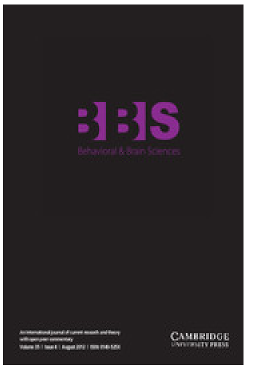

How should implicit learning be characterized?

David R. Shanks and Mark F. St. John

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tance of the null hypothesis. One can always argue that the test of awareness was insensitive, did not match the conditions present at encoding, or was unreliable. Caution is appropriate in calling subjects unaware, but one must ask how an experimenter can be any more certain that subjects do not have explicit knowledge when subjects repeatedly deny noticing a sequence, make errors when forced to guess about its characteristics, and fail to recognize it (Willingham et al. 1993).

Even if unconscious learning has not been satisfactorily demonstrated, consciousness may still be an important attribute in CSs of memory. S&S take the position that consciousness can only take on two values (conscious or unconscious) as an attribute in a CS. A complete lack of awareness may be one end of a spectrum of states of awareness of the contingency at learning; if unconscious learning cannot take place, that does not mean that the rest of the spectrum is irrelevant.

In sum, the implicit/explicit CS appears preferable to the fragment-based/rule-based CS because: (1) the latter is discordant with the amnesic data, whereas the former fits those data and shows that two different theoretical principles – information processing and neurobiology – lead to the same CS; and (2) the chief argument against the implicit/explicit CS (that learning is never unconscious) is difficult to falsify, because one who believes the testing of awareness was not adequate can raise objections that cannot be discounted. Furthermore, if unconscious learning did not exist, it would not sound a death knell for the implicit/explicit CS. For this CS to have validity, there need only be variability in the attribute of consciousness, and predictive power in that variability.

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Authors' Response

How should implicit learning be characterized?

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The commentators on our target article have raised a very broad range of issues, and in our response we will attempt to answer as many of the key points as possible. We thank the commentators for the careful consideration they have given to our arguments. We should say at the outset, however, that although our arguments have been challenged at a number of places, we have not been led to abandon our view of the implicit learning literature. We continue to believe that although the instance/rule distinction provides a meaningful characterization of human learning, it remains highly questionable whether unconscious learning has been adequately demonstrated.

The organization of our response is shown in Table R1. Our plan is first to say something about the appeal of implicit learning, and then to consider the question of unconscious learning: whether our Information and Sensitivity Criteria are appropriate, how consciousness and learning should be defined, what the role of consciousness in learning might be, and whether the further evidence for unconscious learning that commentators discuss warrants a revision of our conclusions. Following these sections, we will consider other dimensions for characterizing distinct learning systems. Is the fragment/instance memorization versus rule-induction distinction a useful and valid one? And finally, can we refine and further specify what is learned and later used by the fragment/instance system?

R1. The appeal of implicit learning

Carlson points out that the idea of implicit learning has an appeal that defies dismissal. Part of this appeal might simply be the cachet of a mysterious process. But more seriously, there clearly is something to implicit learning. Most, if not all, of the commentators agree that there is a real phenomenon, one characterized by **Holyoak & Gattis** as "(*a*) knowledge about covariations in the environment, (*b*) learned by exposure to stimuli exhibiting the covariations . . . (*e*) [knowledge that] is not fully verbalizable and (*f*) [knowledge that] is not manipulable in the sense that it cannot be re-represented explicitly to serve as input to other procedures." We agree; the task at hand is to further analyze this phenomenon. One question is whether the implicit learning process and the resulting information are unconscious. The other question concerns what this knowledge is like and what sort of learning process acquires it.

Several commentators (**Berry, Lindsay & Gorayska, Reber & Winter**) have challenged an important aspect of the overall logic of the target article. We have assumed that the sensible approach is to be skeptical about unconscious learning until it is demonstrated, but these commentators argue that exactly the opposite assumption should be adopted, namely, that unconscious processes have some sort of priority over conscious ones. For example, Berry says that we "adopt the position that unless researchers can prove conclusively that learning is unconscious then we should assume that it is conscious," but we "never justify why conscious processing should be the default assumption." Our argument is simply that explicit learning is the agreed starting point, since no one doubts that learning is at least some of the time accompanied by awareness of what is being learned. What remains to be shown is whether unconscious learning is also feasible.

The position advocated by **Berry, Lindsay & Gorayska,** and **Reber & Winter** is tantamount to holding that unconscious learning does not need to be demonstrated in the laboratory. But no amount of a priori argumentation is going to demonstrate its existence: what is needed is clear experimental evidence. If we do not know in a certain task whether knowledge is conscious or unconscious, it is surely foolhardy to *assume* the latter.

R2. Criteria for establishing unconscious learning

Before discussing potential problems with our conceptualization of implicit and explicit learning, we would like to mention briefly a very interesting article published after

Table R1. *Organization of the authors' response to specific commentaries*

Topics	Commentators
R1. The appeal of implicit learning	Berry, Carlson, Holyoak & Gattis, Lindsay & Gorayska, Reber & Winter
R2. Criteria for establishing unconscious learning	
R2.1. The Sensitivity and Information Criteria	Berry, Brody & Crowley, Carlson, Catania, Dienes & Perner, Holyoak & Gattis, Lindsay & Gorayska, Merikle, Overskeid, Perruchet & Gallego, Rakover, Reed & Johnson, Stadler & Frensch, Svartdal
R2.2. Defining consciousness and learning	Baeyens et al., Catania, Howe & Rabinowitz, Overskeid, Packard
R2.3. Measuring consciousness	Andrade, Baeyens et al., Catania, Dienes & Perner, Ennen, Nagata, Overskeid, Reber & Winter, Stadler & Frensch, Squire et al., Terrace
R2.4. Defining explicit learning and consciousness by verbal report	Catania, Cleeremans, Dienes & Perner, Lindsay & Gorayska, Merikle, Overskeid, Rakover, Terrace, Willingham
R2.5. Do prediction and recognition tasks measure conscious or unconscious knowledge?	Cleeremans, Dienes & Perner, Rakover, Seger
R2.6. The possible causal role of awareness	Catania, Poldrack & Cohen, Squire et al., Svartdal
R3. Further evidence for implicit learning	Overskeid
R3.1. The mere exposure effect	Bornstein, Brody & Crowley
R3.2. Conditioning	Baeyens et al., Kimmel
R3.3. Instrumental learning	Andrade, Berry, Overskeid, Rizzo & Parlangeli, Svartdal
R3.4. Sequence learning	Cleeremans, Reed & Johnson, Stadler & Frensch
R3.5. Amnesia	Cleeremans, Holyoak & Gattis, Kourtzi et al., Packard, Poldrack & Cohen, Reber & Winter, Seger, Squire et al., Willingham
R3.6. Language learning	Goldstone & Kruschke, Lachter, Nagata, Reber & Winter
R4. How should implicit learning be characterized?	Holyoak & Gattis, Kourtzi et al., Merikle, Nagata
R4.1. Consciousness and models of implicit learning	Cleeremans
R4.2. The fragment/instance memorizer	Cleeremans, Kimmel, Marsolek, Perruchet & Gallego, Seger
R4.3. The relationship between memorization and rule induction	Goldstone & Kruschke, Howe & Rabinowitz, Nagata, Packard, Poldrack & Cohen, Reber & Winter

the target article was completed and which, we believe, provides overwhelming justification for our strict criteria. Farah et al. (1993) were interested in the relationship between implicit (or "covert") and explicit ("overt") processing in prosopagnosics, patients who, as a result of brain damage, have difficulty recognizing faces. The typical pattern of findings is that whereas prosopagnosics may not be able to name a familiar, visually presented face, they will nevertheless process that face faster than an unfamiliar face during the course of a task, thus suggesting that some face knowledge is preserved. For example, less time is taken to judge a pair of familiar faces the same than to judge a pair of unfamiliar ones the same. Just as with implicit learning, the suggestion is that the person may have knowledge that is not available to consciousness.

Farah et al., however, have provided compelling evidence that the dissociation between implicit and explicit processing in prosopagnosia is attributable to differential test sensitivity. They constructed a parallel distributed processing model which could be presented with coded representations of faces. Two aspects of the network's performance were monitored: overt recognition and speed of processing. When Farah et al. lesioned their network, they were able to reproduce all of the major dissociations of implicit and explicit performance seen in prosopagnosics, but these dissociations came from a system that has a single (albeit distributed) knowledge source. Thus it follows that the dissociations must have arisen because the implicit tests were better able than the explicit ones to detect small amounts of residual knowledge in the system. [See also Farah: "Neuropsychological Inference with an Interactive Brain: A Critique of the 'Locality' Assumption" *BBS* 17(1) 1994.]

