European Journal of Personality
Eur. J. Pers. **22**: 385–409 (2008)
DOI: 10.1002/per.683

Discussion on 'What is Reinforcement Sensitivity? Neuroscience Paradigms for Approach-avoidance Processes in Personality' by Luke D. Smillie

OPEN PEER COMMENTARY

Neurobiological Paradigms Should Complement but Not Substitute Self-reported Questionnaires

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Abstract

We agree with Smillie (this issue) that the reinforcement sensitivity theory (RST) model needs to incorporate human neurobiological data. However, the introduction of new research paradigms should not hamper all previous research in the field. At least for the coming years, we defend the idea that (a) research carried out to date should be taken into account in any new reconceptualisation of RST; (b) neurobiological data are necessary but not sufficient on their own; (c) research using questionnaires should overcome some recurrent problems and (d) a more comprehensive adaptation of RST to humans is needed. Copyright © 2008 John Wiley & Sons, Ltd.

We certainly agree with Smillie that Gray's reinforcement sensitivity theory (RST) is a psychobiological model with scarce psychobiological research. Neurobiological paradigms should be systematically incorporated to test (RST) predictions in humans. In this sense, this review is relevant to summarise the data obtained until now from different fields of research and to propose new ways to investigate RST. However, several issues should be considered:

RST background. Smillie's target paper seems to depart from zero in his reconceptualisation of RST. This approach is surprising in some senses because it does not take into sufficient account the 20 years of research in this field. The amount of behavioural studies showing data consistent with learning predictions (see Ávila &

Torrubia, 2008; Corr, 2004; Pickering, Corr, Powell, Kumari, Thornton, & Gray, 1997) or the purpose-built questionnaires could be examples of the above.

Neurobiological data are necessary but not sufficient. Most of the empirical work carried out in RST has relied on the use of questionnaires for selecting subjects for research. Questionnaires are still the most useful method to assess individual differences in personality. The research in approach and avoidance processes in the framework of RST is not an exception to this rule and has generated a consistent volume of relevant data (see, Corr, 2008). New neuroscience paradigms, in contrast, are still far from doing this job reliably and enabling a neural signature of the BIS and the BAS to be found. For instance, neuroimaging research has questioned the exact role of BIS and BAS structures in processing reinforcers. Even though the amygdala plays a role in processing aversive stimuli (LeDoux, 1998), there is also evidence showing its role in reward processing (Hampton, Adolphs, Tyszka, & O'Doherty, 2007; Winston, O'Doherty, Kilner, Perrett, & Dolan, 2007). Similarly, the ventral striatum is mainly related to reward processing (Knutson & Cooper, 2005), but also some studies have shown its role in processing painful stimuli (Becerra & Borsook, in press). Considering self-report questionnaires as general instruments to measure coarsely the activity of the BIS and the BAS, the more logical role at present for these psychobiological methods would be to complement personality questionnaires. In this sense, probably psychogenomics and functional neuroimaging are the best candidates to this end.

Research using questionnaires should overcome some recurrent problems. A surprising aspect in Smillie's proposal is the criticism of personality questionnaires as a valid procedure to measure sensitivity to reinforcers. We clearly disagree with this view, but we also admit that some problems exist related to this topic and that researchers should arrive at some consensus in some basic aspects of the model:

(a) *The personality dimensions which are related to the functioning of the RST neurobehavioural systems*. We do not agree with Smillie that neuroticism and extraversion are the personality dimensions most related to punishment and approach systems, respectively. In fact, extraversion is related positively to sensitivity to reward and negatively to sensitivity to punishment, where there is no experimental evidence of the relationship between neuroticism and sensitivity to punishment (see Ávila & Torrubia, 2008). After 20 years, the existence of discrepancies in these basic aspects may diminish the power of RST. The accumulated experience in this field seems to indicate that personality dimensions resulting from the activity of the BIS and the BAS do not seem to directly correspond to classic dimensions of personality and that probably it would be more useful to consider specific dimensions.

(b) *The requirements of scales aimed at measuring activity in these systems*. For years, the RST model was only validated using self-report measures not directly designed for this aim (Torrubia, Ávila, & Caseras, 2008). Further, a number of purpose-built questionnaires were developed using different approaches and conceptualisations of BIS and BAS, but only two have been extensively used (the BIS/BAS Scales and the SPSRQ). Both measures converge in the measurement of avoidance processes because of the high correlation between scales, but discrepancies have been found in the assessment of individual differences in approach tendencies. From our point of view, these arise from a different perspective in the design of the items. The most known, the BIS/BAS questionnaire, has been built using a general conceptualisation of reward and punishment without references to specific reinforcers. We agree with

DOI: 10.1002/per

Smillie that in these cases ‘it seems biologically implausible to suggest that individuals can introspect directly about their reinforcement sensitivity’. In contrast, the items of the SPSRQ (similarly to other personality questionnaires) detail specific situations in some cases with specific reinforcers, which from our point of view are easy to evaluate by responders. The problem with this second approach is that we may ensure that the items represent the most important facets related to appetitive (and aversive) reinforcers in a determinate culture. Maybe an adaptation of a BAS questionnaire to a new language and culture requires a more complex process than a mere translation, and we should control for different variables such as age, education level or relevance of determinate rewards. A consensus should be reached about the main aspects of purpose-built RST scales in order to improve communication between researchers.

(c) *How to take into account ontogenetical variation in sensitivity to reinforcers*. Smillie has written that ‘all these theories agree that approach and avoidance processes are engaged by reinforcing stimuli in the environment’. As we have argued elsewhere (Ávila & Torrubia, 2008), one of the main problems with this concept is the interindividual and intraindividual variability in sensitivity to reinforcers, which is especially evident for reward. This is a cardinal point in RST since all functional neuroimaging and self-reported measures of approach and avoidance processes should take this variability into account. Functional neuroimaging or psychopharmacological studies must specify the reinforcers and may control for their subjective perception. From our point of view, this fact hampers the possibility of deriving precise measures from functional neuroimaging paradigms. For instance, we may use money or pleasant pictures as rewarding stimuli, but we first need to assess the individual sensitivity to these reinforcers, as expected activations of the brain reward systems would not only depend on general reactivity of the system, but also on the specific relevance of the actual reward.

A more comprehensive adaptation of RST to humans is needed. RST adaptation to humans needs a further elaboration (Gray, 1981; Pickering et al., 1997). Recently, we have detailed a new framework to explain behavioural data derived from RST model (Ávila & Torrubia, 2008). Although its development is beyond the scope of these comments, we would like to emphasise the importance of considering the motivational context (and not the stimulus) as the relevant input to the BIS and BAS. Then, activation of the BAS is observed in appetitive motivational contexts (i.e. situations with a strong expectation of reward) generating individual differences in appetitive learning (better in individuals with an overactive BAS) and aversive learning (better in individuals with an underactive BAS). In sum, RST model needs a more developed framework to be applied in humans.

Two Distinct Bases of Inhibition of Behaviour: Viewing Biological Phenomena Through the Lens of Psychological Theory

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Abstract

Constructs concerning reward and threat sensitivity can be organised in several ways (along with other ideas). Which conceptual organisation is used channels interpretations of phenomena ostensibly reflecting the sensitivities. For example, a two-mode organisation in which behavioural inhibition can follow either from threat sensitivity or from effortful control (planful restraint) yields an interpretation of serotonergic function quite different from what many assume. In this view, accumulated evidence suggests that serotonergic function relates to effortful control, rather than threat sensitivity. Neurobiological tools are useful, but their usefulness often depends on psychological theory. Copyright © 2008 John Wiley & Sons, Ltd.

The idea that approach and avoidance tendencies are building blocks of elaborate behaviour is prominent again in personality psychology, as is the idea that individual differences in responsiveness of systems for approach and avoidance underlie dimensions of personality. There are, however, several ways to use those ideas (in conjunction with other ideas). Smillie (this issue) described one organisation, following Gray and McNaughton (2000). I favour a somewhat different organisation, outlined later.

Smillie's main point is the desirability of using new techniques to determine true reward and threat sensitivities (as opposed to self-reported sensitivities).[1] Though the techniques outlined doubtlessly are useful, I offer a caution: Apart from behavioural paradigms, the techniques Smillie touted all require linking an observed biological phenomenon to an inferred psychological one. Unfortunately, there are many psychological third variables in that process. Focusing on a salient but noncritical psychological variable can induce errors in linking the biological to the psychological. These links easily become unquestioned assumptions.

