

**Describing Inner Experience?  
Proponent Meets Skeptic**

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**Part Three**

**Reflections**

## Chapter Ten

### Eric's Reflections

#### Eric Schwitzgebel

Melanie makes a number of interesting claims in these interviews – claims which, if true, reveal much about one person's stream of conscious experience. But the question is, are her claims true? What license do we have to believe them? In my mind, this is the first and most central question that must be answered.

Let's grant this from the outset: Melanie is a sincere and conscientious subject, Russ a careful and even-handed interviewer. What they deliver is probably about as good as can reasonably be expected from open interviews about sampled experiences. If we reject it, we reject the method in general – and in its wake surely also a plethora of related but less careful approaches. We then either resign in defeat or face the difficult task of specifying some better way to garner reports about spontaneously generated emotion, imagery, and the like. If, on the other hand, we are justified in accepting what Melanie says about her experience, then perhaps, by repetitions of this method, we can make some headway in the vexed field of consciousness studies. In the merit or failure of these interviews, we can glimpse a possible future of the discipline.

My position is this. We should tentatively accept the most basic claims Melanie makes about her experiences, pending further evidence. However, we

should view the details she provides, even plausible details confidently asserted, with a high degree of skepticism. So, for example, in Beep 5.1, I think we should tentatively accept – as more likely to be true than not – that Melanie had visual imagery of an intersection and also a feeling or recognition of anxiety sometime roughly around the moment of the beep. We should, I suggest, accept this tentatively, barring countervailing evidence. (Such evidence is not available in this case but could include such things as later recantations or physiological or facial measures suggestive of a different emotional state.) However, even without specific countervailing evidence, I think we should be very wary of the details. I don't think we should accept, even tentatively, what Melanie says about the specifics of the image, about the level of detail in the image, about whether she was actually feeling anxiety at the moment of the beep (as opposed to just “knowing” that she was anxious without an anxious feeling), about what this knowledge or feeling of anxiety was like, about whether she is right to deny the presence of other experiences at the moment, etc. We should, I think, withhold judgment about the accuracy or inaccuracy of such assertions, absent further physiological or behavioral evidence of some sort for or against them. The details of Melanie's reports may be true. But, without some further corroboration, we should not cite them as serious support for particular philosophic or scientific theses about the nature of experience – for example, in defense of a particular account of imagery or emotion. They are, at best, merely suggestive.

I regard this as a moderate view, and the course I would chart for consciousness studies in light of it is a cautious and pluralistic one – neither a wholesale rejection of Russ’s experience sampling, nor the elevation of it over previous approaches. The field is for now, I think, in the unenviable position of possessing a stable of suggestive but unreliable (or at least unproven) methods, to none of which we can harness full scientific confidence.

### **1. We Have Not Established the Validity of Russ’s Interview Method**

Russ rejects my cautious pluralism because he believes his approach to the study of conscious (or “inner”) experience gives substantially more faithful access to experience than does any other contemporary scientific approach. He believes, if I understand him correctly, that we should largely disregard the accounts of experience given by other contemporary scientific methods because he thinks they don’t adequately manage the methodological problems DES is designed to avoid. Of course, as we mentioned in the opening chapters, many philosophers and psychologists over the centuries have claimed they possessed singularly trustworthy methods of studying consciousness. The contradictory results arising from this diversity of methods show that many such claims must be false. The burden of proof is squarely on Russ to show that his method, unlike the others, does in fact merit our trust. In my view, Russ has not shown this.

Russ has emphasized the advantages of obtaining an arbitrary, brief sample of experience, reflected on immediately after it occurs. He has emphasized the advantages of not forcing that experience into a preconceived structure and of restraining the interviewee from making general claims or claims about causation. He has shown in the interview chapters that he is capable of soliciting reports without palpable bias. Melanie, for her part, makes interesting assertions about her experience, assertions that are not obviously self-contradictory and don't crumble into an uninterpretable mess when she is asked to elaborate. This is all good. But it still falls a considerable distance short of showing that we should, as a general matter, accept the deliverances of Russ's method. We need, in addition, some sort of external corroboration. That is, we need to find evidence not grounded solely in interviews of this sort that sheds light on the accuracy or inaccuracy of Melanie's reports. And this book, of course, presents nothing of the sort. It records an exploration, not a verification.

A measurement technique may require external corroboration at the outset without remaining forever hostage to, and judged inferior to, the sources of evidence that first help establish its validity. A scientist intends to create an extraordinarily precise thermometer, let's say. She has good theoretical reason to anticipate outstanding accuracy. Yet she will not accept its deliverances immediately. She compares its measurements with the measurements of other, cruder, thermometers she already trusts to some extent. If it's too far off, she has

cause for concern. She puts the thermometer in a situation where she would expect, theoretically, a very slight rise in temperature – a rise perhaps unmeasurable by earlier thermometers – and hopes her new device registers it. If her device passes enough such tests, she may go back and use it to correct or displace her older thermometers or to revise some of the theories she initially used in testing it. Corroboration doesn't imply subservience. It is no objection to the demand for corroboration that the method in question will likely prove superior to the prior methods (and theories grounded in those methods) to be used in corroborating it. In science, few methods command trust without independent corroboration, at least at first.

Consequently, even if we had excellent reason to think Russ's method superior to all prior methods, prudence dictates that we compare its results to the results of those other methods (as the measurements of the new thermometer were compared to the measurements of old thermometers) and that we check its results against what can be theoretically predicted or retrodicted (as the thermometer was checked to see if it recorded the predicted slight rise in temperature). Direct verification of the first sort is beyond the scope of this book: We employ no independent means of measuring Melanie's experience. Russ does offer some corroboration of the second sort in Chapter Two when he discusses features of Fran's behavior that seem to support her unusual introspective reports. (I find the case of Robert less compelling, for reasons described in Box 2.6.) Russ and I have

also contemplated the possibility of a study correlating experience while reading with cognitively measurable differences in reading comprehension (see Box 5.3). None of this, however, pertains directly to Melanie or to the transcripts at the center of this book. We have no videotape, say, of Melanie's behavior at the sampled moments, against which we could check her reports, no cognitive tests that might shed (tentative) light on matters such as whether she really is more self-conscious than others or particularly prone to detailed imagery.

Perhaps we wouldn't need such external support if Russ's method had no flaws, left no sizable space for error to enter, was indisputably massively superior to all the preceding methods that have produced uneven (but sometimes interesting) results. However, as I'm sure Russ would agree, his method is not as resplendent as that. Even if, in the end, we decide it is better than all preceding methods, granting exemption from the general requirement of external corroboration is extreme.

Russ will dispute with me the extent to which his method leaves room for distortions due to experimenter bias and situational pressures, even with a skilled interviewer (more on this later). However, I think he cannot reasonably dispute that his method (like many others) leaves considerable room for errors of memory and communication and for distortions due to the preconceptions and reconstructions of the subject. Even if there is no significant memory issue in the minute or so after the beep, when the subject is first reflecting on her sampled

experience (which is very optimistic), the interview itself is conducted up to 24 hours later. The interviews touch on many details the subject did not explicitly record, or possibly even reflect on, immediately after the beep. Surely, there is substantial room for error here.

The interviewer exhorts the subject to set aside preconceptions, to be fully receptive to her experience regardless of how surprising it may be, not to confabulate or reconstruct on the basis of theory, to express uncertainty where it seems appropriate, to be absolutely frank. But of course exhortation alone, though it may be helpful, can't guarantee that the subject actually attains all these desirable (?) goals. Nor can we be assured that the appearance of frankness, of open-mindedness, of atheoreticity, indicates their actual presence. Indeed, I doubt it's humanly possible to attain some of these goals even approximately. What would it be to be completely open-minded, atheoretical, unreconstructive in one's memory and reports? Would that be mere infancy? Don't we need pre-existing frameworks, categories, theories, causal maps to remember, even to perceive, anything at all – to have anything other than unreportable, immemorable, “blooming, buzzing confusion” (as William James [1890/1981] puts it)? Melanie's biases and preconceptions can't but inform her reports. Risks to her accuracy ensue, which may be impossible to disentangle from the benefits.

Let's contrast Russ's method with the archetypal method of introspective psychology as practiced by Titchener and others a century ago. The latter method

generally involved setting up a controlled situation with precisely measurable stimuli (color plates, in constant lighting conditions, for example, viewed at a constant distance and angle). Practiced introspective observers reported on their experience as it occurred or immediately afterwards, and in cases of uncertainty, or for verification, the stimuli could be repeated. This method has some of the same virtues as Russ's method, including that it targets specific, brief episodes after only a short (or no) delay. Like Russ, Titchener and other introspective psychologists exhorted their observers to set aside their presuppositions. Also like Russ, they generally attempted to reduce or disarm their own expectations. Experiences weren't sampled arbitrarily from everyday life, however, and Titchener's observers were surely affected by the experimental set-up, by the expectation of experiences of a certain sort (e.g., visual experiences of varying hue), and by the potentially distracting or distorting knowledge that they would shortly be reporting on those experiences. On the other hand, conditions were better controlled and the observers' reports more easily allowed for certain sorts of verification (e.g., checks for consistency with what's theoretically predicted; see Titchener 1901-1905; Schwitzgebel, 2004, 2005). Most importantly, perhaps, the memory demands in Titchener's studies were not nearly as great as in Russ's. Titchener generally asked his observers only to report one aspect of their experience, very swiftly. He didn't ask them to reflect on the experience as a whole. Thus, observers didn't require several minutes to generate their reports, as

Russ's subjects often do. (Nor were Titchener's observers interrupted by the task of turning off a beeper, retrieving pen and paper, etc.) They could focus on making an instant judgment about a single thing. And of course, Titchener's observers generated their final reports on the spot, not after an interview the next day. Titchener also emphasized his preference for trained observers, with considerable introspective experience. Maybe trained observers have more theoretical commitments and bias than observers who enter untrained – but it's not clear that this is so. Titchener stressed that even untrained observers are prone to preconceptions and theories about their experiences and often leap to generalizations quickly after one or a few trials (Titchener, 1899, 1901-1905, 1912).

Or consider the armchair phenomenological investigations of contemporary philosophers such as Charles Siewert (1998, 2006) and Terry Horgan (Horgan & Tienson, 2002; Horgan, Tienson, & Graham, 2003). Siewert (forthcoming-a, forthcoming-b) is particularly explicit about his method, which he calls “plain phenomenology.” He urges phenomenologists to reflect repeatedly and patiently on both their ordinary lived experience and on particular types of invoked experience. He asks them to take special care in drawing conceptual distinctions, to bear in mind the theoretical implications, and to consider a variety of related and nearby cases before reaching their final judgment. For example, Siewert (1998, ch. 8) invites the reader to reflect on the difference, if any, between the

experience of imagining an “M” tilted on its side and the experience of imagining the Greek letter sigma. Are these imagery experiences the same or different, and in what respects? Must there be a difference in imagined shape for a difference in imagery experience? To what extent does it seem that such imagery experiences vary with, and depend on, one’s intentions and concepts? Maybe Siewert’s approach risks, more than Russ’s approach, importing the theories of observers invested in particular answers. On the other hand, careful theoretical reflection and the consideration of nearby contrasting cases may also help forestall confusion. The method runs comparatively greater risks than does DES of unrepresentative selection, and of potentially severe and undetectable distortion of the experience by the act of introspectively reflecting on it as it occurs. But on the other hand, wise selection may help us better appreciate subtle contrasts and discern issues of theoretical import. Furthermore, concurrent introspective reflection reduces or eliminates problems of memory inherent in reflecting on experiences already past, and it may allow us to slow down and focus better on detail.

It’s by no means clear a priori whether Titchener’s introspective methodology and contemporary philosophical armchair phenomenological reflection, which Russ rebuke, contain more potential for error than Russ’s own methodology. Each has its apparent strengths and shortcomings. Maybe Russ’s method will find compelling external corroboration that warrants its elevation

over other introspective approaches, but absent such corroboration, I see no reason to regard DES as vastly superior to other methods, with their flawed and divergent results.

## **2. Should We Credit Melanie's Reports at All?**

We might, then, put every introspective method, Russ's, Titchener's, Siewert's, and all others, on an equal footing: prone to obvious sources of error, inconsistent in their results, relatively uncorroborated, unworthy of scientific credence. Proper scientific caution, we might think, demands that we discard everything Melanie says, pending positive and robust evidence that we're on firm ground.

The problem with this approach is that no swift and decisive corroboration or disconfirmation is in the cards for any method of studying experience. Insisting on firm ground thus means abandoning the theoretical exploration of consciousness. Of course, we should at least try, more than we do, to find external corroborations of subjective descriptions of experience and to illuminate the conditions under which such descriptions are credible. But the results of any such attempts will inevitably be controversial and difficult to interpret for some time to come. As I mentioned in discussing Russ's disagreement with Flavell about how to interpret his children's denial of the experience of thinking (Ch. 3.2), there's

just too much room to posit whatever experience best supports our theory or conforms to our favorite method.

I see no reason to think the task Russ sets Melanie is absolutely impossible. People must have at least some inkling of what's going on in their own present and immediately past conscious experience. That inkling is, I think, surprisingly poor and unstable (as I've argued in other work), but it would be a radical skepticism indeed to suppose that we have no clue whatsoever about the ongoing flow of our experience. Asking people about their present or immediately past experience is not entirely pointless. Suppose someone judges himself to have just been (consciously) thinking about his plans for Saturday. Suppose also the usual sources of error in judgment are minimal, as far as we can tell. It seems churlish not to give him at least tentative credit.

Russ's method builds on that fundamental credibility. Although the interview isn't conducted until hours later, basic features of the experience are explicitly noted within a short time. I see no reason to think that such basic features couldn't, in general, be accurately recalled in the later interview, especially with a notepad as a cue. Russ allows the subject to approach the task in her own terms, solicits a report without overt pressure toward any particular outcome, discourages mere hypothesizing. It again seems churlish, a mere stance, to give her no credit whatsoever, absent some specific reason for skepticism. Minimally, let's say, it seems in most cases more likely than not that the basic

topics of thought or reflection that Melanie reports (e.g., her chair in 1.1, scuba diving in 4.1, her appointment in 5.1) were indeed in consciousness somewhere around the time of the beep. Maybe also (more questionably perhaps) those topics were present in roughly the modes she describes (inner speech or hearing in 1.1 [but for cautions about this case in particular, see Russ’s comments in the next chapter], bodily imagery or feeling in 4.1, visual imagery in 5.1).

Given the uncertain state of consciousness studies and the lack of any well-established general methods, to endorse a blanket skepticism about about such matters exhibits a misguided and crippling purism. However, I do think a blanket skepticism may be in order regarding the details of Melanie’s reports, unless we find further corroboration of them – corroboration either of those reports in particular or of the validity of Russ’s experience sampling method in general. I’ll develop this idea in Sections Four and beyond. But first I’ll describe an experiment of my own. Perhaps this experiment – an awkward experiment, I confess – can in an imperfect way illustrate some of the untapped potential in experience sampling.

### **3. Adapting Russ’s Methodology to Explore the Richness of Experience**

The experiment addresses the “richness” of experience – the extent to which we have constant experience in a variety of modalities. According to the rich view, we have constant visual experience (at least when our eyes are open, maybe

also when they're closed), constant tactile experience (for example of our clothes against skin), constant auditory experience, maybe constant emotional experience, conscious thought, imagery, etc. – all simultaneously (see Box 4.8). According to the thin view, experience or consciousness is limited to one or a few modalities or topics at a time. For example, maybe when I'm wholly absorbed in writing, the background noise of traffic plays no part in my conscious experience at the moment. (I may, of course, still non-consciously process auditory input, so that if the sound suddenly changes or stops, the change or cessation may capture my attention and enter my consciousness.) Most of the day, my shirt rubs against my shoulders. On the rich view, I have a constant, peripheral – maybe quite faint – tactile experience of this. On the thin view, I have no experience of it whatsoever, not even in a faint and peripheral way, most of the time, unless I'm actually focally attending to or thinking about my shirt. I've found people to report divergent intuitions regarding the relative richness or thinness of experience, even when I make my best effort to guard against variation in the use of terms. Some people (but few theoreticians of consciousness) also endorse a moderate view, between rich and thin, on which experience outruns focal attention to some considerable degree but isn't the constant plenum envisioned by the rich view.

What I called the “refrigerator light phenomenon” in Box 4.18 frustrates any attempt to study the richness of experience using concurrent introspection of experience as it transpires. Surely, when I think about whether I'm having visual

experience, I have it. When I think about whether I'm having tactile experience, I have that, too. But I shouldn't thereby conclude that I have constant visual and tactile experience. We can't rule out the possibility that my inquiry itself creates the experiences in these cases, lifting the relevant sensory processing into consciousness. The proper question to ask is whether visual and tactile experience are present when I'm not thinking about them. You can see why it would be appealing to employ a beeper to get at this issue.

I divided 21 subjects (11 philosophy graduate students and 10 non-philosophers) into five roughly balanced groups (for more methodological detail, as well as considerable self-critique, see Schwitzgebel, forthcoming-c). With one group, I did something like Russ's DES procedure (less expertly, I'm sure), but with a few modifications: First, I avoided the phrase "inner experience," which I worried might be interpreted as emphasizing imagery, thoughts, emotions, and the like over ("outer"?) sensory experience. Second, I spent some time explaining what's meant by "consciousness" or "experience" or "phenomenology," citing examples of conscious processes (vivid emotions, focal sensory experiences, inner speech) and non-conscious ones (subliminal perception, immune system response). I invited discussion of this topic. Third, in discussing the first sample, once the participant was done reporting the most salient aspects of her experience, I explicitly asked whether there were also other aspects of her experience, giving examples like feelings of hunger or tiredness; visual, auditory, tactile, or olfactory

experiences; emotions; visual imagery; conscious thoughts; etc., repeating this question with arbitrary examples of potential experiences until the subject denied recalling anything more. Fourth, generally in discussing the first sample, and always on the first interview day, I mentioned the debate between the rich and the thin view, citing arguments on either side and expressing neutrality on the question. However, I did not particularly emphasize that issue. I encouraged theoretical and methodological discussion on a variety of topics, generally recommending cautious restraint in such matters. Broad and open theoretical discussion was encouraged throughout four days of sampling and interview.

Participants in the other four conditions, unlike those in the first condition, were told explicitly that the purpose of the research was to explore the richness or thinness of experience. They were given an explanation of the debate and some arguments and intuitive examples on both sides of the question, and they were asked for their own initial impressions. Like the first group, they were mostly beeped over four days and invited to reflect on the theoretical and methodological issues pertinent to their reports. Each was asked about one aspect of sensory experience. One group was asked simply to report if they were having visual experience at the time of the beep, and if so what that experience was. Another group was similarly asked about experience in the far right visual field, another group about tactile experience, another about tactile experience in the left foot. They collected their samples with these types of experiences in mind, instructed

to make a first judgment of “yes, I had such an experience” or “no, I didn’t” (or “maybe” or “sort of”) as quickly as possible after each beep. Participants were repeatedly assured that it was fine if they had no experiences of the sort in question – that that would be nice evidence for the thin view – and conversely that it would be fine, and good evidence for the rich view, if they found such experiences in every single sample. Participants who leaned toward one view were periodically reminded of the viability of the other view. Occasionally, a participant who claimed to have had an experience of the sort under study was pressed about whether there really was such an experience, or whether she was just reporting some external object in the visual or tactile environment.

Conversely, participants who claimed to be having no experience of the sort under study were occasionally pressed about whether they really thought there was no experience, as opposed to merely vague or secondary or peripheral experience.

Every participant in the full experience condition (resembling DES) and the two visual experience conditions reported some sort of visual experience in most samples – even those initially inclined toward a thin view, with no obvious difference or trend between the three conditions. A majority (8 out of 13) reported visual experience in every single sample. In contrast, participants in the full experience and full tactile experience conditions reported tactile experience in only about half to three-quarters of all samples (depending on how liberally one interprets “tactile” – e.g., whether pain and proprioceptive experiences count).

Somewhat lower rates of experience were reported in the far right visual field and the tactile left foot conditions – though even the “thinnest” participant in the tactile left foot condition confidently reported tactile left foot experience in 2 of her 19 samples. How exactly to interpret these results is a complicated and uncertain matter that I can only partially explore here. However, if we credit the participants’ reports, overall they seem more supportive of a moderate view than either a rich or a thin view.

