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Semantic activation without conscious identification in dichotic listening, parafoveal vision, and visual masking: A survey and appraisal

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Abstract: When the stored representation of the meaning of a stimulus is accessed through the processing of a sensory input it is maintained in an activated state for a certain amount of time that allows for further processing. This semantic activation is generally accompanied by conscious identification, which can be demonstrated by the ability of a person to perform discriminations on the basis of the meaning of the stimulus. The idea that a sensory input can give rise to semantic activation without concomitant conscious identification was the central thesis of the controversial research in subliminal perception. Recently, new claims for the existence of such phenomena have arisen from studies in dichotic listening, parafoveal vision, and visual pattern masking. Because of the fundamental role played by these types of experiments in cognitive psychology, the new assertions have raised widespread interest.

The purpose of this paper is to show that this enthusiasm may be premature. Analysis of the three new lines of evidence for semantic activation without conscious identification leads to the following conclusions. (1) Dichotic listening cannot provide the conditions needed to demonstrate the phenomenon. These conditions are better fulfilled in parafoveal vision and are realized ideally in pattern masking. (2) Evidence for the phenomenon is very scanty for parafoveal vision, but several tentative demonstrations have been reported for pattern masking. It can be shown, however, that none of these studies has included the requisite controls to ensure that semantic activation was not accompanied by conscious identification of the stimulus at the time of presentation. (3) On the basis of current evidence it is most likely that these stimuli were indeed consciously identified.

Keywords: attention; consciousness; dichotic listening; iconic memory; identification; masking; parafoveal vision; perceptual defense; semantic priming; shadowing; subliminal perception

This paper will be concerned mainly with simple tasks in which the stimuli are isolated spoken or written words (and occasionally pictures of single objects). It is assumed that when such stimuli are recognized meaning representations stored in semantic memory have been accessed through the processing of the sensory inputs. Once accessed, these representations are maintained in an activated state for a certain amount of time, a condition that will henceforth be referred to as *semantic activation*.

Semantic activation is often accompanied by a subjective experience, an awareness of the existence of a stimulus and of having recognized it; that is, the stimulus is *consciously identified*. Conscious identification can be indicated by overt behavior, for example, by naming the stimulus, discriminating it as familiar, categorizing it, pointing to a matching object, and so on. Any voluntary discriminative response that can be elicited on the basis of the meaning of a stimulus will be considered *direct evidence* of semantic activation. *Indirect evidence* of semantic activation may also be obtainable in the form of various kinds of influence (bias, facilitation, interference) that the processing of one stimulus can exert on the processing of another stimulus to which it is in some way related.

A prototype experiment illustrating all the basic concepts in this paper is provided by the well-documented semantic priming effect in the "lexical-decision task" (Meyer & Schvaneveldt 1971). This task consists of judging whether or not a visual string of letters is a word. In one version, two strings were successively presented, each requiring a speeded lexical decision (Meyer, Schvaneveldt & Ruddy 1975). The decision that the second string was a word was made more quickly if the first string was a word that was semantically associated with it rather than an unrelated word or a nonword. This semantic priming effect constitutes indirect evidence of semantic activation of the first word. In this particular experiment there was also direct evidence of semantic activation and thereby of conscious identification of that word since subjects also made a lexical decision in response to the first string.

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Marcel (1978; 1983a; Marcel & Patterson 1978) obtained basically the same result using the same task (except that no response was required to the first letter string). An unexpected finding was that the semantic priming effect would also show up when a very brief presentation of the priming word was immediately masked by another visual stimulus – a pattern mask – preventing not only conscious identification of the prime but even detection of its presence. This constitutes a canonical example of *semantic activation without conscious identification* (henceforth SA/CI), which is operationally defined as positive indirect evidence of activation (the semantic priming effect) together with negative direct evidence of identification (inability to make a voluntary discriminative response to the prime).