This conclusion is, of course, exactly parallel to our argument that many examples of apparent implicit learning can be explained by the fact that the explicit test does not meet our Sensitivity Criterion. Below, we describe some analogous simulations we have conducted of implicit learning in amnesia.

R2.1. The Sensitivity and Information Criteria. Several commentators endorsed the Sensitivity and Information Criteria (**Berry, Brody & Crowley, Carlson, Perruchet & Gallego, Reed & Johnson, Svartdal**), whereas others raised concerns over them. **Dienes & Perner, Holyoak & Gattis, Lindsay & Gorayska,** and **Overskeid** all argue that our criteria are too stringent and make it very difficult, in principle, to dissociate learning and awareness. Dienes and Perner, for example, propose a thought-experiment in which a subject has instance-based, but unconscious, knowledge of a grammar; they argue that if the performance and awareness tests are made sufficiently similar to meet our criteria then it is very difficult to imagine a mechanism that would apply the knowledge in one case but not in the other.

We have two responses to this. The first is that there are many people who believe that there *are* studies that meet these conditions, in which one test does reveal knowledge whereas another very similar one does not (see commentaries by **Reed & Johnson, Stadler & Frensch**). The second is that if the criteria are relaxed, then although the likelihood of observing dissociations between our two tasks increases, so does the danger that the dissociation is due to differential sensitivity or a mismatch in the type of information examined by the two tests. We believe that if it is difficult to demonstrate unconscious learning, that is all the more reason to be cautious about its existence.

Catania, Holyoak & Gattis, and **Lindsay & Gorayska** go further and argue that we have effectively equated learning and awareness, with the consequence that we have (in Catania's words) "designed a set of conditions such that learning cannot occur without awareness." But this represents a gross misunderstanding of our position. We have suggested that there are two types of measures. Some, (conditioned responses, sequential reaction times, grammaticality judgments) provide indices of learning. Others (recognition, prediction, verbal reports) may provide useful evidence concerning the content of consciousness. Although it would be very controversial to claim that the learning tests provide pure (or even partial) assessments of awareness, we have shown that when the results of these two sorts of tests are compared, it turns out as a plain empirical fact that learning and awareness are typically associated.

Merikle elaborates some of the issues raised in the target article concerning the logic of single dissociations, arguing strongly that this logic will always founder on the impossibility of demonstrating beyond doubt that a given test of awareness is exhaustive. We are probably not quite as skeptical as Merikle about the possibility of establishing unconscious learning in experiments using the single dissociation logic, but we agree wholeheartedly that his concerns should be addressed more adequately by defenders of implicit learning. **Merikle, Perruchet & Gallego,** and **Svartdal** all advocate the exploration of alternative strategies, an idea we fully support.

Perruchet & Gallego and **Brody & Crowley** raise the very interesting but thorny issue of learning during the test. Perruchet & Gallego refer, for example, to evidence that above-chance performance in a grammaticality judgment test may be due to learning taking place in the test rather than to the retrieval of information from an earlier study phase. As evidence of this, Perruchet (1994) reports performance significantly above chance in subjects not given a study phase. A related effect, noted in the target article, is that subjects may observe their performance during testing and thereby become aware of their implicit knowledge. These possibilities are worrisome, because they challenge the idea that tests (either implicit or explicit) may reveal whether prior learning was conscious or unconscious. Rather than providing evidence about earlier processing, performance on the test is under the control of events happening during the test itself. We fully agree with Perruchet & Gallego and Brody & Crowley that this is a real possibility that requires much further investigation.

Rakover offers an entirely new method of determining whether a task can be learned without awareness, but we believe his procedure is flawed. He proposes that a task is "not susceptible to introspective awareness" if the subject, having been told the rule governing the task but not the specific content of the rule, is unable to discover even a part of the content of the rule. As an example, he cites data reported in the Lewicki et al. (1987) study, which was extensively discussed in the target article. In a pilot study, Lewicki et al. informed subjects that there were rules governing the position of the target on complex trials and offered them $100 for finding any parts of the rules. None was able to do so, from which Rakover concludes that the task is not susceptible to introspective awareness. He then suggests that if such an unsusceptible task is learnable, it follows that the learning must be unconscious. Because Lewicki et al. obtained reaction time (RT) speed-up when subjects were exposed to the RT version of the task, Rakover's analysis suggests that learning was unconscious. The structure of the task could not be determined by conscious analysis but could be learned implicitly.

The problem with such an account, however, is that it is left up to experimenters to specify in the susceptibility test the "rule" they think is relevant. But if this rule is not what is responsible for learning in the later performance test, the experimenter will erroneously conclude that learning was unconscious when it may well have been conscious. As we argued, the RT speed-up in Lewicki et al.'s experiments was almost certainly not due to knowledge of the rules that were required in the susceptibility test. In sum, **Rakover**'s account falls foul of the consequences of not adopting the Information Criterion.

None of the commentators seems to question the status of the Information Criterion. We all appear to be in agreement that the test of awareness must focus on the same information that underlies successful task performance. One point made by **Carlson**, which we readily accept, is that it is not merely stimulus-specific information that is relevant but also self-specific information about such things as intentions. *All* of the information that plays a role in task performance – including such things as goals and intentions – must be known before we can judge whether an awareness test meets the criterion.

R2.2. Defining consciousness and learning. Several commentators (**Baeyens et al., Catania, Overskeid, Packard**) wondered why we had failed to define learning and awareness, but we regard doing so as a futile exercise. Surely the past thirty years of research on human concept learning should have taught us that some concepts are simply ill defined and fuzzy? And surely everybody realises that any definitions of these terms we offer would immediately be challenged? Readers of Dennett (1991) will search the book in vain for a "definition" of consciousness, but just because a concept cannot be given an analytic definition, does not mean it is outside the scope of scientific investigation. [See also Dennett & Kinsbourne: "Time and the Observer: The Where and When of Consciousness in the Brain" *BBS* 15(2)1992.]

With respect to learning, Roediger (1993) offers a clear illustration of the difficulty in finding a suitable definition.

He reports that the average duration of labor in giving birth to first-born babies is 9.5 hours, whereas with later-born babies it is 6.6 hours. Clearly, with second and third children the amount of time the mother spends in labor is much less than with first children. Does this mean that something has been learned which explains the speed up? On the one hand, it could be argued that if repetition priming is an example of learning (as indexed, say, by latency of tachistoscopic identification), then so should this "priming" effect in childbirth. On the other hand, it seems strange to say that the female reproductive system is capable of "learning" and "remembering." We suggest that borderline cases such as this and the ones cited by **Overskeid** (e.g., the immune system "learning" to recognise viruses) illustrate the futility of trying to define learning.

Baeyens et al. claim we are arguing that "any verbal or nonverbal behavior which might reasonably be thought of as reflecting a judgment based on a belief-that-*x* should be treated as an index of awareness-of-*x*," but we certainly never argue for this in the target article. Apart from anything else, such a definition would probably make unconscious learning a logical impossibility. Our approach is wholly pragmatic – we offer no hard and fast criteria as to what counts as a test of awareness. However, we do believe that if a subject has an expectancy of a future event and is able to project that expectancy into a truly instrumental action (as opposed to a reflex), then the expectancy is almost certainly conscious. At present, it is fair to say that we do not know for certain whether responses in a prediction task are instrumental acts or reflexes (see Dickinson 1988 and Jacoby et al. 1993, for possible ways of distinguishing between these). We discuss the prediction task further in section R2.5. In fact, we are largely in agreement with Baeyens et al.'s criteria for what counts as consciousness. Our only dispute would be that it is a truism that "awareness-of-*x* is the presence . . . of a phenomenal state of having-the-subjective-impression-to-consciously-know-that-*x*."

Howe & Rabinowitz take us to task for being unspecific as to what it is that the subject needs to be aware (or unaware) of. They point out that there are three ways in which consciousness may be related to learning: "the organism as conscious while learning, the organism as being conscious of learning, and the organism being conscious of what is being learned." We thought we had been clear about this, but to reiterate, the question is whether or not the subject is aware of the information that is responsible for improved performance on the implicit test (i.e., the third case above). Whether an organism is aware in the general sense, or whether it is aware that it is learning, is simply irrelevant to the issue of implicit learning.

R2.3. Measuring consciousness. In their thoughtful commentary, **Dienes & Perner** suggest that performance on, for example, a prediction test measures the objective rather than the subjective threshold of awareness. Subjects may perform above chance on such a test despite believing that they are guessing, in which case their knowledge should be described as unconscious, and by inference, their learning should be described as unconscious as well.