Assumptions about the psychological meaning of biological data must be reexamined repeatedly. This cannot be done once and for all. Change in psychological model can suggest a change in meaning for the link from biological to psychological. I illustrate this below with a case in which I believe Smillie's discussion reflects a misinference. The psychological model displayed in Smillie's Figure 1 leads easily to the misinference. An alternative model, however, suggests a very different interpretation.

Impulsivity, anxiety and constraint. Consider three theoretical organisations involving approach and avoidance. Smillie's Figure 1 reflects Gray's (1981) initial theory proposing

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[1] It is a little ironic that Smillie expressed concern about the validity of inferences made from human self-reports, but seemed less concerned about building a theory of human personality by observing rats' behaviour and making inferences about their emotional states.

DOI: 10.1002/per

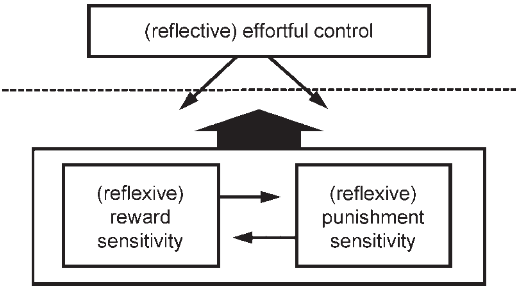

Figure 1. Three temperaments as influences on behaviour. Two reactive (reflexive) systems compete, as they become engaged by cues of reward and punishment, respectively. The resultant tendency is manifested directly in behaviour, if effortful control is low (due to disposition, developmental level, or situational influence). Effortful control (reflective) depends on executive processes. When these processes are engaged, they can countermand the resultant emerging from the basic systems, thus dampening the role of the basic systems. Adapted from discussions by Rothbart and others.

personality dimensions of reward sensitivity (impulsivity) and threat sensitivity (anxiety). Higher reward sensitivity leads a person to display more impulsive pursuit of rewards, given reward cues. Higher threat sensitivity leads greater inhibition, given cues of impending punishment. One clear implication of this view was that impulses are expressed unless they are inhibited by sufficient anxiety. Another was that anxiety is the main determinant of inhibition.

The theory was later revised in a rather profound way (Gray & McNaughton, 2000). The behavioural inhibition system (BIS), formerly the mechanism of threat sensitivity, now concerns conflict. Although Smillie's Figure 2 says the conflict must be approach versus avoidance, his footnote 1 contradicts that, saying that BIS is also engaged by two incompatible approach goals. This is a critical difference. If a conflict between two approach goals engages BIS, then BIS cannot reflect punishment sensitivity. This would seem to be an important theoretical change.

My current view of approach and avoidance, and of issues pertaining to constraint that are not handled adequately by approach and avoidance alone (Carver, 2005; Carver, Johnson, & Joormann, 2008; Carver & Miller, 2006), reflects the developmental theories of Rothbart, Eisenberg and colleagues (Eisenberg et al., 2004; Rothbart, Ahadi, & Evans, 2000; Rothbart & Posner, 1985; see also Caspi & Shiner, 2006). As shown in Figure 1, these theories posit basic approach and avoidance temperaments. However, they also posit the gradual emergence of a temperament of effortful control, which depends on developing prefrontal executive functions. Effortful control constrains behaviour so that gratification can be delayed, and long-term goals can be attained. It also helps people to do things they do not really feel like doing. Effortful control is the core of trait Conscientiousness (Figure 1).

Together, these temperaments yield a two-layered system of behaviour management (for broader review of two-mode models, see Carver, 2005; Carver et al., 2008). At the lower layer, approach and avoidance tendencies compete, as in Gray's earlier view. If incentives are present and anxiety is low, reward pursuit commences; if anxiety is high, behaviour is inhibited. When the superordinate level is in charge, however, both reactive tendencies (approach and avoidance) can be countermanded. Behaviour can be constrained for reasons other than cues of punishment, and behaviour can be emitted despite the absence of cues of immediate reward.

DOI: 10.1002/per

To my eye, the role of BIS in the revised RST (management of conflict) appears to resemble effortful control more than it resembles the avoidance temperament. If effortful control has bidirectional influence, however (promoting behaviour in the absence of immediate reward as well as inhibiting ill-considered impulses), this would not be strictly an 'inhibition' system.[2]

Serotonin function. I said earlier that different psychological models can promote different interpretations of biological phenomena. As an example, consider serotonin-related genes and serotonin function. It is common for discussions of serotonin function to refer to anxiety-related traits, punishment sensitivity or avoidance, but I believe those labels are misleading (Carver & Miller, 2006; Soubrié, 1986). Serotonin relates more broadly to constraint (Depue & Spoont, 1986; Spoont, 1992). Low serotonin function has been linked to aggression (often), anxiety (sometimes) and depression (often); serotonin has been linked to Agreeableness and Conscientiousness, as well as Neuroticism. Probably the most common correlate of serotonin function is impulsivity.

Why, then, so much focus on anxiety? One likely reason is the theoretical assumption that punishment sensitivity is what causes behavioural inhibition. As noted above, there is good reason to believe that self-control involves more than the balance of reward sensitivity to punishment sensitivity. However, this conclusion (which comes from developmental and other literatures) has not yet made its way to the literature on serotonin.

Here is a case where an emergent model suggests the need to reexamine assumptions about the psychological meaning of a biological phenomenon. With respect to anxiety in particular, two-mode models would suggest that anxiety and anxious behaviour have two co-determinants (*cf.* Depue & Lenzenweger, 2005). A sensitive threat temperament generates anxiety; a weak system of effortful control permits anxiety to spill into behaviour. The overt manifestations of anxiety would be dampened by either decreased sensitivity of the avoidance temperament or increased capacity for effortful control. Thus, there are two possible reasons why increased serotonin function reduces anxiety. I believe, however, that a close examination of the literature bearing on serotonin function suggests that the role of serotonin, here and elsewhere, concerns effortful control (Carver & Miller, 2006; Carver et al., 2008; Spoont, 1992).

More generally, although new physiological techniques provide great opportunities, biological phenomena do not completely dictate interpretation. The biological phenomena must be interpreted through the lens of psychological theory. As psychological theories evolve, new understandings of the biological phenomena may also emerge. The interdependence of psychological and biological runs both ways.

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[2] Other labelling problems also plague RST. Gray labelled the core traits impulsivity and anxiety. But impulsivity reflects lack of executive control as much as or more than sensitivity to incentives. There are also divergent views of what 'anxiety' is; current RST treats it as the state that follows from conflict, even approach–approach conflict, thereby apparently disconnecting it from punishment or threat sensitivity.

A Cognitive-affective Extension to Reinforcement Sensitivity

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Abstract

Smillie (this issue) broaches the issue of going back to the roots of reinforcement sensitivity theory (RST) by using neurobiological paradigms to arrive at biologically validated measurement instruments for the revised RST constructs. Our comments here argue for an integration of cognitive and cortical aspects of approach, avoidance and conflict states and highlights the question whether there are more affective-motivational systems than just those three RST-systems. Copyright © 2008 John Wiley & Sons, Ltd.

Smillie (this issue) does an excellent job of summarising reinforcement sensitivity theory (RST) and assessment approaches promising a neurobiological definition of reinforcement sensitivity. In reviewing recent studies using neuroscientific methods and methodologies like molecular genetics, pharmacologic agents and imaging methods (electroencephalographic, EEG, as well as functional magnetic resonance), he highlights the relevance of approach-avoidance processes in contemporary neuroscience and stresses the meaning of RST as an appealing theoretical framework for psychobiologically oriented research on individual differences.

Although the neurobiological part of the revised RST focuses largely on *subcortical* emotive systems, Wacker, Heldmann, and Stemmler (2003) found that its behavioural architecture (see Figure 2 in the target paper) may also provide a superior account for the relationship between hemispheric EEG alpha lateralisation over anterior *cortical* regions and various emotional and motivational states (see Coan & Allen, 2004, for a distinguished review). The common model of anterior asymmetry assumes left anterior activation to reflect approach-motivated states (e.g. reward motivation: Pizzagalli, Sherwood, Henriques, & Davidson, 2005, or anger: Harmon-Jones, Lueck, Fearn, & Harmon-Jones, 2006) and right anterior activation to depend on withdrawal/avoidance-guided behaviour (e.g. Davidson, Marshall, Tomarken, & Henriques, 2000).