The concept of semantic activation is now pervasive in cognitive psychology (e.g., Anderson 1983; Collins & Loftus 1975; Keele 1973; Morton 1969; Neely, 1977; Posner & Snyder 1975a). The example above illustrates two of its properties. First, activation of the meaning of one particular word spreads to semantically related words. Second, activation is maintained for a certain period of time after stimulation. A further property is that the amount of activation is generally assumed to vary continuously between zero and some maximum. This property is sometimes invoked as an a posteriori rationalization of the intuitively unexpected semantic priming effect in the absence of conscious identification of the prime. One need only assume that the level of activation required to achieve priming is lower than that required for conscious identification.

There have, of course, long been claims of dissociability between the availability of the information to the processing system and its availability to conscious awareness. This has been the central thesis of the research on subliminal perception and perceptual defense (for reviews see Dixon 1971; 1981; Erdelyi 1974; Shevrin & Dickman 1980). From the outset, conventional wisdom in cognitive psychology has been very reluctant to acknowledge these lines of inquiry and to integrate them into current information-processing conceptions. Part of the skepticism was based on methodological flaws in early attempts to demonstrate the existence of subliminal perception (Eriksen 1960; Neisser 1967). Another reason for the continuing skepticism about subliminal phenomena may have been the absence of a theoretical framework; however, various recent attempts to account for perceptual defense/vigilance (Erdelyi 1974) or subliminal perception (Dixon 1971; Shevrin & Dickman 1980) with modern theories of attention have not exerted much influence on mainstream cognitive psychology. For a more general renewal of interest in SA/CI to arise, it appears that new findings had to be generated from the very same experimental paradigms currently used in information-processing research.

A first series of studies that did give rise to such a renewed interest was published in the early seventies (e.g., Corteen & Wood 1972; Lackner & Garrett 1972; Lewis 1970; Smith & Groen 1974). These studies were concerned with the fate of the semantic content of irrelevant words included in unattended messages presented to one ear while subjects were attending to different messages presented to the other ear – a condition referred to as *dichotic listening*. These studies showed that the meaning of unattended words (which the subjects were presumably unable to identify consciously) was nevertheless processed since it affected performance on the attended message.

Dichotic listening tasks have played an important role in the development of modern theories of selective attention. These theories differ regarding where in the time course of the processing of concurrent stimuli selection is supposed to occur. Early-selection theories hypothesize that only simple physical characteristics of the unattended stimuli (e.g., intensity, pitch, or spatial position) are processed before input selection occurs (e.g., Broadbent 1958). Late-selection theories hypothesize that the meaning of the unattended stimuli is also accessed prior to selection (e.g., Carr & Bacharach 1976; Deutsch & Deutsch 1963; Duncan 1980). Evidence for SA/CI with unattended words has sometimes been interpreted as supporting (Posner 1978; 1982; Posner & Snyder 1975a) or being compatible with late-selection theories (Duncan 1980). In the same vein, but less influential, was a series of studies showing that the meaning of unattended, not consciously identified, parafoveally presented words can affect the processing of foveally presented attended words (e.g., Bradshaw 1974; Underwood 1976; Willows & MacKinnon 1973).

The strongest impetus for the new concern with the phenomenon of SA/CI, however, was undoubtedly the demonstration of semantic priming from undetectable masked words (Marcel 1978; 1980; 1983a; Marcel & Patterson 1978) and related demonstrations by Allport (1977). These findings have been enthusiastically welcomed by many information-processing psychologists for several reasons. There is now a large body of research devoted to understanding the semantic priming effect. As outlined earlier, the activation of the representation of the meaning of the prime is supposed to spread to representations of the meanings of related words. This process is considered to be completely automatic, that is, unavoidable and not under the voluntary control of the subject. There is a second component of priming, however, that is under voluntary control; the subject may deliberately activate the meaning representations of some words because he expects them to be presented. Any semantic priming effect with consciously identifiable primes is supposed to reflect a mixture of these two components (e.g., Neely 1977; Posner & Snyder 1975a; 1975b). Unconscious priming therefore offers a means of studying the hypothesized automatic activation component of semantic priming isolated from confounding factors such as expectancies elicited by the priming word. Priming effects under severe pattern-masking conditions, if they are reliable and validly interpreted, should have far-reaching implications for the comprehension of the visual masking phenomenon (see Section 4) and, at a broader level, for our understanding of the relationships between conscious and unconscious mental representations (e.g., Marcel 1983b).

