Super siesta!

A nap in the afternoon could prevent information overload. That is the message from a study conducted by Sara Mednick and her colleagues at Harvard University's psychology department [Mednick, S.C. *et al.* (2002) *Nat. Neurosci.* 5, 677–681]. Mednick tested 139 undergraduates on a 'texture discrimination task', 4 times in 1 day. The students had to report the orientation of a target-array of bars presented very briefly against a background of horizontal bars. For each student, the length of time the target-array was visible was manipulated until they achieved 80% accuracy. Mednick found that with each testing session, the students needed more time to respond accurately, but that performance could be restored by a nap of 30 to 60 minutes between sessions. And the longer the nap the better: 60 minutes gave more time for slow-wave sleep, the crucial factor for restoration of performance. A rest without sleeping did not help. Importantly, though, performance could be restored without sleep if stimuli were presented on the opposite side of the screen, suggesting that deterioration of performance was caused by region-specific loss of function in the brain, not general fatigue. So either have a nap after lunch or at least don't do the same thing all day; perhaps have a word with your boss about the afternoon timetable! *CBJ*

Harsh conditions make birds brainy

Living an easy life might not be such a good thing after all. A recent experiment has shown that birds that live in a harsh climate have bigger brains than those that enjoy a mild climate [Pravosudov, V.V. and Clayton, N.S. (2002) *Behavioural Neurosci.* 116, 515–522]. The researchers compared chickadees from two different areas, Alaska and Colorado, and their ability to locate a previously hidden food cache. Alaskan chickadees were more efficient at finding their hidden supplies, suggesting that they had a better spatial memory. Analysing the birds' brains supported this hypothesis, showing that hippocampal formation was significantly larger in Alaskan chickadees. It would appear that the Alaskan chickadee has evolved specific brain properties to adapt to its harsh environment. As the human brain is similar to the bird brain at the cellular and sub-cellular level, this study might help us appreciate how our own brains change during learning and memory. *KR*

In Brief articles written by Heidi Johansen-Berg, Christian Beresford Jarrett and Kate Ravilious.

Letters

Debate on Introspection

Establishing a legitimate relationship with introspection

Response to Jack and Roepstorff

Psychologists' relationship with introspection is much like that between men and women: it is on again, off again and psychologists often feel they can neither live with introspection nor without it. In their often compelling article, Jack and Roepstorff argue that the fertility of the field depends on psychologists reuniting with the practice of introspection [1]. They suggest that, although reluctant to admit it, psychologists have been carrying on a surreptitious relationship with introspection that they should come clean and admit. Although mindful that challenges exist, Jack and Roepstorff suggest that psychologists can safely embrace introspection and spawn important new insights.

I am sympathetic to Jack and Roepstorff's attempt to mend the divisions that have led many, though certainly not all, psychologists to spurn introspection. My discussion (pp. 339–344 in same issue) of the various ways in which people can fail to apprehend their own experience is not meant to undermine reconciliation between psychologists and introspection. To the contrary, by conceptualizing the manner in which introspection can be dissociated from experience, I hope we can anticipate potential rough spots in the relationship before unexpected difficulties further sour psychologist's trust in introspection. As in any relationship, trust necessarily depends on an understanding of differences.

When couples commit themselves to enduring relationships, they must recognize not only the anticipated pleasures but also the hardships. A danger for any relationship is the underestimation of the challenges that it faces. Although Jack and Roepstorff pay lip service to these challenges, their admonitions are insufficiently strong. They suggest, for example, that in order to apply introspective methods successfully, researchers '…need do scarcely more than read the rest of this [Jack and Roepstorff's] article.' This, they argue, is because researchers '…are already highly familiar with the evidence.' This logic sounds dangerously close to Woody Allen's classic line that he is such a good lover 'because I practise a lot on my own'. One's own personal experiences, no matter how compelling they may seem, are a very dangerous proxy for those of others. How many relationships have suffered because one person presupposes that what is good for them must necessarily be good for their partner? It is a similar leap to assume that because researchers feel they can naturally introspect about their own experiences, that they should be similarly able to interpret the introspections of others.

