008; Johnson, Turner, & Iwata, 2004; Poythress et al., 2008).

DeYoung is right to draw attention to the confusion surrounding the states of fear and anxiety, which is seen also in the psychiatric literature. For example, some 'fear' questionnaires, such as the Fear Survey Schedule (Wolpe & Lang, 1977), contain a mixture of fear (e.g. animal and tissue damage items) and (RST-defined) anxiety (e.g. social conflict) items; and these 'fear' and 'anxiety' items too are psychometrically dissociable (Cooper, Perkins, & Corr, 2007)—this statistical separation is supported by different types of evidence, for example predictive validity studies (e.g. Perkins, Kemp, & Corr, 2007; also see this study for a review of the statistical differentiation of existing fear and anxiety scales). Thus, there is evidence for the statistical differentiation of fear and anxiety personality *traits*. This, largely, statistical support of the fear-anxiety *trait* differentiation is supported by the extensive behavioural, lesion and pharmacological *state* evidence upon which revised RST is based (Gray & McNaughton, 2000; for a summary of this evidence, see McNaughton & Corr, 2004; McNaughton & Corr, 2008a).

These observations are far from sufficient to quell the fear-anxiety debate; and we can expect the statistics-based arguments to continue into the foreseeable future. Given this situation, I think it is sensible to *assume*, for the time being at least, that these traits are, to an appreciable degree, separable; and if and when compelling evidence shows that there are not truly separable, then we can combine them in the sound knowledge that they form a unitary trait rather than a 'shot-gun marriage' of the type seen with sociability-impulsivity back in the 1960s.

Somewhat inconsistent with the notion of the inseparability of fear and anxiety (at least in lexical terms), **DeYoung** makes the promising suggestion that the two-factor solution ('Withdrawal', describing people as anxious, self-conscious, and depressed, possibly BIS-related; and 'Volatility', describing people as emotionally labile, irritable, and easily upset, possibly FFFS-related) of the 15 facets of the Neuroticism domain of the five-factor model (DeYoung, Quilty, & Peterson, 2007) might be related to FFFS-fear and BIS-anxiety, respectively. This is a valuable link with the five-factor model and goes some way to integrating RST with the highly influential lexical model of personality. I recommend that these two factors should be used in future RST research to explore their full potential.

State-trait characterisation of the FFFS: Fear or panic?

DeYoung also makes the plausible suggestion that 'panic', rather than 'fear', may be a better descriptive label for FFFS activity, especially if we want a label to describe the high pole of this factor. I agree that the description of the traits associated with the FFFS as 'panicky and irritable' has considerable appeal, especially as it captures the FFFS's panic and fight/anger aspects. However, for reasons given below, 'panic' is perhaps an even more problematic label than 'fear' for the FFFS. In keeping with the theme of common language usage, I agree though that the person at the high pole of the FFFS could be characterised by such adjectives as 'reactive', 'touchy', 'sensitive', 'prickly', 'defensive', 'irritable', 'irascible'. For economy, we might prefer 'emotionally volatile' to reflect the five-factor model 'Volatility' factor discussed above.

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Now, it is true that RST assumes that, at high levels of threat (i.e. perceived and/or actual), FFFS-behaviour leads to flight (if the environment affords escape) or panic-related fight (if the environment does not afford escape). However, if we were to adopt the panic label for the FFFS then **DeYoung** would be justified to assert that this label would not characterise the usual outputs of the FFFF, as most people do not panic, at least not very often. The statement in the target paper that, except in cases of false alarm, activation of the BIS ends when 'behavioural control reverts to FFFS-mediated avoidance/escape' may be interpreted to suggest that feelings of panic should be much more common than, in fact, they are in everyday life. However, RST is quite clear that FFFS activation would not always, or indeed often, lead to panic—active avoidance or escape should be expected to produce panic only under conditions of very short (actual or perceived) defensive distance (see Corr & Perkins, 2006, p. 6)—these conditions are very uncommon (one such condition, comparable to the putative panic felt by the rat when nose-to-nose with a cat, would be driving across a railroad track only to discover that a train was heading, at high, speed, towards you.) I agree with **DeYoung**, that 'In fact, when BIS activation causes passive avoidance of some danger, one frequently neither actively flees nor fights, but rather switches to some other approach behavior that is less threatening'. Under these conditions, we should not expect flight or fight; however, this behavioural switching may still be a form of FFFS-related avoidance: Avoiding/escaping something aversive (however mild) so as to allow an alternative BAS behaviour—this avoidance/escape to a place of safety would, itself, be BAS activating, which fits with the notion of switching to some other approach behaviour that is less threatening.

There are still other reasons to counsel caution in adopting **DeYoung's** 'panic' label for FFFS activation. As already stated, panic is only relevant to high levels of threat (actual or perceived) intensity, so does not adequately describe all FFFS behaviours. RST is explicit in assigning FFFS activation to specific neural models that mediate specific defensive behaviours (see McNaughton & Corr, 2004; McNaughton & Corr, 2008a). Using only one form of output, panic, to label the various other forms of defensive responding would be misleading—'fear' has the merit of not being so closely tied to any one neural modules (the same may be said of 'anxiety' in the case of the BIS).

In addition, there are clinical, genetic and psychometric reasons against the adoption of panic to describe the FFFS. Whilst supporting the distinction between 'fear' (i.e. animal and situational phobia) and 'anxious-misery' (i.e. depression and generalised disorder), as lower-order factors of 'internalising disorders', in a large quantitative genetics study of common mental disorders (Kendler, Prescott, Myers, & Neale, 2003), panic disorder was found to be aligned with the, putatively BIS-related, 'anxious-misery' factor (see also, Kendler, Neale, Kessler, Heath, & Eaves, 1992; Prescott & Kendler, 1998). Other data, though, associate panic with the 'fear' factor of internalising disorders (e.g. Krueger's, 1999, analysis of patterns of comorbidity among common mental disorders).

Yet another reason for eschewing the coupling of panic with the FFFS is recent (unpublished) structural research I have undertaken with Andrew Cooper (Goldsmiths, University of London). On the basis of qualitative (to generate item content) and structural equation modelling (SEM), we developed theoretically faithful measures of the FFFS (Flight/Flight; Avoidance) and of the BIS (Cautious Approach/Risk Assessment/Motor Inhibition; Worry/Disengagement/OCD), as well as BAS measures. Consistent with **DeYoung's** proposal, on *a priori* grounds, we aligned a panic factor (which includes such face valid items as 'I tend to panic a lot', 'I am a panicky sort of person') with the FFFS factors. However, the SEM results offered no support for this hypothesised association;

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and, if anything, this panic factor loaded more highly on BIS factors (~0.65) than FFFS factors (~0.47). We also observed something similar with an 'anger' (i.e. defensive aggression) factor which loaded most highly on BAS-related Response Responsivity and Impulsivity factors (which has been found by other researchers; Carver & Harmon-Jones, 2009—this makes sense in terms of RST; see Corr, 2002). Once again, this Anger finding shows the complexity of the RST—although high fear does lead to behavioural defensive aggression (it serves the FFFS function of escape), it seems to be represented most comprehensively in BAS factors (the distinction between defensive and predatory types of aggression may help to clarify these relations; Corr & Perkins, 2009).