We have two points to make about this. First, in the sorts of studies reviewed in the target article, we do not know whether this is what subjects believe – they may well believe that their prediction performance is above chance (though the unpublished study **Dienes & Perner** cite, in which subjects reported that they were guessing, sounds very promising). Second, and more important, the problem with the subjective/objective threshold distinction is the way in which the subjective threshold is assessed. Subjects are notoriously poor at knowing what randomness is, so when asked whether or not they believe they are guessing, there is no reason to suppose that a veridical answer concerning their state of awareness is given. Moreover, the subjective threshold is often assessed *post hoc:* at the end of a block of trials, subjects are asked to evaluate their performance across that block. It would hardly be surprising in the circumstances that very ephemeral states of consciousness during the prediction trials might be forgotten or discounted by the time of the evaluation. The subjective threshold technique may yield results different from the objective one only because the testing conditions are different and the subjective test is less sensitive. (This is much less of a problem in blindsight, of course, because the subjective judgment about performance is made in the presence of the stimulus to which the subject is responding. We certainly would not want to dispute the importance of the subjective/objective difference in such cases as blindsight).

In citing a test which they construe as measuring unconscious learning but which we view as measuring conscious knowledge, **Baeyens et al.** attempt to point out the unacceptability of our methods of assessing awareness. In the evaluative conditioning study they cite, subjects were exposed in the learning phase to a relationship between line thickness in a geometrical figure and the valence of a subsequently presented picture (positive or negative). In the critical test, subjects saw a geometrical figure and were told that a positive or negative picture would be presented subliminally immediately after it. Subjects had to say whether they thought they had seen a positive or negative picture, even though none was actually presented. Subjects performed above chance on this test: they reported seeing a positive slide, for example, if the geometrical figure had been paired in the study phase with a positive slide.

Baeyens et al. want to offer this as a test of unconscious knowledge, but we see no strong evidence supporting that position. It is equally possible that the subjects, as a result of the study phase, have a (very fragile) conscious expectancy of a picture of a certain valence given a certain geometrical figure. Their expectancy may then bias the response they give, such that this forced-choice test can be interpreted as assessing awareness. We believe the burden of proof is on those who support the idea of unconscious influences to demonstrate that their interpretation is the only viable one. In this case, we suggest it is not.

In support of unconscious processes in general, **Overskeid** claims there is much evidence that our decisions may often be taken by our brains prior to any conscious intention to act, and that we "jump on a running train believing we started it." As evidence, he cites the well-known experiments of Libet (1985), but these have been subjected to so much criticism (see Dennett 1991) that it is highly premature to treat them as conclusive.

A number of commentators (**Overskeid, Reber & Winter, Squire et al., Terrace**) draw strong inferences from the fact that learning can occur in preverbal children and in lowly organisms such as the invertebrate *Aplysia*. The argument is that we are on the horns of a dilemma: either our thesis forces us to conclude that such organisms are conscious, or, if it is assumed that such organisms cannot possibly be conscious, then the forms of learning they are capable of must be unconscious. We believe that neither of these arguments is valid. First, just because a regularity occurs in humans does not mean it holds for other species. No amount of argumentation from animal experiments is going to alter the fact that a correlation between awareness and learning exists in humans.

Second, it cannot simply be asserted that nonhuman organisms are unconscious (see Dennett 1991; Griffin 1992, Nagel 1974). In fact, we believe that little productive purpose is to be gained in discussing animal consciousness at all, since there is no agreed-upon means of deciding what counts as consciousness in animals, and our intuitions clearly differ from those of some commentators. **Reber & Winter** are confident that animals are unconscious, but they provide no reasons for their view. Similarly, after describing evidence of learning in two- to three-month-old infants, **Terrace** states (correctly, in our view) that "whether infants are aware of their expectations is entirely a matter of speculation," but then goes on to conclude that "since consciousness in infants cannot be subjected to empirical study, there is no basis for assuming that infants are conscious of what they learn." But equally, there is no basis for assuming that they are *not* conscious. Terrace gives no rationale for his belief that there is "compelling evidence of learning without consciousness in infants and animals." To repeat, we doubt that the matter is decidable, and in any case it has little bearing on our conclusions. The target article, and the experiments it reviewed, are about adult human learning.

Catania proposes a number of animal experiments analogous to human implicit learning ones (see McLaren et al., in press, for similar examples) to illustrate his belief that it is only verbal reports, and not other forms of behavior such as recognition or prediction, that are of relevance to the concept of consciousness. [See also Lubinski & Thompson: "Species and Individual Differences in Communication Based on Private States" *BBS* 16(4)1993.] For example, in a pigeon analogue of a sequential RT experiment, it would be possible to set up both an RT measure and a prediction measure. Catania suggests that if the pigeon's RT and prediction responses correlated, such that RT improvements never occurred in the absence of above-chance prediction responses, our position would force us to conclude that the pigeon is aware.

We went to some lengths to acknowledge that prediction responses might be susceptible to unconscious influences (see below), so the conclusion that the pigeon is conscious would not necessarily follow. But our main response to **Catania** is to turn his question around: What would he conclude from his pigeon experiment if the RT and prediction measures dissociated? Would one not have to conclude that there is a source of information that is available to one response but not the other, and (if the tasks are very similar) that what characterizes this information is that it is unconscious? Surely, on his reasoning, this would be even stronger evidence for conscious processes (in the control of prediction responses) in nonverbal organisms. Of course, we doubt that such dissociations occur in humans so there is little reason to anticipate them in other organisms, but that is no reason to believe that consciousness is a logical impossibility in animals.

Ennen recalls the Rylean distinction drawn between "learning-how" and "learning-that." She argues that learning-how does not involve acquiring information so it cannot be reported. Therefore, it must be learned implicitly. Unfortunately, although her argument seems sound, (that is, although unconscious learning appears to be a sound logical possibility), there does not appear to be any clear evidence for it. Take conditioning, for example. This is a perfect example of knowing-how, a form of learning that can be characterized as "the incremental modification of perceptual-motor connections over time such that the agent acquires a neurophysiological disposition for improved performance." Yet in humans, conditioning is always accompanied by awareness. Thus Ennen's analysis does nothing to challenge the claim that learning is always accompanied by consciousness.

Stadler & Frensch and **Nagata** suggest that learning is implicit when it is unaffected by intention. It is quite reasonable to define theoretical terms as one wants, but this creates an entity different from the "implicit" learning that is the focus of the target article, because intention is synonymous neither with awareness nor with abstraction. These commentators are surely right that learning can be either intentional or unintentional, but to focus on the learning *process* rather than the learned information is simply to change the subject. Remember that the thesis we are assessing proposes that information can exist in such a form as to be able to project itself onto behavior without being represented in consciousness. This is a thesis about information, not process.

Andrade points to a motivational issue in measuring awareness among patients under general anesthesia. She discusses the isolated forearm technique which reveals high levels of awareness in anesthetized people, but, as she notes, even this test is fallible, because of motivational changes in subjects. Not only is it the case, as we argued in the target article, that evidence for learning and memory during anesthesia is contradictory (see Merikle & Rondi, 1993, for an even more skeptical review), it is also doubtful that anesthetized subjects are always unconscious of events happening around them.

R2.4. Defining explicit learning and consciousness by verbal report. Some commentators (**Catania, Dienes & Perner, Lindsay & Gorayska, Overskeid, Rakover, Terrace**) suggest that verbal report is the best place to draw the line between explicit and implicit learning. **Lindsay & Gorayska** argue that it may be more difficult than we assumed to prove that another test is more sensitive or exhaustive than is verbal report, and therefore only the latter should be adopted as an index of consciousness. But not only does this immediately rule out animals and nonlinguistic humans as being conscious, it also makes unconscious learning a logical necessity. Whereas we are accused of making unconscious learning a logical impossibility, Lindsay & Gorayska appear to be doing the exact opposite! We agree with **Merikle** that establishing a test as exhaustive is likely to prove very difficult, and we doubt that Lindsay & Gorayska have any case for establishing this with respect to verbal report.

The view that there is something special about verbal reports appears frequently in the commentaries, but we see no basis for adopting this strong position. There is no clear boundary between reports and other tests of awareness; instead, we take verbal reports to be merely one of several means of assessing awareness. For example, Marcel (1993) has shown that subjects can be much better at using responses other than verbal reports as indicators of awareness. Marcel presented subjects with a brief light close to the threshold of detection and instructed them to report on each trial whether they saw the light by blinking, pressing a button, and saying "yes." Subjects on the same trial reported with their finger that they saw the light while reporting orally that they did not, and vice versa. Most interestingly, verbal reports did not turn out to be the most sensitive index on detection: blinking, under some conditions, was far more sensitive. In the face of such findings, it is very difficult to assign a special role to verbal reports.

Other commentators point out that there is a qualitative difference between learning in subjects who can retrospectively report and justify their performance and subjects who deny having any knowledge of their performance. However, both the Sensitivity and Information Criteria suggest that verbal recall is a poor place to draw a line between learning systems. Reportable knowledge varies between subjects, their vocabularies, the retrieval cues available, prompting, set, certainty of the knowledge, and so on. We see little justification for a qualitative difference at the point of spontaneous or prompted verbal recall. We do see the level of verbal report as an interesting and legitimate independent variable, but one with quantitative rather than qualitative levels. And we agree with **Willingham** that quantitative differences make for poor taxonomies.