Russ will surely say that in conducting the interviews as I did, I allowed my own biases and presuppositions to inform the results. I acutely feel the merits of this objection. Here are a few not entirely sufficient responses to that concern: (1.) At the end of my time with each participant, I asked her to guess whether I personally leaned toward the rich or the thin view. Subjects were divided on this question, generally saying they felt I was even-handed. (2.) The asymmetry of response between the visual and tactile conditions suggests that situational pressures creating a general bias toward reporting sensory experience can’t fully explain the results. (3.) Explicitly discussing the theoretical possibilities and explaining some of the appeal of both sides of an issue may actually be preferable to allowing such issues to pass undiscussed, since it may serve as something of a check on participants’ initial suppositions. Let me also add that since the experiment (except in the DES-like, full experience condition) centers around a single yes/no/maybe question, the memory demands are considerably less severe

than in the standard DES format. There's no several minutes of recalling and describing the experience, no great likelihood of forgetting the key piece of data between the after-beep scribble and the next-day interview.

In our discussion of Beep 1.1, Melanie explicitly denies having visual experience at the moment of the beep, and in general she denies having any more than 2-3 types of experience at any one moment. She doesn't usually include visual sensory experience in her reports. In these respects, she's typical of Russ's subjects and different from mine. This difference could, of course, be due entirely to design flaws in my experiment or to my deficiencies as an interviewer, or it could point to an inexcusable instability in beep-and-interview methods.

Another possibility is that the difference turns on linguistic or theoretical issues. Russ uses a variety of terms and phrases to talk about what he's trying to get at, all somewhat interchangeably, including "inner experience" (with its hint of favoring "inner" processes over sensory ones), "attention," and "awareness" (see Box 2.1 for his justification of this practice). Unfortunately, the use of "attention" as equivalent to the others seems to invite the thin view. On the rich view, of course, many things outside of the fairly narrow band of focal attention are nonetheless experienced, so "in attention" and "experienced" are decidedly not interchangeable. Notice how Russ asks the relevant question of Melanie on the first day:

Russ: So is there anything else going on at this particular moment?

You're seeing the white parchenty paper...

Melanie: Mm hm.

Russ: And does that seem to be in your awareness, or is it...

Melanie: No it's not. I'm not aware of how my body is positioned or of what I'm holding. It's very much just in my head.

Russ: You're paying much more attention to your thought process here, about "isn't it strange...?" "isn't it funny?" You're obviously seeing the parchment, because that's what started this process, but it's not in your awareness.

Melanie: Yes, exactly.

By using "attention" and "awareness" interchangeably here to mean "experience," Russ implicitly suggests that something outside attention is outside experience, in direct contravention of the rich view. Melanie might thus be forgiven for interpreting Russ's questions as about what she is attentionally focused on or centrally aware of, rather than about a wider panoply of peripheral experience that may or may not exist. Though Russ's first use of the word "attention" in the recorded dialogues is only after Melanie's first denial of experience above, its use here conveys an implicit assumption that may already more subtly have been communicated to Melanie – or even explicitly communicated in Russ's initial interview with her, in which he gave instructions

about how to use the beeper. The difficulty may be further compounded by what I regard as Russ's frequent blurring of the epistemic (having to do with knowledge) and phenomenal (having to do with the stream of experience) senses of "awareness" (see my discussion in Boxes 8.6 and 9.4). If Russ's subjects interpret "in awareness" to mean (epistemically) something like "a matter of explicit knowledgeable reflection" and also (phenomenally) something like "part of the stream of experience," that also could lead to overly thin reports, if the rich view is correct that the contents of the stream of experience considerably outrun the matters to which we are devoting explicit focal attention.

Since Russ avoids general theoretical and terminological discussions, it's difficult to know exactly how his subjects understand him on such matters. By the time I have the opportunity to raise the question in my own way, Melanie has already committed herself to denying visual experience. (If you think it problematic that Melanie would both deny visual experience and not object to Russ's statement that she was "obviously seeing," you probably lean toward the rich view.) After her exchange with me, Melanie's denial of visual experience becomes more qualified – not absolute, but relative, expressed in words like "primarily" and "much more." Such a relative claim is of course consistent with the rich view, which generally assumes a focal center of experience and a periphery that is in some way vaguer or less vivid.

Three more factors may further support Russ's subjects' tendency to disregard peripheral aspects of their experience, if any exist. (1.) Subjects will naturally tend, after the beep, to focus first on what was central in their experience. By the time they start to think about whether they were also having (say) visual experience – if they ever think about that – it may be several minutes after the beep, and their memory may have faded too much for accurate recall. (2.) Subjects will have collected six to eight samples per interview session. Given the details of Russ's questions, if there is any hope of getting through a substantial portion of those samples, peripheral aspects of experience must be excluded. (3.) Russ himself explicitly declares a lack of interest in peripheral aspects of experience, which he thinks probably can't be reported accurately (see Box 4.8). I fully support Russ in wanting that limit on inquiry in standard (unadapted) DES interviews – but that means we are on shaky ground using the reports of subjects like Melanie to undermine the rich view.

I regard the experiment described in this section as preliminary and exploratory. I don't entirely trust my own subjects' reports, and I certainly don't mean to suggest that the reader should accept that in fact people do have visual experience most of the time and tactile experience about half the time. The results require interpretation and are at best only suggestive. (Schwitzgebel forthcoming-c discusses a variety of concerns about the data, suggesting ways in which they can be reconciled with thinner or, especially, richer views of experience.)

However, the debate about the richness of experience has thus far been conducted largely impressionistically, or in terms of questionable general theories of consciousness (e.g., James, 1890/1981; Jaynes, 1976; Dennett, 1991; Searle, 1992; Siewert, 1998). A version of the beep-and-interview method gives us the opportunity to explore the question in a different and maybe better way – a way beyond even what Russ himself envisions. If future researchers discover still other means of exploring this question, and if the results of the various researches appear to converge, then perhaps we will have some solid basis for a scientific opinion.

#### **4. Memory in Introspective and Eyewitness Testimony**

Let me return now to my case for skepticism about the details of Melanie's reports. I propose a blanket skepticism about all but the grossest features of her reported experience. I simply don't trust Melanie accurately to remember the details.

Actually, I don't really trust Melanie's descriptions of the grosser features of her experience either, though I'm willing tentatively to accept them. So let me put the point a bit differently. When Melanie reports the details of an experience – for example when she describes the details of an image or attempts to specify her degree of self-consciousness in feeling an emotion – her reports may accurately reflect the content of the original experience; but I think it just as likely that the

imputed details are erroneous inventions, arising from her theories and preconceptions about experience, from situational pressures, from accidents of language, from distorted and unrepresentative reconstructions formed either shortly after the beep or during the course of the interview, etc. I'm willing to accept that in most cases Melanie preserves some rudimentary memory of her experience as it transpired shortly before the beep, but how that trace is articulated and described in the course of the interview, the specifics in which it's dressed, seems to me very likely to depend as much on factors only tenuously associated, or unassociated, or even negatively associated, with accuracy, as on genuinely remembered particulars.

I could be wrong about this. If Russ or others are able consistently to corroborate reports like Melanie's down to a fine level of detail, then we may be justified in accepting all or most of what Melanie says. Of course, as the field stands now, even the most basic aspects of DES reports remain uncorroborated and will require considerable effort and ingenuity to corroborate. Judgments about how far to believe Melanie can only be speculative.

The task Russ and I set to Melanie invites comparison to the task of reporting the details of an outwardly witnessed event. Although Melanie isn't literally an "eyewitness" of her experience – we see things, of course, not our experiences of those things – her task bears an important resemblance to the task of an eyewitness asked to report some specific event, such as a crime. Like an

eyewitness, Melanie is expected to report details of specific, unique events that she was (presumably!) in good position to record, as the result of a relatively swift and unmediated process beginning with those events. A comparison between Melanie's reports and eyewitness testimony is inviting because although the literature on the accuracy of reports of conscious experience is spotty and controversial, the accuracy of eyewitness testimony has been extensively examined, with some relatively robust findings. Chief among them: Eyewitness reports are prone to what most people find to be a surprising degree of error.

Two passages from Elizabeth Loftus's classic book on the topic (Loftus, 1979) give the flavor:

Two female students entered a train station, one of them leaving her large bag on a bench while both walked away to check the train schedules. While they were gone, a male student lurked over to the bag, reached in, and pretended to pull out an object and stuff it under his coat. He then walked away quickly. When the women returned, the older one noticed that her bag had been tampered with, and began to cry, "Oh my God, my tape recorder is missing!" She went on to lament that her boss had loaned it to her for a special reason, that it was very expensive, and so on. The two women began to talk to the real eyewitnesses who were in the vicinity. Most were extremely cooperative in offering sympathy and whatever details could be recalled. The older woman asked these

witnesses for their telephone numbers “in case I need it for insurance purposes.” Most people gladly gave their number.

One week later an “insurance agent” called the eyewitnesses as part of a routine investigation of the theft. All were asked for whatever details they could remember, and finally, they were asked, “Did you see the tape recorder?” Although there was in fact no tape recorder, over half the eyewitnesses “remembered” seeing it, and nearly all of those could describe it in reasonably good detail. Their descriptions were quite different from one another: some said it was gray and others said black; some said it was in a case, others said it was not; some said it had an antenna, others claimed it did not. Their descriptions indicated a rather vivid “memory” for a tape recorder that was never seen (Loftus 1979, p. 61-62).

And:

... subjects viewed a film of a traffic accident and then answered questions about the accident. Some subjects were asked, “About how fast were the cars going when they smashed into each other?” whereas others were asked, “About how fast were the cars going when they hit each other?” The former question elicited a much higher estimate of speed. One week later the subjects returned and, without viewing the film again, they answered a series of questions about the accident. The

critical question was, “Did you see any broken glass?” There was no broken glass in the accident, but because broken glass usually results from accidents occurring at high speed, it seemed likely that the subjects who had been asked the question with the word “smashed” might more often say yes to this critical question. And that is what we found (Loftus, 1979, p. 77-78).

Distortive influences on eyewitness testimony include information or suggestions built into the wording of questions, the expectations or theories of the witness, the expectations of the interviewer, stress, the solidification of guesses or conjectures into confident assertions as they are repeated over time (“confidence inflation”), and the confusion of what is imagined with what is remembered, to name a few (see, for example, Loftus, 1979; Narby, Cutler, & Penrod, 1996; Wells & Loftus, 2003).

It’s not surprising, of course, that eyewitness testimony is subject to distortion. We don’t need a raft of journal articles to tell us that. What is striking, however, and repeatedly confirmed, is the extent of the distortion. Most people just don’t expect witnesses to be as badly mistaken or as easily influenced as they often are. They don’t expect a majority of eyewitnesses to invent, and describe in detail, a tape recorder they have never seen. They don’t expect subtle differences in the phrasing of questions to have the profound effects they often have on witnesses’ reports.

Since the earliest days of eyewitness research, instructors have been fond of classroom demonstrations of eyewitness inaccuracy. Münsterberg (1908/1927) describes one typical classroom demonstration: A shouting match breaks out between two students; one draws a weapon; the professor intervenes. Immediately afterward, the professor tells the class that the episode was staged and asks them for a written account of the events (in the case Münsterberg describes, some students recounted the events only later). Inevitably in such demonstrations, the students' reports are rife with error. Münsterberg writes:

Words were put into the mouths of men who had been silent spectators during the whole short episode; actions were attributed to the chief participants of which not the slightest trace existed; and essential parts of the tragi-comedy were completely eliminated from the memory of a number of witnesses (1908/1927, p. 50-51).

The students witnessing such demonstrations are generally quite surprised at the results, shocked that they and their peers could diverge so widely in their descriptions of the perpetrators' height, race, hair color, and clothing, in their characterization of key events, in almost every feature of the evaporated scene. This surprise is a crucial pedagogical tool in undermining students' misplaced faith in the accuracy of eyewitness testimony (Charlton, 1999; Gee & Dyck, 2000).

Psychologists have also more formally tested the degree to which people tend to overestimate eyewitness accuracy. Researchers have, for example, asked undergraduates and ordinary citizens to read through descriptions of eyewitness testimony experiments and then to predict the outcome of the experiments. Subjects in such studies often severely overestimate the accuracy of other subjects' eyewitness performance (for reviews see Leippe, 1995; Devenport, Penrod, & Cutler, 1997). In another series of studies, Wells and colleagues (e.g., Wells, Lindsay, & Ferguson, 1979; Lindsay, Wells, & O'Connor, 1989) staged crimes before subject-witnesses. The witnesses were asked to identify the perpetrator from a photo lineup, then testify and undergo cross-examination. Subject-jurors who viewed the testimony and cross-examination generally overestimated the witnesses' accuracy, judging not only the accurate witnesses to be accurate, but also the inaccurate ones. In Wells, Lindsay, and Ferguson (1979), for example, subjects judged not only 76-84% of the accurate witnesses to be accurate (depending on condition) but also 73-86% of the inaccurate eyewitnesses to be accurate. Needless to say, given these numbers, subjects were not especially good at distinguishing accurate from inaccurate eyewitnesses on the basis of their testimony.

Indeed, generally speaking, people seem to be fairly poor at distinguishing accurate from inaccurate eyewitness testimony, except in extreme cases, such as when a witness is blatantly self-contradictory or explicitly avows uncertainty

(Leippe, Manion, & Romanczyk, 1992; Leippe, 1995; Devenport, Penrod, & Cutler, 1997). Our poor judgment on this front may spring from a variety of factors. For example, psychological research suggests that people tend especially to believe confident eyewitnesses, but that confidence correlates only weakly with accuracy, or correlates well only in special conditions (e.g., Wells, Lindsay, & Ferguson, 1979; Wells & Murray, 1984; Bothwell, Deffenbacher, & Brigham, 1987; Sporer, Penrod, Read, & Cutler, 1995; Kassin, Tubb, Hosch, & Memon, 2001; Brewer & Burke, 2002; Weber & Brewer, 2004).

Back to Melanie. Are we to think her better than a typical eyewitness? What we asked her to observe was in some sense closer to her than any outward event – but is that sort of proximity an advantage? In vision, certainly, one can get too close. Things nearby and essential may nonetheless be only poorly seen and rarely reflected on – such as one’s eyeglasses. I may talk more coherently about, and reach more accurate judgments about, the road I’m driving on than the steering wheel I use to drive on it. (I know the road curves 90 degrees; but can I say how far I need to rotate the steering wheel to make that turn?) Likewise, even if sensory phenomenology is in some sense essential to sensory judgment, we may know it only very poorly. As I pointed out in Chapter Three (along with a variety of other reasons to doubt the accuracy of our introspective judgments), we normally observe, attend to, think about, and describe outward events, not inner ones.

The task we set Melanie was an alien one – one that strikes many subjects at first as strange and difficult. Though Melanie gained some practice over the course of the interviews, it seems unlikely to me that her comfort with the task in the end should justifiably exceed the ordinary eyewitness’s comfort in reporting nearby outward events. Immediately after each beep, Melanie knew that she would need to remember and report the experience in question, but at least some criminal eyewitnesses (not to mention subjects in eyewitness experiments) are in a similar position, realizing either immediately after a crime occurs, or even as it is occurring, that they should remember details for later report. Russ and I gave Melanie some feedback about her reports, but that feedback consisted mainly in exhortations to be open-minded, to resist generalizations, and to attend closely to the beeped moment, coupled with Russ’s general willingness to accept confidently-asserted declarations about specific episodes and my varying degrees of theory-laden skepticism. Although our feedback may have had some limited value, I certainly risked affecting Melanie with my theories and Russ may have encouraged a kind of blasé confidence by his readiness to accept confident reports, almost regardless of content. The events Melanie reported were mostly fleeting – momentary images, passing thoughts. Opportunities abounded for theory-laden reconstruction, for unintentional confabulation. No external check or second witness existed to keep Melanie careful and modest.

Here's a further point of difference between eyewitness testimony and introspective report: Normally, when someone witnesses a robbery or car accident, she'll have some sort of schema or sense of the world into which they fit. Such events may be surprising in a certain way, perhaps undermining our expectations and stereotypes, but they rarely impugn our sense of the possible. In experience sampling, however, our most basic conceptualizations are often undermined: We simply must be wrong in much of what we believe about our stream of experience – if for no other reason than that the massive diversity of opinion about basic structural features of human experience considerably exceeds the likely diversity in the experiences themselves (Box 7.4). Much of what is true of experience is going to strike at least some people as, if not inconceivable, at least rather strange.

Consequently, in introspecting we must frequently encounter events that fit our concepts poorly. Such events, especially if they're fleeting and we're unpracticed in reflecting upon them, may be difficult to report accurately and particularly susceptible to theory-based reconstructive distortion. In the relatively rare cases when externally witnessed events challenge our sense of the possible – for example, when the final position of the cars doesn't seem to make sense given the trajectories we seem to recall – our memories, theory-laden and reconstruction-based as they are, appear to be undermined. I'm judging here only from personal experience: I know of no research directly on that issue in the eyewitness literature. However, a slew of studies spawned by Schacter, Cooper,

and Delaney (1990) suggests that memory is poorer for line drawings of “impossible” objects than for (novel) possible ones. On a more introspective note, Gopnik (1993a-b) argues persuasively that children’s memory for past false beliefs is severely hampered when children accept a theory that allows no room for false belief in general. Likewise, surely, what is alien (a cricket match) will generally be harder to remember and report than what is comfortable and familiar (a baseball game). The merely unusual may vanish in reconstruction, or it may be better remembered because striking and salient; but events so foreign to our ordinary conceptions that we lack easy schemata or categories for them – events, if Russ and I are right, that we are quite likely to encounter in introspection – should, it seems, be hard to retain.

People asked to imagine events often confuse those events with events actually experienced (for a review of the extensive literature on this see Johnson, Hashtroudi, & Lindsay, 1993; for connections with eyewitness testimony see Lindsay, 1994). I’ve spent half an hour looking for my keys. Suddenly, I picture them on the kitchen ledge. But am I having a genuine memory of having seen them there, or does the image of them on the ledge seem familiar only because I imagined the keys there before, earlier in my search? After the crime, I imagine the perpetrator with a moustache; later I’m confused about whether I actually saw him that way, or only imagined it. What about events in the stream of experience? Once again, I’m forced to conjecture: I know of no research that looks directly at

whether we can conflate “inner experiences” we actually had with those only imagined later. However, it seems likely the rate of conflation would be comparatively high. If Melanie has a visual image of a shed at the moment of Beep 1.3, then reconstructs that image shortly afterward in taking note of that experience, then reconstructs it again when she is reviewing her notes prior to the interview, then again finally (as she admits) during the interview itself, she runs a considerable risk, I think, of misattributing features of one image to the other. If the information available to me as I entertain that image of my keys on the ledge marks only poorly whether the source of that representation is a past imagination or a past perception, mustn't it even more poorly mark one past imagination from another?

The literature on eyewitness testimony calls into question the very project of this book as Russ and to a lesser extent I conceive it. The reader is invited, as I was invited, to listen to Melanie and reach his own more or less intuitive judgment about how believable she is. But if people tend greatly to overestimate the trustworthiness of eyewitness reports, and if we have only mediocre skills in discerning accurate from inaccurate eyewitness testimony, and if our standards for assessing accuracy arise principally from our experience with what seems to me the comparatively easy and familiar matter of reporting on outward events and judging the accuracy of such reports, then maybe we've been invited into a trap. Melanie's testimony may well be considerably less accurate, and we may be

considerably poorer judges of where it is accurate, than most of us are initially inclined to think.

The point here is not that Russ's method introduces some special source of distortion into Melanie's testimony. The point is that we should be wary of trusting our intuitive judgments about how accurately she is reporting. Melanie's preconceptions, Russ's and my subtly (or not so subtly) communicated expectations, Melanie's potential confusion of the remembered with the merely imagined, the changeable and elusive nature of the events to be described, the universal human investment in being right in what one has said in the past – all these and their kin have larger effects than most of us naively expect. Melanie's testimony may seem trustworthy and yet be surprisingly full of error. Given the novelty of the task and the methodology, we can only speculate how far such error may go, but my sense is that it likely penetrates quite far. If an eyewitness can invent a tape recorder, replete with convincing detail, then much more easily, I think, can Melanie invent a feeling of lightness in her chest, or confuse inner speech with unsymbolized thinking, or be mistaken about the degree of detail in a visual image.

### **5. Pressures of the Interview Situation and Experimenter Expectations**

A large and compelling body of evidence in social psychology (reviewed, for example, in Ross & Nisbett, 1991) has demonstrated that subtle features of a

situation can have a striking impact on behavior. An oft-cited example (from Isen & Levin, 1972) is the following. People who had used a phone booth in a suburban shopping plaza saw another person (a confederate of the experimenter) spill a folder of papers in their path. The situation had been arranged in advance so that some callers had found a dime in the phone's coin return slot immediately prior to witnessing the mishap and others had not. Among those who had not found a dime only one of 25 helped to gather the papers. Among those who had found a dime, fourteen of sixteen helped. Apparently, what we might have thought to be principally determined by durable character traits – how considerate and helpful someone is – can be largely decided by a minor feature of the situation. Hundreds of experiments, using a variety of methods and venues, show similar results.