Indeed it is questionable how confident researchers can be in their own introspections. One of the central implications of dissociations between consciousness and meta-consciousness is that individuals, presumably including researchers, can misrepresent their experiences to themselves. Jack and Roepstorff assert, '…there is also a sense in which subjects simply cannot be wrong about their own experiential states.'

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Presumably they arrived at this conclusion by drawing on the seemingly self-evident quality of their own introspections, and assumed that it must equally apply to others. However, when we consider research on the topic, this conclusion seems less self-evident. If, for example, extensive introspection can cause people to make decisions that they later regret [2], then one very reasonable possibility is that the introspection caused them to 'lose touch with their feelings'. In short, empirical studies suggest that people can fail to appraise adequately (i.e. are wrong about) their own experiential states.

If researchers can neither have complete confidence in their own introspections nor those of their participants, then how do we insure a successful relationship between psychologists and introspection? Three strategies (also fundamental to sound personal relationships) seem important: identifying behaviors that establish credibility, finding common ground that enables mutual understanding, and developing a trust that allows one to know when to give the benefit of the doubt.

In relationships it is often said that 'actions speak louder than words'. This expression does not mean that words are unimportant but rather that they are only meaningful if validated by one's actions. Similarly, when people report strategies, feelings or beliefs, their accompanying behaviors must correspond with these statements if they are to be believed. If, as can happen, a person says that they prefer one item and then selects another [3], then our confidence in their account is shaken. Introspective credibility is also challenged when individuals say they used a strategy (e.g. deliberately moving an object) when they actually had no control [4]. Cases in which reflection alters (and particularly when it impairs) performance also raise questions about the accuracy of the reflections [2,5–8]. In short, when words and actions correspond we can have greater confidence in introspections than when they conflict.

Also central to any relationship is the establishment of common ground, helping to insure that both individuals see eye to eye. In the case of introspection, developing common ground can be achieved in various ways. By refining the use of language, individuals can be trained to consistently map particular expressions on to particular experiences. Such linguistic training enables people to avoid the verbal overshadowing effects that translations of experience can otherwise produce [9]. Common ground can also be established between introspective reports and physiological measures. Jack and Roepstorff cite a study by Lutz *et al.* [10] in which subjects' categorizations of different perceptual experience consistently mapped on to unique EEG responses. Once such mappings have been established, then future introspections can be evaluated by assessing the degree to which self-reports co-occur with established associated physiological responses.

Perhaps the most critical element of any successful relationship is trust. This is certainly the case for psychologists and introspection, where demonstrations of questionable introspections [11] have undermined psychologists' willingness to rely upon this source of evidence. As in relationships however, it is important to have a theory about when to hold people at their word and when to shrug off their comments. We learn to disregard a partner's impetuous remarks after a difficult day, knowing that such comments often do not represent enduring feelings. Similarly, as we refine our understanding of when experiences are likely to become dissociated from meta-consciousness, we can increasingly predict when introspections are accurate and when they are suspect. As in human relationships, we must be aware of when it is appropriate to be suspicious and when we can trust introspection.

Needless to say, some psychologists will be unhappy about forging a relationship with introspection that requires the establishment of some trust. Just as finding a mate is not a necessity for everyone, we can certainly afford to have some researchers avoid embracing introspection. However, if the entire field spurned introspection, psychology would quickly become sterile. Given that many of us already tacitly rely upon introspection as a fertile source of inspiration and validation, we should formalize psychology's relationship with introspection and finally make it legitimate.

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The 'measurement problem' for experience: damaging flaw or intriguing puzzle?

Response to Schooler

Jonathan Schooler [1] aptly warns of the 'potential dangers of using self-reports as an index of consciousness.' He is absolutely right to do so, and his experimental work

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helps illustrate how attempts to measure experience can alter it. In his experiment involving mind-wandering while reading [2] we see how the experimenter's probe causes the subject to switch his attention from merely thinking, to thinking about what he is thinking [3]; this, in turn, causes the subject to stop 'zoning-out' and so alters his ongoing experience. In addition, Schooler provides other intriguing experimental examples of how self-reports can disrupt or alter ongoing processing [4]. It is exactly because the act of probing experience involves and depends upon cognitive processes that we have emphasized the importance of recognizing the interaction between experimenter and subject. The quality of information obtained from reports depends on the experimenter's awareness of the influence of his/her questions, and his/her sensitivity to the subject's communicative intentions. Only by keeping track of this interaction, and making it explicit, can we hope to develop an objective understanding of experience; hence, our claim that this 'second-person perspective' needs to be recognized and incorporated into scientific accounts.