In conclusion of this section, given the above considerations, I believe that it would be premature to align FFFS sensitivity/reactivity with a label of 'panic'. Gray seems to have been wise to label his systems in terms of basic behaviours rather than inferred emotional states—which, themselves, may be conflations of multiple systems activation, the vagaries of language, and the representation of the outputs of these systems in the conscious mind.

Simple statements and complex constructs

Whatever the final form taken by RST-related traits of personality, I agree with the need to be clear as to what is meant by such terms as 'personality' and 'emotion'. **Matthews** charges that these concepts, along the even fuzzier ones surrounding consciousness (see below), are treated in too simplistic a way in the target paper. For example, my statement 'Extraversion is another example of, largely, automatically elicited preferences' is taken to task. The sentence ran on to read '...: whether a person prefers to go to lively party or to have a quiet night at home is not subject to rational judgment; they "just do"—further enquiry would probably serve only to distort this basic preference by adding controlled processing levels of justification and rationalisations'. Whilst not considering it necessary to modify this specific statement, I agree with **Matthews** that higher-order aspects of personality (e.g. Extraversion and Neuroticism) need to be considered more closely, especially as they are probably distributed across multiple, independent processes, both of an automatic and controlled nature, with the latter including 'a host of high-level social-cognitive constructs including self-efficacy, social motivations and strategies for coping and emotion-regulation'. However, here we witness again the emergence of the 'interface' and 'lateness' problems.

In relation to the issue of simple causal effects of emotion, as seen in cognitive processing paradigms such as priming and selective attention, these effects are consistent with the predictions of the model. There is, usually, enough time in these paradigms for emotion to sensitise automatic processes and, thus, produce the effects observed—in this case, 'future' is defined in terms of milliseconds. **Matthews** performs a valuable service in identifying existing problems with the automatic-controlled distinction in emotion effects and I agree that further dedicated work is needed to clarify these issues.

Behavioural approach system (BAS)

With good justification, **DeYoung** draws attention to the complex nature of the BAS; and although not central to the proposed model of behavioural control it is worthy of some attention. Elsewhere, in place of the short-hand account used in the target paper which describes the personality factor associated with BAS as consisting of 'optimism, reward-orientation and impulsiveness', I have adumbrated a sketch of the different putative

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processes involved in approach motivation and behaviour (Corr, 2008). This response may benefit from a brief summary of this work. In brief, on evolutionary grounds, we may assume that the BAS is rather more complex than typically assumed by RST researchers; and, more crucially, is likely to be more complex than either the FFFS or the BIS (this conclusion is based on the 'life-dinner principle' of Dawkins & Krebs, 1979; see Corr, 2008). To explicate BAS processes, the concept of *sub-goal scaffolding* was developed, which was designed to explain the separate, though overlapping, stages of BAS behaviour, consisting in a series of appetitively motivated sub-goals (also see discussion below of 'action-goal inclinations'; **Morsella & Hubbard**).

The model of *sub-goal scaffolding* was designed to reflect the fact that, in order to move along the temporo-spatial gradient to the final primary biological reinforcer, it is necessary to engage a series of behavioural processes, some of which oppose each other (e.g. involving, at the early stages, behavioural restraint and planning, but, especially at the final point of capture of the biological reinforcer, impulsivity). Therefore, simply being a highly impulsive person (i.e. not planning and acting fast without thinking) would be detrimental to effective BAS behaviour. For these reasons, 'impulsivity' is probably not the most appropriate name for the personality dimension that reflects the totality of BAS processes (Franken & Muris, 2006; Smillie, Jackson, & Dalgleish, 2006). I agree with **DeYoung** that impulsivity is a 'compound trait resulting from the strengths of multiple systems' (which could include also weak BIS disinhibition), and it is likely to reflect not only BAS sensitivity but also variation in the strength of controlled processes that inhibit pre-potent responses. In conformity with the *sub-goal scaffolding* perspective, weak top-down controlled processing is likely to lead to the impaired inhibition of pre-potent responses at the early stages of approach behaviour and, thus, an absence of behavioural restraint when it is most needed. Other authors have also distinguished between impulsivity in functional and dysfunctional terms (e.g. Dickman, 1990), which provides further evidence of its complex nature.

There is evidence that, at the psychometric level, the BAS is indeed multi-dimensional. For example, the Carver and White (1994) BIS/BAS scales measure three aspects of BAS: Reward Responsiveness, Drive and Fun-seeking. In accordance with the concept of *sub-goal scaffolding*, Drive may be seen to be concerned with actively pursuing desired goals; Reward-Responsiveness with excitement at doing things well and winning; and Fun-seeking with impulsivity. In passing, in my aforementioned (unpublished) RST-SEM research with Andrew Cooper, a factor of 'Goal-planning' correlated with a 'Drive/Persistence' factor of the BAS—although, perhaps not unsurprisingly, is also loaded onto the BIS sub-factors, indicating that cautious, risk-assessing, approach behaviour also entails some degree of goal planning.

BEHAVIOURAL INHIBITION SYSTEM AND ERROR DETECTION

Several commentatories focus specifically on the role played by the BIS in the specification of the behavioural control model. **Hoffman** calls attention to, what he sees as, the undue prominence given to the BIS, whilst acknowledging that 'there is no doubt that the BIS is involved in error detection, behavioral inhibition and the triggering of subsequent controlled information processing'. He contends, in my view correctly, that any comprehensive model of behavioural control must take into account a far wider range of functions enabled by

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controlled processing, such as reasoning, the simulation of alternative behavioural plans in the face of obstacles, decision-making, and other instances of complex problem solving.

The BIS was incorporated into the behavioural control model as a starting point to focus, principally, on basic defensive behaviour—which might be a preferable research strategy at such an early stage of model building (**Morsella & Hubbard**). The main focus of the target paper was on the 'interface' and 'lateness' problems in multi-level behavioural control; and the inclusion of the BIS, though it plays an important role in the proposed model (especially in relation to error detection), stands apart from these two problems. In the target paper, an attempt was made to justify the prominence assigned to the BIS, albeit not with complete success (**Hoffman**); and it did, to some extent, consider executive functions. Nevertheless, I concede that this BIS-centric orientation will need to be supplemented by other controlled-reflective functions, and in any further development of the model a greater emphasis on executive cognitive functioning would be desirable (e.g. flexible switching between multiple processing goals; maintenance and updating of goal-relevant information in working memory and attentional inhibition). I do not see these theoretical approaches as mutually exclusive to the BIS and there is no reason why they could not be integrated in a more refined version of the model of behavioural control proposed.