Two commentators (**Cleeremans** and **Willingham**) make the related proposal that consciousness varies on a continuum. As training continues, initially unconscious knowledge becomes increasingly conscious. Changes in the degree of consciousness would account for observed changes in the amount of information reported verbally as well as for changes in prediction accuracy and even reaction time. However, we prefer the interpretation that the information represented in consciousness increases in amount and in strength (i.e., certainty). Our argument, via the Sensitivity Criterion, is that verbal report places a high threshold of certainty and retrievability, whereas other tests, such as the prediction task, place a lower threshold. The lower threshold allows less certain information to be demonstrated.

Besides quantitative differences, of course, the content of verbal reports may vary qualitatively. As we discuss in the target article, learning (and testing) protocols may demonstrate heavy reliance on memorization and instance learning or they may demonstrate heavy use of reasoning and problem-solving skills and hypotheses. Note that although we believe that verbal reports should be treated with care, it is not the case (as **Catania** suggests) that we have ruled out verbal reports as relevant to judging a learner's awareness. They are certainly relevant; they may just not be exhaustive.

R2.5. Do prediction and recognition tasks measure conscious or unconscious knowledge? Several commentators (**Cleeremans, Dienes & Perner, Rakover, Seger**) raise the issue, addressed at some length in the target article, that prediction and recognition tests may not be "process-pure," that is, they may not provide pure indices of conscious knowledge but may be contaminated by unconscious knowledge. We reiterate that we believe this is a perfectly valid objection which will only be resolved by further investigation. At present we probably do not have sufficient information to resolve the issue.

Dienes & Perner criticise prediction and recognition tests, because such tests do not require subjects to describe the underlying basis of their responses: a subject may make a correct prediction without being able to *justify* that prediction. But this would only be relevant if we thought that justification played a role in the performance measure (e.g., RT). That is, if subjects only responded quickly in the RT phase when they could describe the basis of their expectation about where the next signal would appear, then to meet the Information Criterion, the awareness test would have to pick out only those cases where justification was available. But in general we have no reason to believe that justifications play a role in performance tasks, in which case Dienes & Perner's position has little direct support.

R2.6. The possible causal role of awareness. We made no inferences concerning the possible causal role of awareness. Our aim was merely to show that, as an empirical fact, awareness is necessary for learning. Some commentators (**Catania, Poldrack & Cohen**) wonder about the specific role that consciousness plays in learning. The view that awareness is necessary for learning could entail a range of possibilities, with a causal role merely representing one extreme. At the other extreme, awareness may have no causal role with respect to learning but may be correlated with it via a third factor.

Far from being tangential to the question of implicit learning, (as **Svartdal** claims) the correlation between level of performance and verbal report demonstrates the coupling between performance and consciousness. We have no particular reason for favoring one interpretation of the role of consciousness over any other. We note, however, that subjects may consciously choose a learning strategy, such as hypothesis testing or memorization, and that this choice affects learning. In this sense, at least, consciousness affects what is learned.

Squire et al. claim that in cases where the two are correlated, consciousness is epiphenomenal to learning. Thus, despite the fact that conditioning cannot be shown to occur in the absence of awareness, the latter plays no causal role and is merely a nonfunctional "shadow" that accompanies learning. But, as Lovibond (1993) argues eloquently, the correlation between conscious knowledge and learning is incredibly close. The epiphenomenalist position therefore looks decidedly strange, because if consciousness is an epiphenomenon, it ought to be possible to dissociate it from learning. There is no evidence that this is possible, however. (We argue below that amnesia does *not* represent a case in which awareness and learning are uncoupled.)

R3. Further evidence for implicit learning

Inevitably, commentators have cited studies that we failed to cover in the target article. We apologise if, by wittingly or unwittingly excluding certain pieces of evidence, we have reached erroneous conclusions.

Some commentators take us to task for considering such a limited and artificial range of learning tasks and for drawing more general conclusions about human learning than is warranted by these studies. To an extent, we cannot help but agree, but we also believe that complex real-world learning is largely reducible to simpler learning. By considering such basic learning paradigms as Pavlovian conditioning, we therefore hope to encompass more complicated and realistic learning situations. Besides, we know of no careful studies of the contents of people's consciousness when they are learning to play tennis (see **Overskeid**).

R3.1. The mere exposure effect. **Bornstein** maintains that data from experiments on the mere exposure effect provide compelling evidence for unconscious learning. In such experiments (e.g., Kunst-Wilson & Zajonc 1980), subjects show an increased preference for stimuli which have been repeatedly exposed for brief durations, but they cannot recognise those stimuli. We have no dispute over the robustness of such effects, but we do have strong reservations about whether they demonstrate implicit learning.

It is odd that **Bornstein** persistently refers to these experiments as involving stimuli perceived without awareness, because the evidence that the stimuli in most of these experiments are subliminal is very weak. For example, they are often presented unmasked, and a forced-choice present/absent test is rarely included (see Bornstein et al., 1987, for an exception). This is important, because if the original stimuli were genuinely subliminal, then any preference for them at test would have to represent a role for unconscious information.

Be that as it may, dissociations clearly do emerge between subsequent preference and recognition judgments, whether the stimuli were consciously perceived at study or not, and this finding may also provide evidence (albeit less direct) of unconscious processes. In this case, the idea would simply be that there is a source of information available for making the preference but not the recognition judgment, and this information may therefore be the end-product of an unconscious learning system operating in tandem with the conscious system. However, we pointed out that hypermnesia seems to occur for recognition responses in these experiments (Merikle & Reingold 1991), and we cited this as evidence that in making recognition judgments, subjects may initially discount a source of *conscious* information such as perceptual fluency (**Brody & Crowley** propose a similar account in their commentary). **Bornstein** does not provide an alternative explanation of the hypermnesia finding.

Moreover, it turns out that some very recent evidence supports our position and directly refutes the view that the preference and recognition judgments are based on different sources of information. Whittlesea (1993) has shown that the dissociation of affect and recognition can occur *even for items that did not appear in the study phase.* In his experiment, he required subjects to rate words for pleasantness and also to judge whether they had been recently presented; in fact, the critical words had not been preexposed. Because of a manipulation of the context in which the words appeared, some words were processed more fluently than others, and this led them to be rated as pleasant, but these same words were not called "old." Thus the dissociation of affect and recognition occurs even for novel items.

This result directly contradicts the idea that above-chance affect judgments are based on unconscious information encoded during the study phase. Instead, as Whittlesea argues, the result must be attributable to the different ways in which information (such as perceptual fluency) concerning a test item may be interpreted in affect and recognition tests.

R3.2. Conditioning. Our conclusion that awareness is necessary for conditioning is disputed by **Kimmel,** who cites as positive evidence a study by Grings (1965) in which subjects acquired autonomic CRs but "when questioned after three days of conditioning . . . revealed no knowledge about stimulus relations." Kimmel also cites a study (Kimmel et al. 1983) in which subjects could be shown to have learned about one of two coloured lights that signalled shocks but were unable to report whether it was the blue or yellow light, and another study (Lachnit & Kimmel 1993) in which subjects apparently learned to respond appropriately to CSs even if they were unaware of the relevant reinforcement contingencies.

Kimmel seems to think that such results are persuasive since "the subjects . . . were questioned very closely, and the questioners were motivated to detect any sign of awareness. At the very least, Shanks & St. John should describe what they consider inadequate in interview procedures from studies claiming conditioning without awareness, instead of rejecting them peremptorily or acting as if they do not even exist." We did not dismiss these studies peremptorily; rather, we consider them to have exactly the same flaws as other studies that we did cite. These flaws include using an awareness index that fails to provide the subject with the same quality of retrieval cues as the conditioning measure.

It does not matter how closely the subjects are questioned or how motivated the questioners were. If the subjects' knowledge is fairly ephemeral, then any decrement in the conditions of testing between the implicit and explicit tests will lead to less information being revealed. We leave it as an exercise for the reader to decide whether results such as those **Kimmel** cites provide conclusive evidence of unconscious learning. Incidentally, we have been unable to evaluate the Lachnit and Kimmel (1993) study that Kimmel cites, because that report does not in fact include any discussion or data concerning aware versus unaware subjects.

We do not believe we have given an unfair description of **Baeyens et al.**'s data. When subjects are given a concurrent test of awareness (rather than just a postexperimental questionnaire), more of them appear to be aware of the conditioning contingencies. Is this because the concurrent measure is more sensitive, or because it actually creates awareness? Baeyens et al.'s belief in unconscious learning requires them to exclude the former possibility, but we see no reason (and they provide no evidence) for such a conclusion. There are any number of reasons why a concurrent test might be more sensitive than a *post hoc* one. For further criticism of the data on evaluative conditioning, see Davey (1994).