In contrast, our alternative Behavioural Inhibition–Behavioural Activation Model of Anterior Asymmetry (BBMAA, Wacker et al., 2003) relates the left anterior region to motivated behaviour arising either from the BAS or the FFFS and the right anterior region to goal conflict-induced momentary interruption of goal-directed action with concurrent reassessment of behavioural options mediated by the BIS. Hence, putting RST-systems squarely within the BBMAA (see Figure 2) explained patterns of results inconsistent with the common approach-withdrawal model, and also provided new and testable hypotheses about anterior asymmetry.

We recently demonstrated (Wacker, Chavanon, Leue, & Stemmler, 2008) that an avoidant motivational state presumably mediated by the FFFS is associated with less right-lateralised anterior activation than a state of behavioural inhibition assumed to be

DOI: 10.1002/per

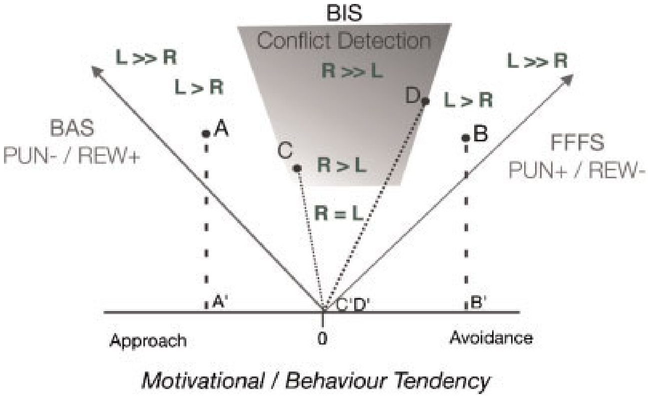

Figure 2. The Behavioural Inhibition–Behavioural Activation Model of Anterior Asymmetry (BBMAA) maps the BAS, BIS and FFFS systems along with their motivational and behavioural effects onto anterior EEG alpha asymmetry. A–D represent different combinations of concurrent BAS and FFFS activation with projections A′–D′ as resulting behaviour tendencies. An approach-motivated behavioural tendency A′ results from stronger BAS than FFFS activation (situation A), an avoidance-motivated behavioural tendency B′, from a predominant FFFS activation (situation B; vertically projecting dashed lines). Both of them are associated with left-sided cortical activation. Behavioural inhibition (C′, D′) and associated right-lateralised cortical activation results from the BIS detecting an approach–avoidance conflict (radially projecting dotted lines). Greyed area indicates combinations of opposing goals signalled by the BIS as a conflict. The size of this area varies among individuals and indicates individual differences in conflict sensitivity. L, left-sided cortical activation; R, right-sided cortical activation. REW+, signals of reward, REW−, nonreward, PUN+, signals of punishment, PUN−, nonpunishment.

mediated by the BIS. Moreover, self-reported FFFS activation was associated with left-lateralised anterior activation supporting our alternative model based on the revised RST. This example reveals the strength of the RST as a useful psychobiological model that helps to operationalise psychological constructs, offer testable predictions and explain inconsistent effects.

Despite the fact that for many biological personality researchers RST is an attractive neurobiobehavioural framework, we believe that some important theoretical specifications might need more emphasis. Smillie's is indeed a broad claim, to 'operationalise reinforcement sensitivity [....] by looking to other literatures—most notable the behavioural, cognitive and clinical neurosciences—for paradigms which have been linked with specific, relevant brain functions' (Smillie, this issue). Here, we plead for an even stronger incorporation of cognitive and cortical correlates, advancing the view that those correlates and their paradigms definitely join in answering the entitled question 'What is reinforcement sensitivity'. One cognitive paradigm we promote is decision-making since conflicts are at the very core of decisions, such as which dessert to choose (approach-approach) or passing an exam (approach-avoidance).

In order to operationalise state BIS-activation beyond classical approach-avoidance conflict detection, we developed an approach-approach conflict paradigm that resembles a gambling/decision-making task. Using this approach-approach conflict paradigm, we could show that different conflict intensities and presumably BIS-activations were associated with early stimulus-processing (N2-component of the event-related potential, see Leue, Chavanon, Wacker, & Stemmler, submitted) and with later occurring lateral frontal asymmetry (Chavanon, Wacker, Leue, & Stemmler, 2007) suggesting different BIS-related processing stages, which are separable in time. This study sheds some light on basic cognitive functions like decision-making and its impact on behaviour.

Central executive functions like updating, goal shielding and decision-making seem to play a pivotal role in goal-pursuit no matter whether it is about an approach or an avoidance goal (e.g. structuring and restructuring cognitive networks in line with goals, means and situational affordances). A major challenge for the future of RST seems to be the differentiation and integration of cognitive subprocesses and timelines beginning with perception and attention via expectation and the determination of possible violations of one's expectations (e.g. updating of information due to a non-stationary environment) to an evaluation of the progress of goal attainment. Disentangling the separate timelines of these subprocesses has been started in animal as well as human research and has shown that brain systems calculate potential gains and losses and update those estimates in the course of behaviour (Kable & Glimcher, 2007).

Our last comment revolves around the broader layout of RST. One aspect is its learning theory foundation and the target paper makes it very clear that reinforcement sensitivity is its central psychological construct. Since its inception, this orientation has kept the flock of personality psychologists in favour with RST quite low in number. Are signals from the environment, unconditional and conditional, the only or even most important information driving the activity of motivational systems in the brain? This is probably true in some cases, but probably not in most or all. Goals, strivings, life tasks but also embodied desires and emotions all have a strong motivational impact on behaviour. Then the question arises whether RST, in describing moment-to-moment motivational tendencies, is a subtheory of a more general motivational personality theory.

A second aspect is the confinement in RST with just two behavioural tendencies, approach and avoidance plus the inhibition of behaviour. Again, molecular behaviour can be broken down into just these three classes. But isn't personality psychology since Allport cognizant of the importance of the *meaning* of a behavioural act instead of just its physical attributes? Consistency of behaviour is not revealed by registering specific acts, for example lying or stealing, but by probing the intention of the actor, for example retaliation or exploitation. This is where emotions come into the picture. The more basic ones, such as expectancy/wanting, warmth, lust, fear, anger, separation distress/sadness and contempt/disgust, are strongly motivating, they have different goals and make for quite different intentions. They probably are biological systems and orchestrate many response systems of the brain. Finally, they could provide just those embodied cognitive-affective units which are most relevant to personality psychologists and clinical psychologists as well as psychiatrists alike.

Psychobiological Research is Crucial for Understanding Human Personality

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Abstract

Human personality is regulated by a developmentally complex hierarchy of three major systems of learning and memory: the procedural learning of habits and skills, the semantic

DOI: 10.1002/per

learning of facts and propositions and the self-aware learning of intuitions and narratives about episodes in one's life. Reinforcement sensitivity theory (RST) considers only habit learning and so is an incomplete model of human personality. It neglects persistence of intermittently reinforced activities and the character traits that regulate conflicts among emotional drives. Nevertheless, psychobiological research is crucial to improve understanding of human personality. Copyright © 2008 John Wiley & Sons, Ltd.

Human beings learn by means of a hierarchy of systems of learning and memory, which includes procedural learning of habits and skills, semantic learning of facts and propositions, and self-aware learning of pre-verbal intuitions and autobiographical narratives about specific episodes in the continuity of one's life (Tulving, 2002). Reinforcement sensitivity theory (RST) focuses on the habit systems, which are an important part of personality but certainly not a complete account. The hierarchical complexity of human personality has strong implications for effective research strategies to understand personality. This complexity could easily undermine the effectiveness of most of the recommendations made by Luke Smillie. Nevertheless, I still confirm and encourage the spirit of much of what he says.