It's well known that such subtle situational pressures can greatly compromise a psychological study, through their effects on both experimenters and subjects. Expectations conveyed by or to experimenters, in particular (which we might think of as part of the social situation surrounding the experiment), can have a surprisingly large influence on the outcome of research (e.g., R. Rosenthal, 1976). In one famous study (R. Rosenthal & Fode, 1963), undergraduates acted as experimenters, running supposedly "bright" or "dull" rats in a maze (the rats were actually from the same population). The "bright" rats performed considerably better than the "dull" rats, and continued to improve over the course of the

experiment. Presumably, they were better encouraged, better treated, and given the benefit of the doubt in multiple difficult-to-track ways. Similar effects have been found, disturbingly, with children in the classroom, even with only minimal experimenter contact (R. Rosenthal & Jacobson, 1968/1992).

Closer to the present topic of study, Intons-Peterson (1983), using advanced undergraduates, has shown substantial experimenter expectation effects on subjects' reports of their imagery experiences and on imagery-related tasks, even when many of the most overt sources of potential experimenter influence are eliminated. For example, undergraduate experimenters gave subjects a mental rotation task, requiring the subjects to quickly judge whether a visually presented outline of a hand was a left hand or a right hand (as seen from the back). Hands were presented at different angles of rotation (always from the back), and prior to each presentation the subject either received a "perceptual prime" (a left or right hand in canonical upright position, presented for comparison with the target hand) or was asked to visually imagine such a comparison hand. All presentations and time-recordings were done by computer. When experimenters expected better performance in the perceptual prime condition than in the imagination condition, the computer recorded performance times in accord with that expectation. Conversely, when the experiment was conducted by experimenters with the opposite expectation, the opposite result was found. Outside observers brought in to check for subtle sources of experimenter influence (e.g., in voice modulations

and facial expressions) had difficulty discerning any such differences between the two groups; Intons-Peterson did, however, find substantial differences in the experimenters' pauses while reading the instructions.

Situational and experimenter-effect influences tend to be stronger than most people (or at least most Westerners) expect (Ross & Nisbett, 1991; Choi, Nisbett, & Norenzayan, 1999). Thus, we must be cautious in relying on our intuitive sense about the extent to which Russ's and my expectations, and the pressures of the interview situation, may have influenced Melanie's reports. Here, as with eyewitness testimony, an untutored sense of Melanie's believability may lead us astray: She may be considerably more swayed by us than the reader would naively expect. In fact, Melanie later wrote: "I struggled during the first set of samples, and, I will admit, for most of the experiment with a desire to gloss over what I was really experiencing and try to say what I thought was expected of me" (personal communication, September 2004). This later statement (if it is to be believed) supports my impression that Melanie felt potentially distortive pressure from what she took to be our expectations.

The open structure of Russ's interviews allows plenty of opportunity for experimenter expectations to come into play, especially if such subtle factors as the length of a pause are considered relevant. So I don't think we're safe inferring from the lack of palpable bias on Russ's part that his expectations had only negligible distortive effect. I myself, of course, made much less effort to hide my

biases, and in one case at least I'm inclined to think they had a discernible effect: in Melanie's move from reporting very detailed imagery with almost no indeterminacies to her reporting more indeterminate imagery (see Box 5.11).

One situational pressure that may be easily missed is the pressure on Melanie to provide some kind of fairly specific description of her experience. She has worn a device for exactly that purpose; to confess ignorance would be a defeat; other subjects apparently can do this; two professors await her report with interest. Intuitively, one might think it nonetheless quite open to Melanie – especially given Russ's and my verbal endorsements of caution – regularly to say she doesn't recall very well, for her to provide only a very rough sketch and then stop, to open the door to uncertainty. Such restraint would probably better reflect her (and most of our) actual capacities. And if the general picture that Ross and Nisbett draw is right, such situational pressures toward specific and confident reports may be substantially more compelling than they seem to untutored intuition. Furthermore, Russ's persistence in asking for details, while in many ways laudable, may amplify this pressure (for example, in Beep 4.1, where Melanie struggles to describe the experience of craving to go scuba diving; see Box 7.3).

Inaccuracies of memory may thus conspire with subtle situational pressures – pressures both to conform to our (perceived) expectations and to confidently produce details of some sort or other – to create substantial inaccuracies in

Melanie's reports. And the vaguer the memory, the more ineffable and elusive the targeted experience, the more room for such factors to operate. If the task is intrinsically very difficult – if we're simply not capable of accurately reporting that kind of detail – confabulation, or simply taking one's best stab, without much sensitivity to whether confidence is justified, may be practically forced.

Let me mention also that situational pressures doubtless affect the interviewer, as well as the interviewee. In particular, I'd like to emphasize one pressure that I think may run pretty deep in the DES situation: the pressure to accept what the subject says, especially when she's reporting confidently on a moment of experience conscientiously sampled and carefully scrutinized in the interview. For the interviewer to remain unsatisfied in such a condition undermines the apparent basis of the activity. The subject has been asked to describe her experience and no flaws have been found in her report. What more could the interviewer want? If the interviewer consistently remains skeptical, the subject may legitimately wonder if she has been lured into a winless task. I'm quite familiar, from numerous informal interviews, with the awkward tension that arises when I ask someone her opinion about some aspect of her experience and then express a disinclination to believe the resulting statement. It feels much more natural and comfortable to come on board, agree, be collaborative rather than judgmental.

In his 1990 book, Russ explicitly states that the subject and interviewer should try to reach agreement. Indeed, Russ had his subjects examine and criticize the interviewer's final descriptions of their experiences. Russ's highly collaborative method no doubt vividly conveys a respect for the subject and a concern for deeply scrutinizing what the subject might antecedently have thought to be irrelevant details. The interview may benefit enormously from conveying these impressions. However, it may also become difficult for the interviewer to achieve the distance and detachment necessary to view the subjects' reports in a sufficiently skeptical light.

#### **6. Further Concerns Particular to Reporting Conscious Experience, and “Bracketing Preconceptions”**

I've recommended a general skepticism about the details of Melanie's reports. In light of these concerns, should we still, at least tentatively, accept the gross features of her reports, as I suggested in Section 2? Should we accept that Melanie experienced, at or around the time of Beep 1.1, a thought in inner speech or hearing about the peculiarity of having to plan the inheritance of her new chair, as well as an experience, perhaps visual (“rosy-yellow?”), of the humorousness of that thought – never mind details about the pacing and vocal characteristics of the speech, the location and exact tint of the glow? Should we accept that Melanie experienced, at Beep 5.1, a visual image of some sort, of an intersection, and

some kind of feeling or knowledge of anxiety? Such gross features seem much more likely than the details to have been reflected upon and written down immediately after the beep, and thus to have been accurately ascertained and preserved in memory, relatively unchanged, from the moment of experience to the moment of report. One might also suspect that gross features would be less subject to revision or confabulation under situational pressures than a nuance or detail.

But how likely are we to get it right about the gross features of our conscious experience in the first moment of introspective reflection? Let's set aside questions of long-term memory for the moment and consider short-term memory or even concurrent introspection. Returning to the eyewitness analogy: How likely is Melanie to have "seen" things correctly in the first place? Except in unusual circumstances of visual illusion or magic shows, we're generally unlikely to misperceive the gross features of nearby events witnessed in good conditions. We might misperceive the thief's hair color in the sun, but we wouldn't misperceive his blue getaway car as red or see him as driving off thataway (south) when he's actually driving the opposite direction (north) – much as we might, surprisingly often, misremember such matters later. An eyewitness who immediately (within a few seconds) explicitly notes such easily perceptible features of the event, and who keeps her notes for consultation, is considerably less likely to misremember those features later than one who waits a few minutes

or hours. If Melanie's immediate knowledge of the gross features of her own experience is as good as an eyewitness's knowledge of large, nearby events, we might likewise be justified in accepting the first thing or two she notes in each sample.

However, I would argue that our introspective and immediately retrospective knowledge of our own experience is generally not as good as our knowledge of the most easily perceptible outward events in our vicinity. This is the reversal of Cartesianism that I advocated in Chapter Three. Preconception, expectation, lack of practice, weak linguistic and conceptual tools, the instability and skittishness of experience (combined, perhaps, with its complexity), conspire to produce introspective judgments that are often grossly false, even regarding the most basic features of current or immediately past experience. As I suggested in Chapter Three, there's little reason to think we get it right, even in the most careful reflection, about such things as the basic structural features of our imagery (regarding, for example, how detailed it is in the periphery and whether it arrives instantly or is built up a piece at a time as we think about different aspects of the imagined scene) and emotional experience (regarding, for example, whether it is experienced as entirely bodily or whether there's some non-bodily cognitive component) and the clarity of peripheral vision (as I argue in Box 4.18). (For further development of these points, see also Schwitzgebel, 2002a, forthcoming-b, and in preparation.) Philosophers, psychologists, and ordinary folk persistently

disagree about such matters, and it seems indolent utopianism to suppose that everyone is simply right about their own experiences and wrong about everyone else's – especially given the lack of evidence for cognitive differences between people corresponding to their different experiential reports.

We can even neglect and invent whole modalities of experience, as I've argued in the case of echolocation (the ability to hear the location and properties of silent objects through attunement to how they reflect and alter environmental sounds). Many people – even, historically, many blind people who've actively used echolocation in navigating around walls and obstacles (and also, famously, Nagel, 1974) – deny any auditory experience of or capacity for echolocation; yet most can be brought to change their minds with a few introspective experiments. (Close your eyes and say “shhhh...” while a friend moves her hand around in front of your face; you can hear where her hand is; see Schwitzgebel & Gordon, 2000, for details.) Likewise, there's a lively controversy about whether there's a distinctive experience of thinking over and above the experience of imagery and inner speech (for a brief review, see Schwitzgebel, in preparation; see also Russ's discussion of “unsymbolized thinking” in Ch. 11.1.7.4).

I won't argue these points farther here, but I draw the following conclusion: Even at the first instant of reflection about her experience, Melanie might be quite badly mistaken about it. Introspection is more difficult than ordinary perception. Convincing or reminding ourselves of this difficulty is crucial in our evaluation of

Melanie's accuracy. Thus, I think we must add to the concerns discussed in the previous two sections another major source of error, one that undermines even the first and most basic aspects of Melanie's reports: the intrinsic difficulty of the observation. Although Russ has done good work in trying to reduce certain sources of error (as discussed in Chapter Two), the fundamental difficulty of the observation remains.

We cannot, of course, given our current state of knowledge about experience in general and Melanie in particular, prove gross error in any of Melanie's reports. However, let me list some of the relatively gross claims about which I'm most suspicious: that Melanie literally visually experienced a "rosy-yellow glow" in Beep 1.1 (see Box 4.7); that she was as consistently and robustly self-conscious as she claims on Days 5 and 6 (Beeps 5.1, 6.1, 6.2, esp. Boxes 8.9, 9.3); that her imagery was as detailed as she says in Beeps 1.3, 2.1, 2.2, and 5.1 (see esp. Boxes 4.18, 5.4); that she literally imagined individual overlapping echoes of "nice long time" in Beep 6.4. In some sense, such matters are details. They're not the sort of thing an untrained reporter would probably first notice about her experience – and maybe, indeed, Melanie did not reflect on such matters swiftly after the beep (without her notes [see Box 4.15], we have no way of knowing). At the same time, these are the kind of basic structural facts about experience that should interest a researcher in consciousness studies. Furthermore, in general, I think it quite possible that Melanie is missing whole modalities of experience that are

difficult to discern and report – such as perhaps imageless or “unsymbolized” thinking, if it exists, or unattended visual experience – focusing on and remembering, instead, only those aspects that happen to come to mind first or are easiest to parse.

Why, you might ask, am I more skeptical of these particular reports – the ones described at the beginning of the previous paragraph – than others? I have no reasons internal to the interviews. I can detect no telltale signs of error in Melanie’s patterns of speech, for example, or any special hesitation, uncertainty, or inconsistency on Melanie’s part in making these reports (though I confess that I may have a tin ear for such things). My reasons are entirely external: Melanie’s reports here poorly match my pre-existing impressions about what’s common in experience, based on my understanding of my own experience and my reading of the psychological and philosophical literature. Now Russ will surely object here that in so evaluating Melanie’s reports, I’ve failed to “bracket preconceptions,” and thus am not giving his method a fair shake in its own terms. Russ and I have been through this dialogue multiple times (e.g., Box 9.9). Let me add, here, a few thoughts to it.

First, I acknowledge the appeal of “bracketing preconceptions” for the purpose of conducting a friendly, relatively unbiased interview. Surely an interviewer can err through too lively a commitment to seeing the subject a particular way; perhaps indeed this is the error most to be avoided. But it’s one

thing to bracket preconceptions (insofar as possible or desirable; see Section 1 above) as part of an interview technique, and quite a different thing to discard all prior (non-DES) evidence about experience in one's later evaluation of that interview. I don't know whether Russ really means to recommend the latter course; but sometimes it seems to me he comes across that way, for example in his reference to earlier methods as "failures" and in his tendency to disregard previous literature in interpreting his results. This point is central to understanding the role Russ envisions for DES vis-a-vis other methods. So let me re-emphasize here that one could only justifiably take the extreme position of disregarding all prior evidence in one's evaluation of Melanie's assertions if it were somehow already established that Russ's experience sampling method was so superior to all other sources of evidence as to automatically trump anything contrary. As I argued in Section 1, I don't think that has been established. Unless we've decided to accept DES as our sole guide to the truth about conscious experience, it makes no sense entirely to forgo our previous inclinations – whether the fruit of other methods or general plausibility arguments – in reaching our final judgments about how far to believe Melanie's reports.

Second, although I said in the first section of this concluding essay that we have no means of comparing Russ's picture of Melanie's experience with the picture that other methods would have produced, that may not be quite true. No direct comparison between methods is possible here, but maybe we can make

indirect comparisons. If Russ's picture of Melanie generally comports with what we'd expect based on prior methods, that provides a kind of support for it; if not, that may raise concerns. It seems to me that the above-cited claims comport worse with my sense of prior research, and my own experience, than other of Melanie's claims. In particular, all those claims strike me as relatively unusual. I think I am, then, justified in being somewhat more suspicious of them (see Box 9.9 for further development of this point). Of course, we don't want to rule out a priori that DES interviews could reveal anything undiscovered by previous approaches. I am not saying that Melanie must be wrong, just that I'm worried.

Third, I acknowledge that my own sense of plausibility and likelihood differs from others'. This is problematic. I claim no unusual introspective expertise. I've read widely on consciousness and reflected somewhat on my own experience, but no more than others who disagree with me about various substantive issues. The phenomena are elusive, the literatures complex, contradictory, and confused. So I can't say that I feel myself to be on any especially solid ground when I am inclined to accept one piece of Melanie's testimony more than another based on prior impressions.

Reflecting in this way, I begin to feel near total darkness about experience. Can I really make any good judgments about the better and worse in Melanie's reports? When I consider my own poor antecedent knowledge about conscious experience, my self-assurance begins to fail. I was inclined to mistrust Melanie's

reports of detailed imagery because I have a general impression that visual imagery is sketchier, less determinate in its details, especially when quickly generated, than many people suppose. But on the basis of what have I arrived at this opinion? I've already said that there is no single, dependable method for studying consciousness. Maybe, then, I believe what I do about imagery because there's a consensus among researchers applying a variety of methods, individually weak but jointly persuasive? No, there is no such consensus. I must admit by my own lights, then, that I could easily be quite wrong in my opinions about imagery. Indeed, it was my genuine dissatisfaction with my own (and the field's) condition on such matters that led me to Russ in the first place, looking for something better, or at least something additional. So maybe Melanie is quite right about her imagery (her rosy-yellow glow, her self-consciousness, her echoes), and I am wrong.

Conversely, however, maybe in other cases I should mistrust Melanie more than I do. When she reports a feeling of conviction in 6.1, or a lightness in her chest in 6.2, or imagery (of any sort at all) in Beeps 1.3, 2.1, 2.2, and 5.1, I feel no particular suspicion. But if the state of the field, and my own epistemic state, is as much a mess as I think – and if I am right in insisting that gross introspective errors are generally quite possible – perhaps I'm too easily taken in by what seems to me the plausibility of these reports. Does conviction really carry some sort of positive phenomenology, a “feeling” of conviction? Or is conviction just a

state of reaching a definite judgment, perhaps accompanied by general arousal or specific imagery, but without any distinctive experiential element of its own? Is “lightness in the chest” a misleading metaphor (but one that, for some reason, ensnares me more than “seeing rosy-yellow” [Box 4.7] or “hearing echoes” of inner speech [Beep 6.4])? Could Melanie’s imagery all be invented after the fact? I feel I’m losing my grip on what good reasons there are for thinking Melanie hasn’t gone astray in these ways.

In his talk of “bracketing preconceptions” and of the need to reject “armchair” speculations and earlier failed methods, Russ conveys doubt about the value of people’s – including my – prior (non-DES) sense of what’s credible or relatively less credible in Melanie’s reports. Perhaps I should join Russ here and mistrust myself. However, I can’t afterward bring myself to the next move Russ recommends: trusting his interview techniques instead. Melanie’s internal consistency, her evident conscientiousness, her happy confidence alone, I’ve argued, can’t justify our credence, even if Russ has succeeded in producing an admirably neutral context for reporting. But now it seems only a short step to radical uncertainty about Melanie’s reports. I have no idea where to doubt and where to believe, so I am left only doubting. And worse: Since I have no reason to think myself any better an introspector than Melanie, my own introspective judgments come under skeptical threat as well. In any reflection I could very

easily be wrong, and my prior sense of plausibility is too ill-founded to be of much help.

But utter uncertainty (about anything sufficiently broad: the external world, other minds, the future) is philosophical madness, isn't it? And few philosophers even among the radical skeptics have dared entirely discard our knowledge of our own ongoing conscious experience. How could we be totally in the dark about that? I just experienced some "auditory imagery" or "inner speech" or "inner hearing" (if I try to be too precise here, I might lose hold of it): I heard or spoke, silently, the sentence I was about to type. I can't seem to bring myself sincerely to doubt that claim or to assign it any but the very smallest probability of being false, despite all the reflections that have led me here. And if Melanie seems to be reporting something similar in her own experience – well, there we have a beginning!

So maybe it's only modesty and caution I should recommend, and not utter skeptical uncertainty. For what is nearer to hand and riper for discovery than our own experience? Yet even the meekest and most tentative reflections about experience are bound soon to conflict with what others have said, so widespread and fundamental are the disagreements in consciousness studies.

At the most general level, I suppose I haven't moved far from where I began before meeting Russ – tempted by radical skepticism, suspicious of every method, doubtful about the future of the field. At the same time, this temptation, suspicion,

and doubt, this kind of half-convinced pessimism, is not a deep conviction that introspective science must fail. In fact, in the long run I feel hopeful that we will make some sort of progress, simply by virtue of applying our good minds to it hard enough and long enough in enough different ways. And Russ has convinced me that beep-and-interview methods deserve as large a role, for now, as anything else.

## **Chapter Eleven**

### **Russ's Reflections**

#### **Russ Hurlburt**

We have traversed a crooked path over the course of this book, following randomly selected concrete instances of Melanie's experience into whatever thickets they happened to lead. Now it's time to straighten things out, for me to say what I thought happened here and why the path was worth the effort. I do so in two parts. In section 1, I discuss my own observations. In section 2, I reply to Eric's observations from Chapter Ten.

Before I do that, I wish to emphasize how much I respect Eric's participation in this project. Despite his skepticism, he was willing first to try out DES for himself; then to recognize the conflict of serving as his own subject and to agree that we should find a more neutral subject (ultimately Melanie); and then to participate in the making public of this interchange that took place in an arena where I was far more experienced than was he. That is the heart of good science: as much as possible to subject one's own views to the scrutiny of reasonable but not-necessarily-like-minded others. In passing, let me say that over the years I have made similar would-you-like-to-participate-in-DES offers to many other philosophers and psychologists, nearly always with the same result: their retreats make Roadrunner look like he is dragging an anchor.

I think Eric and I did a good job of avoiding a “My theory is better than your theory” interchange. Instead, we have brought our quite different views into a candid collaboration / confrontation where both of us expected to be altered and would have been happy to be proven partly or entirely wrong.

Also at the outset I emphasize that I agree with Eric that skepticism about introspective reports is highly desirable; that the base rate of successful introspections is small; and that Melanie’s sincerity and conscientiousness and my carefulness and even-handedness does not in any way guarantee that my conclusions about Melanie’s experience are correct. It is the size and extent of the skepticism, not its desirability, that is at issue here.