If it is true that we were once biased against accepting putative demonstrations of SA/CI because we lacked an adequate theoretical framework to account for such a possibility, we are now in the opposite situation. Theoretical constructs such as semantic activation or late-selection models of attention make the existence of the phenomenon plausible. As a matter of fact, the concept of unconscious priming has been promptly integrated into current conceptions of information processing by some authors (e.g., Allport 1980; Henderson 1982). Dixon, who has already gathered and reviewed evidence for the existence of subliminal perception from eight different bodies of research (Dixon 1971), has recently incorporated new evidence from pattern masking, parafoveal vision, and dichotic listening into an updated version of his book (Dixon 1981). It is my opinion that the pendulum has now swung too far in the direction of uncritical acceptance of the idea of SA/CI. Because of our new theoretical presuppositions we are now in danger of acknowledging as evidence for this hypothesis results that should be considered inconclusive on methodological grounds.

My contention is that most, if not all, claims for SA/CI in dichotic listening, parafoveal vision, and visual masking are in reality based on the failure of these experimental methods to reveal whether or not the meaning of the critical stimulus was available to consciousness at the time of presentation.

This paper will attempt to provide the reader with the arguments supporting this contention. Claims to demonstrate the SA/CI phenomenon will be evaluated at two levels. The first level is the more stringent one, involving an assessment of the extent to which the data satisfy a criterion specifying necessary and sufficient conditions for the existence of SA/CI (see Section 1). This amounts to asking whether, within the three bodies of research to be reviewed, a positive answer can be given to three questions: (1) Do existing experimental paradigms provide the requisite methodological conditions for meeting the criterion? (2) If they do, are there in fact data satisfying the criterion? (3) If existing data are equivocal, can methodological improvements be proposed? It will soon become apparent that few experiments are designed so as to yield a positive answer to question 1. This appears to leave two alternatives. One is to discard ambiguous data from the outset and to deal only with the handful of remaining results; however, there are some good reasons for not adopting this overexacting alternative. In the current early stage of development of cognitive psychology, it would be unproductive to adhere exclusively to very stringent criteria. Moreover, this rigor would not convince those who are ready to rely on less stringent criteria. A more fruitful alternative would be to assess the extent to which the equivocal results are compatible with what is already known in the field. This alternative is especially advisable because although dichotic listening, parafoveal vision, and visual masking have been extensively investigated, few studies have directly addressed the issue of SA/CI per se.

This paper is divided into five sections. Section 1 states the criteria for establishing the phenomenon of SA/CI. Sections 2, 3, and 4 survey and assess the data relevant to the issue in dichotic listening, parafoveal vision, and visual masking, respectively. Section 5 summarizes the main conclusions.

1. Establishing the phenomenon and methodological issues

Dixon (1971, p. 18) has proposed three criteria for establishing subliminal perception. Slightly modified in order to address the specific questions discussed in this paper, these criteria are (1) positive indirect evidence of semantic activation together with negative direct evidence of stimulus identification at the time of presentation; (2) positive indirect evidence of semantic activation together with inability to report the semantic content of the stimulus retrospectively; and (3) positive indirect evidence of semantic activation that is qualitatively different from what would be observed with conscious identification, assuming criterion 1 was met.

These criteria are neither equally powerful nor equally compelling. Two points should be discussed. One concerns the difference between criteria 1 and 2; the other makes criterion 3 explicit.