Pickering's commentary is more in sympathy with the inclusion of the BIS, and he concurs with the general aims of the proposed model. However, he questions its ability to carry out the functions with which it is charged. I agree with him that 'a more precise rendering of the theory will ultimately be needed', which 'should include consideration of the neural mechanisms by which inhibitory signals bring about behavioural switching'. As a leading RST theorist and researcher, his comments pinpoint crucial aspects of the model.

The statement in the target paper, concerning the role played by the BIS in controlled processing, is consistent with Pickering's (1997) statements on this topic; as it is also consistent, more generally, with Gray's (1982) own work which always emphasised the role of the BIS in risk-assessment and behavioural inhibition (which entails the scanning of the external world and stored regularities (memory) in conflict situations[1]). The target paper positions the BIS as part of a more extensive executive control system; however, I would not want to say that the BIS *is* the executive control system, although it may play a privileged role, especially, in response inhibition and, probably, in the transmission from automatic to controlled processing.

Role of hippocampus

The issue of the involvement of the hippocampus, as well as other distributed BIS structures in the detection of conflict and behavioural inhibition, is highlighted by **Pickering**. The intention of the target paper was not to downplay the important role played by the hippocampus; nor was it to elaborate on the existing BIS literature—although some of the ideas put forward concerning its distributed functions have been informed by extensive discussions with Neil McNaughton, who is the leading researcher on the neurology of the BIS. One excerpt from a recent email (7th April 2010) may help to clarify the role of hippocampus in the BIS.

[1]Although not as explicit in the pre-2000 original version of RST, the BIS was always deemed to be sensitive to conflict, the paradigmatic case involving *conditioned* aversive stimuli (the conflict resides in the difference between the unconditioned aversive stimuli and the conditioned threat of it: when avoidance/escape is not possible, the experimental animal has to face something it is highly motivated to avoid/escape).

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> In our [McNaughton & Corr, 2004] original 2D model the hippocampus actually does not have any special place. It's just one of several modules. It is in the centre of the hierarchy and so could be viewed as most important; also we have the amygdala as arousal and hippocampus as behavioural inhibition proper, so it could be seen as the basis of the key functions of the BIS. That said the hippocampus has an unusual structure, isolated from the rest of the brain. It seems to have evolved for a special function that ought to be distinct from other structures. So it is likely to be a key locus of concurrent goal conflict detection. There is good evidence for areas like anterior cingulate being involved in conflict processing in the form of error detection; and the theory has always had successive goal conflict (i.e. a conflict between two goals one of which must be held back so the other can be dealt with first) as being frontal cortex and not hippocampus. Also, with simple act/action inhibition, as in mirror drawing or the stop signal task, we have always seen this as independent of hippocampus and based on the basal ganglia (to which I would now add inferior frontal gyrus and presupplementary motor area). So there are many types of conflict that will not engage hippocampus but it may still be that whenever we have true concurrent goal conflict that hippocampus must be involved.

Pickering is right, however, to highlight the role of the hippocampus, and considerably more work will be needed to clarify which structures are involved in conflict detection and response inhibition under different circumstances. Too much remains unclear and unknown for strong conclusions to be reached at this point in time, although I would note that there are data that support the claim that the BIS is a distributed structure which supports the move away from the 1982 hippocampus-centric view. For example, Moeller and Robinson (2010) review a large literature which shows that reactivity to error feedback is related to anterior cingulate cortex (ACC)—itself part of the distributed BIS—which is also activated by subjective experience of distress and pain; and, furthermore, that patients with anxiety-related disorders show enhanced activation of ACC when stimuli are discrepant from expectations. Specific BIS-related predictions are dependent, either singly and/or jointly, on the following parameters: (a) the magnitude of (actual or perceived) threat (i.e. defensive distance); (b) the required speed of the adaptive response; and (c) the simplicity-complexity of the information processing needed to decide on the most appropriate response. The proposed model remains agnostic as to these BIS details; however, such detail will need to be added as the model moves from a general sketch to a detail-rich specification. For the moment, establishing the major tenets is the necessary task in this process of model building.

The principal point I want to convey is that the hippocampus is not the only structure that detects conflict, and in some circumstances may not be the most important, as conflict detection can occur across the distributed BIS hierarchy. Arguably this proposal, rather than narrowing the range of BIS-triggering events, as claimed by **Pickering**, may widen it.

As regard **Pickering's** comment concerning how the BIS would 'know' what areas had generated the error, I relied upon existing BIS theory (see Gray & McNaughton, 2000). Consideration of space in the target paper prevents extensive discussion of this matter. Fortunately, it has been discussed elsewhere (Andersen, Moore, Venables, & Corr, 2009). These authors argue that the neural signature of the BIS—namely, the hippocampal theta rhythm—serves the information function of communicating between the septo-hippocampal system and neurocortical areas. We have shown that theta coherence (phase-locking) occurs when experimental participants are engaged in emotional rumination, as opposed to non-emotional rumination. This finding is consistent with Gray & McNaughton's (2000) view that increased phase locking between the SHS and the neocortex maintains the discreteness of individual cycles of recursive calculations during goal-conflict resolution. Such circuits would 'know' the nature of the error-generated signal.

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Response inhibition and switching

The experimental findings that **Pickering** cites as posing a possible challenge to the model provides the opportunity to make explicit some of the details of response inhibition and switching that were merely implicit in the model (they are part of the wider RST foundations upon which the model rests). First, in relation to the peripheral mismatch study (Dawkins, Powell, West, Powell, & Pickering, 2006), in which associative mismatch produced inhibition on a focal choice reaction time task, my interpretation of these data is no more than, what I consider, a standard RST-based account; namely, that such peripheral mismatch would be sufficient to engage the BIS *to the extent that the peripheral mismatch stimuli were processed as a predicted sequence of stimuli*. Once triggered by mismatch detection, the BIS should be expected to inhibit all ongoing behaviours, whether focal to the mismatch stimuli or not. This interpretation is supported by the association of the RT inhibitory effect and measures of trait anxiety/neuroticism, 'albeit in the opposite direction to prediction (more anxiety, less RT inhibition)'. How might we account for this opposite pattern of findings? Consideration of the details of RST may suggest a plausible answer.