## **1. Russ’ Views**

### **1.1. About Melanie**

We discussed 17 samples with Melanie and on that slim basis learned quite a bit, I think, about her. We discovered that she engaged in an active self-monitoring of her own actions: observing her mouth closing while speaking in Beep 1.3, observing being bent over at sink in Beep 2.4, observing the fogginess of her experience in Beep 3.2, observing her forgetfulness of the parking brake in Beep 3.3, observing that her eyes were looking straight ahead while talking in Beep 6.1, observing the bodily aspects of feeling happy in Beep 6.2, observing her brow furrowed in concentrating and the positioning of her feet in Beep 6.3.

We discovered that she paid thematic attention to the sensory aspects of her environment: the green color of the TV screen in Beep 1.4, the coldness in her toes in Beep 2.3, the coldness and gooiness of the toothpaste in Beep 2.4, the bodily bobbing up and down in her imagination in Beep 4.1. These awarenesses are not merely paying attention to the objects in her environment, but paying particular attention to the sensory aspects of those objects.

We discovered that she had detailed visual images: of the soldier on a dirt road in Beep 2.1, of Stukas in Beep 2.2, of a shopping-list pad in Beep 2.3, of the Bicycle card joker in Beep 4.2, of an intersection with apartment buildings in Beep 5.1; and we discovered that she created those details even in the absence of the correct knowledge of what those details should look like: the Stukas were F-18s in Beep 2.2, the joker was incorrectly imagined in Beep 4.2.

We discovered that she had feelings, sometimes expressed bodily: of sadness/dread pressing on her chest in Beep 2.2, of yearning in Beep 4.1, of conviction that she was correct in Beep 6.1, of happiness (a lightweight feeling in her lungs) in Beep 6.2, of concentrating in Beep 6.3. But we also discovered that sometimes her feelings were apparently ongoing in her body but are not directly experienced: of being exasperated but not experiencing it directly in Beep 3.3, of concern and resentment not being directly experienced in Beep 4.2, of anxiety about being late not being experienced in her body but being thought about in Beep 5.1.

Melanie, by her own retrospective report, was surprised by some aspect of all these characteristics. She had apparently no knowledge at all of the fact that she was as absorbed by the sensory aspects as she was; she knew she had visual images but was surprised by the incorrectness of their detail; she was unaware of the emotional processes ongoing outside of her awareness.

### **1.2. How Far Does Russ believe Melanie?**

I believe, pretty much as does Eric, that there is reason to accept at least in broad strokes the veridicality of Melanie's reports. Certainly there is reason to quibble about some things: as we have seen, her reports on the first sampling day or so might reflect more her presuppositions than her actual experience; her images may be incorrect in some of their details because the interview took place the next day; and so on. But none of these quibbles is enough to overturn the overall accuracy of the observations. If we were particularly concerned about the first few sampling days, we could discard those beeps and sample with Melanie for a few more days. If we were particularly concerned about the forgetting or confabulating of image details, we could give Melanie a tape recorder and ask her to dictate the image details immediately following the beep rather than rely on written notes. Thus I believe that Melanie's accounts are pretty darn good; we could incrementally but not dramatically improve on them if we wished.

I have sampled with several hundreds of subjects at the same or greater level of detail and skepticism as we applied with Melanie. I am convinced that the general statements that we made about Melanie (that she engaged in active self-monitoring of her own actions, that she paid thematic attention to the sensory aspects of her environment, etc.) do not apply to all subjects or even to most subjects. I do not wish to claim that we discovered Melanie's essential uniqueness, but I do believe that, for example, most of my subjects do not engage in the kind of active self-monitoring that Melanie did. Whether observed differences reflect actual phenomenological differences or merely expressional differences is of fundamental importance. I assure the reader that for 30 years I have interrogated subjects in what most would say is excruciating detail on this particular issue, and during that time I held no particular position on the desirability of one outcome or the other. Those observations have forced me to conclude that people's experience actually differs from one person to the next – that these differences are not merely differences in reporting style. I would have been just as happy if the universe had turned out otherwise, but it didn't.

**1.2.1. Raw vs. Exposed Reports.** In evaluating Melanie's accuracy, we need to make a distinction between what I call "raw reports" and "exposed reports." A raw report is what a subject unaidedly reports about her inner experience; an exposed report is the result of the DES expositional interview, the

result of clarifying to the extent possible the subject's inner experience. As we have seen, I think that Melanie's raw reports contained much that was believable and much that was not to be believed (particularly early in her participation in this process). Melanie was, I think, a typical subject in this regard. I was skeptical of her early raw reports, for example, of her inner thought voice of Beep 1.1 (see Boxes 4.1, 4.8, 9.10, and my discussion of faux generalities below). However, I found much less to be skeptical about in her raw reports from the final few sampling days.

I think our exposed reports of Melanie's experience, the understanding of her experience that Melanie and I (and to some extent Eric) shared at the end of each interview about her experience, contained very much that was believable and very little that was not to be believed. Thus, for example, I do believe Melanie's reports about the detailed nature of the images of the soldier on the road (Beep 2.1) and that the Stukas really looked like F-18s (Beep 2.2). I am perfectly willing to accept that a few of the details in those images may have been confabulated or otherwise mistaken – Melanie and we are not infallible – but I see no reason to believe that Melanie confabulated all or most of the details. The Stukas-as-F-18s is a good example (see Box 5.9). If one denies the existence of details in images, where did the F-18-ness come from? What would have motivated Melanie, in telling us about her experience, to say that these Stukas looked like F18s? It's hard to believe she was simply trying to please us by giving such an outlandish

report. The more plausible explanation is that she was somehow seeing in inner experience something that looked like an F-18. I can accept the possibility that the F-18ness was not a part of the image at the moment of the beep, but was created only in the telling about the beep, but then one has to explain why an image at the moment of the beep can't have details when an image at the time of the report can be detailed. That is of course possible, but I can see no reason to force such a complicated explanation in the absence of any direct evidence.

I accept the fact that the exposed reports so obtained may not be a complete account of Melanie's momentary experience; see the discussion below in section 2.2. However, in general I agree with Eric's limited approval: I do think that "what [our interviews of Melanie] deliver is probably about as good as can reasonably be expected from open interviews about sampled experiences."

**1.2.2. Faux Generalization.** When Melanie uses the terms "all the time" and "whenever," (for example, in Beep 1.1: "It's my inner thought voice, so it's the one I recognize and hear all of the time whenever I'm thinking"), she shows that she is making what I have called a faux generalization (see Box 5.17). Her statement has the appearance of a truly inductive generalization, as if she had observed a series of instances of thought voices, noted that they all have the same characteristics, and reported that generalization. But it is highly unlikely that her statement is actually the result of such a truly inductive process. That statement is

much more likely the result of the cognitive heuristics such as availability, recency, salience, accessibility that Kahneman and Tversky (and others) have described.

DES has shown that such self-characterizations are often not true and occasionally dramatically not true. I have had seemingly normal graduate students say they experience frequent images, but sampling produced none. I have had other seemingly normal graduate students say they have no visual images, but sampling produced many. Now it is certainly possible that some faux generalizations are true – Melanie may well have the kind of inner voice she described – but by and large they cannot be trusted. That’s why part of the DES strategy is to discourage faux generalizations, to encourage subjects to suspend their belief in their own self-characterizations, to focus on the actually occurring instants on which true generalizations can be built.

Subjects typically understand this quite readily. If I say something like, “Well, your self-characterization might or might not be true; let’s try not to be influenced by it one way or the other and see what emerges in the samples,” most subjects are not offended and recognize that value of such an approach. As a result, most of the time, the expression of faux generalizations gradually disappears during sampling. One might argue that I punish the expression, so the expression disappears while the belief lingers on. I don’t think that’s true; most subjects would convincingly say that it’s not true. Melanie was a quite typical

subject in this regard. I believe she came to see that her faux generalizations interfered with her ability to observe her experience accurately and that she gradually developed the skill of suspending them. As a result, her raw reports became more accurate, and it became easier for us to filter out remaining inaccuracies and do a better job of creating accurate exposed reports.

### **1.3. Inner Speech**

It is useful to comment on Melanie's lack of inner speech because many theorists hold that all thinking is inner speech and that inner speech should therefore be ubiquitous across all DES subjects. Baars, for example, claims that “human beings talk to themselves every moment of the waking day” (Baars 2003, p. 106).

It is usually very easy for DES subjects to report inner speech and very easy for investigators to recognize it. Subjects who have frequent inner speech (and there are many such subjects) make for generally the easiest, least ambiguous sampling studies. Inner speech often involves full sentences, naturally inflected, with the same kind of pauses, stutters, voice pitch and rate, emotional tone, and so on as external speech.

Melanie was not like that. First of all, she had no clear cut examples of inner speech. She reported inner speech twice out of the four samples on the first sampling day, but those reports are dismissible, I think. It is often the case that

DES subjects, like Melanie, frequently report talking to themselves on the first sampling day and rarely make such reports later in sampling. I take that to be the result of the subjects' initial presupposition that thinking is inner speech. The questioning of the first day is designed to bracket all presuppositions including that one. If subjects later report no inner speech, then I attribute the early reports to the presupposition and the later lack of report to the successful bracketing of that presupposition.

The closest Melanie got to inner speech was sample 3.1, where she was thinking “peri-, peri-,” to herself, the first part of the word “periodontist” that she was trying to remember. But questioning revealed that she wasn't really sure whether she was saying “peri-” or experiencing it in some other way. I take no position on whether this “peri-, peri-” experience was or was not (vaguely or faintly experienced) inner speech; I think trying to force such a conclusion is a mistake (see section 2.2 below). Certainly Melanie did not have the kind of clear and frequent inner speaking phenomenon that is common among many DES subjects.

At sample 3.3, Melanie experienced her own voice saying the first part of the sentence, “Why can't I remember about the parking brake.” Melanie, like most DES subjects, apparently made the discrimination between inner speech and inner hearing confidently and experienced this sample to be inner hearing. Inner speech, the more common phenomenon across subjects, is experienced to be

“going away,” “produced by,” “under the control of” the subject, “just like speaking aloud except no sound.” Inner hearing, by contrast, is the experience of a sound “coming toward,” “experienced by” rather than produced by, “listened to” rather than spoken, “just like listening to a CD.” The typical subject is not confused between inner speech and inner hearing any more than they are confused between speaking aloud and hearing a tape recording of themselves speaking.

#### **1.4. Why the Personal Is Important**

In one sense, this book is entirely about Melanie; in another sense, it is not about Melanie at all. Clearly our main intent was to find out something about inner experience, about interviews, about the difficulties of apprehending another’s conscious experience; our major aim was not to find out something about Melanie as a particular individual. Melanie herself, really, means little to the reader.

So why do we have to spend so much time with Melanie? Couldn’t Eric and I just juxtapose our theoretical positions like semiprecious stones in a tumbler and turn on the critiquing process? I think not. Melanie’s moments are, to stretch the metaphor, the carbide grit in the tumbler. It’s not merely that our theories grind on each other as the tumbler rotates, but that our theories grind on the facticity of Melanie’s reports in which both Eric’s and my views are bathed. We

need Melanie to keep us in the concrete, to prevent us from flying away to the abstract.

In The Stranger, Camus has Meursault say to the priest, “And yet none of his certainties was worth one strand of a woman’s hair.” I take that as an artistic critique of theory, formal knowledge, science, philosophy, psychology, abstract ideas in general. Perhaps to overdraw the point somewhat, the more one is involved in theoretical / formal / scientific / abstract knowledge, the less one confronts, contacts, encounters, is impacted by, touches real people. It’s not merely that a person has a limited capacity, as if time spent in theory reduces the time spent encountering real people. It’s not merely personal idiosyncrasy, as if some personalities incline to theory while others incline to people. On the contrary, theory (or formal knowledge in general) tends to hide real people, to split one away from real people, to create the illusion (I might better say “delusion”) that it approaches real people while at the same time eliminating the knowledge that real people are actually being lost in the process.

I don’t think that theory must hide real people, only that it overwhelmingly has. A science of people might well be built up one hair (with apologies to Camus) at a time; a few (Oliver Sacks comes to mind) have pointed in the right direction. Eventually, perhaps, many hairs might be braided into a beautiful and secure scientific coiffure. But at this time, it seems to me, psychological science and philosophical analysis has for the most part maintained

that we can do the coif without paying attention to the individual hairs, a view I think is fundamentally mistaken.

I think an interest in theory and in formal knowledge in general tends actively to interfere with an interest in personal truths. Interests, including interest in theory, are sets of skills, involving real bodies / minds engaged in real activities, strengthening this muscle, building that coordination, and so on. A proper theoretical interest involves the skills of standing still while the surroundings change and of suppressing the individuality of the theorist. A good theory is one that is independent of the person stating it – you’d be critical of a theory if it held when you state it but didn’t when I state it. And the best theory is the one that is most independent of the characteristics of the target as well – the more universally true the better. Universal truth doesn’t care whether we’re talking about Eric or me or Melanie. So the best theory (or best formal knowledge in general) is the one that is the least interested in real people, least interested in the theorist or in the subject.

Thus one of the main aspects of the general / theoretical skill is to suspend the personal, to act as if the personal didn’t exist or wasn’t important, so that the general / theoretical speaking can take place independent of the speaker and independent of any particular person spoken about. If one exercises that skill, gets good at it, one usually develops the skill of ignoring the personal, of holding the personal at bay.

The problem is that formal knowledge can create the illusion that it approaches real people when in reality it turns its back on real people. Psychology (philosophy, too, probably) graduate programs emphasize formal knowledge and as a result spend little time teaching how to observe people accurately. It's a striking omission, so ubiquitous it is rarely noticed. To observe people accurately (or even to try to observe people accurately, or even to observe one's failure to observe people accurately) is in many ways incompatible with (even antagonistic to) modern psychology.

In some ways the situation is similar to the distinction between the classical and the jazz musician. Classical performance is a skill that involves suppressing many of the bendings and other licenses that are the hallmark of jazz performance. Classical performers get good at such suppression; that's why opera singers who sing popular tunes or symphony orchestras that play jazz almost always sound stilted and awkward. But there are a few exceptions (Wynton Marsalis comes to mind), performers who can advance both the classical and the jazz mediums.

I do not in any way wish to contend that the personal is more important than the general / theoretical. The ideal psychological scientists, in my view, would be at home in both worlds, Marsalis-like. I do contend that the corrective that currently needs to be applied is to push strongly in the direction of the

personal. That's why we involved Melanie, to keep us focused on at least one flesh-and-blood person while we discussed general issues.

**1.4.1. Personal Truth.** So I think we want a “hairy” science, one that starts with personal truths and builds toward the general / theoretical. By “personal truths” I mean that which is both personally true and truly personal. Suppose we know that Melanie’s femur is 16 inches long. That is a true fact about Melanie as a particular person – we can say that feature is personally true of her. But it is not truly personal – it does not reveal much about what Melanie is really like. Certainly it says something about Melanie as a person – she’s of about average height – and that fact is indeed important to such things as her promise as a volleyball player. But that fact is not truly personal. By contrast, our general observation of Melanie’s samples that she frequently makes self-reflective, self-monitoring observations is truly personal. This observation allows Melanie to emerge as Melanie really is, of herself, by herself, for herself, not as a member of one of my favored theoretically created groups, not by comparison to some standardization group in a psychological test such as the MMPI, not as an instance of some universal truth, but as Melanie revealed to herself on her own terms one moment of lived experience at a time. Out of the nearly infinite welter of experiential phenomena that might present themselves to Melanie, self-analytical phenomena do present themselves to her repeatedly.

We have looked at only 17 moments of Melanie's existence. That's obviously a very small sample, but even so a substantial pattern emerges. Clearly we need to observe more samples to be confident of any general statement and its limitations and range of convenience, and we should do so. Then psychological science, if it is to be efficacious, can start with a true understanding of what Melanie is really like, and similarly of what John and Jane and Maria and Sam and Julio are really like and how they are the same and/or different from Melanie and each other.

Thus I think good theory is possible but rare. It would be truly personal – it would acknowledge and start with the messy, tangled idiosyncrasy of the objective reality with which we're dealing. DES is an attempt to create a personal starting point. One may dispute whether the attempt is successful, but at least it is an attempt. One might be able to advance different, perhaps more effective attempts; that would be terrific, from my point of view.

**1.4.2. Developing a Taste for Specific Moments.** It seems to me that psychological science must develop a taste for the exploration and accurate reporting of concrete moments of experience. One might argue that this taste is perhaps the result of my many years of creating such reports using DES, but I think it is the other way around. DES is at least as much the result of the taste for accurately described concrete moments as the cause of the cultivation of that taste.

I do think the cultivation of that taste is possible, and the first step is to accept that that cultivation might be valuable.

### **1.5. Discovery vs. Confirmation**

Modern psychological science is very impatient, it seems to me; for the most part, its practitioners can see little value in wading through the details of randomly selected moments in hopes that some substantial personal truth might emerge. But if there is to be any significant discovery in the science of psychology, it will have to be built up out of real generalizations (as thoroughly distinct from faux generalizations) about personal truths (as thoroughly distinct from valid measures).

There are two related but separable dimensions to this impatience, the personal / impersonal and the discovery / confirmation dimensions. I have just discussed the personal / impersonal dimension and made the case that science needs more emphasis on the personal end of that dimension. Now I make the case that science needs more emphasis on the discovery end of the discovery / confirmation dimension.

Psychological science typically proceeds by making a generalization (often called a hypothesis) before it collects its data and then seeking to validate that generalization (test the hypothesis) afterwards. There is nothing inherently wrong with that procedure; good science would be at a substantial loss without it.

However, the creation of the to-be-validated generalization is an important part (arguably the most important part) of the validation process, because a validation study is quite fully constrained by the original generalization. An experiment, for example, lays out its possible results at the time of its design, before the data are collected; the interpretation of an experiment is mostly a selection from among the possibilities laid out in advance (including the possibility that the generalization is false).

Unfortunately, in my view, modern psychological science pays inadequate attention to the creation of hypotheses in its impatient rush to validate them. I don't think that that sequence can be effective in the long run, because (among other reasons) it elevates the status of presuppositions rather than diminishes it: A hypothesis is entirely (or almost entirely) shaped by the presuppositions behind it. Psychological research should spend much of its time carefully observing personal truths, advancing tentative real generalizations, making more observations, and revising the real generalizations, and only then advancing hypotheses that might explain those generalizations. It is through that process that discoveries will be made, which can later be validated by standard psychological science.

Thus the order of generalization-making is a fundamental structural issue for the field. A validation study states the generalization at the outset and tests that generalization. Eric's rich / thin investigation is of this type: his

generalization that all or most subjects have visual experience all or most of the time shaped the very structure of his entire experiment. By contrast, our investigation of Melanie is a discovery study, which collected the data first and made generalizations only later. We did not set out, prior to interviewing Melanie, to determine whether Melanie had frequent self-monitoring experience. On the contrary, we observed Melanie the way Melanie was, with as many of our presuppositions held at bay as possible. On the basis of that series of observations, we arrived at the emergent real generalization that Melanie had frequent self-monitoring experience. Subsequently, after the observation of Melanie and many others, we might advance hypotheses such as that people who have frequent self-monitoring also X. Then it would be appropriate to validate that hypothesis.

I am entirely in favor of validation studies – I’ve written a book about how to do that process (Hurlburt, 2006). My criticism is only that psychological science expends far too much of its energy far too soon on validation, and expends far too little energy perfecting the careful observation skills that can lead to genuine discovery.

### **1.6. On the Science of Inner Experience**

As we have said all along, we are not here particularly interested in Melanie; our main interest is in the science of inner experience and what our discussions of Melanie have to say about it. The main question for me, then, is that if we accept

the characterization of Melanie's sampling presented above, and if we accept that the same characterization applies to many if not most subjects, are the data obtained from DES good enough on which to build a science of inner experience? I think the answer is Yes, at least in an engineering sort of way. The engineer knows about a variety of building materials: steel girders, wood two-by-fours, etc. He also knows that he should not have absolute confidence in any particular steel or wood beam due to imperfection in materials, inconsistency in processes, incorrect installation, etc. Instead, he accounts for his lack of single-beam confidence by incorporating the margin of error and redundancy that his science has found desirable. The science / art of engineering is thus not to specify exactly how a particular object will perform, but to understand the materials available and the tasks at hand, to try to match them appropriately, and to design for expectable flaws.

I think it likely that a science of experience can be constructed in a similar fashion. We can accept the fact that we don't believe all confidently-asserted-and-robustly-vetted reports; however, at the same time we can have confidence in a science of experience built out of a redundant set of independent reports, as long as the independent reports are pretty good. What we need is a science that, at least approximately, credits apprehensions of experience to the extent that they are creditable, and discredits apprehensions to the extent that they should be discredited, and uses reports in appropriate venues. We don't have to be perfect in

this regard; we need enough redundancy that we can continue to support the science even if an occasional subject is mistaken, or doesn't understand, or lies, etc.