Smillie's most important point is that it is crucial for psychology to move beyond the stagnation caused by overreliance on factor analysis to describe personality. Personality self-reports need to be understood in relation to observations from genetics, neuropharmacology, neuroimaging and social-cognitive descriptions of the situation in which learning is occurring. However, a bottom-up focus on biological mechanisms is likely to be as unproductive as a top-down emphasis on personality self-reports. In my experience, progress in understanding the complex psychobiology of human personality demands an interactive hypothetico-deductive approach (Cloninger, 2004), which I think is the best way to interpret and apply Smillie's insights. It will help to recognise both the strengths and the limitations of Gray's original contributions in order to understand both my agreements and disagreements with Smillie.

Gray made a seminal contribution to understanding personality when he showed that neurotic introversion predicted sensitivity to aversive stimulation whereas neurotic extraversion predicted sensitivity to rewarding stimuli (Gray, 1981). He recommended that within-person differences in rates of response to reinforcement should define the rotation of personality axes. Guided in part by Gray's insight, I initially distinguished three temperament dimensions based on individual differences in associative conditioning: Harm Avoidance (behavioural inhibition or anxiety proneness), Novelty Seeking (behavioural activation or impulsivity) and Reward Dependence (behavioural maintenance, including sociability and persistence) (Cloninger, 1987). Later Persistence was separated from Reward Dependence when all four temperament dimensions were shown to be independently inherited (Heath, Cloninger, & Martin, 1994) and psychometrically distinct (Zuckerman & Cloninger, 1996). Persistence was defined in terms of individual differences in resistance to extinction of previously intermittently reinforced behaviours.

Each of the four temperament dimensions has been subject to extensive research confirming unique genetic antecedents, brain networks and clinical correlates, as detailed elsewhere (Cloninger, 2004; Gillespie, Cloninger, Heath, & Martin, 2003; Turner, Hudson, Butler, & Joyce, 2003). For example, Corr et al. (1995) and Corr, Kumari, Wilson, Checkley and Gray (1997) showed that high Harm Avoidance scores predicted augmented

DOI: 10.1002/per

startle responses when subjects were viewing unpleasant pre-startle images, whereas low Harm Avoidance scores predicted reduced startle responses when subjects were viewing pleasant pre-startle images. Individual differences in Persistence in human volunteers are strongly predictive (r = .8) of the partial reinforcement extinction effect (PREE) through non-linear effects on a neural network involving the ventral striatum, orbitofrontal cortex and anterior cingulate cortex (Gusnard et al., 2003). Such findings illustrate the utility of biological studies as recommended by Smillie.

However, the complexity of the nonlinear relations between personality scores and brain responses required a hypothetico-deductive approach in which both trait description and experimental manipulations were planned to test specific hypotheses. Smillie's insight that it is crucial to understand personality in terms of specific psychobiological mechanisms remains valid, even though there are more than two or three systems needed to understand the dynamics of reinforcement. RST should certainly be extended to take Persistence and PREE into account.

Each of the four temperament dimensions is complex, as expected because it is the behaviour and fitness of organisms as a whole that are subject to evolutionary pressures, not individual genes or neural systems. The components of personality are nonlinear dynamical systems influenced by interactions among many genetic and environmental influences regulating multiple partly overlapping neural systems. Both top-down and bottom-up approaches implicitly assume linear relations among variables, as depicted in Smillie's two figures. Unfortunately, both strategies are inadequate to understand non-linear dynamical systems. Luke Smillie's recommendations can be interpreted in a way that is sound if grounded in a more complete model of human personality and applied as a hypothetico-deductive strategy to refine both personality description and its associated psychobiological mechanisms iteratively.

RST is not merely incomplete because it limits itself to two or three dimensions of associative conditioning. I found that different configurations of temperament were associated with different rates of personality disorder on average, but that both mature and immature people had the same range of temperament configurations. I realised that reinforcement sensitivity was an incomplete account of human personality and so extended my model to measure both temperament and character (Cloninger, Svrakic, & Przybeck, 1993). The character of a person can be described in terms of three branches of mental self-government that regulate conflicts among the emotional drives: Self-directedness (i.e. responsible, purposeful), Cooperativeness (i.e. tolerant, helpful) and Self-transcendence (i.e. self-forgetful, spiritual). These character dimensions are essential to measure differences in the maturity and integration of personality, which are not captured by reinforcement sensitivity. Jeffrey Gray appreciated this limitation, and recommended that studies of reinforcement sensitivity focus on involuntary phenomena, like the startle reflex, in order to minimise the influence of higher cognitive processing. Smillie should avoid the major error of trying to reduce human personality to reinforcement sensitivity.

Consider the error made by behaviourists in trying to explain the development of language in terms of conditioning alone, which is comparable to trying to explain human personality in terms of reinforcement sensitivity alone. The development of language depends on a rich innate endowment of human beings that cannot be explained by a person's genotype or their postnatal experiences (Chomsky, 1980). Personality development is at least as complex as that of language (Cloninger, 2004). Half of the variability in human personality is unique to each individual. Gene–gene and gene–environmental

DOI: 10.1002/per

interactions have as great an effect (25%) on personality as the average effects of individual genes (25%). As a result, the key to progress on the psychobiology of personality is the joint consideration of self-reported and observed behaviours and associated psychobiological mechanisms in a hypothetico-deductive programme of research. Despite my awareness of these challenges, like Smillie, I encourage others to let go of their over-reliance on factor analytical models of personality and work to understand the psychobiology of personality.

An Intermediate-level Approach to Personality: Dissolving the Bottom-up and Top-down Dilemma

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Abstract

Smillie's target paper is a timely reminder of the considerable progress that is being made in the neuroscience of personality. He notes, however, that it is curious that neuroscience-inspired models rely so heavily upon self-report measures, and contends that the use of basic neuroscience paradigms might lead to more concrete understanding of reinforcement processes involved in approach-avoidance. In this commentary, I counsel caution about adopting such a 'bottom-up' approach, and argue that an 'intermediate-level' position is needed: one which operates at the interface of basic neuroscience and statistical-descriptive ('top-down') approaches. Copyright © 2008 John Wiley & Sons, Ltd.

This commentary focuses on a major issue identified by Smillie (this issue), namely '... it is curious to observe so strong a reliance upon self-report measures in the area of personality psychology which has perhaps delved deepest into neuroscience paradigms and biological driven perspectives'. Smillie goes on to note that basic neuroscience research '...has yielded a number of paradigms that may facilitate a more concrete understanding of reinforcement sensitivity [and] they might be argued to more directly and objectively index those functions than self-reported introspections'.

Smillie's paper serves the useful purpose of highlighting some of the main problems to be addressed by approach-avoidance theories of personality (e.g. *reinforcement sensitivity theory*, RST) and, in particular, draws our attention to the problem of defining, in both theoretical and operational terms, 'reinforcement'. However, as these scary apostrophes are meant to indicate, within this broad family of theories, reinforcement has a number of different meanings. Unlike a Skinnerian definition of reinforcement, which is defined in terms of experimental 'operations' and 'responses', with no intervening central states and where the definition is precise (although arguably, this precision is bought at the price of theoretical sterility), approach-avoidance personality theories separate reinforcement defined in terms of (external) operations (e.g. presentation of 'reward') and reinforcement

DOI: 10.1002/per

defined in terms of (internal) central states of emotion/motivation—'reward' here only makes theoretical sense if the experimental subject perceives the operation as rewarding.

Typical (bottom-up) nonhuman animal-based experiments deftly side-step the issue of individual differences by using inbred rat/mouse strains and securing rigorous control of the environment (e.g. inducing thirst by water depletion). As a result, the distinction between reinforcement itself and reinforcement sensitivity is blurred, but it is central to personality psychology.

The reduction or elimination of individual differences can provide the experimental grounds on which neuroscientific theories of behaviour may be built, and we see this in 'state' RST of Gray and McNaughton (2000; for a review of the entire RST field, see Corr, 2008). But, as personality psychologists, we are interested in the 'trait' version of the theory: how differences in the operating parameters of neural systems give rise to differences in 'state' response to stimuli, and how these differences, over the longer time frame, lead to stable personality factors and processes (e.g. Neuroticism). Smillie makes a plea for a proper search for endophenotypes of personality, focusing on four main areas: neuroimaging, pharmacology, behavioural (e.g. category learning) and psychogenomics (a generic name I coined, in 2006, to refer to the delineation of the genetic bases of psychological measures and processes—the latter of which includes 'silent' internal processes such as sensory preconditioning).