To begin with, it is important to recognise a layer of complexity that is often overlooked in RST: The opposing behavioural outcomes of punishment, consisting in (a) response inhibition *and* (b) arousal-induced response invigoration. That is, upon presentation of punishment, there is an inhibition of behaviour as well as an induction of arousal that serves to strengthen any ongoing response (in the Dawkins et al., 2006, study, the primary task). Therefore, there is antagonism between (inhibition-based) response decrement and (arousal-based) response increment. The Gray and Smith (1969) Arousal-Decision Model states that, at low punishment intensity (e.g. as related to goal-conflict, as might be assumed in the experiment under discussion), the effects of arousal may well have been stronger than the effects of inhibition. Thus, it is perhaps of little surprise that anxious-neurotic individuals showed the least amount of inhibition, according to this explanation, by virtue of them showing the greatest amount of arousal-related response invigoration. The plausibility of this interpretation is strengthened by the statement, 'We were able to reverse the direction of anxiety correlations if the unattended mismatching stimulus (E in the above example) was pre-experimentally conditioned to be aversive'. Indeed; this is predicted by standard RST: Conflict + aversion should be expected to produce response inhibition that is stronger than the response invigoration from arousal induction, and, under this condition, neurotic-anxiety people should be expected to show the highest level of response inhibition.

Such details of inhibition and arousal are important when predicting the outcomes of any experiment that involves conflict and/or aversive stimuli; and they should be at the forefront of our mind when designing RST experiments. In order to be sure that the results are in accordance with theory, parametric manipulations of conflict, threat and arousal magnitudes must be achieved, which would allow formal modelling of type performed by Gray and Smith (1969) and McNaughton and Corr (2008a), and of the type favoured by Pickering (see Pickering & Corr, 2008). **Pickering's** observation of the important role placed by neural network modelling of specific inhibitory effects is timely. As an example of the fecundity of this general approach to the dynamics and structure of personality, an RST-inspired neural network theory of personality recently appeared in *Psychological Review* (Read, Monroe, Brownstein, Yang, Chopra, & Miller, 2010).

Pickering goes on to discuss other experimental findings as a possible challenge to the proposed model, namely those of Moeller and Robinson (2010). I agree with his statement that '...a detailed analysis of personality effects in potentially BIS-engaging processing

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tasks like these is needed to establish the boundaries of Corr's account'. However, as indicated in relation to the Dawkins et al. data, as so often, the devil is to be found in the detail. Discussion of existing data, based on experiments not specifically designed to test the predictions of the model, have limited value. They allow far too many *post hoc* explanations; and may, therefore, not go very far to 'establish the boundaries' of the model.

We may ask though, are the response switching and neuroticism findings of Moeller and Robinson (2010) *inconsistent* with the proposed model? Before attempting to answer this question, a few words need to be said on the issue of neuroticism, which was the personality construct of interest in this study. Neuroticism is likely to be a compound measure, mixing different neuropsychological processes (e.g. FFFS and BIS)—or, from five-factor model perspective, relatively separate factors of Withdrawal and Volitality (see above)—which can, and according to RST often are, in opposition (e.g. faster FFFS-mediated active avoidance with slower BIS-mediated passive avoidance). Also, as indicated by **Matthews**, constructs such as Extraversion and Neuroticism are likely to reflect variance at controlled levels of processes, and not simply the automatic levels of control.

In support of these comments, Moeller and Robinson (2010) themselves note that correlations between self-report neuroticism and various threat-related scales are well established, but systematic associations between neuroticism and experimental manipulations of threat are much less clear-cut. This outcome may reflect little more than the interaction of the specific aspects of FFFS and BIS activation and task parameters: Only when there is concordance between the outputs of the FFFS/BIS (and the BAS) and the response demands of the task should we expect to find positive associations—under other conditions, we may find only null or negative associations (see the above example of how punishment-related arousal invigorates ongoing BAS-related behaviour). Thus, for any meaningful test of RST, there must be a clear mapping between experimental task parameters (e.g. speed of response) and the expected outputs of the FFFS, BIS and BAS (for further discussion of this matter, see Corr & Perkins, 2006).

There is nothing new in the above statements: These principles are central to revised RST (McNaughton & Corr, 2004; McNaughton & Corr, 2008a). Specifically, as shown by the concept of 'defensive distance', a threat stimulus of a fixed intensity leads to different behavioural reactions depending on the individual's *perceived* defensive distance (i.e. fearful*ness*), and with each distinct defensive behaviour (e.g. avoidance vs. freezing) different cognitive, emotional and behavioural processes are engaged. General psychophysiological measures (e.g. electrodermal activity) can be useful when measuring activity of the whole defensive system functioning; however, it is altogether a different matter when we want to measure activation of specific neural modules of the FFFS and BIS.

On the basis of this discussion, it is not surprising that Moeller and Robinson (2010, p. 2) note, 'Neuroticism's hypothesised link to threat reactivity processes has been met with mixed success...for example, the straightforward idea that neuroticism should predict the faster recognition of threatening stimuli has not faired well...several studies have linked neuroticism to greater reactivity to negative stimuli or punishment-related feedback, but the results have been more complex than originally envisioned'. Hans Eysenck certainly favoured the notion of a unitary factor of neuroticism, but Jeffrey Gray never did (for him it reflected the combined sensitivities of the BIS and BAS); and in revised RST, the distinction between FFFS-fear and BIS-anxiety has further complicated the picture (see above). Therefore, the 'straightforward idea' is neither straightforward, nor is it a RST-related idea—I would note that Moeller and Robinson (2010) did not refer specifically to

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RST, but discussion of their views help to clarify the interpretation of their data to which **Pickering** calls attention. Thus, for these reasons, neither RST nor the proposed model should be expected to account for experimental findings that have been designed in ways that conflate different, often opposing, processes. With justification, I could appeal to this line of reasoning to avoid any attempt to interpret the findings of Moeller and Robinson (2010); however, existing RST already provides an off-the-shelf explanation, if one is desired—if nothing else, this explanation shows that such data are not outside the explanatory remit of RST.

I do not find it surprising that, in the Moeller and Robinson (2010) study, high neuroticism individuals slowed down on repeated trials, when we should expect this response decrement as a result of BIS-related conflict detection triggered by error feedback. This is an example of behavioural inhibition. Also, I do not find it surprising that, when there was a response switch, such individuals were faster, when we should expect this response facilitation as a result of FFFS-related active avoidance? Standard RST can account for these effects—although this ‘explanation’ is unsatisfactorily postdiction.

The proposed model of behavioural control need not add much to this account. However, what it does add is a number of predictions that could be used to confirm or falsify its basic tenets. Specifically, the model postulates: (a) slowed RTs on repeated responses are BIS-mediated and, therefore, should be anxiety related; (b) faster switching is FFFS-mediated and, therefore, should be fear (or some appropriate proxy) related; and (c) the transition from automatic to controlled processes takes time, requiring controlled processes to adjust the cybernetic weights of the automatic system. In order to test the proposed model, it would first be necessary to decompose neuroticism into its respective FFFS-fear and BIS-anxiety components, which is something that was not attempted by Moeller and Robinson (2010). As the automatic cybernetic weights have to be adjusted following error feedback, highly neurotic individuals would then be primed to switch quickly which, indeed, was found in the above study. And when they repeated the same response, then conflict should be expected and, thus, slower RTs, which was what was found. In order to test the adjusted weights hypothesis, it would be necessary to undertake a temporal blocks analysis to determine whether the effects for highly neurotic individuals follow a temporal course over the task. In addition, it would be highly valuable, by experimental means, to impair controlled processing (e.g. by dual-task processing) which, it should be expected, would disrupt the adjustment of the automatic weights and lead to sub-optimal performance (e.g. slower switching times).