### **1.7. Bracketing Presuppositions**

Many if not most of Eric's and my points of disagreement have in some way to do with whether or not or to what extent we should bracket presuppositions. The bracketing of presuppositions is a central or at least major peripheral issue in: the problematic of retrospective and armchair introspection (Boxes 4.2, 4.11, 5.6, 5.7, 5.17, 8.5, 8.7); inner vision has the same characteristics (angle of field, etc.) as exterior vision (Box 4.6); visual experience always occurs (Box 5.7, etc.); visual experience is sketchy (Box 8.2); imagery should parallel external seeing (Box 8.3); accepting more easily things that seem plausible (Boxes 5.7, 8.5); the issue of auditory imagery vs. inner speech (Box 5.7); people are mostly alike (Boxes 7.4, 8.5); the cultural impact on reports of thinking (Boxes 7.12, 7.13); the situational impact on reports of inner experience (Box 8.1); emotionality does not involve color (Box 4.7); that the laws of physics and time apply to inner experience (Box 9.8 and the surrounding discussion); on the standard of evidence (Box 9.9); and so on. I now take this opportunity to expand my views on the bracketing of presuppositions, an essential feature of what I would call a good science of inner experience.

My quarrels with Eric about the bracketing of presuppositions have not been mere cavils; on the contrary, they have reflected the fact that I believe that the bracketing of presuppositions is, at least at the present time, the central issue of consciousness studies and psychological science. Get the bracketing of presuppositions adequately right and consciousness studies can advance; don't get it right, no advance.

The concept of the bracketing of presuppositions is not, of course, my invention; Husserl and other phenomenologists have discussed the "bracketing," the "setting aside," the "putting out of play" of presuppositions as part of the reductions that occur in a phenomenological analysis. I have adopted the same terminology, although we should note that my use of the term "bracketing presuppositions" is somewhat different from that of Husserl. Husserl's intention by bracketing presuppositions is to arrive at pure ego, pure consciousness, the differentiation of the perfection of evidence, apodictically secure philosophy. My use of the term "bracketing presuppositions" is quite pedestrian by contrast, because my goal is adequacy, not purity, perfection, or apodicticity. I want investigators to bracket presuppositions to enable them to move from being substantially blind to being "pretty darn good" at conducting an expositional interview. That's a substantial move, but less than Husserl advocated.

**1.7.1. Bracketing Presuppositions Is Necessary.** I have explored the inner experience of several hundred people at the same or better level of detail and skepticism as we did with Melanie. Furthermore, I have observed and/or participated in the training of a few dozen investigators. On the basis of those explorations, I am convinced that most subjects misrepresent their own experiences to a greater or lesser degree, usually as the result of incorrect presuppositions about the characteristics of their own experiences. I am also convinced that most psychologists, philosophers, and laypersons (including those who are attracted to methods like DES) misrepresent their subjects' experiences, usually as the result of incorrect presuppositions about the characteristics of experience.

The common denominator in both misrepresentations is the failure by subject and / or investigator to bracket presuppositions. Both the subject and the investigator should set aside or put out of play the notions that distort the reports about experience.

The adequate bracketing of presuppositions is a necessary condition for the "independent" requirement of my summary "we can have confidence in a science of experience built out of a redundant set of independent reports." A presupposition, whether conscious or unconscious, known or unknown, forces all observations into alignment, forces all observations to have the same flaw, thereby ruling out independence from one report to the next. That is just as

destructive to the science of experience as it would be to the engineer who used beams that all had the same flaw – when the conditions are just right, all the flaws work in concert and the structure collapses.

The main skills of the investigator's task are to bracket the investigator's own presuppositions and to help the subject bracket the subject's own presuppositions. There is nothing fundamentally complex about that, but in practice it is quite difficult. Presuppositions run deep, and people (subjects and investigators) are blind to their own most important presuppositions.

**1.7.2. Helping the Subject Bracket Presuppositions.** I want to help the subject bracket whatever presuppositions about inner experience she may have. As an example, I want to help Melanie bracket her presupposition of talking to herself, which we discussed above in the Inner Speech section. On her first sampling day, Melanie described herself as talking to herself, apparently on the presuppositional belief (with Baars and most of the Western tradition) that all thinking involves talking to oneself. The bracketing task in such situations is neither to accept Melanie's talking-to-herself reports as being accurate nor to reject them as being false. Instead, I want somehow to convey to Melanie the desirability of suspending judgment (bracketing) about whether talking to herself is an accurate description of her experience. I want to convey to Melanie that it is okay with us either way: if she does in fact talk to herself, we want to hear about

it in her reports. If, on the other hand, she does not talk to herself, we want to hear about whatever phenomenon is present other than such talking.

**1.7.3. Bracketing the Investigator's Presuppositions.** Melanie's inner speech is an example of helping the subject bracket her presuppositions. DES also asks its investigators to bracket their own presuppositions. We have discussed at length several of Eric's presuppositions, for example that he believes that people do not experience plentiful detail in their visual imagery (see Boxes 4.18, 4.19, 5.1, 5.4, 5.11, 7.8, 8.2, 8.3). That view of images doubtless is the product of some combination of armchair introspection (Eric's own imagery is perhaps not detailed), his reading of the imagery literature, his informal questioning of others about their imagery, and other influences. DES asks Eric to bracket that presupposition (among all others) when exploring the experience of any individual subject such as Melanie. Bracketing presuppositions in this instance means being sensitively open to the possibility that Melanie has detailed imagery, but being equally open to the possibility that Melanie does not have detailed imagery. That is, the task is to be open to Melanie's visual phenomena as they unfold themselves to Melanie and through her to Eric and me. Bracketing means that we should structure a level-playing-field situation for Melanie, that we should not let our own (or Melanie's) presuppositions knowingly or unknowingly bias us (or her) in favor of one outcome or in opposition to another. Bracketing does not

mean that Eric should pretend that his own experience or his reading of the imagery literature doesn't exist. Presumably he has profited to some degree from his self-observation and from that reading, has a greater understanding of the traps, pitfalls, successes, and blind alleys that that literature includes. On the basis of that understanding he should feel free to ask skillful questions that assist Melanie to distinguish between this aspect and that, to make as sure as possible that Melanie understands what is being asked in known-to-be-problematic areas, and so on. But he should not, it seems to me, prejudge the answers to those questions. Melanie maintained that she has detailed imagery despite Eric's probing (actually, in some cases leading) questions. That should count against his presupposition that all imagery is not detailed.

I have conducted many DES investigations where subjects provide detailed imagery reports that were at least as convincing as Melanie's. I have tried to keep those studies independent in the sense that I have worked hard at bracketing presuppositions about such things as detailed imagery. As a result, I have discovered to my satisfaction that there is a range of detail in visual imagery. Some subjects have no visual imagery whatsoever; some have visual imagery that is not detailed, some subjects have much visual imagery, some of which is detailed and some of which is not; some subjects have much visual imagery, all of it detailed. It's harder for me than for Eric to discount all those results because I have seen them all first hand and can vouch for their thorough examination

because I was there. The existence of a range of imagery detail across subjects in my opinion provides evidence that I have bracketed whatever presuppositions I might have in this regard.

**1.7.4. An Example.** Twenty-five years ago, in the early 1980s, before DES existed as a relatively formal method, I was giving subjects beepers and asking what was going on with them at the moment of the beep. Occasionally, usually on about the third day of such sampling, subjects would say they wanted to quit; that they weren't any good at sampling; that they didn't wish to waste my time; they couldn't observe their own thinking; that I should get someone who could perform the sampling task better than they could. I typically said something to the effect of, Why don't we discuss today's samples and see what the problems are, and after that if you want to quit, okay. At the end of such early interviews, I myself would agree with the difficulties the subject was reporting – sampling did seem impossible for us.

But eventually I noticed that similar sequences were happening across several subjects; not all subjects, to be sure, but enough to provide a pattern. I therefore worked harder to understand what was happening. It turned out that these subjects were having frequent (what I came to call) unsymbolized thinking – thoughts that have clear, differentiated content but no discernible features that “carry” that content: no images, no words, no other kinds of symbols. For

example, a subject might be thinking, at the moment of the beep, something that if expressed in words would be, “I think I’ll make a ham sandwich – no, I’ll have a hot dog.” But there were no words (no “ham sandwich”; no “hot dog”; no “no”) and no images (of a sandwich or a hot dog), and no other discernible symbols. The subject was clearly thinking, clearly thinking specifically of a ham sandwich and specifically a hot dog, and clearly changing his mind from one to the other, but there was no way to describe how that thought appeared, other than to say it did appear.

The problem was that my subjects and I all shared the (commonly held) presupposition that, of course, thinking had to be in words or in some other kind of symbols. It never would have occurred to us to suspect the existence of thinking without words or symbols. The subjects were distressed – they were pretty sure about what they were thinking but they were totally unable to report it in a way that was acceptable to them. Even more distressing, as they got better at paying attention to what was happening at the moment of the beep, as naturally happened in a few sampling days, they seemed to get worse at reporting it – they now were beginning to observe themselves thinking without words or symbols, which they knew was impossible. Therefore, they saw themselves as bad subjects and wanted to quit. I was of little help because our presuppositions colluded, and we were stuck.

What eventually got us through this impasse was my willingness to bracket presuppositions, even those presuppositions that were so basic, so ingrained, so taken for granted, so unquestioned, as that thinking was in words or symbols. I wasn't particularly skillful at that bracketing, but I had read enough phenomenology and Eastern meditative traditions to open myself to its possibilities. Once the presupposition that thinking had to be in words or symbols was successfully bracketed, it was relatively easy to recognize the pattern and to talk about that kind of thinking from then on.

This example illustrates the interplay of the presuppositional points we have been discussing. The subjects and I happened to share a presupposition about the nature of thinking. That made it difficult for me to ask evenhanded questions, for the subjects to give accurate answers, and for me to interpret what was being said and not said. When I finally could bracket my presupposition, it made it much easier for me to help the subjects bracket theirs, and the knot was untied. This example also illustrates the insidiousness of presuppositions: we didn't know, at that time, of the very existence of the presuppositions that needed to be bracketed. Of course we thought that thinking was in words or symbols; how could it be otherwise? It didn't (at the outset) occur to us to bracket that presupposition any more than it would occur to us to bracket the fact that we need air to breathe. Of course we need air to breathe. The presuppositions that are the

hardest, and also the most important, to bracket are those which exist prior to any understanding that they should be bracketed.

To bring this example to a close, let me add one additional anecdote. During the past 25 years, I have declined to participate myself as a DES subject on the logic that I, like other investigators, am fallible. I recognized that investigators are likely to favor (value, etc.) their own characteristics, so it seemed prudent to me that if I was to be an evenhanded observer of the characteristics of the inner experience of others, I should avoid being captured by the particular idiosyncratic characteristics of my own experience. One way to facilitate that was simply not to know what were the idiosyncratic characteristics of my own experience. Therefore I declined to sample myself.

Last year, the students in my sampling lab, understandably curious about what my own sampled experience might look like, prevailed on me to let them sample me. I agreed, on the logic that now, after 25 years of practice, I was probably pretty secure in my ability to bracket my own personal characteristics. So I wore the beeper and reported back to the lab for a joint sampling interview. At the outset I somewhat apologetically reported that I wasn't a very good subject; I said that probably as a result of my years of sampling I had lost the ability to pay attention to my own experience, that sampling with me wasn't likely to be very useful, that they should probably find a better subject, but they could go ahead and ask about my experiences. It turned out that my own samples contained much

unsymbolized thinking and that I hadn't recognized that in myself despite 25 years of recognizing it in others! I had said to my student interviewers exactly the same kinds of things that my subjects had been saying to me for 25 years, but I didn't recognize it.

Presuppositions die hard.

**1.7.5. The Beep as the First Bracketing Step.** The first step of an exploration of inner experience should be to apprehend that experience as it is in its occurrence, to get to the experience itself.

I believe that the reflective task is made substantially easier by the DES beep, which provides a substantial head start in the bracketing of presuppositions by selecting the precise experience to be reflected upon (Hurlburt & Heavey, 2004). It makes that selection at, or at least very nearly at, the moment that the experience is occurring and signals the subject to consider this particular experience, not one a few seconds, hours, or days earlier. Without the beep (or some other equally or more effective means of selecting a particular experience) the subject has to work her way backwards to the target experience, and that retrospection is subject to a host of pressures. Without the beep, a substantial bracketing of presuppositions is therefore required to get retrospectively to the experience itself, unadulterated by similar events that occurred at different times, different events that occurred at about the same time, fantasized events that never

occurred at all, and so on. The beep simplifies the bracketing task. Certainly the beep does not guarantee that we will get to the experience itself – effective bracketing of presuppositions is still required – but it makes it very much easier.

#### **1.7.6. Random Sampling as a Second Step in Bracketing**

**Presuppositions.** The randomness of the DES beep is an extremely effective bracketing-presuppositions tool because (a) the unexpected nature of the random beep catches the subject off guard, thus outsmarting the mind’s habitual presuppositional activity; and (b) the experiences to be investigated are chosen randomly, not to match some presupposition or to avoid some presupposition, not because an experience is thought to be important or thought to correspond to some theory. The random nature of the beep slices through all those presuppositions and produces an even-handed collection of non-presupposition-driven experiences; then those experiences can be examined to determine whatever characteristics emerge from them. For example, we did not at the outset of sampling with Melanie presume that inner speech is important, nor did we presume that it is unimportant. On the contrary, we randomly sampled Melanie’s experience and if inner speech turned out to be important, fine; if not, fine. Data, as free of presuppositions as possible, drove conceptualizations such as that inner speech was not a major aspect of Melanie’s experience.

Part of Eric's critique of DES from the point of view of classical introspection is that the classical introspectionists controlled their experimental conditions better than does DES. However, from the standpoint of bracketing presuppositions, the controlling of conditions is a liability rather than an asset, because the controlling of conditions is in fact a substantial concretization of presuppositions. Control presupposes that the experimenter knows before the data are collected what is and what is not important. There is a time when such control is desirable, but that is not until the phenomena are clearly understood as they naturally occur.

#### **1.7.7. Armchair Introspection as a Failure to Bracket Presuppositions.**

I have railed against armchair introspection frequently during the course of this book because I think consciousness studies has relied far too heavily on the armchair and its failure to bracket presuppositions. Above I have used Baars, the prominent researcher in consciousness studies who claimed that "human beings talk to themselves every moment of the waking day" (Baars 2003, p. 106), as an example of failed armchair introspection, because one of the most robust findings of DES is that there are many people, apparently like Melanie, who talk to themselves only rarely if at all. I suspect that Baars' position comes from armchair introspection buttressed by analysis; I suspect that Eric's view that

people are mostly the same and his doubt that Melanie's images are richly detailed come from similar procedures.

At Box 4.5 Eric reported his own armchair introspective investigation of Melanie's report of speeded up but not compressed or rushed inner speech. He observed, correctly as I recall, that his own inner speech, when he wore the DES beeper, was at about the same rate as his external speech; that is what most (but not all) DES subjects report about inner speech. Then he, armchairwise,

walk[ed] across campus deliberately producing inner speech and attempting to observe its pace as I did so. I found myself getting tangled up, feeling like I often produced the speech twice, once in forming the intention to produce a specific instance of inner speech and then again in carrying out that intention (as though I didn't realize the intention was already executed in the forming of it). I also found myself unsure of the pacing especially of the first of these two acts of inner speech – indeed, unsure even of whether the first was in fact an act of inner speech at all.

It is simply not adequate, I think, to support or discount a position because it happens to agree disagree with one's own armchair. Eric's tangle comes from the inadequate armchair method, not from some fundamental difficulty or impossibility of introspection.

It appears that Eric used his armchair introspection to discredit his own DES experience. At Box 4.5, he stated that, apparently as a result of observing the

tangles in his armchair introspection, “In my own earlier sampling, in fact, I believe I reported that my inner speech was paced at roughly the same rate as my external speech, but now I find myself wondering if I was correct in that observation.” It seems to me that, when observed contemporaneously, he wasn’t as tangled when responding to the DES beep as he was in his armchair introspection. That strikes me as straightforward evidence that DES is in some important ways better than his armchair. There are other potential explanations (I pressured him in the DES situation, we weren’t careful enough in the DES situation, and so on) but those seem quite convoluted by comparison.

Eric might respond that perhaps his cross-campus-walk observation was just a particularly bad armchair introspection and a bad application doesn’t invalidate the general armchair process. I would agree with the logic but I don’t think that evades the criticism. There is no way that I can see to discern whether his observation is good or bad short of a series of validation studies, and as I have said above, resting validation studies on inadequately grounded hypotheses is not in my view a productive way for science to proceed.

**1.7.8. Bracketing Presuppositions in Experiments: Flavell.** Eric sometimes rankles when I suggest that his resistance to accepting Melanie’s reports is due to his armchair introspections, saying that his views are the result of not just his armchair introspections but also of his reading of the various

literatures. I accept that; the problem is that the same presuppositions that drive his armchair may also drive the literature he reviews.

For example, in Chapter Three, section 2, Eric described the “vast mistakes” that children make when describing their thinking, basing his observations on his review of studies of young (5-year-old) children by Flavell and his colleagues. I believe it likely that Eric has some of the same presuppositions as does Flavell, and as a result both may be substantially mistaken about the thinking of 5-year-olds. (I greatly respect the Flavells, who have tried to understand inner experience when such work was eschewed; nonetheless, it seems to me that their conclusions may not reflect their own data.)

Here are the instructions that a typical Flavell experiment puts to children. The child is seated on a carpet, and the experimenter says:

I’m going to ask you a question, but I don’t want you to say the answer out loud. Keep the answer a secret, OK? Most people in the world have toothbrushes in their houses. They put their toothbrushes in a special room. Now don’t say anything out loud. Keep it a secret. Which room in your house has your toothbrush in it? (Flavell, Green, & Flavell, 1995, p. 57).

The experimenter then moves the child to a table and asks the child if she had been thinking while seated on the carpet, and if so, about what. Most older children (and adults) say that indeed they were thinking, that they were saying to

themselves “bathroom” or seeing an image of their bathroom, or the like. However, the majority of 5-year-olds deny the existence of such thinking while seated on the carpet; if they acknowledge they were thinking, they typically report themselves to have been thinking about something other than the bathroom. As a result, Flavell and his colleagues conclude that 5-year-olds lack the ability to observe the thinking that was taking place: “children lack the disposition and the ability to introspect. Lacking introspective skills, they would be unlikely spontaneously to notice and reflect on their own mental experiences and, consequently, unlikely to attribute such experiences to others” (Flavell, Green, & Flavell, 1995, p. 52).

It seems to me that a much more straightforward conclusion might be that 5-year-olds don’t experience thinking. It appears that Flavell and his colleagues don’t reach this conclusion because they don’t take seriously enough the possibility that children’s thinking may be different from adults’ and older children’s:

We can imagine a number of possible reasons why ... 5-year-olds tend to perform poorly on introspection tasks. (1) The 5-year-olds had no thoughts of any kind..., and therefore had nothing to report. This explanation seems implausible on its face. It is tantamount to saying that, unlike older people, young children do not have a continuous or near-continuous stream of consciousness when in a conscious state.

There is also empirical evidence against it.... Flavell et al. (1995) found in several studies that 5-year-olds would often deny having had thoughts even when it was not just likely, but virtually certain, that they had just had some (e.g., about which room they keep their toothbrush in) (Flavell, Green, & Flavell, 2000, p. 108, emphasis added).

It seems to me that the “virtual certainty” that thinking was ongoing is the result of the Flavells’ presuppositions about thinking. They cannot accept that “young children do not have a continuous or near-continuous stream of consciousness when in a conscious state” even though their own 5-year-old subjects consistently deny it, no matter how hard the Flavells work at setting up situations where 5-year-olds might report that stream. The Flavells don’t accept their own subjects’ reports because that would be “implausible on its face,” which seems to me to be a presupposition trumping a whole series of observations. That is, I think, a large mistake.

I think it is simply not true that it is “virtually certain” that 5-year-olds were thinking in that situation. By “thinking” Flavell and his colleagues (and most others, including me) apparently mean some kind of conscious introspectible process (like talking to yourself, or seeing an image of the bathroom, or the like); part of a Flavell instruction to children is that “our brain or mind is sort of like a flashlight. It shines on just a few things at a time, and while it shines on some things, it can’t shine on others” (Flavell, Green, & Flavell, 1995, p. 61).