Here, we need to note something of importance. RST, for example is not a theory about reinforcement *per se*; it is concerned with central processes that mediate reinforcement (i.e. motivationally salient stimuli). We run the risk of lapsing into a definition of reinforcement purely in terms of reward and punishment (as experimental operations), when what we ought to be focusing on is the facility of reinforcing stimuli to activate affective and motivational central systems, hence the need to focus on reinforcement *sensitivity*. Whether we choose to call these internal systems 'reward' and 'punishment' is a matter of preference, but they should not be confused with the eliciting reinforcing stimuli (a fixed level of 'reward' may be pleasurable for one person, but frustrating for another; see Corr, 2002a,b).

Furthermore, relieving nonpunishment (RNP) and frustrative nonreward (FNR) are 'rewarding' and 'punishing', respectively, but they are not produced by 'reward and punishment, threats and incentive' (Smillie, this issue)—at least, not as defined as external stimuli: they only make sense in terms of the sensitivity of the internal mediating system. Only by a *conceptual* understanding of these central processes can we ever expect to arrive at a sensible interpretation of the effects of reinforcement. The problem for Smillie's position, as I understand it, is that many of the processes central to personality are not evident from a purely endophenoptype approach.

Personality theory is not, and indeed cannot, be a theory of state processes alone (defined either by the endophenotypes outlined by Smillie, or by any other endophenotypes). The understanding of the neuroscience of state processes, especially those relating to reinforcement sensitivity and conflict, *are* fundamental to explicating personality processes; but this approach is *necessary*, not *sufficient*. Personality psychology would seem to demand something more: personality is a 'down-stream' manifestation of multiple interacting state processes, none of which, by themselves (or in simple additive combination), provide the unitary of construct required to define personality (a similar point is made by McNaughton in his commentary). For this major reason, I do not share Smillie's apparent enthusiasm for a predominantly 'bottom-up' approach, to the (relative) exclusion of top-down processes. For theoretical coherence, personality research must address the unity of constructs (e.g. Anxiety), and these constructs cannot be reduced to, or explicated by, neural processes alone (as operationalised by endophenotypes).

Where do we go from here? In place of 'top-down' and 'bottom-up' approaches (which, in any case, in their strict form, do not survive close scrutiny), I would advocate an 'intermediate-level' position. I contend that we need (a) psychometrically rigorous and (b) theoretically faithful measures of personality (neither tied exclusively to basic neuroscience data/knowledge nor statistically derived constructs). We can achieve this intermediate-level position by a *rational* approach to construct and measurement development, designed to achieve concordance with underlying theoretical processes (e.g. the neural modules comprising the *Fight–Flight–Freeze System*). Then, we can relate these psychometric measures to existing (a) 'bottom-up' neural processes (e.g. dopamine and category learning) and (b) 'top-down' personality measures (e.g. Extraversion and Neuroticism). This proposal is not new. It is one of the legacies of Jeffrey Gray—who may lay claim to be the progenitor of the family of approach-avoidance personality theories–which derives its theoretical fecundity from combining the strengths of the conceptual nervous system (cns; including trait personality constructs) and central nervous system (CNS; comprising state neural processes). Like Gray before us, we, too, might expect theory development most fruitfully to sprout at their interface.

Is RST the Newtonian Mechanics of Personality Psychology?

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Abstract

Smillie (this issue) addressed the potential of neuroscience to identify the biological substrates that define the behavioural approach, avoidance and fight–freeze–flight systems that define reinforcement sensitivity theory (RST). Noting both the practical significance of insights into personality from RST and the difficulties in identifying its biological substrates, we suggest that these difficulties may indicate that the theory itself needs further testing. Copyright © 2008 John Wiley & Sons, Ltd.

In his comprehensive review, Smillie (this issue) has outlined four ways in which neuroscience paradigms offer new means to pin down the biological substrates that contribute to the behavioural approach, avoidance and fight–freeze–flight systems that underlie individual differences in personality according to reinforcement sensitivity theory

DOI: 10.1002/per

(RST). As he notes, the potential of RST, or any biologically oriented theory of personality, actually to explain personality is dependent on the identification of the biological mechanisms that contribute to the systems that are considered fundamental to the theory. As he also notes, the identification process has not proceeded completely smoothly to date, as each new study seems to raise as many questions about validity and clarity of measurement as it addresses. Smillie is certainly correct that the proliferation of relatively economical neuroscientific tools makes possible new studies that may help to resolve these questions. But, at least from the studies Smillie has reviewed, most approach these techniques as new opportunities to gather evidence supporting the existence of the systemic constructs the theory proposes rather than opportunities to test whether the theory has proposed the most biologically relevant systems.

Consideration of the behavioural approach, avoidance and fight–freeze–flight systems that RST proposes has definitely generated both demonstrably effective clinical applications and compelling associations with manifested personality traits and motivational and emotional responses. So did Newtonian mechanics in the observable physical world in which we live. Its limitations and inaccuracies were readily apparent, however, as we attempted to understand the physical world at the levels of both interstellar space (the macro level) and particle physics (the micro level). This may be directly relevant to RST in two ways.

First, RST is limited to articulating systems involved in environmental cues to reward and punishment, yet much of both animal and human behaviour (the macro level) is not directly motivated by these cues (Revelle, 2008). Thus, much of personality remains to be explained even if we completely nail the biological underpinnings of the RST systems. It is no mystery what this 'rest of personality' is: it is learned representations of patterns of stimuli, with both habitual patterns of action or restraint of action in response to those stimuli and actions to control and manipulate the circumstances generating the stimuli.

A relevant example comes from the animal literature, which has also contributed substantially to the development of understanding of the RST systems. In rats, maternal infant care is associated with life-long emotional reactivity that can be characterised as sensitivity to punishment (Cavigelli & McClintock, 2003; Francis, Diorio, Liu, & Meaney, 1999). Active maternal care tends to produce animals that have learned, at the macro level, to explore novel environments and to produce more controlled biological responses to induced stress. Poor maternal care, including rough as well as neglectful treatment, tends to produce animals that have learned to refrain from exploring and to show increased biological responses to stress (Meaney et al., 1994). Effects of maternal care are transmitted across generations by subsequent maternal care by the females when they become mothers, and DNA gene methylation (a micro level effect) appears to be the operative mechanism (Weaver et al., 2004). Importantly, rats treated roughly by their mothers show greater attachment to, rather than avoidance of, their abusive mothers (a macro level effect), apparently because attachment increases the chances that the pups can obtain other macro level effects in the form of food and other support until weaning (Sullivan, Landers, Leaman, & Wilson, 2000). Obviously, both reward and punishment systems are operative here, yet the macro level attachment behaviour reflects some level of choice on the part of the pup to tolerate the short-term punishment in order to improve the chances of long-term reward in the form of survival. There is a known mode of micro level transmission of the effect that appears to garble the RST systems.

This example also has implications for the second way the analogy of Newtonian mechanics may be relevant to RST. Recent work in both clinical psychology and

DOI: 10.1002/per

psychometrics has distinguished between distress propensity and distress tolerance (van Overveld, de Jong, Peters, Cavanaugh, & Davies, 2006; Simons & Gaher, 2005; Stein, Schork, & Gerlernter, 2008, Taylor, Zwolensky, & Cox, 2007). Like the rat with the abusive mother, even the person with the most hyper-reactive avoidance system (whether BIS or fight–freeze–flight) may have or develop the ability to tolerate punishment in return for some long-term reward. Indeed, recent work in clinical psychology, in particular cognitive behavioural therapy, suggests that psychological functioning improves as much by increasing tolerance as by decreasing distress. Perhaps tolerance is a system that RST should incorporate in its search for micro-level biology.

Moreover, clinical psychology, and psychology in general, is oriented towards understanding distress as distinct from enjoyment for obvious practical reasons. This orientation towards distress may have evolutionary origins as well, as perception of threat is often more important to survival than perception of opportunity. But it seems at least possible that the distinction between avoidance and approach, or distress and enjoyment, is secondary to the system's overall reactivity. RST has developed evidence for the separation of approach and avoidance systems, but all of this evidence depends on a pre-existing categorisation of the stimuli as positive or negative. That is, our understanding of the biological substrates involved in the RST systems is based on experiments that delineate the biological responses that are activated *after* a stimulus has been categorised as positive or negative. What if the truly operative construct at the micro biological level is reactivity to stimuli *before* categorisation? Testing possibilities that compete with the most fundamental tenets of a theory is the basis of scientific inquiry, and should be part of RST research.