With respect to direct evidence of conscious identification, criteria 1 and 2 contrast the ability to *identify* some critical stimulus immediately at presentation with the ability to do so after a certain delay. Criterion 2 is often the only one available, especially in the dichotic listening studies that have been taken as evidence for SA/CI. In these cases, the critical stimulus is embedded in a long sequence of items, and then at the end of a trial (or even after a series of trials) subjects are asked whether or not they noticed something. Criterion 2 is therefore very weak, because when subjects fail to report a critical stimulus it is impossible to ascertain whether this is because of unavailability at the time of presentation or because of forgetting during the retention interval. One could argue that criterion 1 suffers from the same drawback, since a response to a stimulus never occurs instantaneously at presentation, and presumably our conscious awareness of the world is always the awareness of some recent past rather than that of the very precise current instant. None of this seems particularly disputable, and hence I can think of no way to distinguish between complete absence of conscious identification and conscious identification followed by such quick forgetting that no response can be elicited by the stimulus. At present we can hardly go beyond an operational definition that equates conscious identification with the ability to respond discriminatively to a stimulus at the time of presentation and, by default, to define the absence of conscious identification as the lack of this ability.

Semantic priming in a lexical-decision task will now be used again to specify criterion 3. Assume that there is a baseline condition in which a target stimulus requiring a lexical decision is presented alone and a priming condition in which the priming stimuli are semantically related or unrelated to the target; several qualitatively different patterns of results could then emerge, of which the following three are theoretically meaningful: (1) both a facilitative effect from related primes and a detrimental effect from unrelated primes; (2) only a facilitative effect from related primes and no effect from unrelated ones; (3) only a detrimental effect from unrelated primes and no effect from related ones. From qualitatively different experimental effects one generally infers distinct underlying processes. Assuming that qualitatively different effects are indeed observed in conditions supposed to lead to semantic activation with and without conscious identification, this fact does not in itself constitute evidence for SA/CI in one of the conditions because qualitatively different effects can be observed in the processing of consciously identifiable stimuli. For instance, Neely (1977) and Posner and Synder (1975a; 1975b) contrasted conditions in which either the first or the second pattern of priming effects just described was observed, but in none of their conditions was the identity of the primes unavailable for self-report. It follows that Dixon's criterion 3 should be considered a powerful but incidental corollary to criterion 1.

It follows that criterion 1 is the only essential one, to be taken as the necessary and sufficient condition for the existence of SA/CI. One of the central issues to be addressed in this paper concerns how criterion 1 can be implemented. It is clear that no special difficulty will be associated with gathering indirect evidence of semantic activation. As already pointed out, one can rely on all kinds of priming and associated methods to achieve this goal. The fundamental methodological problems pertain to establishing and controlling conditions that effectively prevent conscious identification of the critical stimulus at the time of presentation. A description of each of the three experimental paradigms according to the distinction proposed by Norman and Bobrow (1975; 1976) between data-limited and resource-limited processes offers a perspicuous way to characterize much of the methodological issue.

1.1. Data-limited and resource-limited processing

According to Norman and Bobrow, task performance depends on the quality of the data (e.g., the sensory quality of the input, the quality of the memory traces) that are processed and on the quantity of processing resources (e.g., processing effort, memory capacity) allocated to the processing operations. Resources are always limited, so if several tasks are performed concurrently, they must share the resources available. Norman and Bobrow introduce the further notion of the performance–resource function. Two regions can be distinguished in such a function. In one, performance improves as more resources are invested in the task. In this case performance (or processing) is said to be resource-limited. In the other region of the function, performance has reached its maximum and can no longer be improved by the allocation of more resources. In this case performance (or processing) is said to be data-limited. It is important to note that performance in the data-limited portion of the performance–resource function can reach *any* level, including perfect accuracy. (These concepts have been further developed by Navon and Gopher, 1979; see also Wickens, 1984a, for a very clear exposition.) Although doubts about its heuristic value have recently been raised by Navon (1984), this dichotomy still provides us with a convenient way to characterize the tasks to be analyzed in the rest of this paper.

It will be argued that in a dichotic listening task, performance on both messages is resource-limited. In parafoveal vision performance is basically data-limited and possibly resource-limited in certain circumstances. In visual masking, performance is data-limited. Since voluntary control of attention is a way of varying how much of one's resources one allocates to each of the concurrent tasks, the main methodological problems in dichotic listening (and to a lesser extent to parafoveal vision) are accordingly the following: (1) to determine whether focusing attention on the relevant stimuli is enough to prevent the semantic content of the irrelevant stimuli from being analyzed, and (2) to ensure that no redistribution of attention can occur during the task. In visual masking, the main difficulty is to ensure that the mask is efficient enough to prevent conscious identification, which is a typical problem of threshold determination.