So the question is, Is it possible to be confronted with the “Which room in your house has your toothbrush in it?” situation without thinking about the bathroom while seated on the carpet? I think the answer is Yes, and I will advance four reasons. First, what I call the “subtractive” logic: My DES studies show that if I were to beep adults in the pause of this situation, some would report inner speech (such as “In the bathroom”), some would report images (such as seeing the toothbrush holder in the bathroom), and some would report unsymbolized thinking. Because inner speech is present only some but not all of the time, it follows that inner speech is not necessary to correctly answer aloud, “In the bathroom.” For the same reason, neither images nor unsymbolized thinking is necessary to give a correct answer. It therefore seems plausible, by subtraction as it were, that since none of the forms of inner experience are necessary, that it might be possible to say “In the bathroom” aloud without having had any form of inner experience present to awareness. I acknowledge that it is common that people employ one or the other (or several) forms of inner experience in this situation, but I see no reason to believe that it is necessary to employ one or another of these forms.

Second, I have sampled with some adults who, in my conclusion, did not have inner experience. I have written about that with respect to schizophrenia (Hurlburt, 1990), but some nonschizophrenic people also apparently have no inner

experience on some occasions (Hurlburt & Heavey, 2006, Chapter 2). In the Eastern tradition, adept meditators report having no inner experience.

Third, the (scanty) DES evidence from children. I described in Box 5.8 the image a 9-year-old boy described of a hole he had dug in his backyard. The image was incomplete, he said; if I had beeped him a few minutes later, he would have had time to put his toys into the image. If, as is plausible, image-making is a skill that is acquired gradually, we should extrapolate from adults' (or older children's) to 5-year-olds' thinking with caution.

Fourth, I think it quite reasonable to suppose that children learn inner speech long after (possibly years after) they learn to have external speech (Vygotsky, 1962).

Thus I think it quite plausible that 5-year-olds have not developed the skills of image making or of inner speech. By analogy, I think it quite plausible that they have not developed any skills which an adult or older child would call "thinking." Therefore I think it quite plausible that 5-year-olds are being straightforwardly descriptively correct when they deny that thinking was ongoing in Flavell's tasks. Finding that to be "implausible on its face" is a substantial failure to bracket presuppositions, a substantial, unwarranted over-reliance on what seems familiar to an adult.

Let's be clear: I'm not arguing that 5-year-olds have in fact not developed those skills; in fact I do not consider myself an expert on children's thinking. I'm

arguing that it is plausible that they have not developed those skills, and that it is highly undesirable to dismiss that possibility on its presuppositional face. In fact, I believe that the best evidence we have on children's thinking comes from the Flavell studies, but it seems reasonable to me (but not to the Flavells or Eric) to interpret those studies as showing that 5-year-olds do not have a stream of consciousness similar to adults.

My antipathy toward armchair introspection is that what seems natural or familiar in the armchair may then infect subsequent interpretations of the literature and experiment, leading (for example) Eric to use the phrase "vastly mistaken" when describing Flavell's 5-year-old's failure to report thinking. The core problem is the failure to bracket presuppositions; if presuppositions were bracketed adequately then I would have no objection whatsoever to armchair introspection (the Buddha comes to mind). However, I fear that Eric's and the Flavells' armchair enhances presuppositions rather than brackets them. It's hard to imagine a more consistent set of results than those provided by Flavell's 5-year-olds, and yet, apparently, neither the Flavells nor Eric are listening. I blame the armchair. Perhaps I should blame the presuppositions, not the armchair. However, presuppositions become accepted, reified, entrenched in the armchair, as if the armchair provides some independent evidence. The armchair is a dangerous place for consciousness studies.

By the way, for a similarly skeptical view of children's dreaming, grounded in careful sampling and interview techniques that seem to point against the armchair assumption of similarity between children's and adults' experience, see Foulkes (1999). Here is Foulkes' summary of his findings about the dreams of children aged three to five:

The single most amazing finding of ours... was how puny the dream process turned out to be. However rich and detailed preschoolers' dream "reports" may be when they are elicited at some delay by parents, clinicians, or other credulous interviewers, when children are awakened during REM sleep periods and asked, on the spot, if and what they were dreaming, the most common response by far is that they were not dreaming anything at all. (Foulkes, 1999, p. 56)

### **1.8. The Desirability but Difficulty of Objective Observations**

Eric has stated frequently that he would more readily believe reports about inner experience such as Melanie's if they were confirmed by objective (experimental) results. I have agreed that objective results would be desirable, but so far, no one has undertaken studies that I think would be adequate.

I would make a rough distinction between theory-driven and experiential-phenomenon-driven objective studies. All of the objective studies that Eric has

considered are theory-driven; those that I would find adequate are experiential-phenomenon-driven.

For example, the mental rotation studies that are used to explore imagery are theory-driven. In an early such study, Cooper (1975) presented subjects with two random forms and asked them to decide whether the second form was reflected or rotated from the first. Cooper found that the greater the angle of rotation, the greater was the time necessary to perform this task, and therefore concluded that subjects were rotating mental images at some constant rate in the performance of this task.

The Cooper (1975) study and those that it spawned attempt to discover the characteristics of imagery without examining the experiential phenomena of imagery. However, as we have seen, DES studies show that people are quite different in their image experience: some have clear, detailed images all or nearly all the time; some have images not at all or hardly ever. It seems quite reasonable to wonder whether people who engage in detailed imagery nearly every waking moment would approach an imagery task in a substantially different way from that of people who rarely if ever engage in visual imagery in their everyday lives. Furthermore, mental rotation tasks may be quite irrelevant to the way people actually use imagery in their everyday lives.

I think it is possible to create tasks that would objectively explore images but that are based on the actual experiential phenomenology of images. Here are two examples:

DES observations of reading show that some people produce images as they read while others speak the read text in inner speech (see Box 5.3). It seems reasonable to conjecture that there should be objective differences between those two groups – imagers may read faster, for example, than those whose reading is time-locked to the rhythm of external speech; imagers may make comprehension errors related to the inaccuracy of their images; and so on.

The second example follows from the DES observation that a minority of subjects have frequent what we call visual sensory awareness: they pay attention not so much to the objects of their environment but the sensory aspects of those objects: the color of the 7-Up can, the roughness of the stucco on the house, the particular way the skin folds around a smile, and so on. I've observed that one such person also finds it impossible to fuse random dot stereograms. I further notice that the three-dimensional figures that emerge from fused random dot stereograms have a substantially different phenomenology for me (who can fuse them easily) than do the stereograms themselves. The stereogram itself, before it is fused, seems real, whereas the staircase that emerges from the fused stereogram, while absolutely compelling in its three-dimensionality, seems derived, or constructed, or created, rather than perceptually real. All that leads me to

speculate that people who habitually pay attention to the real sensory details of perception will have difficulty fusing stereograms because they have to leave those real sensory details behind in order to allow the more imaginary three-dimensional figure to emerge.

I don't know whether that speculation about sensory awareness and stereograms is true, but the example highlights the features of the experiential-phenomenology-driven objective studies that I would find compelling. First, a careful observation of the phenomenology of experience needs to be conducted. As the result of many DES studies, I know that most subjects who have frequent sensory awareness don't recognize that about themselves until sampling demonstrates it to them, and many people who believe they have frequent sensory awareness become convinced by sampling that they were mistaken. Thus it is not adequate merely to ask people about their experience, and it matters little whether that asking is done orally or by questionnaire. Second, subjects will have to be selected on the basis of careful examination of phenomena – it is not adequate to lump all subjects into the same pot. Third, such studies are conceptually easy (create two groups, one with much sensory awareness and one with little, and explore their stereogram abilities) but labor intensive (sensory awareness is relatively rare, and to determine its presence requires something like several days of DES exploration).

As far as I know, no study that differentiates readers on their experience of reading, or that differentiates stereogram fusers on their experience of visual sensory awareness, or any other such study that differentiates on the basis of carefully examined experiential phenomenon, has been performed. The closest study that I know of examined the inner experience of two groups, one whose subjects were objectively measured rapid talkers and one whose subjects spoke at more average rates (Hurlburt, Koch, & Heavey, 2002). They found that fast talkers have richer, more complex experience than normal-rate talkers.

Thus I agree with Eric on the desirability of objective investigations, but only if they are properly performed. I believe that objective studies that are sensitively tied to carefully explored experiential phenomena can be done, and that their results will likely be illuminating.

## **2. Replies to Eric's Reflections**

### **2.1. On Eric's Rich vs. Thin Study**

Eric's adaptation of DES to explore the richness of experience is in many ways an admirable study. First, I think he is correct in his analysis that the beeper method is better than concurrent introspection in this situation. Second, the study he describes is quite ambitious: 21 subjects is a lot for a descriptive sampling study. Third, I have listened to some of the tapes of Eric's interviews, and I can

attest that he does in fact give his subjects a quite balanced introduction to the concepts “rich” and “thin.”

Eric’s subjects, despite his efforts, reported tactile experience in only half of samples, and tactile experience in the left foot in less than that. Thus out of the welter of possible facets of consciousness only some few are apparently manifested to Eric’s subjects. Eric and I might disagree about the number of facets that are manifested; but whether that number is one or three or seven or “a lot” isn’t nearly as important as the recognition that, at least as reported by his subjects and mine, experience does not include a broad array of many simultaneous experiences (that is, is not rich).

I think it may be a mistake to try to determine precisely how many facets are manifested because that determination depends far too closely on the details of the definitions like “attention,” “faint,” “peripheral,” “experience,” and so on. I might think that Eric too-heavy-handedly leads his subjects to describe visual experiences, but that leading probably adds only about 1 to the number of features his subject reports, perhaps increasing it from 2 to 3 or from 4 to 5. Eric might think that I too cavalierly disregard subjects’ peripheral experience, but that disregard probably subtracts only about 1 from the subject’s number of experiences. Thus, at a plus-or-minus-one-or-so level, Eric and I agree that our subjects can accurately identify only some limited number of facets as being experienced at any moment.

I think that to try to go further than that plus-or-minus-one-or-so level would be to fall into a trap that contributed greatly to the demise of classical introspection a century ago. As is well known, Titchener and the introspectors at Würzburg disagreed about the existence of imageless thought. Titchener held that all thoughts had imaginal cores. The Würzburgers, in opposition, maintained that they had discovered “imageless thought,” occurrences of thinking that had no perceptible images whatsoever. Titchener retorted that all thoughts indeed had imaginal cores, but some of these cores were so faint as to be imperceptible to all but the most highly skilled introspectors. Thousands of introspective hours were spent on both sides trying, without success, to resolve the issue. The failure of introspection to agree about such a fundamentally important issue was seen as highlighting the inadequacy of introspection as a method, and as a result (certainly there were other important factors; see Danziger, 1981) introspection was discredited for the ensuing century.

However, Monson and Hurlburt (1993) showed that the introspecting subjects in both Titchener’s and the Würzburg labs gave highly similar if not identical descriptions of the phenomena that the Würzburgers called imageless thought; the disagreement came not in the descriptions of the phenomena but in the interpretation of those descriptions. Titchener and the Würzburgers were so focused on their differing views of the fundamental building blocks of thinking

that they failed to appreciate how similar were their observations and descriptions of phenomena.

Any attempt to use DES or any other phenomenological method to determine the exact number of phenomena existing simultaneously in experience is likely to fall into the same trap that snared Titchener and the Würzburgers when they attempted to use classical introspection to settle the imageless-thought debate. In both situations, investigators on both sides observed similar phenomena. In both situations, the things being investigated were (often) exceedingly faint. In both situations, much depended on the fine points of definition.

Two further comments on Eric's rich-thin commentary. First, he concludes that "we are on shaky ground using the reports of subjects like Melanie to undermine the rich view," and by that apparently means that experience, despite the thin-to-moderate reports of Melanie and his own subjects, may actually be radically rich. I agree that it is entirely possible that experience is radically rich, but I also think that the evaluation of that possibility is beyond the reach of phenomenological investigation. Eric's study is pretty good, and I think there is not going to be a phenomenological study that is fundamentally better (in a plus-or-minus-one sense) than his. Thus I think phenomenological studies are going to incline toward some moderate position in the thin-rich continuum. It is still possible to claim that experience *really* is radically rich and that all

phenomenological studies miss that richness, but (a) that claim has to be supported on some other basis and (b) it's not fair to use that claim as an argument against phenomenological investigations.

Second, when Eric asks subjects about visual experience in the far right visual field or about tactile experience in the left foot, he is asking them to ignore the most important aspects of experience (except in those rare cases where experience happens to be focused on the far right visual field or the left foot). In essence, he is asking his subjects to perform tasks that are almost impossible (unless you accept the strong form of the rich position) while at the same time avoiding the tasks that are centrally easy. As we will see below (in section 2.2), I think that is highly problematic. Eric's interest in whether consciousness is rich or thin causes him to put more and more stress on things that are less and less clearly available to consciousness. DES is the opposite: it puts less and less stress on things that are less and less clearly available.

## **2.2. DES and Titchener's Introspection**

Eric compares DES to Titchener's introspection and concludes, "It's by no means clear a priori whether Titchener's introspective methodology, which Russ rebukes, contains more potential for error than Russ's own methodology. Both have their apparent strengths and shortcomings." In reaching this conclusion, Eric points out that Titchener controlled conditions better than DES, but as we have

seen above (in section 1.7.6), I think that is often a liability rather than an asset because control reifies presuppositions. Eric also points out that Titchener's studies typically have fewer memory demands; I will return to that issue in section 2.3 below, where I will conclude that it is not a severe problem.

At the same time, I believe that classical introspection made two major mistakes that DES avoids (three mistakes counting the experimental control issue), and I wish the reader to balance them against the criticisms of DES. First, the classical introspectionists failed to bracket presuppositions adequately and as a result failed to notice that their observations were quite consistent. I discussed that mistake earlier in this chapter (in section 2.1) and showed how DES avoids that mistake.

Second, Titchener often emphasized the subtle at the expense of the obvious (Hurlburt & Heavey, 2004). Titchener was primarily interested in fundamental mental processes and what we might call the psychophysical aspects of consciousness. His laboratory conducted experiments like comparing the relative brightness of two different colors; discerning a very low tone sensation from a sensation of atonal noise; making the quantitative assessment that two sensations are each an equal distance, in different directions, from a third; distinguishing difference (or combination) tones; reporting the characteristics of the "flight of colors" (complex afterimages); and perceiving non-obvious visual

illusions. All those are obscure, although (according to Titchener) fundamental processes.

DES seeks to describe obvious incidental occurrences, not fundamental processes. If inner speech happens to be present at the moment of the beep, DES describes that speech; if an image happens to be present, DES describes that image; and so on. DES is not interested in obscure or the hard to detect processes, whether theoretically fundamental or not.

As a result, Titchener and DES come at phenomena from opposing directions. Titchener had to train his introspectors to suppress the very things that DES seeks to discover – whatever happens to be passing through awareness at any given moment –whereas DES trains subjects to ignore any interest in fundamental processes and simply pay attention to the details of whatever incidental occurrence happens to be ongoing. The DES task is substantially easier than that of Titchener’s introspection, and this accounts for the large differences in training time required.

I use as an example the difference tone studies because (a) they are rather typical of Titchener’s introspection; (b) Eric has provided a very useful discussion / example of Titchener’s difference-tone training procedure on the Web at <http://www.faculty.ucr.edu/~eschwitz/SchwitzAbs/DiffTone.htm>; and (c) they illustrate the introspectionists’ (and Eric’s) overconcern with small effects.

A difference tone (sometimes called a combination tone) is a lower tone that is heard when two higher notes are played loudly together. For example, if you hear two trumpets playing loud tones, one at 800 and the other at 1000 hertz. You may also hear a third, quieter tone whose frequency is 200 Hz, the difference between 800 and 1000 (thus the name “difference tone”), as if three instruments were playing instead of two. Difference tones are well known musical effects, but they are quite subtle in comparison to the actually played notes: the two original notes are loud while the difference tone is soft. Eric goes to great lengths to ensure that his Web visitors can detect the difference tones, telling them to increase the volume so that it is “unpleasant but not painfully loud,” to get better audio speakers, to practice with remedial stimuli, to take breaks to “cleanse the palate,” and so on. Difference tones are thus soft sounds audible only when two other voices are very loud, and are therefore by no means the most salient feature of the experiential world. Surely, then, we must accept that the difference-tone demonstration is asking subjects to do difficult things.

The difference-tone demonstration shows that subjects have difficulty noticing and/or identifying phenomena that are known to exist, an important observation that justifiably should give rise to some skepticism about introspective reports. However, it is important to keep that skepticism targeted at the subtle/difficult introspections and not to overgeneralize, as has sometimes been done, to all introspection tasks. I would go one step further: If subjects have

difficulty making difficult introspections, then until the science of experience matures substantially, we shouldn't ask that of them. Instead, we should prefer instead to ask them to make the relatively easy introspections that have been overlooked for at least a century.

### **2.3. DES Has the Same Defects as Does Eyewitness Testimony**

Eric believes that the eyewitness testimony literature calls into question the veridicality of DES reports, noting that people tend greatly to overestimate the trustworthiness of eyewitness reports. He further maintains that it is comparatively easy to report outward events, so Melanie's testimony about inner experience may well be considerably less accurate than typical eyewitness testimony.

I think Eric is correct to characterize eyewitness reports as being largely untrustworthy. I agree that eyewitnesses can be influenced by the specifics of the questioning. I agree that most people substantially overestimate the accuracy of eyewitness testimony. However, I see two main reasons that Melanie's reports are likely to be considerably more accurate than eyewitness testimony.

First, I created DES in full knowledge of the eyewitness testimony literature and designed it specifically to minimize eyewitness-type errors. For example, the eyewitness literature suggests that it is better to have witnesses tell their stories initially in their own words and in their own manner before external

questioning begins. We ask DES subjects to do that. Eyewitness testimony suggests that words such as “smashed” can have substantial impact by comparison to words such as “hit.” DES explicitly tries to be neutral in its word choice, usually using the subject’s own words. The eyewitness literature suggests that the less retrospective the report, the better; DES minimizes retrospection. And so on.

Eric himself, having observed my interviewing at close range, has identified few or no specific areas where when we sampled with Melanie we failed to follow the principles that the eyewitness literature (and other literatures) suggest would be desirable. Thus, Eric’s argument apparently is that even though I (or Eric and I) have conducted these interviews with a very high level of skill and with substantial sophistication about the problematics of eyewitness testimony, there is still the possibility that our interviews fall prey to the same kinds of errors that plague eyewitnesses. I agree that that is possible, but it seems likely that because we (I think quite successfully) substantially reduced, if not eliminated, precisely those influences that the eyewitness literature finds important, what eyewitness pressures that remain are likely to be orders of magnitude more subtle than the kinds of factors explored by the eyewitness literature. Therefore I think that it is not fair to apply the criticisms that justifiably apply to eyewitness testimony to skillfully implemented DES.

Second, the eyewitness paradigm differs from DES in that the eyewitness observations are always one-shot, no-preparation, no-training events, usually in emotion-arousing situations, whereas DES involves substantial training, support, clarification, and practice. To apply an eyewitness-testimony study's results to DES, the study would have to have approximately the following instructions:

As you go about your everyday activities for about three hours this afternoon, you will encounter six incidents of potential petty theft. Each instance will be identified immediately afterward by a clear, unambiguous signal. Furthermore, these events will be arranged so that they always take place directly in your view – for example, you will not need to turn around and look to see the event.

Within 24 hours of seeing these six events, an insurance agent will call you and ask you to report what you have seen; therefore we ask that immediately following each event, you take whatever notes you think might be relevant so that they might remind you of the details of the event. That agent will not be interested in what happened before or what happened after the petty theft, but only what was happening at the moment of the signal.

During the course of the interview, the insurance agent (who, by the way, is extremely highly skilled) will help you to understand what is meant by “at the moment of the signal,” help you to refine your

observational skills, help you to clarify what you should have been paying attention to and what not, help you to clarify what might be useful to record in your initial notes, and so on.

Then the next day, you will observe another six potential petty thefts, following which the skilled insurance agent will again help you to improve your skills. That will be repeated for a half dozen or so days, perhaps 36 potential petty thefts in all. We may largely ignore your observations of the first 6 or so petty thefts, because you will be mastering the skill of theft-observation.

Furthermore, the agent will work for neither the prosecution nor the defense, and is genuinely interested only in discovering what you saw, not what is best for his/her client nor indeed in what actually happened.

I don't think there any eyewitness testimony studies that are in the same universe as that. And I don't think that that vignette overdraws the situation; in fact, I think that that minimizes the skill-of-the-insurance-agent part of the situation. The thought experiment to be undertaken is, Would the eyewitness in such a study learn to be an accurate reporter of petty theft details? I think so. A highly accurate reporter? I think so. A perfect reporter? I think not.