In conclusion, in principle, there is nothing in the Moeller and Robinson (2010) findings that pose a serious challenge to the proposed model; although I would be the first to demand that dedicated prospective research is needed to confirm specific predictions based on the proposed model.

THE CAUSAL STATUS CONSCIOUSNESS

For some commentators, the value of conscious awareness in behavioural control was taken for granted (e.g. **Morsella & Hubbard**). **Revelle, Wilt and Condon** state that consciousness must ‘assist with the resolution of longer-term problems related to the temporal integration of experience’. However, **Matthews** considered it to be redundant and positively confusing construct.

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To begin with, I freely admit a certain degree of equivocation in my association of consciousness with controlled processes (**Matthews**). We still have no idea how the brain generates phenomenal states, and nor do we have any coherent theory as to why such states are necessary—that is, why cannot the functions ascribed to consciousness be carried out, like so many other functions, in non-conscious mode? We can point to possible behavioural benefits of such states (e.g. Gray, 2004), based on a systematic investigation of the types of processes that have privileged access to consciousness and those that do not (**Morsella & Hubbard**). However, I remain sympathetic to the position adopted by **Matthews**, because in many respects the invocation of conscious states complicates—and has the potential to confuse—theoretical accounts of behavioural control. However, assuming that consciousness does exist (in some form), and permitting a functional role for it (which many people would not), I think it is incumbent upon biologically respectful scientists not to dismiss it out of hand; although, I agree, that it should not be used to fill explanatory gaps for the sake of mere convenience.

Concerning a definition of consciousness (**Matthews**), I made it clear in the manuscript that the 'hard question' of consciousness is not addressed (see footnote 3). That is, the *how* and *why* the brain generates it. This remains a mystery in neuroscience and psychology and is not something I want (or can) address. When I discuss the important work highlighted by **Morsella and Hubbard**, further refinement of this problem emerges. For the moment though, what I mean by 'consciousness' is the phenomenal state of being subjectively aware of perceptual objects, whether external objects (birds flying) or internal ones (awareness of leg pain). It is the *experience* of percepts (qualia; e.g. colour and sound) constructed by the brain that I would emphasize—Nagel (1974) is well-known for claiming that phenomenal states exist when there is *something it is like*[2]. As Morsella (2005) notes, although such words as 'subjective experience', 'qualia', 'awareness', sentience' and 'consciousness' are difficult to describe and analyse, they are relatively easy to identify (look around the room in which you are reading these words—the *experience* you are now having is the referent of these words). I do not know why we experience these subjective states and I am content to assume that the controlled processes that interface with the automatic machinery of behavioural control are non-conscious. But I am unwilling, yet, to dismiss evidence that points to a functional role for consciousness.

Why we should not (and cannot) dismiss consciousness

A number of reasons exist to suggest that it may be too soon to dismiss conscious awareness from the causal chain of events in behavioural control. Gray (2004) claimed a number of such reasons, based on the principle that if it could be shown to have evolved by natural selection then there are strong grounds for assuming it serves a functional role, and is not epiphenomenal. Other reasons are provided by the work cited by **Morsella and Hubbard**.

To the average person in the street, to say that consciousness does not have a major function would invite derision; and a similar reaction would be seen, albeit in a more polite form, from within personality psychology (e.g. **Revelle, Wilt & Condon**). How could you be reading and comprehending the words on this page if this were not the case? (Granted,

[2]This, of course, does not mean these phenomenal states have any function, or, indeed, that they are real in any other sense they we seem to experience them (this could, itself, be a grand illusion; see Corr, 2006, p. 595–597).

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there may exist zombies, who behave identically to conscious beings, but who have no subjective experience—but is not this possibility also very fanciful?).

The following reasons for retaining the construct of consciousness, at least for the present time, are relevant. First, is the subjective experience of being consciously aware. This *is* psychological data—and of no small scientific interest in itself. Second, is the causal question, and several areas have pointed to the role played by inhibition of pre-potent responses, and there are other reasons for assuming causal potency (**Morsella & Hubbard**; see below). To anticipate some of these causal functions, consciousness may provide a forum for the simulation of disparate information for detailed processing, especially at the interface of conflict (e.g. bottom-up and top-down data; mental simulation allows goal planning, and response consideration and selection—this line of reasoning is consistent with Jackendoff's (1987), intermediate level theory of consciousness). Third, the apparent fact that this subjective forum is egocentric makes sense in terms of Darwinian selfish genes: Short of actually performing an action and waiting for the consequences (which may prove lethal), the best strategy of predicting an outcome is to simulate it in an appropriate model (**Morsella & Hubbard**).

Gray (2004) laid emphasis on the following feature of consciousness: (a) the inter-individual consistency (as far as we know) of qualia (e.g. colour of different fruits)—a lack of such a functional value would tend to lead to genetic drift and less obvious consistency; (b) qualia that allow the classification and differentiation of evolutionarily-significant environmental stimuli (e.g. nutritious vs. poisonous foods); and (c) the events of which we become conscious are neither a random nor complete set of those events of which we could, in principle, become conscious, given that the brain receives information about them (this feature of conscious awareness is also highlighted by **Morsella & Hubbard**). I would add behavioural evidence to suggest that nonhuman animals too have something similar to human consciousness (this conclusion is supported by the considerable psychological similarities observed across the phylogenetic scale; see McNaughton & Corr, 2008b).

In contrast to **Matthews** in particular, **Morsella and Hubbard** argue in favour of the causal efficacy of consciousness; and they offer empirically based reasons for rejecting the epiphenomenal position. Morsella (2005, p. 1001) states, 'The valence and other properties of the phenomenal percept are in some ways isomorphic to ongoing action. It is not the case, for example, that pleasant states are associated with avoidance behaviours or that unpleasant ones are associated with approach behaviours—in other words, tissue damage does not happen to feel good and drinking when thirsty does not happen to feel bad. In conclusion, it is not easy to discredit phenomenal states as an object of scientific inquiry. Difficult problems remain'. I concur entirely.

I know these arguments will not satisfy all; and nor should they because considerably more theorising and empirical work will be required before we are in a position to reach any firm conclusions. At this point in time, I recommend that we acknowledge that a theory of behavioural control that does not consider what the vast majority of people, including psychologists, believe to be of fundamental importance, itself needs careful justification.