We might say there is a 'measurement problem' for experience. As cognitive scientists our goal is to relate the subjects' experiences to their cognitive and neural processes, yet the equation is confused by the fact that any attempt to observe experience will itself engage and alter processing. One result is a trade-off: immediate measurements of experience are accurate, but disrupt ongoing processing [2,4]. Retrospective reports aren't disruptive, but they depend on the vagaries of memory.

The English have a saying: 'A bad workman blames his tools.' The good worker concentrates on figuring out how to use those tools better. There are two kinds of response to the problems of measuring experience. One response is to point the finger and give up – to see problems in measurement as fundamental flaws. This has been the dominant response in psychology for more than fifty years [5–8]. The other response is to take responsibility for the problem, to work constructively to make reports as accurate as possible; and, if necessary, to re-conceptualize what is being measured in order to make sense of measurement anomalies.

What is interesting about Schooler's article is that he appears to be torn between these two responses. On the one hand, he is trying, and succeeding, to help us conceptualize experience better by attacking the naïve assumption that people are always explicitly aware of their own experiences (in doing so, he generates an account closely related to that of Jack and Shallice [9]). Naturally, the evidence he uses to convince us that this is wrong derives directly from self-reports. And yet, Schooler also lapses into blaming his tools! Ironically, it is at just at the points where Schooler stops taking responsibility for introspective evidence, and instead attempts to point the finger, that we see problems in his thinking.

Schooler tells us the equation of consciousness with reportability is problematic because it depends on the assumption that we are always explicitly aware of our own experiences. Yet, this does not appear to be true when we are careful to distinguish different types of report. Suppose we ask a subject to report the varying location of a simple visual stimulus. This task does not require the subject to think explicitly about, or report on, their mental states – it only requires them to think about the stimulus. Nonetheless, under normal circumstances, the subject's report will express or reflect their conscious experience. We may call this type of report a *1st-order* report. Other examples of 1st-order reports include reports of how we remember the world to be (recollections) and concurrent verbal protocols [10].

1st-order reports reveal the contents of consciousness. At the time of the experiencing, subjects know the contents of their experience directly – they can't be wrong about them. We might be wrong about where the stimulus is, but we can't fail to know where we *think* it is. On the other hand, we can be wrong when we attempt to remember what we saw, and we can be wrong when we make *2nd-order*, 'introspective', or 'self' reports. Examples of 2nd-order reports are statements like, '*I saw the stimulus clearly*' and '*I was daydreaming just then*'. 2nd-order reports require the subject to think explicitly about, and represent, their own experience; that is, they involve meta-awareness. Just as we might misrepresent the world outside, so we might misrepresent our own experience.

Schooler is wrong to think that the link between consciousness and reportability always depends on meta-consciousness. Reportability is usually taken to mean 1st-order reportability. Nonetheless, he is still right to think that meta-consciousness is crucial to understanding the conscious/non-conscious distinction. The reason is that some behaviours that look just like 1st-order reports are not reports at all; they are just guesses – responses that are meaningless to the subject and that do not reflect any conscious experience. The prototypical example is the blindsight patient, who can reliably point to the location of stimuli in the absence of any visual awareness. We can call this sort of behaviour a *0th-order* or *null* report. The only direct way of telling whether subjects are making genuine 1st-order reports or null reports is to ask them to make a 2nd-order report.