Eric makes two additional eyewitness points. First, that what we asked Melanie to observe was closer than any outward event, and that the observation of such close-up characteristics is "strange and difficult" by comparison to the

attending to outward events. Eric acknowledges that Melanie improved in this regard, and I think adequately. But if he thinks Melanie's improvement was not adequate, then we should be able to minimize this difficulty by sampling over more days. Furthermore, observing an outward event is not as easy as Eric implies: Usually eyewitnesses are not looking at the event at its outset and must orient themselves to it and thereby usually don't see the important initial details; furthermore, eyewitnesses are not always in a good position (too far away, obscured by darkness or a tree, etc.) to see the event.

Second, Eric thinks that introspectors are in a worse position than are eyewitnesses because introspectors often find themselves reporting experiences they would have thought were impossible (unsymbolized thinking, for example), whereas eyewitnesses often find themselves reporting events that they would find merely surprising, not impossible. I think that is a justifiable criticism of one-shot introspection but not of DES: the explicitly iterative, acquire-this-skill-over-several-days feature of the DES procedure is specifically designed to mitigate that criticism. The typical subject who eventually will report unsymbolized thinking finds that phenomenon unthinkable on the first sampling day, threatening but perhaps possible on the second day, open-mindedly possible on the third day, and obviously occurring on the fourth. It is the art of the interviewer to open up possibilities gradually while at the same time maintaining a dispassionate even-handedness about actualities.

Eric's point is that overcoming perceived impossibility is more difficult than overcoming perceived unlikelihood, but I disagree with that. Some presuppositions are more difficult to overcome than others, but that has little to do with whether impossibility or merely unlikelihood is at issue. If a robbery witness holds the presupposition (more often called the prejudice) that most robberies are committed by blacks, his testimony is likely to be distorted even though he would easily recognize the possibility that a robber could be white. That presupposition/prejudice is false, just as false as is the presupposition against unsymbolized thinking. But I think overcoming the robbery prejudice is more difficult (for a variety of shameful reasons) than overcoming the presupposition against unsymbolized thinking, even though the person might at the outset think unsymbolized thinking is impossible.

Thus an important difference between DES and eyewitness testimony is that DES explicitly and repeatedly tries to identify and bracket such presuppositions prior to the observations in question, and that is simply not possible in eyewitness events.

The eyewitness testimony literature is important because it points out some of the things that can be problematic in reports. It is highly useful in that it can be and has been mined for suggestions about how to avoid errors in reports. Eric and I agree that DES does avoid some of those potential errors, and we agree that DES cannot be said to have avoided all of the potential errors. Eric and I

disagree on the extent to which DES may have been successful in reducing the influence of the theories and expectations of the witness and interviewer; I think he may not have seen enough good DES interviews and he thinks I may be captured by the overconfidence of the witness. Because there are substantial differences between DES and eyewitness testimony, I think it is unfair simply to extrapolate eyewitness criticisms to DES. However, I do think it is fair to be wary and to seek ways to evaluate DES informed by the failures of eyewitness testimony. I look forward to those studies.

#### **2.4. DES Relies Too Heavily on Memory**

Eric is concerned that errors of memory may lead Melanie to confabulate, if not in the minute or so after the beep when the subject is first reflecting on her sampled experience, then in the interview itself up to 24 hours later. I agree with Eric as a matter of principle that any human science researcher must be concerned about the veridicality of memory, and in fact, have spent much of my career criticizing retrospective measures.

However, the historical fact is that my colleagues and I have explored the data collection aspect of DES investigations in a variety of informal ways. We have followed subjects into their natural environments and conducted expositional interviews on the spot, and we have arranged situations where subjects sampled in my presence. Those efforts minimize the problems of retrospection but increase

the experimenter's intrusiveness. I have positioned myself "on the edge" of the subject's environment, for example, by sitting in my car parked outside a subject's house, and asked the subject to come to me immediately after being beeped. That's a compromise between minimizing retrospection and increasing intrusiveness. We have conducted the "standard" DES except that we have insisted that the expositional interview take place on the same day as the sampling, thus reducing the retrospectiveness (from about 24 hours to about 4 hours) and eliminating whatever distortions might be caused by the intervening sleep. We have allowed longer-than-24-hour delays between sampling and interview. We have asked subjects to take long notes and short notes. We have asked subjects to tape record responses rather than write them, on the probability that more detail would be provided in the recorded statement.

The result of all these explorations is that as far as I can tell, it doesn't make much difference how the initial recording is made or when the interview is performed as long as it's within about 24 hours of the sampling. Certainly I agree that the shorter the retrospection the better, but only up to the point that the shortness of the interval causes experimenter intrusiveness. Certainly I agree that the longer the retrospection, the more likely that some details will be confabulated. However, I have granted all along that some details are confabulated, and I don't see a small increase in confabulation as overly serious. I

do not think that a trained subject will confabulate the main characteristics as the result of a less-than-24-hour delay.

I grant that most of these explorations have been informal and all of the resulting observations were made by one group, namely me and my colleagues. But at the same time, I believe we were serious and committed observers, willing to alter the DES procedure in whatever way we thought was necessary to obtain what I thought were accurate reports. My sense is that the length of the delay is more crucial early in the training of a DES subject, and that the interval can probably be relaxed somewhat with a subject who, because of DES experience, knows what is being asked and what is at stake.

I can easily imagine that future, more formal studies might show that 24 hours is too long for some purposes. If that happens, then I of course would recommend shortening the interval between obtaining the sample and discussing it.

## **2.5. Subtle Interview Pressures May Have Large Effects**

Eric believes that the large and compelling body of evidence in social psychology (reviewed, for example, in Ross and Nisbett, 1991) has demonstrated that subtle features of a situation can have a striking impact on behavior. I agree that situations have important influences on behavior. I agree that most people substantially underestimate the importance of the situation. However, Eric seems

to use that literature as a strong argument against our believing Melanie, and I disagree with that conclusion for much the same reason that I disagreed with his use of the eyewitness testimony literature against our understanding of Melanie.

There are two main points here. First, the effects of subtle features on behavior have been substantially overstated, in my view, by the situationism literature in general and Eric in particular. Eric refers to the situational manipulations repeatedly as being “subtle,” and in this regard he follows in the path of Ross and Nisbett (1991) and many others. For example, Ross and Nisbett introduce their section titled “Social influence and group processes” with the following paragraph:

This chapter’s review of classic studies of social influence and situational control will emphasize two themes: first, that social pressures and other situational factors exert effects on behavior that are more potent than we generally recognize, and second, that to understand the impact of a given social situation, we often need to attend to its subtle details (Ross & Nisbett, 1991, p. 28).

However, most, of the manipulation examples that Ross and Nisbett cite are not at all subtle. Here are their lead-off topics: Sherif’s (1937) confederate consistently and substantially misrepresents his “autokinetic effect” estimates, giving “estimates that were either consistently much higher or consistently much lower than those typically made by subjects left to make judgments on their own” (Ross

& Nisbett, 1991, p. 29, my emphasis). Asch (1951, 1952, 1955, 1956) had 10 “subjects” in a room, of which nine were actually confederates who substantially misrepresented their length estimates, “with no hesitation or expression of indecision, offered a patently wrong answer” (Ross & Nisbett, 1991, p. 30, my emphasis). Newcomb (1943) found that “young women from predominantly upper-middle-class families entered Bennington College between 1935 and 1939, sharing the generally conservative Republican political views and voting preferences of their parents. Within a couple of years, after having been exposed to the Bennington milieu, the students’ views and preferences had shifted far to the left of those of their family members and of most other Americans of their social class” (Ross & Nisbett, 1991, p. 35). The several-year influence of roommates, friends, professors, activities, and so on can hardly be called subtle; neither should “much higher” or “patently wrong” untrue verbalizations.

Ross and Nisbett’s (1991) summary illustration (in its own section called “Putting it all together”; Ross & Nisbett, 1991, p. 52-58) was Milgram’s shock-administration study, which may be as instructive for us as it was for Ross and Nisbett. Two people appear at the laboratory to become subjects in an experiment. In plain view of both subjects, slips are drawn, and one subject becomes the “teacher” while the other becomes the “learner.” The experimenter instructs the teacher to administer ever-increasing doses of electric shock to the learner, saying things like, “The experiment requires you to continue; you have no

choice,” (Ross & Nisbett, 1991, p. 57, emphasis added). (Unknown to the “teacher,” the “learner” is actually a confederate of the experimenter, and no shocks are actually administered.) This experimental manipulation was not subtle; “you have no choice” is subtlety’s diametric opposite.

Thus Ross and Nisbett’s chapter leads the reader to believe that subtle influences have dramatic effects, but their primary and most dramatic examples are not at all subtle.

Certainly there are studies where subtle influences do have somewhat large effects – I would probably call Isen and Levin’s (1972) dime-in-the-phone and Intons-Peterson’s (1983) hand-position priming studies that Eric cites “subtle.” However, the results of those studies are usually substantially less dramatic than the than the ones Ross and Nisbett emphasize. Thus I agree that Ross and Nisbett, and many others, have convincingly demonstrated that the situation is indeed important in determining behavior. However, Ross and Nisbett, and many others often overemphasize the subtlety of these situational determinants and the strikingness of their effects.

Second, one of Ross and Nisbett’s main conclusions is that the situational influences that they identify can be effectively mitigated by “opening up the channel factor.” Ross and Nisbett define “channel factor” as follows:

When we find an apparently small situational circumstance producing a big behavioral effect, we are justified in suspecting we have identified a

channel factor, that is, a stimulus or a response pathway that serves to elicit or sustain behavioral intentions with particular intensity or stability (Ross & Nisbett, 1991, p. 46).

According to Ross and Nisbett, as a result of this channel factor, Milgram's "teachers" administer extremely high doses of electric shock to the learners. However, Ross and Nisbett believed that undermining one, some, or all of the features of the channel factors in Milgram's study would have dramatically weakened the effectiveness of the communication:

Suppose that the experimenter [Milgram] had announced at the beginning of the session that, if at any time the teacher wished to terminate his participation in the experiment, he could indicate his desire to do so by pressing a button on the table in front of him. We trust the reader agrees with us that if this channel factor had been opened up, the obedience rate would have been a fraction of what it was (Nisbett & Ross, 1991, p. 57).

Thus, Ross and Nisbett themselves recognized that the obedience-engendering characteristics of situations can be substantially mitigated. Merely giving the subject the right to terminate participation, Ross and Nisbett apparently thought, was enough to wipe out or at least dramatically reduce Milgram's situational influence.

DES was created in full recognition of the situational influences that Nisbett, Ross, and many others have observed on communications. DES incorporates clear, unequivocal, multiple “channel-opening” instructions to forestall such influences. For example, we explicitly and repeatedly told Melanie that she could withdraw at any time; that saying “I don’t know” or “I don’t remember” was a perfectly legitimate response, that we valued her best effort over any predetermined expectation; that it was quite possible that things wouldn’t be clear and that that was okay; that the task was perhaps impossible; that we would learn as much or more from her inability to perform a task as we would from her ability to perform it easily; that we much preferred her unexaggerated candor to any attempt to figure out what we wanted to hear; etc., etc., etc. Not only did we say such things repeatedly, but we meant them sincerely; and not only did we mean them sincerely, I think Melanie recognized that we meant them sincerely. Therefore, by Ross and Nisbett’s own argument, we, I think, successfully undermined the channel effect and therefore should not expect large obedience effects.

I have quibbled with Ross and Nisbett’s and Eric’s use of the word “subtle” not for merely pedantic reasons, but because the “subtle” pressures that might be applied by us to Melanie are not the same as the “subtle” pressures Ross and Nisbett reviewed. I fully accept that a justified claim of subtle pressure might still be made by saying that despite our repeated, explicit instructions to the contrary;

and despite the fact that an avowed skeptic, knowledgeable about the situationism literature, watched the procedure every step of the way and could not identify any explicit pressure communications; we still somehow subtly conveyed to Melanie, by word, deed, or situation, that she must give responses that went far beyond what she could remember. However, we should recognize that that would truly be a very subtle pressure, far more subtle than most if not all of the situations examined in the situationism literature. It therefore seems unfair broadly to invoke Ross and Nisbett against our DES interview of Melanie.

I have no reason to deny categorically the existence of such a subtle pressure in general, nor its existence in our interaction with Melanie, but it does seem more unlikely than likely that such subtle pressures as we may have applied had a large effect.

### **3. A Note about the Form of this Book**

As we described in Chapter 1, our collaboration, including Melanie's participation therein, was originally intended to be a private conversation between Eric and me. Once we decided to shape this ongoing conversation into a publicly available book, we were faced with the problem of how to continue our frank and private, tentative and exploratory conversations alongside our public ones, and so we developed the following scheme. We agreed that with respect to any given topic, we would initially communicate with each other in what we called the Personal-

Russ-Eric (or PRE) mode. Either of us could initiate a PRE communication. If I wrote in the PRE mode, I would try to say exactly what I really thought, without undue need for documentation or undue concern for Eric's feelings or sensitivities. My PRE comment invited a reply by Eric in the same PRE mode: He could give his view on the same issue; he could agree or disagree with what I had said; he could ask me for clarification or simply supply his own clarification; etc. Once we felt we both understood what was really going on in the conversation, we trimmed and simplified the exchanges, or discarded them as being irrelevant now that issues had been clarified. Consequently, much of what follows each of the "Russ:" icons in this book is actually my point of view as clarified, corrected, expanded, contracted, and otherwise adjusted by Eric; similarly the "Eric:" comments have been shaped by me. A few of the Russ: comments were actually originally written by Eric, and vice versa.

There are two reasons that I think this mode of collaboration is worthy of comment. First, it was a genuinely engaged collaboration. There is no talking past one another in this book – we tried as best we could to clarify with/for each other exactly what was meant at every sentence in this book. This is not an essay in favor followed by an essay in opposition that leaves neither party unchanged (or, worse, hardened). On the contrary, this is teamwork by two trusting but differing individuals that leaves both (and hopefully the reader) bettered by the process. I

now find myself thinking about things and saying things in ways that Eric would think and speak, and Eric finds himself voicing my thoughts as well.

Second, Eric's and my collaborative process seems directly in the spirit of the DES values. In the PRE mode, Eric and I tried to be as direct with each other as possible about specific, concrete issues (to what extent did Russ believe what Melanie said at 24:05 in transcript 4, for example); we accepted those communications as being preliminary attempts that were aimed at some particular thing but not necessarily reported in high fidelity; we helped the other to clarify what was thought and what was said; we improved those skills over time; and so on. That process of trying to be as clear as possible with each other about specific concrete issues is very similar if not exactly the same as our trying to be as clear as possible with Melanie about her specific, concrete experiences. Everything in this project was tied to specific, concrete instances. I think that's rare; we found it to be quite valuable.

#### **4. Conclusion**

This study has left me quite optimistic about the future of the science of inner experience. I'm confident that it is possible to fashion introspective methods that keep most of the risks that we have discussed at bay, that we should not tar all methods of exploration of inner experience with the broad brush that tarred introspection. I'm confident that there is much of importance to be learned about

inner experience, and that it is possible to explore it with accuracy. Much hinges on science's ability to learn and teach observational skills, including, very importantly, the skill of bracketing presuppositions. It can be done; whether it will be done only time will tell.

I would have said all that before embarking on this project with Eric. In that light it might seem that I have been unmoved by our interaction, but that is substantially untrue. On the contrary, my thinking about all the issues raised in this book has been clarified, deepened, dis-exaggerated, amplified, corrected. That has been so thoroughgoing a process that it is now impossible to remember a time when I wasn't so clarified, deepened, and so on.

The question, I guess, is whether this clarification is of value to the reader. I hope so. I think the issues that Eric and I have debated are important. I do believe that the care that Eric and I have taken to "get it right" with and for each other is evidenced in every box and every discussion in this book, with the result that the issues are left pretty well exposed without too many (implied or assumed or taken for granted) distracting excesses – he has trimmed mine away, and I his. We may not have resolved those issue, but I trust that this project has provided a step in the right direction.

And as my last word, thanks again to Melanie.

## Chapter Twelve

### Response to Russ and Some Parting Thoughts

Eric Schwitzgebel

#### 1. Response to Russ's Reflections

At a certain level of description, Russ and I believe the same thing. We're both inclined to accept the grosser aspects of Melanie's reports and to treat the details cautiously. We both think that beep-and-interview methods of the sort described in this book are an important tool for studying consciousness. Yet, in a way, this appearance of common ground is misleading: We diverge considerably in our assessment of the virtues of the present method relative to other existing and prior methods, such as the methods of early introspective psychology (e.g., Titchener) and philosophical armchair reflection (e.g., Siewert). We differ substantially in how far to trust the gross features of Melanie's reports – that is, her “exposed” reports (see Chapter 11 Section 1.2.1) as brought out by Russ: I trust only tentatively while Russ is pretty confident. And we differ in what counts as a detail small enough not to warrant even tentative acceptance: I recommend skepticism about all but the general topic and main content while Russ is willing to accept a considerably finer level of detail.

Our interaction has, as both Russ and I think, substantially deepened our thinking without – perhaps predictably – moving us very much from our original positions. For my part, though, I have become convinced that interviews of this sort are as trustworthy as any other method of studying consciousness – which is to say, not very trustworthy at all, but better than nothing. I’ve begun to envision potentially suggestive experiments turning on developments of the method, which in conjunction with a variety of other methods and considerations might begin to show something.

I’m in sympathy with much of what Russ says in the first part of Chapter Eleven – especially regarding faux generalizations, inner speech, the desirability and difficulty of objective observations, and the importance of the personal. I like his personal stories about failing to bracket presuppositions, and I find his treatment of Flavell interesting. His points against me have some merit, too, though I’m not persuaded in the end.

Our different assessments of Flavell reflect and illustrate, I think, two fundamental disagreements between us that may underwrite our different summary assessments of Melanie’s trustworthiness: First, Russ is more optimistic than I about the accuracy of immediate retrospection (given certain precautions). And second, he’s readier than I to see radical differences in people’s inner lives. He isn’t put off by seeming strangeness or by the fact that people’s reports diverge widely.

Such broad differences of perspective aren't easily rebutted in a short space. I can only repeat some general considerations I've touched on throughout this book: the fundamental physiological and behavioral similarity between people; the general lack of behavioral differences corresponding to differences in experiential report (e.g., in the literature on imagery); the instability of our claims about experience, even when conscientiously introspected or immediately retrospected (e.g., people's tendency to change their opinions about peripheral visual experience and echolocatory experience in conversation with me; see Box 4.18; Schwitzgebel & Gordon, 2000; Schwitzgebel, in preparation); the history of divergent and changing views in philosophy and psychology; the fleeting and (probably) complex nature of experience; the novelty of the task; the room for reconstruction, bias, theory-driven distortion, and capture by metaphor; the difficulty of conceptualizing experience and articulating it verbally. All this together suggests, to me, that when people differ in how they describe the basic structure of their experiences, even when their reports are collected as carefully as Russ collects them, these differences may often fail accurately to reflect real differences in the experiences themselves. At a basic, structural level, we may be fairly similar inside, though we answer questions about our experience differently.

Russ's charming story about failing to recognize his own unsymbolized thinking (Ch. 11.1.7.4) prompts further reflections along these lines. Maybe unsymbolized thinking, if it exists, is particularly difficult to conceptualize and

articulate, so that at first some people will misreport it as something else or miss it entirely. Russ has suggested (Ch. 11.1.3) that people tend especially to overreport inner speech on the first interview day, due to a preconception that inner speech is a typical mode of thought; some shift later to reporting more unsymbolized thinking. I wonder then whether a subject reports unsymbolized thinking or not may depend at least as much on her personality or approach to the task as on her underlying experience. Surely some people are better at changing their minds and recognizing the unexpected than others. Some will be more willing to appear uncertain and inexpert, to go through the awkwardness Russ characterizes as typical of the first report of unsymbolized thinking. Some might tend to ignore “unsymbolized” aspects of their experience, if any, in favor of more easily reportable visual imagery and the like. Some might be less conscientious about whether their thoughts really do involve inner speech or not, if reports of inner speech meet with the approval of the interviewer. Maybe, for example, it’s in Russ’s personality to be willing to say “I’m a bad subject, I don’t have any idea what to say about this sample” and less in Melanie’s personality, and it’s this characterological difference that underlies their difference in reporting, rather than a real difference in underlying experience.

Russ suggests I might grow less skeptical if I were exposed to as many subjects as he has been, each interviewed as carefully as we have interviewed Melanie. He may be right. However, conversely, Russ’s judgment may be as

colored and distorted by his long dedication to the method, and the social and emotional commitments that flow from that, as mine is impoverished by limited exposure. I see no ideal standpoint.