Irrespective of the causal-functional status of the concept of consciousness, it *is* important in personality psychology, as would be quickly revealed by a cursory inspection of personality psychology textbooks: They are replete with consciousness-laden constructs (e.g. values, beliefs, self, meta-cognition, qualia). In addition, the vast majority of personality measurement tacitly assumes that it is possible consciously to introspect on emotion, intentions, desires, behaviours, etc. (although there are inherent problems with this assumption; **Poropat**). Among personality psychologists, consciousness is widely

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used as an explanatory construct. For example, **Revelle, Wilt and Condon** emphasise the importance of long-term goals; but how can one has such goals (whether it is preparing for a lecture or saving for one's children education) without some form of conscious awareness—in fact, such goals have little meaning without the invocation of consciousness. These commentators go as far to say that 'consciousness fulfills the momentary role of error detection' and consciousness must 'assist with the resolution of longer-term problems related to the temporal integration of experience'.

Personality psychology may well have been wrong to put so much store in consciousness-related constructs—it has to be admitted that such constructs are too often used in a theoretically promiscuous manner. **Matthews'** critique of the role assigned to consciousness in the target paper, to the extent to which it is valid, must be seen also as a *major* criticism of the whole field of personality psychology. I think he is quite correct to highlight this problem. If his claim of the causal impotency of consciousness is valid, then does it not follow that many important concepts in personality psychology, including our very sense of self, is equally epiphenomenal? I doubt that many personality psychologists would be willing to accept this conclusion—although, one day, the accumulating force of evidence may demand this acceptance. Personality psychology would be rather theoretically threadbare without consciousness-laden constructs.

Is consciousness redundant in standard cognitive models of behavioural control?

Despite the reasons given for not, pre-maturely at least, dismissing consciousness as a causal force, do not existing cognitive models do a good-enough job of modelling and predicting relevant behavioural phenomena? **Matthews** asks, 'The question for Corr is to specify what standard cognitive neuroscience models lack that a separate "consciousness" construct provides'. This is a highly pertinent question. He notes that, for example, the Norman and Shallice (1986) model 'captures many of the features of control models elaborated in the target article, without assigning causal powers to consciousness'. I too am impressed by the explanatory power of Norman and Shallice's (1986) model to account for behavioural phenomena such as 'capture errors' or 'utilisation behaviour' (i.e. an inability to suppress a strongly triggered but inappropriate behaviour—e.g. the example of William James dressing for bed instead of dressing for dinner); the inability to act (akinesia; attributed to the inability to resolve selection between competing behaviours); and 'persistence of behaviour' (stereotypy or perseveration).

However, there are several limitations to such models, as well as some major strengths. The first limitation is the comparative neglect of the 'lateness' problem—there remains a real disagreement as to the extent of this problem (see above). The second limitation is the ambiguity surrounding the role played by conscious awareness in such models (see below). The third limitation is the scientific status of such models: To what extent do they adequately model *actual* psychological processes and experiences? In relation to this point, **Revelle, Wilt and Condon** present arguments for including RST constructs into artificial intelligence models of behaviour in order to improve their realism—however, computer-inspired models omit *phenomenal state*. As a matter of routine, we model weather systems, and these have predictive utility, but these simulations are *not* the weather. Morsella, Hoover and Bargh (in press) say that just as artificial models of transportation (e.g. wheels, jet engines, etc.) are not suitable models of biological locomotion, so too classical models of cognition, inspired but not limited to, von Newmann's computer approaches, are not suitable models for understanding action and perception. There is clearly residual doubt

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over the adequacy of formal cognitive models of higher-order psychological processes, arguably more than acknowledged by **Matthews**.

One's position on these matters would seem to turn on the following issue. Is the purpose of cognitive modelling to describe and explain *actual* cognition, emotion, and behaviour, as *experienced* by you and I; or is it to provide *formalised* models capable of abstracting key operating principles that, although denuded of experiential elements, do a superb job of predicting behaviour in parsimonious terms? To my mind, these positions are quite different, and not incompatible. I would not want to go too far down the consciousness path if it were at the expense of rigorous formal cognitive models that have the merits of theoretical parsimony and predictive power. However, when so doing, we may want to acknowledge that they are not intended to model other features of psychology (e.g. subjective experience) that appear highly relevant to the layperson and to (many) psychological scientists.

The need for consciousness in cognitive models

Before acceding to the theoretical superiority of formal cognitive models, we should first scrutinise a little more closely the claim that they have managed to eschew consciousness as an explanatory concept. It would seem that, in the case of the Norman and Shallice (1986) model, 'willed' actions require some element of deliberate and controlled processing, including a conscious component. In particular, controlled processes are needed under conditions such as planning and decision-making, cognitive trouble shooting, ill-learned or novel behaviour, threatening or difficult tasks, and inhibition of a pre-potent response. These processes are usually not devoid of a conscious component—although we may still want to argue that this component is causally impotent. For example, in their treatment of this influential model, Eysenck and Keane (2005, p. 180) state that, in contrast to the automatic 'contention scheduling' process (which resolves conflict by selecting one of the available schemas on the basis of environmental information and current priorities), even before we get to the higher-level supervisory attentional system, 'There is generally more conscious awareness of the partially automatic processes involving contention scheduling than of fully automatic processes'. Furthermore, according to Dienes and Perner (2007, p. 293), control and awareness are intimately related, and although some forms of control can occur quite unconsciously, other some forms of control, such as planning or overcoming strong response tendencies (the 'executive tasks'), are so commonly associated with conscious awareness. They go on to state, 'it would seem bizarre if they occurred without it. In fact, unconscious executive control is not possible in the theories of Norman and Shallice'. According to Shallice (1972, p. 383), 'The problem of consciousness occupies an analogous position for cognitive psychology as the problem of language behavior does for behaviourism, namely, an unresolved anomaly with the domain of the approach'. We know only too well the consequences for behaviourism of failing to grasp the language nettle.

In relation to supervisory-type cognitive models, **Morsella and Hubbard** argue that behavioural control theories should try to avoid a guiding homunculus-like agent in charge of suppressing one action in order to express another action. They point to both *a priori* and empirical grounds for their position (e.g. Kimberg, D'Esposito, & Farah, 1997). As Dennett (1991, p. 188) notes in relation to the flood of multimodal information giving rise to the new problem faced by evolution of high-order control, 'There wasn't a convenient captain already on board, so these conflicts had to sort themselves out without any higher

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executive'. This position is consistent with the specifications of the model of behavioural control proposed in the target paper.

CONSCIOUSNESS: AN ACTION-GOAL MODEL

I have so far opposed the wholesale discounting of consciousness, especially its causal role, but I have not yet suggested a plausible account of it in terms of behavioural control—as already noted above, **Matthews** drew attention to my equivocation in the target paper. Might there be a viable explanation available sufficient to guide future research?

In a wide-ranging commentary, **Morsella and Hubbard** agree with many of the tenets of the model and, on the basis of their own and others thinking and research, suggest how it might be improved. Significantly, they like **Revelle, Wilt and Condon**, but in stark contrast to **Matthews**, take seriously the causal role of consciousness in behavioural control. Their commentary goes to the heart of what types of stimuli enter conscious awareness, and why: This dissection strategy holds the promise of going a long way to suggesting causal functions.