Neuroscience undoubtedly can help us understand how biology contributes to personality, which is the goal we all share. But maybe RST will contribute most not by sticking to its existing Newtonian mechanical explanations but by developing theoretical and experimental paradigms that, like special relativity and particle physics, address the pieces of the puzzle that haven't quite fit at either the macro or micro levels.

Challenges to Personality Neuroscience: Measurement, Complexity and Adaptation

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Abstract

Reinforcement sensitivity theory (RST) makes an important contribution to understanding the neuroscience of personality. However, it faces significant challenges. This commentary identifies three that appear pivotal—development of adequate measurement models, accommodating the complexity of mappings between component neural processes and behaviour and establishing that individuals vary in generic strategies for adapting to environmental reinforcers. Copyright © 2008 John Wiley & Sons, Ltd.

DOI: 10.1002/per

Smillie's target paper provides an accessible summary of the state-of-the-art in reinforcement sensitivity theory (RST: Corr, 2004), but its original contribution is primarily methodological. The paper discusses four different experimental paradigms that might be used for theory testing and development. Smillie provides some well-chosen example studies that illustrate how research may proceed. I agree with the author that these lines of research are likely to be informative. However, in this commentary I will focus on some of the potential pitfalls that may lie in wait (see Matthews, 2004, 2008a, for more detailed analyses of RST).

Measurement issues. Smillie discusses bottom-up approaches to elucidating the neuroscience of personality. For example, we can isolate relevant genes or neurological attributes, and trace the consequences of individual differences in these biological building blocks for behaviour and personality. However, the paper sidesteps the key issue of how to build a psychometrically sound measurement model from such research. If we measured sensitivity to reward through (1) an assay of relevant quantitative trait loci (QTLs), (2) measurement of behavioural responses to dopaminergic drugs and (3) through imaging activity in reward centres, would we find a single latent trait for sensitivity? Or would we find only paradigm-specific traits unrelated to any over-arching construct?

The psychometric approach remains to be explored, but there are some reasons for pessimism, or at least caution. The vicissitudes of attempts to validate the Eysenck (1967) theory of personality through psychophysiology illustrate the difficulty of adequate measurement of broad constructs such as arousal or conditionability. Indeed, the predictive failures of the Eysenck theory discussed by Matthews and Gilliland (1999) exemplify the dangers of basing a theory on a construct (cortical arousal) that resists valid measurement. Will sensitivities to reward and punishment prove to be any more tractable as dimensional constructs? It remains to be seen. Smillie's (this issue) statement that dopamine (DA) supports multiple functions, and, in any case, DA should not be equated with reward sensitivity, indicates potential sources of measurement difficulty.

Emergence and complexity. Like most biological theories, RST assumes that simple mappings exist between parameters of neurological functioning and personality. This assumption may not be correct. As Smillie acknowledges, genes interact with one another, as do neurotransmitter systems. Indeed, Corr's (2004) recent revision of RST incorporates a joint subsystems hypothesis that allows for interaction between the brain systems described by the theory. Zuckerman's (1991) critique of biological theories of personality—to which Smillie does not do full justice—emphasises that traits are not necessarily isomorphic with any single neurological construct. Traits emerge from multiple neurological and cognitive components, and a given neural system may contribute to multiple traits.

Turkheimer (2000), writing about behaviour genetics, points out that the dominant feature of personality development is its interactivity. Not only do multiple genes interact, but gene expression is modulated by environmental factors, which in turn affect the future environments to which the person is exposed. Thus, causal, mechanistic processes leading from genes (or environmental factors) to behaviours are not readily identified.

Smillie (this issue, footnote 3) seeks to insulate the neuroscience of personality from high-order cognition. This may not be possible: all emotions—whether simple or 'highly interpreted'—may be shaped by the top-down influence of cognition and verbal, semantic analysis (Averill, 1997). In relation to the experimental paradigms recommended by Smillie:

DOI: 10.1002/per

(1) gene expression is influenced by environmental exposures (*cf.* Turkheimer, 2000), which, in turn, are affected by cognitively informed voluntary choices of activities;
(2) drug effects on behaviour are influenced by expectancies;
(3) activations of brain regions are influenced by high-order cognition, mediated, for example by descending pathways from neocortex to amygdala;
(4) decision-making is powerfully shaped by cognitive strategies and heuristics evidenced by biases such as framing effects.

If traits emerge from multiple neural and cognitive processes, it may not be possible to explain their impact on behaviour solely through neuroscience; the methodological challenge is to differentiate personality phenomena better suited to either neurological or cognitive-symbolic explanations (Matthews, 2008a,b).

Traits as individual differences in adaptation. Biological theories of personality have persistent difficulties in handling the person–environment interaction which is a central tenet of modern personality theory. Just as different arousing stressors may have differing effects on behaviour, so too it is unclear that different rewarding and punishing agents have equivalent effects as moderators of personality—behaviour associations. Consistent with the interactivity principle previously described, the problem is, in part, that people are likely to attach different meanings to equivalent reinforcers, with consequent differences in behavioural outcomes. A variety of cognitive biases linked to personality show that personal meanings diverge from the objective qualities of stimuli (Matthews, Schwean, Campbell, Saklofske, & Mohamed, 2000).

Environmental factors may perhaps be parsed into levels of reward and punishment, but such an approach may be over-simplified. The cognitive-adaptive theory of personality (Matthews, 2008a) proposes that traits correspond to more differentiated sets of environmental challenges central to human existence: challenges which may be managed through different strategies. Specifically, extraversion relates not to handling of reward in general, but to management of social pressures and challenges. Likewise, neuroticism relates to anticipation of predominantly social threats, rather than to punishment sensitivity in general.

These correspondences may reflect the difficulties, within a human ecology, of adopting a generic strategy for responding to reward and punishment stimuli. The strategic choices for dealing with a dangerous snake (straightforward fight or flight) may be quite different from the options available for dealing with an aggressive family member. Neither avoidance nor violence is likely to be very effective in the latter case (see Matthews, 2004, for an account of the special adaptive challenges of social threat). This is not to say that brain motivation systems are irrelevant to these traits—rather, that individual differences in reinforcement sensitivity are just one element of a more wide-ranging adaptation distributed across multiple neural and cognitive systems.

Conclusion. RST is an important personality theory, and the various empirical approaches described by Smillie are well worth pursuing. We will not know whether RST succeeds until more evidence is available. This commentary identifies three challenges for this research. Can we derive broad-based, psychometrically sound traits using neuroscience measures? Can we establish robust mappings between neural attributes of the individual and behaviour? Can we establish that individuals differ in their strategies for adapting to rewarding and punishing environments in a generic sense? The worst-case scenario is that research is not able to move beyond a series of disconnected demonstrations that there are linkages between personality, genes and brain systems, so that evidence cannot support any detailed theorising. A more optimistic scenario is that RST will grow to accommodate the issues raised here.

Unscrambling the Personality Omelette

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Abstract

Some conceptual issues are important if reinforcement sensitivity is to be analysed as Smillie suggests. To assess sensitivity, pharmacology should involve chronic administration and drugs that modulate neurotransmitters rather than act like them. Sensitivity may involve more than a simple increase in output of systems for a given level of input. A unitary sensitivity factor at the behavioural level can involve many genes acting on multiple transmitter systems. These modulatory systems must be separate from the areas that control the measured behaviours. Finally, sensitivity of the behavioural inhibition system will be distinct from sensitivity to threat and affect. Copyright © 2008 John Wiley & Sons, Ltd.

Smillie (this issue) considers how to answer the question 'What is reinforcement sensitivity?'. He starts by quoting me and so it is not surprising that I agree with much of what he says, in particular: (a) his focus is on assessing reinforcement sensitivity as directly as possible; (b) he advocates multiple, quite distinct, methods for this assessment and (c) he does not prejudge the answer—although classes of answer are indicated at various points. This said, there are a number of issues where I think a different perspective or emphasis could help those attempting to 'unscramble the personality omelette'.