R3.3. Instrumental learning. We applaud **Svartdal**'s use of an interesting alternative to the standard logic of dissociation to demonstrate unconscious instrumental learning. Svartdal recommends providing explicit instructions that focus the subject's attention on one aspect of responding (e.g., position), but arranging for reinforcement to be under the control of variations in another dimension of responding (e.g., force). With such a procedure, Svartdal (1992) showed that variations in the dimension of responding that was in fact reinforced became larger and larger, apparently without the subjects being aware of this change.

As **Svartdal** notes, though, such an experiment is still likely to be dogged by the problem of correlated hypotheses. He dismisses the claim that correlated hypotheses may have been the basis of his subjects' responding, because when asked to formulate advice to another hypothetical person on how to master the task, subjects typically gave very general answers and failed to verbalise any such correlated hypotheses. But until we know what the putative correlated hypothesis might be, it is very difficult to know what we should be looking for in these protocols. If the hypothesis is only partially used, or if several different correlated hypotheses are used concurrently or on different trials, or if the hypothesis is very complex, subjects' inability to give useful verbal reports would hardly be surprising. Because there can be no doubt that correlated hypotheses play a crucial role in many instrumental tasks (Dulany 1961), we reiterate our conclusion that the existence of such hypotheses makes it very unlikely that clear evidence of unconscious learning will emerge from instrumental tasks.

Berry rightly points out that we should have discussed the study by Stanley et al. (1989), which used the yoking procedure that we commended in the target article. However, we believe that this study provides far from convincing support for implicit learning. In their first three experiments, Stanley et al. observed that verbal protocols from subjects who have had extensive practice at a control task (sugar production or person interaction) could significantly improve the performance of naive subjects, indicating that a good deal of knowledge was available to verbal report.

In Experiment 4, Stanley et al. (1989) selected a sample (12 out of 53 subjects) of people learning the control task who had a clear transition point where their performance showed a marked and relatively sudden improvement. Stanley et al. found that yoked subjects given the pretransition verbal protocols from these subjects showed just as much benefit as yoked subjects given the posttransition protocols. From this Stanley et al. concluded that a sharp performance increase is not immediately accompanied by an increase in the quality of verbal report, because otherwise the yoked subjects given posttransition transcripts should have shown a much greater benefit than those given the pretransition transcripts.

Although this result is certainly interesting, we believe that there are at least two problems with Stanley et al.'s interpretation. First, a statistical artifact may have been introduced by the procedure of selecting subjects who showed a sudden transition in performance. Naturally, random variation accounts for some of the fluctuations in performance in these tasks, but if it is merely chance that is responsible for the selected subjects' transitions, then there is no reason to expect their accompanying verbal protocols to show any marked improvement after the transition.

Second, Stanley et al. found in Experiment 3 that naive subjects given a simple rule did not perform at the maximum level achievable with that rule. Subjects were told to "select the response level half-way between the current production level and the target level," which if followed could lead to 83% responses on target. However, subjects achieved only 50% correct responses. The obvious conclusion is that even when subjects have access to a useful rule, they will not always use it. Perhaps they are not confident that a rule is correct until they have tested it by comparing what happens when they do and do not follow it. But if that is the case, then there is no justification for supposing that the yoked subjects in Experiment 4 should have shown an immediate improvement in performance when given the posttransition protocols. In sum, the yoked design Stanley et al. used is very promising (and without question shows that subjects may have access to far more knowledge than would be predicted by other forms of questioning), but it has not established that any significant amount of knowledge is unconscious.

Rizzo & Parlangeli argue that in complex control tasks where some real-world schema is available to the subject, subjects will use that schema as a basis for rule learning. They report that in some circumstances a subject will be unable to learn to control a system formally identical to one they can learn to control if the former retrieves schema-based knowledge that is incompatible with the contingencies of the task. Rizzo & Parlangeli suggest that this is inconsistent with the existence of an independent implicit learning system, because such a system would be unaffected by the specific content of the task. We are not sure that this line of reasoning is watertight, but it is certainly interesting that such differences can emerge. Rizzo & Parlangeli also report obtaining strong correlations between performance and verbal report.

Overskeid claims that instrumental learning is possible during sleep, but we see no good reasons to assume that people are completely unaware when they are asleep. If it is difficult, as **Andrade** suggests, to prove beyond doubt that people are unconscious when they are anesthetized, then it will surely be a good deal harder to prove it when they are asleep. Moreover, numerous well-controlled studies have failed to reveal any signs of learning during sleep (British Psychological Society 1992; Wood et al. 1992)

R3.4. Sequence learning. Our interpretation of Stadler's (1989) study is very nicely refuted by **Stadler & Frensch.** We argued that the failure of the prediction test to reveal any savings (although the RT data indicated that learning had occurred) may have been due to interference and hence rapid forgetting during the test, which provided no feedback to maintain accurate performance. Our hypothesis was that evidence of savings may have been present on the earlier test trials but abolished on later trials as interference built up. Stadler & Frensch report that prediction performance was at chance even from trial 1.

Because we have argued that this study goes a good way toward meeting the Sensitivity and Information Criteria, we are happy to acknowledge that the results appear to provide good evidence of unconscious learning. Our only outstanding concerns would be (1) that the assessment of awareness was based on a fraction of the number of observations that were used to demonstrate RT speed-up, introducing possible problems of statistical power, and (2) that we continue to believe that withholding feedback in the prediction test works against the possibility of finding significant savings. But because the awareness test provided no hint of savings, the former concern is probably unimportant, and the latter concern can easily be avoided by providing feedback and comparing prediction performance with that of a control group trained on random patterns. Stadler's (1989) study clearly merits replication and further exploration to reveal why this experiment obtained evidence of a dissociation between RT speed-up and prediction performance whereas so many others, reviewed in the target article, have failed to do so.

Reed & Johnson also report a new experiment that seems to meet our criteria while still obtaining a dissociation between performance and awareness. In that experiment, subjects trained on the repeating reaction time sequence 12134231432 were then tested on the sequence 123413214243, which is matched in terms of single-event and pairwise frequencies. The increase in response latencies that Reed & Johnson (1994) observed must therefore be attributable to genuine sequence learning, involving at least knowledge of second-order structure. That this knowledge was implicit is suggested by the fact that performance on generation and recognition tests was at chance. In the generation test, for example, subjects saw sequences of 2 target locations from the training phase and they then generated the 10 locations they thought had followed the target sequence in the training phase. Reed & Johnson found that subjects were no more likely to make a correct first response following the 2 targets than were control subjects trained on a random sequence. The recognition test also revealed no evidence of explicit sequence knowledge.

We are somewhat less convinced than with Stadler's (1989) experiment that this provides compelling evidence of unconscious learning. First, with a relatively small number of trials per subject, the generation and recognition tests may not have been powerful enough to detect small savings from the training phase. Second, it is not at all clear why Perruchet and Amorim's (1992) subjects were able to perform above chance in generation and recognition tests whereas **Reed & Johnson**'s were not. It is true that there are several procedural differences between the tasks, but the discrepancy remains troublesome. Third, and most important, we have found in a replication of Reed & Johnson's experiment that subjects can perform well above chance in a generation test. We trained subjects on the same sequence and under the same conditions Reed & Johnson used, but we then required the subjects to predict target locations in a standard prediction test containing 96 trials, with feedback about correct target location. On the first cycle of 12 trials, subjects made reliably more correct predictions (45.2%) than would be expected if they only had knowledge of first-order structure (33.3%). Our subjects, therefore, did appear to have access on the generation test to second-order sequence knowledge. Of course, firm conclusions must await additional data.

Cleeremans suggests that "there is no evidence whatsoever that subjects have conscious access to . . . distributional information about the stimulus material." We disagree. Even if prediction performance is to some extent contaminated by unconscious processes, the results (including those of Cleeremans & McClelland 1991, but especially those of Perruchet & Amorim 1992) suggest that subjects have extensive conscious access to the relevant distributional information. More important, there is no evidence that they do *not*.

R3.5. Amnesia. Many of the commentators (**Cleeremans, Holyoak & Gattis, Kourtzi et al., Packard, Poldrack & Cohen, Reber & Winter, Seger, Squire et al., Willingham**) were concerned by the lack of weight we gave to data from memory-impaired patients. The argument for the significance of such data is clearly stated by Poldrack & Cohen. They claim that "it has been well documented that amnesic patients can exhibit normal learning of (motor, perceptual, and cognitive) skills, and can do so in the absence of the ability to recollect the learning experiences." If learned information can project itself onto performance without being available to consciousness, then surely we have *prima facie* evidence of implicit learning.

We acknowledge that such results provide, at first glance, an apparently strong case for an influence of unconscious information. However, we believe that the commentators have failed to give a fair or complete picture of the data from patients with anterograde amnesia. We believe there are many places where their argument needs a substantial amount of extra work, and in the present section we enumerate various reasons why amnesic data may be explained without appeal to implicit learning.