The action-based model of control summarised by **Morsella and Hubbard** respects the finding that, under some circumstances, there exist different skeletomotor inclinations (i.e. 'action goals') toward the same stimulus situation and this produces conflict which requires, in some form, resolution. In addition, they note that encapsulated control systems often have different principles and procedures, shaped by different phylogenetic imperatives. The proposed model is consistent with their general perspective. Importantly, it shares with theirs the assumption that, at this stage of theory development, it is preferable to focus on the inter-goal dynamics among basic systems: Either the FFFS, BIS and BAS, or even simpler systems such as those controlling inhaling and exhaling. Some detailed discussion of their model is required in order to show how it may be used supplement the proposed model.

Their model rests on the observation that the process of resolving skeletomotor *action-goal conflict* requires phenomenal states (the most basic form of consciousness) by virtue of the primary function of these states being to resolve conflict by permitting 'crosstalk' among action systems, in some sort of workspace, whose contents are broadcasted globally. By this means, processes are integrated that otherwise would remain independent (encapsulated). Their general view on the question of consciousness is shared by other theorists (e.g. Baars, 2002; Dehaene & Naccache, 2001; Merker, 2007—although they often fail to specify which kinds of information require phenomenal states for integration and which kinds do not; Morsella, 2005).

According to Morsella (2005), phenomenal states are necessary, not to integrate perceptual-level processes (as in intersensory conflicts), but to integrate conflicting action-goal inclinations toward the skeletal muscle system, as captured by the principle of *Parallel Responses into Skeletal Muscle* (PRISM). In this framework, incompatible skeletomotor plans trigger strong changes in consciousness (see evidence in Morsella, Gray, Krieger, & Bargh, 2009), whereas conflicts occurring at other stages of processing (e.g., intersensory conflicts), or not involving skeletal muscle, do not lead to such changes (Morsella et al., 2009). Without these states and the crosstalk they afford, action can be influenced by one system or another (as in the case of 'unintegrated' actions such as reflexive inhaling or pain withdrawal), but it cannot be influenced by more than one system simultaneously (as in the case of 'integrated actions' such as holding one's breath; Morsella & Bargh, in press. This

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model also argues, again on the basis of empirical evidence (Morsella & Krauss, 2004, that the guiding influence of motor actions (e.g. arm and hand movement) is to influence cognition and feelings, which is opposite to what we often think about cognition-action relations.

Morsella (2005) poses the crucial question: What are the phenomenal states summoned to action? In other words, how do task demands differ between those that are consciously penetrable and those that are not? Assuming that consciousness achieves something that non-conscious does not, then this contrastive approach should help to identify the role of phenomenal states.

Morsella's (2005) theory further postulates that phenomenal states are necessary not to express or suppress actions but to suppress action *tendencies* of opposing systems; and without this mediation it is impossible to suppress or attenuate response tendencies involved in such things as blinking, reactions to muscle fatigue and pain withdrawal. This account explains why it is impossible to asphyxiate oneself by holding one's breath, for this voluntary act is only possible whilst still conscious. There is other evidence to show that automatic tendencies can only be curbed with conscious mediation (Baumeister, Heatherton, & Tice, 1994; Dunton & Fazio, 1997). The emphasis on resolving goal-conflict often requires top-down cognitive control, the machinations of which may not always be consciously accessible (Crick & Koch, 2000; Morsella, 2005)—in line with the view that cognitive control and consciousness are distinct brain processes, the proposed model differentiated alpha and beta controlled processes. This material fits well the assumption of the proposed model that one, but not the only, of the major functions of controlled-reflective functioning is response inhibition.

As noted by Morsella et al. (2009), the assumptions upon which the PRISM model rests are far from new; in fact, they are quite old—e.g. Sechenov (1863) proposed that conscious thoughts should be regarded as inhibited actions—although they fell out of favour during the cognitive revolution. Thorndike (1905, p. 11) said, 'The function of thoughts and feelings is to influence actions...Thought aims at knowledge, but with the final aim of using this knowledge to guide actions'; and, in a similar vein, James (1890, pp. 520–524) stated, 'Thinking is for doing'. These examples of ideomotor theory reflect the belief that the mental image of an instrumental action tends to lead to the performance of that action, unless there are inhibitory 'acts of express fiat' (i.e. an inhibitory veto). The idea of encapsulated systems, each of which developed over a different evolutionary timeframe, and with different functions and operating principles, seems to demand some form of hierarchical control to ensure the smooth running of the motor system. This is one important idea that underlies RST too (McNaughton & Corr, 2009).

Concerning the nature of conscious awareness, a central argument in the account given of controlled/consciousness processes by **Morsella and Hubbard** is that action-goal representations that are crosstalked phenomenally tend to be perceptual-like representations of the consequences of the motor efference (e.g. the image of a finger flexing) rather than of the efference itself, which is unconscious. According to them, perceptual-like representations are what seem to underlie the images in dreams, episodic memory, and the observations of the actions of others and of oneself—the discovery of mirror neurons is also consistent with this view. This perspective is consistent with Gray's (2004) view of consciousness, which was adopted in the construction of the proposed model, that much of the content of consciousness is perceptual in nature.

The material considered above is consistent with proposals that the function of consciousness is to construct an internal percept-rich simulation both of the external world

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and of one's current place (and dispositions) within it. This approach helps to explain everyday behavioral phenomena, for example driving a car at 70 mph along a motorway. The visual (and auditory) flow of information is enormous and almost certainly beyond the capacity of the perceptual system comprehensively to register and process. Selective attention may serve to reduce processing demands to focus on the important; but what exactly is important and how can this information be best known and acquired? What is important about car driving, as well as much else, are action-goals: What needs to be accomplished? In this instance, staying in the correct lane, responding to other vehicles on the road, etc. This action-goal focus accounts well for the well-known perceptual phenomenon of inattentional blindness (Simon & Chabris, 1999); as well as other evidence to show that so much of our attention and cognition is action-based (Morsella, 2005) and seems to obey evolutionary considerations—for example, as noted by Dawkins (1976, p. 49), 'The main way in which brains actually contribute to the success of survival machines is by controlling and coordinating the contraction of muscles'.

In terms of neurology, according to Morsella (2005) PRISM perspective, the process that considers the opposing action-goal tendencies reside in the neural circuits of the ventral thalamocortical processing stream (Goodale & Milner, 2006; Sherman & Guillery, 2006), where information about the world is thought to be represented in a unique manner (e.g. representing the invariant aspects of the world; allocentric coordinates), unlike that of the dorsal stream (e.g. representing the variant aspects of the world; egocentric coordinates).