The 'omelette' of my title derives from Jeffrey Gray's view that the modifications that I made (Gray & McNaughton, 2000) to the state version of his original theory (Gray, 1982) had shattered its conceptual unity. As he put it in a section heading of the book (p. 282) 'Humpty Dumpty had a great fall'. In his writing of the personality chapter of the book (pp. 333–349), he attempted 'Putting Humpty Dumpty together again'. However, at the neural level, I am convinced that his choice of original analogy was right and that 'All the King's horses and all the King's men [cannot] put Humpty together again'—or at least, not as an egg. The reverse process is required. 'You can't make a [personality] omelette without breaking [neural] eggs' but, of course, if you break a number of eggs and then follow the right recipe you can produce a largely unitary and highly desirable omelette.

I will not attempt a recipe for such an omelette. As I have already said, a strong point of Smillie's paper is that it offers methods for approaching this desirable end without making the error of trying to achieve it too fast. Rather I will go over what I see as some of the critical ingredients (many touched on Smillie but, if so, with what is in my opinion insufficient emphasis).

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First and foremost is the distinction between 'reinforcement' and 'reinforcement sensitivity'. The latter must clearly be seen as a conceptual parameter that relates the amount of reinforcement input to some output. It is not reinforcement itself. There is good reason to suppose that the dopaminergic system is the brain's reward system (Reynolds, Hyland, & Wickens, 2001). As Smillie suggests, the effects of cocaine (because it blocks the reuptake of dopamine and so increases its effect) can give us an indication of likely effects in changes in reward sensitivity. But, caution should be exercised since acutely administered cocaine is itself rewarding. It may not provide a good model of chronic changes in *sensitivity*. This problem does not occur with the normal use of specific reuptake inhibitors of noradrenaline and serotonin as these are usually administered chronically. The benzodiazepines, essentially the paradigm drugs for Gray's (1982) theory, have a particularly interesting action in this context. They modulate (and can do so up or down) the effect of release of the neurotransmitter GABA, have little effect if GABA is not released and their antagonists are not GABA antagonists (for reviews see Haefely, Martin, Richards, & Schoch, 1993; Sieghart, 2006). They thus alter the sensitivity of the system rather than acting (as does a drug like muscimol) just like the transmitter itself. Stable endogenous levels of a benzodiazepine agonist hormone or endogenous neurosteroid (Hosie, Wilkins, da Silva, & Smart, 2006) are, thus, the mostly likely basis for a factor of BIS sensitivity (but not neuroticism or general threat sensitivity).

Second, is the nature of 'sensitivity' itself. Greater sensitivity might be expected to produce greater responses. But the Maudsley Reactive rats, our current best animal model of neuroticism, do not have a uniformly greater response to aversive events than the non-reactive (Broadhurst, 1957). Rather, they can best be viewed as having a wider dynamic range with roughly the same mean. They have greater responses to weak stimuli but can have lesser responses to strong stimuli (McNaughton, 1989, Figure 11.2). This factor alone could give rise to many results that are apparently counter to prediction in recent tests of RST.

Third, an apparently unitary personality system could result from interactions among multiple transmitter systems with the sensitivity of each such system depending on a variety of different biochemical and so genetic elements. Both specific noradrenergic and specific serotonergic drugs can ameliorate depression and do so only over long time scales suggesting a common impact on other neural or biochemical factors. Levels of serotonin, for example will be affected not only by changes in reuptake systems (which can be serotoning specific) but also breakdown by monoamine oxidase (which will affect noradrenaline as well) and by changes in various points of the serotonin synthesis pathway. Thus, multiple genetic influences could impact on either or both neurotransmitter system and result in long-term changes in a final common path that is detected, at the behavioural level, as an apparently single personality factor.

Fourth, no specific behaviour or neural element in a defense hierarchy should be identified with sensitivity. The shattering of Gray's original theory into many neural pieces allows each to account for a specific aspect of defensive behaviour: freezing, defection, risk assessment, etc.—and of the varying symptoms with anxiety disorders. But which of these occurs in any particular situation depends on defensive distance (R. T. Blanchard & D. C. Blanchard, 1990a,b) and this is, essentially, the immediacy or magnitude of perceived threat. Thus, sensitivity acts on a system that selects among the 'shattered' elements and must therefore be, relative to them, a unitary system. The monoamine systems are prime candidates here because they project to all levels of the systems controlling fear and anxiety.

Finally, and perhaps most difficult to deal with in the simple verbal terms that can give rise to personality questionnaires, is the relation of the BIS to anxiety and memory. The output of the BIS is not anxiety itself. With clinical anxiety, the drugs that define the BIS can take weeks to have a full therapeutic effect. They can be viewed as producing the type of anterograde amnesia typical of hippocampal damage but in a milder, aversive-specific form. Further, 'anxiolytic' drugs tend to be amnestic—as is reasonable given their hippocampal final common path for action. Critically, the theta rhythm that is at the core of both the old and new theories of the BIS can predict both anxiolytic and pro-cognitive drug action (McNaughton, Kocsis, & Hajós, 2007). It follows that BIS sensitivity affects cognitive–emotional interactions that can alter anxiety or memory but only at one remove. The search for BIS sensitivity (that can then modulate aversive responses when conflict is present) should not, therefore, be confused with the more general sensitivity to threat that operates via the FFFS and may underlie personality factors such as neuroticism.

Switching the Perspective: From Neuroscience to Personality

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Abstract

The review by Smillie (this issue) proposes a critical perspective on traditional personality psychology. He gives an overview on neuroscientific approaches to Gray's reinforcement sensitivity theory (RST) and suggests that a 'bottom up approach' to personality is necessary to overcome a circular reasoning from questionnaire items to biological underpinnings of personality. Smillie's view is challenging for the traditional lexical approach but on the other side this is the way how personality research is likely to be successful in the future. Copyright © 2008 John Wiley & Sons, Ltd.

Individual differences in personality traits are defined as habitual differences in the response to emotional stimuli. However, many although prominent personality theories neglect the origin of emotion, namely the brain, in their conceptualisation of personality. This is a profound shortcoming that was already addressed in the 1970s by Gray (1973) who deliberately investigated the processing of emotional stimuli in animal and human models of behaviour. Considering biological variables with respect to personality research is a gigantic challenge partly caused by the complexity of the brain. The problems encountered by this endeavour are also reflected in the need for a refinement of reinforcement sensitivity theory (RST) by Gray and McNaughton (2000) about three decades after the first version of RST has been published.

The reconceptualisation of RST is the straightforward response to recent findings derived from the use of neuroscientific techniques that had not been available in the past. We must be aware that the processing of emotions is substantially regulated by phylogenetically older parts of the brain that are highly conserved across species (Panksepp, 1998). Therefore, Gray was completely right to use animal models for the development of his RST. Formerly specific methods suitable to investigate the neuronal basis of personality were only applicable in animal models. Alternative methods with similar results are now available in humans like, for example imaging studies and molecular genetics and are already applied with respect to RST (e.g. Reuter, Schmitz, Corr, & Hennig, 2006; Reuter et al., 2004). Now we have to integrate these methods to take the inheritance of Jeffrey Gray.

It is obvious that the neuroscientific approach to personality research is in a great dilemma when biological measures of reinforcement sensitivity are only accepted if they correlate with personality questionnaires. Smillie describes this research strategy as the ‘top down approach’. The top down approach starts with a psychometric conceptualisation of personality and later on strives to identify the biological origins of personality based on correlations with self-report questionnaires.

It would be a challenging view to turn the tables with the consequence that items of personality questionnaires have to correspond to behavioural or biological measures of reinforcement sensitivity and not *vice versa*. This strategy is called the ‘bottom up approach’. Presently, it seems that the ‘top down approach’ is still regnant. For instance, a genetic association study reporting a positive association with a questionnaire trait is celebrated, a negative finding is interpreted with the conclusion that the gene loci under investigation are not related to personality. Smillie brings it to the point by writing that presently we have to ‘validate a potential neural signature of reward sensitivity by correlating it with a questionnaire’. In my view, the opposite should be the case and Smillie again provides an excellent argument for this position ‘it seems biologically implausible to suggest that individuals can introspect directly about their reinforcement sensitivity—that is, consciously access the operational parameters of the BAS, BIS and FFFS—and report this on a personality questionnaire’ (see also Pickering, 2008).

The question arises why personality psychologists do not directly change their strategy from the ‘top down’ to the ‘bottom up’ approach. First, this would mean to break with an old tradition and second, neuroscientific methods are complex, expensive and need expertise that cannot be acquired within a short time. It is plausible that these are bad reasons not to turn to the neuroscientific approach.