As a preliminary consideration, it is important to note that there are an enormous number of theories of amnesia, and how they bear on the issue of unconscious learning varies. The view that what is impaired in amnesia is the ability to have conscious declarative memory of prior learning episodes is only one of many theories, and it is by no means clear that this theory is correct (see Mayes 1988). If amnesia in fact turns out to be a selective deficit of contextual processing (Mayes 1988), or of configural learning (**Kourtzi et al.**; Rudy & Sutherland 1992), or of spatial learning (Hirst & Volpe 1984), or if it turns out to be attributable to any of the huge number of other factors that have been postulated, then its relevance to the issue of unconscious learning will be very limited.

Another reason why data from amnesics may not provide additional illumination concerning implicit learning is that the pattern of data seen in amnesics can readily be obtained with normal subjects anyway. As mentioned in the target article, memory-intact people can be induced to perform at above-chance levels on indirect tests of memory such as preference tests, while performing much more poorly (and perhaps even at chance) on recognition tests (e.g., Kunst-Wilson & Zajonc 1980). As explained in the target article and in section R3.1, we see little reason to regard such results as evidence of unconscious learning. Poor recognition memory may occur simply because subjects discount a source of conscious information. But perhaps evidence from amnesia is more compelling?

It may be helpful to consider a specific example that is typical of the behavior of amnesics. Knowlton et al. (1992) trained normal and amnesic subjects in the typical manner of an artificial grammar study, and found that they performed at approximately equivalent levels in classifying test strings as grammatical or nongrammatical (normals = 66.9% correct, amnesics = 63.2% correct). However, on a recognition test in which old grammatical and new nongrammatical strings were presented, the amnesics (62.0% correct) performed significantly worse than the normal subjects (72.2% correct). Thus, on the test of conscious knowledge of which strings were study items, the amnesics were impaired, but this had no significant effect on their ability to use those items to guide classification.

Cleeremans, Holyoak & Gattis, Kourtzi et al., Packard, Poldrack & Cohen, Reber & Winter, Seger, Squire et al., and **Willingham** all believe that results such as this provide evidence of implicit learning. Yet the logic of the argument for this conclusion is rarely spelt out (though see Reingold & Merikle 1988 for an excellent discussion). Presumably, when fleshed out it would run in one of two ways, depending on the exact outcome of the recognition test:

1. If the amnesics are at chance on the recognition test but perform above chance on the implicit classification test, that means there is information that is available to the implicit but not to the explicit test. Subjects will be strongly motivated to use all available *conscious* information when performing the explicit recognition test, hence, the information that is projected onto the implicit but not the explicit test must be *unconscious*. And if that information is unconscious at the time of retrieval, then it is plausible to assume that it was registered unconsciously.

2. If the recognition test reveals above-chance performance, then the argument is slightly more complex and presumably runs as follows. In Knowlton et al.'s (1992) experiment, for example, it is no longer possible to say straightaway that the amnesics had access to a source of information on the implicit test that was not available to them on the explicit test, because recognition and classification are measured in quite different ways. What we can say is that the normal subjects have access to more information on the recognition test than do the amnesics. Because this information is assumed to be conscious, the only way the normals and amnesics could perform equally well on the *implicit* test is if the test is unaffected by conscious information (because normal subjects have more conscious information but they do not perform any better). Therefore above-chance performance on the implicit test must be attributable to *unconscious* information, and the argument proceeds as before.

With respect to the sorts of implicit learning experiments that have been conducted with amnesics, it is the second of the above arguments that is relevant rather than the first. In these experiments (e.g., Knowlton et al. 1992), explicit and implicit memory are assessed in quite different ways using different response scales, and recognition is typically above chance. We know of no clear cases where identical measures are used (e.g., forced-choice recognition versus classification), and in any case our reservations about the mere exposure effect in normals would apply equally to such cases.

Thus we must evaluate the second of the above arguments. There are many places in which it could be challenged, but for present purposes it is vital to note that the line of reasoning only works if performance is truly equivalent in the amnesics and normals on the implicit test. If the normal subjects perform even only marginally better on that test, the argument will not go through, because it is possible that the extra conscious knowledge the normal subjects have (as indexed by superior performance on the explicit test) is what explains their superior performance on the implicit test.[1] We would then be left with no grounds for concluding that unconscious knowledge is playing *any* role at all.

We will argue that the data do not in fact provide strong support for the argument, because, contrary to what is usually claimed, implicit tests of knowledge typically *do* reveal impaired learning in amnesics (in situations where their explicit knowledge is poor). It is simply not true that amnesics perform normally on implicit learning tasks for which their explicit memory is impaired: at most, what can be said is that on some implicit tasks we do not know for certain whether they are impaired or not, whereas on others they are definitely impaired.

As the data cited above show, Knowlton et al.'s (1992) amnesic subjects performed worse (albeit nonsignificantly) than their control subjects, and this is typical of implicit learning studies with amnesics. For example, Kolodny (1994) found that amnesics were worse than normal subjects at learning to classify unfamiliar paintings. Squire and Frambach (1990) found that although amnesics were able to learn to control a complex task, their performance was worse than that of normals. Knowlton and Squire (1993) found that amnesics could correctly categorize the unseen prototype pattern of random dot stimuli, though at a numerically but nonsignificantly lower level of accuracy than could normals. Nissen and Bullemer (1987) observed that although amnesics could improve their performance in a sequential reaction time task, they learned less than normals, as indexed by the fact that their RTs increased by a smaller amount when they were transferred to a random sequence.

Similarly, Charness et al. (1988) found that amnesics were impaired in learning patterns in the Hebb digits task. Amnesics such as H. M. were impaired at learning puzzles such as the Tower of Hanoi (Gabrieli et al. 1987). Finally, despite repeated claims that conditioning occurs at normal rates in amnesics, there is clear evidence of impairments in conditional discrimination learning (Daum et al. 1991) and simple operant conditioning (Channon et al. 1993), if not in simple conditioning.

Note that we are not arguing that amnesics *always* show impaired learning. Rather, we are claiming that in those cases where their *explicit* memory for some information is impaired, their implicit memory for that information will be so as well. Thus the finding, cited by **Squire et al.**, that amnesics show normal speed-up in learning to read mirror-reversed text is irrelevant unless it can be shown that they have poorer explicit memory for the knowledge responsible for their improved performance. That their memory for the *content* of the text they have read may be poorer than that of normals is not sufficent to establish this fact. Reading mirror-reversed text may be based on something totally unrelated to text memory, such as knowledge of single mappings from reversed to normal letters.

Defenders of the view that amnesics show normal implicit, but not explicit, learning would almost certainly reply that, even granting the above cases, there are numerous other implicit tasks, such as repetition priming, in which the performance of amnesics is unquestionably normal (although they have impaired explicit memory), and that these cases provide incontrovertible evidence of implicit learning. For example, **Squire et al.** assert that priming is intact in amnesia. Again we have to disagree. The main problem is that amnesics almost invariably perform at lower baseline levels than normals, in which case it becomes very difficult to determine what counts as "normal" learning. Numerous studies have shown that, in absolute terms, the amount of repetition priming observed in amnesics and normals is equivalent, but Ostergaard (1994) pointed out that these amounts of priming occur against a background of different overall baselines. Because priming in normals is correlated with baseline latency, and because amnesics have a slower overall baseline, they need to show a larger absolute priming effect to achieve the same relative effect as normals. Amnesics show the same amount of priming in absolute terms, however, despite their slower baseline; hence, it follows that amnesics are *impaired,* not normal, on these tasks.

Part of the difficulty here is that to claim that amnesics learn normally in some tasks is to accept the null hypothesis that a difference does not exist (e.g., Knowlton et al.'s 1992 experiment), but often it is questionable whether the experiment had sufficient statistical power to reject the null hypothesis. Of course, one simple way around the problem would be to show that amnesics can perform *better* than normals on some implicit task (e.g., as a result of having been given more study trials or a shorter retention interval), coupled with *poorer* explicit memory. Our position seems to gain support from the fact that there appear to be no published cases of this sort. Granted, there are some studies in which amnesics have been tested after a shorter retention interval than normals such that performance on the implicit memory test is equated, and where the amnesics still appear to show a selective impairment of episodic memory (e.g., Schacter et al. 1984), but these results have proven very difficult to interpret, because the difference in performance on the implicit and explicit tests may be a function of retention interval (see Mayes 1992). A crossover interaction would be much more persuasive.

We believe that impaired learning in amnesia, as indexed by implicit tests, is the norm rather than the exception. That being the case, the argument that amnesic data demonstrate implicit learning comes completely unglued, because it is quite possible that the pattern of impairment seen in amnesia can be interpreted in terms of a single functional knowledge system: it is no longer necessary to assume two distinct forms of knowledge (conscious and unconscious) contributing to task performance.