Russ's particular criticisms of me in the second part of his chapter – expressing our different stands on what can and should be “bracketed,” our different sense of the impact of subtle social pressures, our different assessments of the comparison to eyewitness testimony – these issues also seem mostly to turn on broad matters of perspective and judgment in the face of conflicting evidence. I won't repeat my reasons for relative pessimism here. I don't think Russ has proven me wrong. Rather, he has articulated a different interpretation of the mass of difficult and discordant data. I acknowledge that his perspective is a reasonable and attractive one, arising from a long career of careful study. Reading his chapter, I almost believe it. I'd like to believe it. I hope the next few decades will shed more light on the matter.

## **2. What Should We Want From These Interviews?**

I may also be more dissatisfied with these interviews for another reason: Whereas Russ is principally interested in the central, most easily reported aspects of Melanie's experience and in the particular, idiographic picture of Melanie that emerges, I've wanted mainly to explore general, structural issues in consciousness. The questions I focus on seem often to be particularly difficult to

answer, requiring subtle discernment or conceptual sophistication or an accurate recollection of details not salient to Melanie in the moments after the beep. Consider for example my questions about whether Melanie's experience is rich or thin (Beep 1.1), whether her emotional experience is exhausted by bodily sensations (e.g., Beep 2.2), how broad the range of clarity is in her visual experience (Beep 1.3). Presumably, Melanie might more easily go wrong in such matters than about, say, whether she had an image of airplanes in Beep 2.2 or was feeling confident in Beep 6.1. Thus, Russ and I hope for different things from these interviews, not equally easy to achieve. Russ wants a mostly accurate view of central features of Melanie's experience, and he thinks we've attained that. I want insight into some of the big structural and theoretical questions about consciousness, and I'm not sure we've attained that.

Have we achieved what Russ wants? Well, I'm not sure. I find something to hesitate over in each major portion of Russ's summary of Melanie's idiosyncrasies. Russ says Melanie is especially self-observational; but I wonder whether that sense might derive simply from Melanie's having fallen into different ways of expressing what is essentially similar between people, given the inherent difficulty conceptualizing and communicating about self-consciousness (see Box 9.5). Russ says Melanie tends to pay attention to sensory aspects of her environment – not just to the objects themselves but to the sensations they produce in her; but I'm concerned about the difficulty of distinguishing memory

of external objects from memory of the sensory experiences those external objects produce in us (see Beep 2.4, p. \*\*\*\*). Russ says Melanie has detailed visual imagery; but I worry that that detail may be created in the reconstruction of those images without having been present in the original imagery (see Box 5.4). Russ accepts the basic characteristics of Melanie's reports of her emotional experience; but I remain concerned about the difficulty of accurately reporting the structural aspects of emotion (see Beep 2.2, p. \*\*\*\*, and Boxes 5.14-5.15). Still, despite these worries, I can't help but feel that we do have at least some tentative sense of Melanie's experience and how it might differ from the experiences of others. Maybe she inclines more toward visual imagery, for example. We might profitably contrast our data about Melanie with similarly obtained data from other subjects, or we might attempt to associate Melanie's measurable abilities and behavioral patterns with her reports. Acquiring any usable data is already no mean accomplishment, as Russ emphasizes in his conclusion.

But should we want more? Or better, can we reasonably hope for more – something more general, more theoretical, more foundational? Or am I, in wanting insight already into general, structural questions about consciousness, displaying the “impatience” (Ch. 11.1.5) or the “asking too much” (11.2.2) that Russ rebukes? Should we restrain ourselves, start only with the rough approximation of particular individuals, and postpone the grand theoretical questions? That, at least, would be highly unusual in the history of science. From

the beginning, every science I know of has sought answers to broad, theoretical questions in tandem with the accurate portrayal of particular details.

At a minimum, Russ has convinced me that candidly facing a subject whose reports about experience seem (to the interviewer!) strange and surprising – whether that subject is ultimately correct and trustworthy or not – opens up avenues for inquiry and argument that may previously have remained unnoticed. Does inner speech or hearing really have to elapse over time (Beep 6.4, p. \*\*\*\*\*)? Could emotional experience literally possess color for many people (Box 4.7)? People diverge more than most of us tend to assume in what they find obvious or plausible about conscious experience, and frank interview is one way to cast light on those divergences.

Furthermore, even if Melanie's answers by themselves aren't entirely convincing, it's possible that a pattern of answers from a variety of subjects may nonetheless illuminate even subtle and difficult structural issues – especially if that pattern is corroborated by data from other methods. For example, I find Russ's encounter with unsymbolized thinking suggestive. And surely this is a deep, structural question about conscious experience if anything is (see e.g., Carruthers, 1996; Siewert, 1998; Horgan & Tienson, 2002; Wilson, 2003; Pitt, 2004; Robinson, 2005).

### **3. The Future of Consciousness Studies**

If the study of consciousness is to thrive, researchers must face up to, and not minimize, the serious methodological problems at the heart of the discipline. Our everyday, intuitive impressions about inner experience are divergent, unstable, and ill informed. Philosophers can't simply reflect from the armchair and expect to find the truth within easy reach. Psychologists and neuroscientists can't simply ask their subjects about inner experience and expect accurate, trustworthy reports representative of how experience transpires in everyday contexts. Common sense and modern philosophy are badly mistaken if they suggest that ordinary people, reflecting in ordinary ways, have a good and ready grasp of their stream of experience. Introspective methodologies that build uncritically on that supposed grasp will inevitably fail.

Nonetheless, most researchers in consciousness still depend on some combination of unsystematic armchair reflection and naive report; and attempts to systematize and regulate, to sort the good from the bad, garner very little agreement between laboratories (or armchairs). Chaos and dissension reign, even about the apparently simplest facts and methods, undermining the basic data of the field. In every science, of course, there's some dispute about what data to credit and dismiss, but in consciousness studies the dissent and divergence are so extreme as practically to cripple the enterprise. Until this situation is resolved, the field will remain a pandemonium of theories with little common ground. Consciousness studies is not yet a mature or progressing science.

Russ and I disagree to what extent careful interview can overcome these difficulties. Russ is optimistic; I am less so. We can't claim to have resolved this question in this book. What have we done then? Maybe we've helped illuminate the issue by casting our different, and conflicting, lights upon it? Well, I'm not sure I'm ready to endorse even that characterization. Our lights play tricks on us. I don't know if this book is in any way an advance. I will say this, at least: We have offered this interchange in the spirit of friendly conflict and in faith in Socrates's (and Mill's, and Habermas's, and Feyerabend's) precept that open dialogue will eventually show a way forward.

## **Appendix A: Box Titles and List of Threads**

### Box Titles:

Box 2.1. A note about terminology: “inner experience” or “conscious experience”?

Box 2.2. Summary of sampling methods

Box 2.3. The truth, the whole truth, and nothing but the truth

Box 2.4. Open-beginninged questions

Box 2.5. Nisbett and Wilson’s critique exempted DES, and indeed (contrary to myth) consciousness generally

Box 2.6. How Compelling is the Case of Robert?

Box 3.1. Churchland’s thought experiments

Box 3.2. Dennett on introspection of current conscious experience

Box 3.3. How should we interpret Flavell’s children?

Box 3.4. On experience while reading

Box 4.1. What is “thinking”?

Box 4.2. Doubts about Melanie’s “inner thought” voice

Box 4.3. Present tense or past tense?

Box 4.4. Fast or normally paced speech?

Box 4.5. Evidence that Melanie is careful. The pace of inner speech, continued

Box 4.6. Bracketing the known characteristics of the outside world

Box 4.7. Color in emotional experience

- Box 4.8. The periphery of experience
- Box 4.9. Should we believe Melanie's report of her first sample?
- Box 4.10. Focusing on a single moment and the dynamics of experience
- Box 4.11. Melanie's awareness of the mechanical aspects of speech #1
- Box 4.12. Self-awareness in perception
- Box 4.13. The timing of the beep
- Box 4.14. Melanie's awareness of the mechanical aspects of speech #2
- Box 4.15. On the subject's notes during DES
- Box 4.16. Imagery violating the rules of visual perspective?
- Box 4.17. Leading the witness
- Box 4.18. The experience of vision. The refrigerator light phenomenon
- Box 4.19. Detail by detail
- Box 4.20. Eric's doubts about report 1.3
- Box 5.1. Asking for details of an image
- Box 5.2. The comparison of images and media
- Box 5.3. Little is known about the phenomenology of reading
- Box 5.4. Dangers of recreating the image. Detail in imagery
- Box 5.5. Images don't exist separately from the seeing of them
- Box 5.6. Indeterminate images
- Box 5.7. Melanie's and Eric's believability as subjects. "Auditory imagery" and  
"inner speech"

- Box 5.8. Do children construct images slowly?
- Box 5.9. When is an F-18 a Stuka?
- Box 5.10. Nonleading questions
- Box 5.11. Complying with a demand for sketchy imagery?
- Box 5.12. Melanie does not give in to all demands
- Box 5.13. Melanie's report of emotion more qualified than previous reports
- Box 5.14. This exchange asks too much of Melanie
- Box 5.15. The James-Lange theory of emotion
- Box 5.16. People say things that are not true
- Box 5.17. People don't necessarily know what they frequently do
- Box 6.1. Bodily emotion without emotional phenomenology? Emotional phenomenology without self-awareness?
- Box 6.2. Feeling fact of body
- Box 6.3. Variations of self-awareness
- Box 6.4. The difficulty of issues around self-awareness of emotion
- Box 6.5. Thought and inner speech
- Box 7.1. Is Melanie inferring rather than recalling?
- Box 7.2. More on subjunctifiers
- Box 7.3. Is there excessive pressure for a specific description?
- Box 7.4. Are people mostly alike?
- Box 7.5. A concern about introducing unrecorded details

Box 7.6. Recalling and reconstructing

Box 7.7. How often does Melanie admit ignorance?

Box 7.8. Melanie's conflation of underspecification and lack of memory

Box 7.9. The language of accurate reports

Box 7.10. Melanie's reaction to skepticism

Box 7.11. Self-awareness of emotion

Box 7.12. Do we think in our heads?

Box 7.13. Bracketing presuppositions

Box 7.14. On the word "because"

Box 7.15. Should we believe Melanie's report of kinesthetic imagery?

Box 7.16. Did we leave out an aspect of the experience?

Box 8.1. On how one starts questioning

Box 8.2. Would Melanie need a veridical copy to answer Eric's question?

Box 8.3. "Strikingly" and bracketing presuppositions. Detail in imagery

Box 8.4. On the use of the word "experiencing" here

Box 8.5. Knowledge of anxiety without the experience of anxiety. Judging others

by oneself, continued

Box 8.6. On Russ's use of the word "awareness"

Box 8.7. Against armchair introspection

Box 8.8. How most people experience emotion

Box 8.9. Is Melanie's emotional experience different from others'? And  
emotional self-awareness

Box 9.1. Was Melanie having visual experience in this sample?

Box 9.2. Is Melanie's external speech generally accompanied by certainty or  
uncertainty?

Box 9.3. Is Melanie attached to seeing herself as self-analytical?

Box 9.4. More on Russ's use of "awareness"

Box 9.5. Consolidating Melanie's sense that she is self-analytical

Box 9.6. Melanie's carefulness

Box 9.7. Melanie's experience of activity

Box 9.8. Mozart's claim to hear a symphony instantaneously

Box 9.9. Should unusual reports be held to a higher standard of evidence?

Box 9.10. Do people know when they're being metaphorical?

Box 9.11. Is DES an example of irreducibly "first-person" science?

List of Threads:

Bracketing presuppositions: 2.6, 3.3, 4.6, 4.10, 5.7, 7.4, 7.13, 8.3, 8.5, 9.9. Also

Ch. 10.6, 11.1.7.

Emotion: 4.7, 5.13, 5.15, 6.2, 6.3, 7.4, 7.9, 8.5, 8.8, 8.9.

Human similarity and difference: 2.6, 3.3, 4.1, 4.7, 4.18, 4.20, 5.3, 7.4, 7.12, 8.8,

8.9, 9.9. Also Ch. 12.1.

Influence of metaphors: 4.7, 5.2, 9.10.

Inner speech and hearing: 4.2, 4.4, 4.5, 4.11, 5.7, 6.5. Also Ch. 11.1.7.7.

Interview techniques: 2.3, 2.4, 4.3, 4.6, 4.15, 4.17, 4.19, 5.10, 5.12, 5.17, 7.3, 8.6.

Also Ch. 2.2.

Limits of DES: 4.10, 5.14, 9.2, 9.10. Also Ch. 11.2.1, 12.2.

Loose language: 2.1, 4.1, 5.16, 7.9, 8.6, 8.9, 9.4. Also Ch. 3.3.

Melanie's Trustworthiness: Attunement to distinctions: 4.5, 6.4, 7.8, 8.9, 9.6.

Melanie's Trustworthiness: Details: 4.13, 5.14, 7.3, 8.1, 8.2, 9.6. Also Ch 10.4,

10.5, 11.2.1, 12.2.

Melanie's Trustworthiness: General: 4.9, 5.7, 5.16, 7.15, 7.16. Also Ch. 10, 11

passim.

Melanie's Trustworthiness: Influence of generalizations: 4.2, 4.11, 4.14, 4.18, 7.1,

7.14, 9.3. Also Ch 10.4.

Melanie's Trustworthiness: Interview pressures: 4.17, 5.1, 5.11, 5.12, 7.3, 8.1.

Also, Ch. 2.3.1, 10.5, 11.2.5.

Melanie's Trustworthiness: Memory: 4.8, 5.4, 7.5, 7.6. Also, Ch. 10.4, 11.2.4.

Melanie's Trustworthiness: Subjunctifiers and confidence: 5.13, 7.2, 7.7, 7.8.

Melanie's Trustworthiness: Unusual claims: 4.7, 4.11, 4.14, 8.9, 9.3. Also Ch.  
10.6.

Non-visual imagery: 5.7, 7.9, 7.15.

Reconstruction: 5.4, 7.1, 7.6. Also Ch. 3.3, 10.4.

Retrospective and armchair generalizations: 4.2, 4.11, 5.6, 5.7, 5.17, 8.5, 8.7. Also  
Ch. 11.1.7.7.

Richness: 2.4, 3.4, 4.8, 6.2, 9.1. Also Ch. 10.3, 11.2.1.

Rules of inner reality: 4.4, 4.6, 4.13, 4.16, 9.8.

Self-Awareness: Melanie's unusual 4.11, 4.14, 6.4, 7.11, 8.8, 8.9, 9.3, 9.4, 9.5.

Self-Awareness: General 4.12, 6.1, 6.2, 6.3, 6.4, 8.6.

Sensory Experience: 3.4, 4.8, 4.18, 9.1, 9.7. Also Ch. 10.3.

Visual imagery: Detail: 4.18, 4.19, 4.20, 5.1, 5.4, 5.11, 7.8, 8.2, 8.3. Also Ch. 3.2.

Visual imagery: Structure: 4.16, 5.2, 5.5, 5.6, 5.8, 5.9, 8.1. Also Ch. 3.2.

## **Appendix B: Beep Summaries**

These are summaries of Melanie's "exposed" reports, as the phrase is defined in Chapter 11.1.2.1. We leave it to the reader to determine with how much skepticism they should be met.

1.1. Melanie was unpacking a chair and saying to herself, "How funny it is that I just received the chair and now I have to plan who is to inherit." The experience was more like inner hearing than inner speech and proceeded at a fast pace without feeling rushed. She associated a rosy yellow glow, which seemed to completely surround her, with the humor of that thought. Although her eyes were aimed at parchment paper, Melanie had little or no experience of it at the moment of the beep.

1.2. Melanie was going from the hallway into the kitchen and saying to herself, this time more in inner speech than inner hearing, "You can think you're really busy but even during those busy times there are periods of empty time." She also had visual experience of the stove and the microwave.

1.3. Melanie was having dinner with her boyfriend and had just finished saying the sentence, “I remember the shed now.” She was aware of her mouth closing as she was finishing speaking. She had a fairly detailed visual image of the shed.

1.4. Melanie was watching the MGM logo with the lion frozen in mid-snarl and the words ‘Ars Gratia Artists’ at the very beginning of a videotape. Her boyfriend was saying to her, “Didn’t the lion used to [beep] roar?” She was hearing and comprehending what he was saying and at the same time paying attention to the green color of the screen.

2.1. Melanie was reading a book about World War II in Kefalonia, Greece. The main character was asking a British soldier when the British are coming to liberate the island during World War II. Melanie was not attending to the act of reading; instead, she was seeing an image of that scene, with lots of sunlight on a dirt road, with the green olive trees and shrubs, and a woman – the main character – speaking to a soldier.

2.2. Melanie was reading a book about World War II Stuka aircraft. She was seeing an image of a line of military planes against a blue-sky background with a couple of white clouds. The imaged planes looked like F-18s, but were

understood to be Stukas. She had definite feeling of sadness / dread like a pressing on her chest.

2.3. Melanie was standing in the bathroom and looking around, trying to make up a shopping list. She was seeing an image of a white pad of paper and of her hand writing the word “conditioner” while also saying the word silently to herself. Also at the same time, she was aware that her toes were cold.

2.4. Melanie was brushing her teeth, aware of being slightly bent over the sink and aware of the rhythmic motion of her hand. She was also aware of the cold and gooiness of the toothpaste.

3.1. Melanie’s boyfriend was asking a question about insurance letters. Melanie’s focus was not on what he was saying but on trying to remember the word “periodontist.” She was thinking “peri-, peri-,” to herself, with the sense that this was the beginning of the word she was searching for. She felt she knew the word and was “waiting for the word to come.” Although she initially said that she heard “peri-” in her own voice, she later felt unsure whether the word fragment was actually experienced auditorially or whether it was instead “slightly visual.”

3.2. Melanie was walking to her car. She was dimly aware, at the moment of the beep, that she was walking toward the car. She had an indistinct visual experience of the car, its big black shape but not its details. At the center of her experience was a feeling of “fogginess” and worry. She described the feeling of fogginess as involving being unable to think with her accustomed speed and as feeling “out of synch.” In addition, Melanie was in the act of observing this fogginess. Her worry was felt as being behind the eyes, involving a heaviness around the brow line.

3.3. Melanie was in her car, shifting from reverse to drive with the parking brake still on. At the moment of the beep she was feeling exasperated at herself, hearing, in her own voice, the phrase, “Why can’t I...” Melanie had the sense that the sentence, had it not been interrupted, would have concluded with a phrase something like “remember about the parking brake.” This episode of inner hearing was distinctly located in her head, moving from the region near her right ear toward the region near her left ear.

4.1. Melanie’s boyfriend was talking about life-threatening sports. Melanie was thinking about scuba diving, feeling an intense bodily yearning to go diving, like her body was going forward, and she was apparently also cognitively recognizing that yearning. She also experienced her body bobbing up and down as if in waves, though she was not actually moving.

4.2. Melanie was reading a book. She had an image of a playing card with a joker on it, dressed in a Harlequin costume with a jester hat and pointy shoes, a jumpsuit with colorful triangles on it, and a big bicycle wheel. Looking back after the beep, Melanie was aware of the emotions of concern and resentment ongoing in her body at that time, but they weren't experienced by her at the moment of the beep.

5.1. Melanie was considering the appointments she had later in the morning, particularly the time pressure of getting to her second appointment, which was across town from the first. She had a mental image of sitting in her car and being stopped at a red stop light at a generic intersection. She could see the stoplight and the road stretched out in front of her and her hands on the steering wheel. Melanie was also cognitively aware that she was anxious, but the feeling of anxiety was not in her awareness at the moment of the beep.

6.1. Melanie was having dinner with her boyfriend, talking about how they divide up the games in the World Series between the National and American fields. Melanie was saying, "But that doesn't make any sense because that means that one stadium gets the World Series games five times [beep] if you play all seven games." Melanie was experiencing a mental feeling of conviction that what she

was saying was correct, which included an awareness of her eyes looking straight ahead.

6.2. Melanie was playing a video game and had said to her opponent, “You’re crowding me” in a joking manner. At the moment of the beep she was feeling happy, experienced as lightness in her lungs. She also experienced a cognitive awareness of the bodily aspects of this feeling.

6.3. Melanie was still playing the arcade game, standing in front of the arcade machine with her arms crossed, concentrating on what was on the screen. She was very aware of the fact that she was concentrating, and in particular she was noticing that her brow was furrowed, that she was chewing on her lower lip, and that she had her arms crossed. She was also aware of the way her feet were placed and the way she was standing. The content of the video screen was only about 20% of her awareness.

6.4. Melanie was picking flower petals out of the sink. Her experience was divided pretty evenly between the activity of picking up the petals and hearing overlapping “echoes” of the phrase “nice long time” from a recently completed (but no longer ongoing) episode of inner speech.

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