We are of the opinion that personality psychology will have no rosy future at all if new approaches from the neurosciences are not integrated. Only the inclusion of biological variables will bring us away from the simple behaviourist S-R model to the point where we ask why contingencies between a certain stimulus (e.g. signalling reward) and a certain response are observable. It is the affective and motivational properties of the stimulus, modulated by cognitive evaluations, which determine behaviour. However, emotion, motivation and cognition have their origin in the brain. So why not addressing the roots of behaviour originating from the brain?

This plea for the neuroscientific approach is not intended to discredit traditional personality psychology nor do we want to belittle the merits of some successful decades of personality research. What we need is the open mindedness for new techniques. Even if we do not want to become experts in, for example fMRI, genetics or EEG, there exists the chance to cooperate with those researchers who are familiar with these methods.

Without any doubt we still have a long way to go. We can criticise that we do not exactly know what a certain BOLD response is indicating about underlying brain structures and we are far away from knowing the genetic basis of personality. Obviously emotions underlying personality are also complex (Ortony, Norman, & Revelle, 2005). Therefore, we have to concentrate on endophenotypes of emotions (Eisenberg et al., 2007). This has already been done by using experimental approaches like the affective startle reflex modulation or reinforcement learning paradigms (e.g. Cohen, 2007; Corr et al., 1995).

So what are the open tasks to guarantee a prosperious future to personality psychology? First, we have to combine experimental approaches to RST with neuroscientific approaches. A multi-method approach will be most suitable, that is the combination of different neuroscientific techniques applied simultaneously to endophenotypes of RST in an experimental setting. Second, we have to promote the 'bottom up' approach of personality research. This means that we have to identify those self-report items describing personality that suit best to biological and experimental conceptualisations of RST and not dogmatically *vice versa*. Third, personality researchers have to aquire neuroscientific techniques or import them by interdisciplinary cooperations. Fourth, we have to prove intellectual open mindedness to a fundamental new look on personality research that is triggered by the 'bottom up approach'.

However, we have to agree with Luke Smillie. It is likely that the combination of the 'bottom up and the top down approach' is most promising because a puristic 'bottom up approach' bears the risk to extrapolate biological mechanisms to RST where this is not legitimate.

Personality is More than Reinforcement Sensitivity

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Abstract

Personality is the coherent patterning across time and space of Affect, Behaviour, Cognition and Desire (the ABCDs of personality). Smillie (this issue) shows the power of RST to address how we feel (Affect) and act (Behaviour) but ignores how we think (Cognition) and want (Desire). Reinforcement sensitivity theory (RST) needs to consider how cognitive framing and goal structures modify the basic approach and avoidance processes of human activity. Incorporating multilevel models of cognitive controls of affect and behaviour into RST has the potential to broaden its framework for applications to complex human endeavours. Copyright © 2008 John Wiley & Sons, Ltd.

For years, psychologists have grappled with two fundamental behavioural dimensions: the tendency to approach and engage in activities associated with reward and the tendency to inhibit and disengage from activities associated with punishment. The basic nature of these

DOI: 10.1002/per

dimensions is demonstrated by their appearance in species ranging from the flatworm to the human (Schneirla, 1959), and their pervasiveness is demonstrated by their appearance in high-level constructs such as achievement motivation in college students (Atkinson, 1957, 1974; Elliot & Church, 1997; Elliot & Thrash, 2002). Research conducted in order to identify the biological causes of approach and avoidance behaviour is of paramount importance. Smillie's target paper is a *tour de force* of the biological underpinnings of approach and avoidance dimensions from the conceptual framework of reinforcement sensitivity theory, RST (Corr, 2002a,b, 2008; Gray, 1981, 1982; Gray & McNaughton, 2000; Pickering & Gray, 1999).

RST is a bottom-up, biological model (based primarily on rat data) organised around approach and avoidance dimensions involving three separate biological systems: Behavioural Activation, Fight/Freeze/Flight and Behavioural Inhibition (BAS-FFFS-BIS). Smillie has summarised the recent evidence (see also Corr, 2008) for how these three systems interact to produce affects and behaviours associated with human personality. Although Gray and McNaughton (2000) argued that RST's bottom-up approach is necessary to understand the biological mechanisms of personality and contrasted this approach with the top down descriptive taxonomic approach associated with either the 'Giant 3' dimensions of Eysenck (1967, 1997) or the 'Big 5' of Costa and McCrae (1992) or Goldberg (1990), we agree with Smillie's suggestion that top down concepts provide scaffolding for the models of the biological processes.

To us, personality is the integrated patterning over time and space of Affect, Behaviour, Cognition and Desire (the ABCDs of personality). That is, a person is understood by his or her coherent patterning of feelings, actions, thoughts and goals. The three systems of RST are primarily associated with a subset of the ABCDs: affective and behavioural outcomes. They are thus compatible with personality descriptions that emphasise affect—for example Tellegen, Watson, and Clark (1999) or Watson and Clark's interpretation of the Giant 3 in terms of positive emotionality, negative emotionality and behavioural constraint (Watson & Clark, 1992). However, RST does not well address another subset of the ABCDs: cognition and desire.

Matthews (2008b) has argued that animal-based models such as RST are not applicable to humans and proposes that an understanding of human behaviour requires a cognitive explanation. We believe, however, that an adequate model must be biologically and evolutionarily plausible in terms of its affective and behavioural components and may be derived from animal data, while at the same time it must go beyond the rodent and be able to explain the cognitive complexity of the human. Consider the case of a college student failing to learn difficult material. Consistent with RST, anxious students given this situation tend to disengage from the task, experience heightened levels of state anxiety and as a consequence, do poorly. But when the task is reframed by emphasising that the material is difficult and that the failure is not due to the students' ability, but rather to the difficulty of the task, the performance of the student improves drastically (Born, Revelle, & Pinto, 2002; Triesman, 1992; Weiner & Schneider, 1971). This simple example shows how the cognitive interpretation of a situation can modify the behavioural and affective sensitivities associated with RST and introduces the concept of levels of control (Broadbent, 1971; Carver & Scheier, 1982; Revelle, 1993). Much of our affect and behaviour is automatic or routine with little cognitive control. It is only when situations demand less routine responses that cognitive controls and goal structures override the more basic RST systems and become important in guiding our actions and feelings (Ortony, Norman, & Revelle, 2005).

DOI: 10.1002/per

Cognitive interpretations also contribute to the inherent ambiguity involved in defining reinforcement, which Smillie in particular and RST in general ignore. Except for unconditioned rewards and punishments and biologically prepared stimuli (Öhman & Mineka, 2001), a challenge for RST is to identify what is rewarding and punishing. To the naïve midwesterner, a windsurfing board is nothing more than a fiberglass plank. To those who have fallen off such a board into very cold water, the board is a cue for punishment and a source of anxiety. To those more experienced sailors, such a board is a cue for excitement and reward.

A fundamental challenge to any theory of personality is how to measure the basic constructs. Because we are unable to introspect accurately about the activity of our BAS, FFFS or BIS (Pickering, 2008), Smillie is correct for going beyond self-report measurements. However, we believe that Smillie has made a mistake by giving primacy to biological measures of reward sensitivity over behavioural measures. Biological and behavioural markers are both indirect measures of the latent variable, reinforcement sensitivity. Latent variables are manifested through the tangible and quantifiable imprints they make on the world, which include biological markers such as dopamine reactivity as well as behavioural markers such as attending lively parties. As such, we see no reason for behavioural markers to be subjugated to a lesser status.

It is important to realise that RST is not just of interest to the biological modeler, but is of relevance to the broader psychological community. One example illustrating RST's broad applications comes from Furnham and Jackson (2008) who discuss how RST concepts can be seen as distal causes that combine with situational demands to affect behaviour in organisational settings. More generally, this approach fits within the potentially fruitful paradigm of testing social psychological theories discussing the tendency to engage in promotion of gains versus prevention of losses (Liberman, Molden, Idson, & Higgins, 2001) in terms of the three systems of RST. We believe that similar efforts would significantly broaden the scope of RST beyond that presented by Smillie, possibly presenting a unified theory of personality across the ABCD domains.

DOI: 10.1002/per