By analogy to Farah et al.'s (1993) model, we assume that knowledge is represented in the weights of a parallel-distributed memory system which can be probed both implicitly and explicitly. When lesioned, such a system would have sufficient knowledge to perform fairly well on an implicit test that can benefit from small and fragmentary amounts of knowledge, but the retained knowledge may not be sufficient to allow for the complete trace redintegration that is typically required in an explicit test. This is tantamount to saying that the explicit tests used to evaluate conscious knowledge in amnesics are generally less sensitive to small amounts of residual information in memory than the implicit tests used to measure performance.

Whatever the merits of our argument, it may well be objected that we have provided no direct evidence in favor of it. We have therefore carried out some computer simulations to try to reproduce Knowlton et al.'s (1992) pattern of data in a single system. The first author, in collaboration with N. Hursey, set up a standard back-propagation network to classify strings as grammatical or nongrammatical. The network was presented with grammatical and nongrammatical strings in the training phase and learned to produce the correct classification decisions across one set of output units. In addition to being taught to classify the strings, the network was simultaneously trained to reproduce the input string across a second set of output units, as in an "encoder" network. After "lesioning" (in fact involving the addition of random noise [mean = 0.0] to the weights) the network's classification and recognition performance was evaluated. For recognition, we measured the accuracy with which each input string was reproduced across the relevant output units.

The critical result was that classification accuracy was only mildly affected by the "lesion" but the network's ability to recognize the training patterns correctly was severely impaired, reproducing the exact pattern that Knowlton et al. (1992) observed in their human subjects. Such a result should not be surprising: it merely extends to amnesia the results reported by Farah et al. (1993; see sect. R2), and confirms the claim that recognition judgments are less sensitive to partial information than are grammaticality judgments. Our simulation also illustrates why it is so important that amnesics are impaired, even if only marginally, on implicit tests: it is because a connectionist network would have great difficulty modelling a case in which performance on only one type of test is impaired.

On the hypothesis that there is a single source of conscious information that is used on implicit and on explicit tests, amnesics would be predicted to perform worse on both; and this is exactly the pattern of results that is observed. In sum, we believe that much of the data from amnesic subjects performing implicit learning tasks of the sort reviewed in the target article can be interpreted by means of a single functional system, with some memory tests simply being more sensitive than others to small amounts of fragmentary information in the system. It is certainly possible that the behavior of amnesics is relevant to the question of unconscious learning, but we believe that advocates of that view will have to do a great deal of work to establish it.

R3.6. Language learning. Language acquisition is frequently cited as a prime example of unconscious rule learning (e.g., see commentaries by **Lachter, Nagata,**

Reber & Winter). Everything from spelling to grammar appears to be rule governed, and even though native speakers perform well on implicit tests of linguistic knowledge (e.g., grammaticality judgments), they are rarely able to report the relevant rules. Such a result is evidence for implicit learning provided the Information Criterion is met: as long as the rules identified by linguists are indeed the basis of peoples' implicit linguistic competence, their inability to report them suggests that the rules are represented in an unconscious form.

A number of researchers, however, have begun to challenge the idea that linguistic knowledge is rule based; they have shown instead that aspects of language may be learned by distributed instance memory. For example, Rumelhart and McClelland (1986) and Plunkett and Marchman (1991) describe connectionist networks that can learn to perform the mapping of English verbs from their root forms to their past tense forms. Sejnowski and Rosenberg (1987) and Seidenberg and McClelland (1989) constructed networks that learn to transform written words into spoken words, and Elman (1990) and St. John and McClelland (1990) showed that networks can learn simple aspects of English grammar and semantics (these are representative examples among a host of others). All of these network models learn by repeated exposure to instances. Over time, regularities in the data are incorporated into the weights. This instance-based learning procedure is the same procedure that was used by Dienes (1992) to model artificial grammar learning and by Cleeremans and McClelland (1991) to model serial reaction-time learning.

Lachter questions what children could be aware of that would allow them to make grammaticality judgments about intricately structured sentences. One might ask whether there are any distributed processing network models, such as those described in the target article, that can perform sophisticated grammaticality judgments. The current answer is no, but some network models are beginning to approach this level of sophisticated sentence processing. Elman (1993) described a network that accurately processes center-embedded sentences, and St. John (1994) found that a network can accurately process sentences with subject- or object-relative clauses. These networks could be modified to produce actual grammaticality judgments for artificial language strings.

Are these models focusing on the important structural organization of the languages on which they are trained? In analyses of the hidden unit representations that these networks develop, Elman (1993) has found evidence that they learn aspects of the phrase structure of the language. Given that these networks are developing a sensitivity to linguistic structure, the possibility that future distributed instance/memorization models will attain a sophisticated level of processing and be capable of making grammaticality judgments appears quite promising.

The research into network models of language learning is controversial and far from complete. For example, Pinker (1991) and Lachter and Bever (1988) argue that the detailed phenomena of the regular verb past tense require a rule system (see **Goldstone & Kruschke**'s commentary). What we can conclude is that it has not been shown beyond doubt that linguistic knowledge is in the form of rules. That being the case, peoples' inability to articulate such rules is neither here nor there. Time, and further research, will tell what informational processes underlie children's linguistic behavior and how far network models can be taken to model aspects of language learning without appealing to unconscious rule-learning systems.

R4. How should implicit learning be characterized?

Which dimensions make for sensible distinctions between learning systems? A common answer has been to use the dimension of consciousness, but we have not found a distinction between conscious and unconscious learning to be warranted. We also discussed in section R2.4 of this response the possibility that a distinction between verbally reportable and unreportable knowledge violates the Sensitivity and Information Criteria. Instead, we propose that the distinction between verbally reportable and unreportable knowledge is a useful, though fallible, indicator of a real distinction between rule learning and instance learning.

Several authors take issue with the rule/instance distinction or offer alternative ones. **Nagata** suggests that our view of what is learned in artificial grammar experiments does not differ markedly from Reber's. Whether or not this is the case, the distinction between rules and instances remains a valid one. Just because artificial grammar experiments may not distinguish between rule and instance learning, it does not follow that other experiments, such as that of Regehr and Brooks (1993), do not.

Holyoak & Gattis suggest the additional criterion of poor transferability and flexibility that, together with instance-based knowledge and limited reportability, seem to characterise what goes by the name "implicit learning." This matrix of characteristics seems to fit with and elaborate on the fragment/instance memorization system. Somewhat similarly, **Kourtzi et al.** propose a distinction between learning that depends on stimulus recoding and learning of stimulus-response associations. Direct S-R learning is assumed to be less flexible than stimulus recoding. We are not sure whether these two kinds of learning are meant to look different in on-line protocols or are meant to be subject to different independent variables, but in general we welcome the further exploration of these alternative/additional distinctions and their comparison to the instance memorization/hypothesis testing distinction we proposed.

Merikle says that the attempt to (dis)prove any given dissociation is doomed; simple dichotomies never seem to get settled (cf., Newell's 1973 "You can't play 20 questions with nature and win"). We feel that this position is too strong, because researchers have gathered a huge amount of data and learned a great deal about human learning systems from considering simple dichotomies.

R4.1. Consciousness and models of implicit learning. Our speculations about how to model awareness in a PDP model are criticised by **Cleeremans** because such models are not aware of the abstraction process across items. Thus, assuming that the abstraction process in humans is analogous to the learning processes in a PDP network, this would appear to constitute a straightforward case of learning without awareness. To be absolutely clear, we are not proposing that networks are conscious. Rather, we are assuming that they provide *models* of human learning and consciousness.

We agree wholeheartedly with Johnson-Laird's (1983, p. 449) point that "like the weather, [consciousness] is explicable by theories that can be simulated by computer programs, but it can no more be embodied within a computer than can an actual anti-cyclone: only organisms with brains can be conscious." For reasons that no one understands, some of the operations of the parallel network that is the brain happen to be accompanied by awareness of the content of what is being processed. We suggest that artificial networks are capable of modelling, albeit in a rudimentary way, the computational processes of the brain, but that these processes are only accompanied by consciousness in real brains. Nonetheless, we can quite happily hypothesise that it is a certain type of processing operation (in brains or artificial networks) that correlates with conscious experience.

We strongly disagree with **Cleeremans**'s assertion that "the very fact that obviously nonconscious and elementary networks are able to account for human performance . . . has always struck me as evidence that awareness is in fact ancillary in these tasks." It would be bizarre to conclude that because a computer can simulate the flight of a spaceship, therefore rocket fuel is unnecessary for reaching the moon. For human learning, awareness may be as necessary as rocket fuel is for space travel.

R4.2. The fragment/instance memorizer. We agree with a number of commentators that rather than arguing about consciousness, we ought to examine and characterise the real differences between implicit and explicit learning. It was in this spirit that we took up the question of what is truly going on in implicit learning, namely, instance and fragment memorization.