Schooler's second attempt to find fault with reports occurs when he interprets 'verbal overshadowing' experiments as showing that reports fail to reflect experience accurately. It is not clear that these experiments illustrate anything more than the fact that report-tasks themselves involve cognitive processes, and so can interfere with other processes. Schooler's contention that these tasks illustrate 'translation dissociations' depends on the questionable assumption that verbal overshadowing experiments involve the subject making self-reports about their experience. In Schooler and Engstler-Schooler [11] the critical manipulation, which interferes with recognition, involves asking the subject to provide a detailed description of a face. Yet the task of attempting to describe a face (its features, etc.) is quite different from the task of attempting to describe *what it is like* to look at and try to remember a face. Schooler appears to be confusing a 1st-order report task with a 2nd-order report task. The experiment shows only that one 1st-order report task interferes with a different 1st-order report task; and it appears this happens precisely because the tasks involve experiencing the faces in different ways (i.e. focusing on features rather than engaging in 'holistic' processing). Our bet is that the subjects could have reliably confirmed this difference in self-reports!

There are those who will believe that, because we focus on the advantages rather than the difficulties associated with reports, we show ourselves to be naïve.

Perhaps, but there is a difference between naivety and optimism, and there is also a difference between critical thinking and cynicism. Before pointing the finger, critics of report measures might do well to examine their own thinking.

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How can we share experiences?

Comment from Chris Frith

Psychology was defined by William James as 'the science of mental life', but psychologists seem to have developed a great reluctance to study 'mental life' by any but the most indirect routes. The most extreme example of this lack of directness is seen in functional brain imaging. For some researchers it seems that, if you have a brain scanner, then you no longer need to study mental activity because brain activity is a truly objective alternative. For studies directly concerned with the neural correlates of consciousness (NCC), this approach obviously cannot work because, at the very least, your volunteer needs to indicate through introspection whether or not he saw a stimulus (e.g. change blindness) or which stimulus he saw (e.g. binocular rivalry). Jack and Roepstorff [1] make the important point that the viability of any functional imaging experiment, whether or not it concerns the NCC, depends upon the volunteer doing what he or she is supposed to be doing; in Jack and Roepstorff's terms, following the script provided by the experimenter. The key element here is communication. The experimenter has to instruct the volunteer and, the volunteer has to indicate to the experimenter's satisfaction that the instructions have been understood. The problem is well illustrated by an event alleged to have happened during the rehearsal of a piece by Stockhausen. At one point in the score the orchestra was instructed to do whatever they liked for a few bars. But the composer stopped the rehearsal at that point and angrily stated, '*That's not what I meant at all.*' There are obvious parallels here with the 'rest' condition that has been such a contentious feature of functional imaging studies. If you could discover what was going on in your volunteer's mind during the rest condition you might well say 'That's not what I meant at all.'

Jack and Roepstorff make the bold suggestion that we should ask our volunteers about their 'mental life' during the scan, and, in particular, try to ascertain how well the script they were following corresponded to that intended by the experimenter. This should become an obligatory feature of functional imaging experiments (like getting informed consent), but it will not always be easy. Communication must happen at the level of meta-consciousness, to use Schooler's term [2], becausethe scanning experience must be re-represented so that it can be reported. Schooler discusses a number of elegant experiments demonstrating that meta-consciousness can be dissociated from experience. This is particularly likely with unusual experiences, or experiences that are intrinsically non-verbal. This is very relevant to my own work as I am interested in the unusual experiences associated with neurological and psychiatric disorders. How can I discover what it is like to have delusions of control? Jack and Roepstorff suggest that sharing experiences requires the adoption of a second-person perspective in which a common frame of reference can be negotiated. I suggest that in Schooler's terms this would require that both participants in the communication come to adopt the same procedure for translating between experience and meta-consciousness. This is clearly possible – expert wine tasters learn how to communicate their experiences verbally. A major programme for 21st century science will be to discover how an experience can be translated into a report, thus enabling our experiences to be shared.

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Experimenting with introspection

Comment from Shaun Gallagher

I would like to endorse both the possibilities of using introspection in experimental science, as proposed by Jack and Roepstorff [1] and others, and the various cautions about its use, as

indicated by Schooler [2] and others. To elaborate further on their arguments, let me suggest that introspection itself, even as it is used in experimental science, is not a unified concept.