Morsella and Hubbard's commentary suggests an important enhancement to the proposed model. Although they agree that all suppressed actions are 'voluntary', inhibition is only one case of the kind of inter-system crosstalk associated with conscious awareness. They note that it is also involved in 'gain-for-pain' behaviors, such as 'voluntarily' increasing one's rate of inhalation in exchange for reward. I agree with this view, that inhibition is not the only possible outcome of voluntary control, and conflict resolution can, indeed, involve excitation of a response (although, this may not be possible without first inhibiting pre-potent rate of a given tendency, e.g. inhalation/exhalation).

Although the proposed model of behavioral was not elaborated sufficiently to suggest specific pathways to instantiate automatic and controlled processes, this extant literature can now be incorporated.

IMPLICATIONS OF MODEL FOR PERSONALITY PSYCHOLOGY

Few commentators commented specifically on the implications of the model for personality psychology more generally. **Poropat** was the major exception. He believes that that model does not go far enough and overlooks opportunities, especially the integration of social influences with conscious processing. He adds that further examination of the manner in which social and especially linguistic interactions affect behavioral controls would add greatly to the value of the model by broadening its theoretical range and generalisability. I agree with his view, and here add a few brief words on this topic.

The social aspect of personality is of no small consequence, and although not discussed in the final target paper (although it had been included in a previous draft), the proposed model does hold implications. Linguistic interaction should be expected to have a significant impact upon personality development via a process of engaging controlled and conscious processing. For a start, social-cognitive psychological approaches focus on the interplay of personality and social processes (Jensen-Campbell et al., 2009), and address

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such questions as the extent to which personality characteristics (including traits) arise out of social interaction; and the reciprocal influence of personality on social interaction and the role of culture in influencing these relationships? At least some variants of social-cognitive psychological theory lay more focus on idiographic and humanistic perspectives (for summaries, see Caprara & Cervone, 2000; Cloninger, 2009; Smith & Shoda, 2009), which raises the question of how these perspectives relate to the nomothetic framework of the target paper; that is '(1) the relevant evolved systems that are operative in every intact human brain and (2) the parameters of those systems that vary from person to person to produce personality trait differences' (**DeYoung**). The linguistic transmission of these social influences should be considered of prime importance (**Poropat**), the understanding of which may become more tractable with the greater synthesis between lexical and biologically inspired approaches in personality psychology, of which the proposed model is one example.

We may, indeed, be heartened by the possibilities opened-up by the recent data linking the five-factor model with neuropsychology; this holds out the promise of a synthesis between the lexical approach, social knowledge transmission and brain-behavioral relations. **DeYoung** puts forward the intriguing proposal that, whilst it is relatively easy to relate Extraversion and Neuroticism to BIS, BAS and FFFS factors, it may also be possible to relate different aspects of controlled-reflective processes to Conscientiousness, Agreeableness and Openness/Intellect. Specifically, he argues that the trait that seems most directly to reflect the ability to inhibit pre-potent responses is Conscientiousness (DeYoung & Gray, 2009; DeYoung, Hirsh, Shane, Papademetris, Rajeevan, & Gray, in press); and Openness/Intellect seems to reflect individual differences in the systems that govern attention and the conscious perception and manipulation of information. This line of work, especially as it is based in neuropsychology and neuroimaging, is an important new development. The possible associations between Conscientiousness, willed voluntary action and free-will should be prime targets for future research. However, I need to re-iterate that, in any such research, the 'interface' of controlled and automatic processes, and the 'lateness' of controlled processes, and especially consciousness, would need to be adequately acknowledged.

I believe that a multi-level model of behavioral control is essential to help explicate the processes underlying social, cultural and political factors on the development of personality. For example, people hold beliefs about themselves, the world, and the future; and these belief systems confer meaning on events and are involved in selecting goals, planning behavioral strategies and understanding oneself and others. For example, Thorne and Nam (2009) discuss how people use story-telling to make sense of, and communicate, their beliefs about the world. In terms of the lexical approach, Agreeableness may be involved in self and other-perceptions (**DeYoung**). How beliefs, as consciously accessible concepts, influence automatic behavioral reactions/routines (cf. **Revelle, Wilt & Condon**) is a fundamental issue, and has been of major interest to social psychologists. However, to realise the full potential of the proposed model, especially its social-linguistic implications, as **Poropat** notes, many missing links will need to be added; for example, the manner by which the various components of the model interact and how the 'cybernetic weights' are constructed and applied.

In conclusion of this section, a few words may be appropriate on the role played by theory and experiment. **Poropat** admits to being 'a little envious of the theoretical structure' that continues to develop around RST, noting that much of the discussion of lexical models of personality do little more than extol the value of describing consistencies

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in personality variation, or resort to unobservable, circularly defined basic tendencies—although we have already seen the significant progress summarised by **DeYoung**. In relation to the greater synthesis of RST-type biological models and lexical explanations, we may not want to put too much reliance on factor analysis given its inherent problems (Block, 1995; Lykken, 1971), which **Poropat** refers to it as 'useful but clumsy tools for theoretical work'. Along similar lines, **Revelle, Wilt and Condon** state that '. . .the utility of constellation-like personality variables (e.g. Big 5) is evident but may be limited if we are really trying to understand personality. These commentators are right to suggest that models are required to get under the skein of personality description, and the DOA-CTA model (Fua, Revelle & Ortony, 2010) discussed by **Revelle, Wilt and Condon** represents one highly promising approach as do the computer simulations of the processes involved (See also Fua, Horswill, Ortony, & Revelle, 2009; go to: http://www.cs.northwestern.edu/~ian/).

One pleasing outcome of the target paper and the commentaries is the identification of valuable links between RST, the model of behavioral control and the lexical approach to personality. This forging of experimental/cognitive and differential constructs within personality psychology may bode well for the success of further attempts to unify personality psychology with general psychology.

CONCLUSION

It is, indeed, a privilege to receive such thought-provoking and wide-ranging critiques, reflecting the intellectual élan and the generosity of spirit of all commentators. In pointing to both the strengths and weakness of the proposed model, issues in need of further clarification and development are identified. Further work will be needed to address them. However, I am still of the opinion that it remains important not to lose sight of the relevance of the 'interface' and 'lateness' problems; and I urge personality psychologists to take due regard of them in their theoretical and empirical work. It is evident from the commentatories that these problems are adequately recognised by some psychologists, whereas for others they were either not acknowledged or discounted. Given their *potential* importance, it would seem prudent at this stage of theory building to take them into consideration when constructing and testing models of behavioral control. I would be the first to applaud the demonstration that these problems are more apparent than real—at least, then, they would not continue to 'haunt' the consciousness of some personality psychologists.

Irrespective of one's preferred position on these matters, we would all perhaps agree that collaboration is the best way forward; as noted by Corr and Matthews (2009, p. x),

> It can be agreed, though, that there is never been a greater need for proponents of different research traditions to talk to one another in the service of theoretical integration.

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