We believe that the available data are best explained in terms of learning rules versus instances, and we are impressed by the data **Marsolek** cites in favour of distinct anatomical correlates of instance and rule learning. The next big question is, what does the instance/fragment memorization strategy look like, and how does it differ from the rule-learning strategy? **Cleeremans** says we need a process model, and we agree. But we feel that a step along the way toward that goal is to examine the sorts of information this system uses: what are instances and fragments, and is this information sufficient to model subjects' levels of performance? We reported several studies (Cleeremans & McClelland 1991; Dienes 1992; Perruchet & Pacteau 1990; Reber & Allen 1978; Servan-Schreiber & Anderson 1990) that address this question.

Still, many commentators wonder what is learned in implicit learning. What is this instance/fragment information like? Is it an explicitly represented list of instances, or is the representation less explicit, less individualised? And how is it acquired? We have suggested that knowledge of instances or fragments is represented across a large number of weights in a parallel processor. This distributed processor system is well characterized by Dienes's and Cleeremans & McClelland's connectionist models – as we point out in the target article. Still, it seems from **Cleeremans**'s comments as well as those of others (**Seger, Perruchet & Gallego**) that we need further exploration of how these distributed processors work and how we can characterise their knowledge.

Whether or not subjects are aware of the abstraction *process*, they are plainly aware of the resulting knowledge of fragments and fragment frequencies (Perruchet & Pacteau 1990; Reber & Allen 1978), and likely continuations of strings and sequences (Dienes et al. 1991; Dulany et al. 1984). This information is available and even reportable at test time.

If we accept the equation of consciousness with activation, then subjects can be aware of the *level* of activation due to different frequencies and variabilities of fragments during training. More frequent fragments will be encoded more often, their weights will grow stronger and produce higher activation levels. As a result, these fragments will seem familiar, both during training and at test time. When new letters violate a common fragment or a light violates a common sequence, the subject may "feel" the violation as a mismatch between past predictions and current encodings via a general reduction in the activation of the network. Either way, the activations of the network provide information that the subject may report as knowledge of the items. With learning, the weights increase and therefore the activations increase. Should we call this change an increase in awareness or an increase in certainty of knowledge? We think the overwhelming evidence falls on the side of interpreting this increase as a change in certainty and quantity of knowledge.

We agree with **Cleeremans** that it is hard to imagine how knowledge encoded in these weights could be inspected directly. But, as we tried to point out above, we do not think direct inspection is necessary for the information in the weights to be consciously available.

Such knowledge does not require an explicit and individuated encoding of each stimulus. Rather, we point to **Cleeremans**'s commentary as a good description of how a distributed processor acquires the statistical regularities of a corpus of stimuli and encodes that information in a distributed format. Contrary to **Kimmel**'s and **Seger**'s comments, distributed processors can perform a good deal of abstraction from a corpus of instances. Maybe the term "instance memorization" is not really adequate to convey this automatic form of statistical abstraction. Perhaps "distributed instance/fragment encoder" would better designate the process we wish to consider.

R4.3. The relationship between memorization and rule induction. A claim is made by **Reber & Winter** that our account fails to come to grips with the requirement that the memorial representation acquired in an artificial grammar learning experiment must be of a form that permits generalization and transfer to novel stimulus items with different physical instantiations. We do not disagree about the facts; even though we argued in the target article that much of the data from such studies can be understood simply in terms of instance or fragment memory, we did conclude in sections R2.5.3 and R3.2 that transfer to novel items with different physical instantiations is indeed possible (e.g., Altmann et al., in press).

However, contrary to what **Reber & Winter** argue, we maintain that this presents no great difficulty for our conception of implicit learning. In addition to memorizing fragments and instances of the study strings, subjects also engage in a certain amount of hypothesis testing, from which they induce abstract rules such as: "the first two symbols of a string cannot be the same." Such rules allow them to perform slightly better than chance on a grammaticality test with entirely different items such as melodies. However, when tested on items in the same format as the training items, both the induced rules and the memorized fragments can contribute to the grammaticality judgement process, and hence performance is better than in the changed-format condition.

Packard suggests that the notion of a "rule" is not useful for characterizing types of learning, because a rule is merely a guide or principle and "almost any task can be construed to involve rule learning." Of course, as with the terms "learning" and "consciousness," a great deal of ambiguity is possible concerning what one means by the term "rule," but we had a specific concept in mind in the target article. A subject is responding on the basis of a rule if performance to test items is uninfluenced by similarity (e.g., to particular training items). Studies such as that of Nosofsky et al. (1989) illustrate how formal rule models can be constructed and how predictions based on instance memorization and rule use can differ at the quantitative level.

The relationship between distributed instance models that can perform some abstraction on the one hand and rule-learning systems on the other is a complex one. **Goldstone & Kruschke** and **Howe & Rabinowitz** question whether it is possible, given our present state of knowledge, to conclude that memorization and rule induction reflect the operations of distinct *systems*. We agree that conclusions should be tentative. We also agree with **Goldstone & Kruschke, Nagata,** and **Poldrack & Cohen** that there is enormous debate over what is an instance, what is a rule, the extent to which they interact, and what constitutes a model of either. But we doubt that memorization and rule induction can be seen as the endpoints of a continuum. Similarity either has a detectable effect or it does not: this is not a continuum.

Our concern is to explain the differences in learning and test behavior in experiments such as that of Reber et al. (1980) where explicitly instructed subjects performed quite differently from observation instructed subjects. Though we are not aware of any studies in which on-line protocols were taken from subjects in these conditions, our prediction is that the two groups of subjects would look radically different. A hypothesis-testing strategy is quite different from the distributed encoding of instances. It looks different in on-line protocols, as judged from studies of hypothesis testing (Bruner et al. 1956; Klahr et al. 1990), and a different set of variables affect behaviour (Mathews et al. 1989; Reber et al. 1980).

For example, Reber et al. (1980) found that the order of presentation of the stimuli only affected test performance for the verbally instructed subjects. Of course, as **Goldstone & Kruschke** might agree, it is likely that even these verbally instructed subjects, who were presumably following a hypothesis-testing strategy, would show some influences of particular instances in their behavior, but we would argue that these result from the influence of a separate instance-encoding system.

Some commentators (**Goldstone & Kruschke, Poldrack & Cohen**) cited Barsalou's (1990) claim to the effect that abstraction and exemplar theories are logically indistinguishable as evidence of the difficulty of maintaining a contrast between inducing rules and memorizing examples. But although Barsalou's argument is interesting, his conception of an abstraction is very different from our notion of a rule, so we believe the relevance of his argument is limited. For Barsalou, an abstraction is a representation in which property information is centralised. For us, a rule does not encode information about studied exemplars at all, but rather is a generalisation about regularities in the exemplars.

To conclude, then, we agree that distributed instance models produce a significant amount of abstraction and therefore blur into what we think of as rules and rule-formation models. However, there is strong evidence for a very different strategy of hypothesis testing that looks quite different from memorization.

We fully agree with the commentators who argue that future research should take the direction of finding the differences (and mechanisms) that distinguish implicit from explicit learning. We think one profitable avenue will be to investigate the sorts of factors that will differentially influence a strategy of learning instances from one of testing hypotheses.

NOTES

1. **Nagata** and **Reber & Winter** seem to believe that if only a "small" amount of explicit knowledge is available, whereas a "large" amount of implicit knowledge can be shown, then implicit learning has been satisfactorily demonstrated. With respect to amnesia, these commentators would presumably argue that the small benefit the normal subjects show over amnesics in the implicit test cannot explain the large discrepancy on the explicit test, in which case argument 2 is still viable. Because the normal subjects have far more explicit knowledge than the amnesics, it follows that if the two tests relied on a common source of knowledge, the normals would have to perform considerably better than the amnesics on the implicit test. But their excess explicit knowledge is unable to boost performance very much on the implicit test. Hence the latter is principally sensitive to unconscious information. We believe this view may be quite widely held, but it is very important to emphasize that it is entirely fallacious. The problem is that a difference of x% correct on an implicit measure is as comparable with a difference of y% correct on an explicit measure as are apples with oranges. Just because we may use a similar measure (e.g., percent correct) in each case does *not* mean that they can be directly compared, because the tests will differ in all sorts of other ways. For example, the chance level of performance is often different in the two tests. It is in general not possible to translate a difference on a given response measure into information, and hence inferences cannot be drawn about whether a subject has "more" or "less" explicit than implicit knowledge. For the same reason, **Reber & Winter**'s claim that implicit learning is less affected than explicit memory by such factors as individual differences and IQ is completely unproven. In the Reber et al. (1991) study, for example, IQ correlated less well with performance on an implicit grammaticality judgment task than with performance on a quite different series continuation task assumed to require explicit processing. But there is no reason to believe that the response scales for these two tasks are comparable or that the difference between them is attributable to the implicit-explicit dimension.

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Letters *a* and *r* appearing before authors' initials refer to target article and response respectively.

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