Schooler lists various terms used to signify introspection, or what he calls meta-consciousness. In most cases, what is indicated is something that is 'second-order' relative to first-order phenomenal experience. This kind of second-order reflexive activity can be very simple, and as such can be found in a large variety of experiments designed to minimize dependency on introspection. In such cases, experimenters might ask their subjects for quick reports about what they experience. '*Do you experience* (see, hear, feel, etc.) *X or not*'? In some cases, to avoid the effects of verbal misinterpretation the subject is asked to simply push a button once she experiences *X*. This still depends on a quick and minimal introspection of the first-order experience (seeing, hearing, feeling, etc.) to be reported. Marcel has demonstrated good reason to be cautious even about this kind of minimal procedure [3]. Specifically, across different report modes (button push, eye blink, verbal '*Yes*') it is possible for different reports to be generated for the very same trial.

More prolonged forms of introspective self-observation may be called for in some experiments. For instance, if asked to report on an emotion, a subject is required to make considered judgments about her own first-order phenomenal experience. Rather than asking whether or not the subject experiences *X*, an experimenter might ask the subject what exactly she does experience. In such cases the subject is asked for a description, which is, in fact, an interpretation. The important question here concerns that on which the interpretation is based. If no instruction is given, a naïve subject is likely to give their report in folk psychological terms. To avoid the subjective aspects of such interpretations, the scientist often instructs the subject, or provides a pre-established set of categories from which the subject chooses the relevant interpretation. A significant bias, however, can enter into the experimental procedure just here in this attempt to be objective. The scientist might feel that some degree of objectivity is met because there is a set of scientific categories in play. But what is it that makes them scientific rather than folk psychological? What is the source of these categories? In cases where such categories are meant to be descriptions or interpretations of first-order experience, then either the experimenting scientists must draw the categories from their own phenomenology (simply reinstating subjectivity), or from a more anonymous phenomenology devised by other scientists in other experiments. But under threat of infinite regress, the phenomenological buck has to stop somewhere, and someone's introspective self-observation has to answer to the possibilities of temporal and translation dissociations raised by Schooler and many of the traditional critics of introspection.

There are two good responses to such issues – one internal and the other external to the experiment itself. The first takes a more systematic approach to introspection by using procedures that allow the subjective experience of experimental subjects to inform the experimental analysis. This is what happens in the study by Lutz *et al.* [4], cited by Jack and Roepstorff. By instructing subjects to set aside standard (folk psychological) conceptions and theories, instructing them to focus on the first-order experience itself, and asking 'open' questions, Lutz and his colleagues employed a version of systematic phenomenology that allowed subjects themselves to define the proper analytic categories. That is, the categories were generated in the very first-order experience that the experimenter wants to know about, rather than in some other, often anonymous, first-order experience, the relevance of which is a matter of interpretation generated outside of the experiment itself.

This kind of procedure will not work in every type of experiment, nor will it necessarily deliver the best results even in those where it is possible. As Marcel notes [5], in some cases (for example, in attempts to measure the effect of unconscious processing, as in priming) tasks that use introspective procedures, no matter how well they are defined procedurally, might not reveal the degree of effect as clearly as procedures that measure the effect indirectly, without introspection. For such reasons, the second good response to the problems of introspection is the one recommended by Jack and Roepstorff: triangulation. That is, interpret introspective results in an adjudicative mix with results generated from other kinds of experiments and observations. In the experiment by Lutz *et al.*, for example, the interpretation of introspective findings was reinforced by consistent EEG recordings.

These considerations suggest three rules for keeping introspective techniques scientific. Although the first two might seem on first glance to be in opposition to each other, they actually raise an important question about what counts as replication in experiments involving introspection.

Rule 1: keep the introspective procedure explicit and systematic. This will assist in possible replication by other researchers.

Rule 2: minimize dependency on extra-experimental, pre-established categories. On the face of it, this seems to prohibit replication. That is, if one cannot use the categories developed in the experiment one is trying to replicate, how is replication possible? It is important to realize that a prohibition against the use of pre-established categories is, in the case of studies of first-order experience, a prohibition against a certain kind of bias, but it is not a prohibition against using the same categories used in the previous experiment. The very same (or very close) categories might in fact be generated within each experiment by following the same introspective procedure. And if very different categories are generated from one experiment to the next, then something is still in need of explanation. This very possibility motivates the final rule.

Rule 3: triangulate.

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