One final distinction among the paradigms is worth mentioning. Although all three are assumed to prevent conscious identification of the meaning of the critical stimuli, they differ in the amount of nonsemantic information of which subjects are aware. In both dichotic listening and parafoveal vision, it is clear that subjects always know that concurrent irrelevant stimuli are being presented; moreover, subjects are able to identify some of their physical characteristics, such as intensity, pitch, color, size, and global shape. This is also the case for some visual properties under the less severe pattern-masking conditions. However, under the more stringent masking conditions, even the detection of whether or not something is presented before the mask can be prevented.

2. Dichotic listening

2.1. General features and findings

At any given moment, the number of events that occur simultaneously is too great to permit conscious analysis of each of them. It is possible, however, to attend selectively to one of the events, thereby becoming conscious of its meaning. From a theoretical point of view, the two most important questions are: What is the mechanism of selection and to what extent is the nonselected information processed? With competing auditory messages, it has been clearly shown that the subjective ease and objective efficiency of selecting one of the messages are directly related to the degree of physical difference between them. Furthermore, selection, although necessitated by resource limitation, is in itself a resource-demanding process (Johnston 1978; Johnston & Heinz 1978; 1979; Kahneman 1973). Spatial separation between the concurrent messages is certainly the most effective physical cue for attentional selection (Kahneman 1973). This is one of the reasons for the extensive use of dichotic presentation, which represents the most extreme form of spatial separation between auditory messages. It is achieved by the use of headphones that transmit one message to one ear only while a different message is simultaneously conveyed exclusively to the other ear. Each ear is thus used as a different channel of communication.

Two types of task have been studied: monitoring and shadowing. In *monitoring* tasks, subjects have to detect targets or recall items presented in one or both channels. Monitoring can be performed under a divided-attention condition, in which attention has to be paid to each message, or in a focused-attention condition. In the latter case full attention has to be paid to the *relevant message* occurring in the *primary channel* while the *irrelevant message* presented in the *secondary channel* has to be ignored. The shadowing task was devised by Cherry (1953). It consists of repeating a message word for word while listening to it. With competing messages, selective shadowing of one of them cannot be performed without paying considerable attention to it; shadowing is hence a means of inducing focused attention in dichotic listening.

The results of Moray and O'Brien (1967) are representative of performance in a dichotic listening task. Subjects monitored monaural or dichotic lists of digits to detect occasional presentations of letters. Performance in the monaural condition was better than overall performance in the divided- and focused-attention conditions, which did not differ. There was therefore what has been called by Navon and Gopher (1979) a "concurrent cost," which means that not all available resources can be voluntarily allocated to the processing of each message. Performance with the relevant message was relatively better and performance with the irrelevant message was relatively worse in the focused-attention condition than performance with either message in the divided-attention condition. Hence, subjects can voluntarily trade off some resources between channels, which implies that performance with each message is resource-limited in dichotic listening.

Is there evidence for SA/CI of irrelevant stimuli in selective listening to one of two dichotic messages? For reasons that will soon become evident, criterion 1 cannot be implemented in this paradigm. It is possible, however, to draw some conclusions by contrasting studies in which direct and indirect evidence for semantic activation has been collected independently.

The initial research of Cherry (1953) showed that subjects were aware only of physical aspects of the irrelevant message but not of its semantic content; language changes or reversed speech, for example, went unnoticed, according to subjects' reports after the experiment. Subsequent studies, however, indicated that the meaning of some stimuli presented in the irrelevant channel sometimes reaches consciousness. In an experiment by Moray (1959) instructions presented in the secondary channel were reported on 51% of the trials on which they were preceded by the subject's own name compared to 11% when the name of the subject was not mentioned. Treisman (1960) showed that on some occasions (6% of the trials) subjects were unable to avoid following the logical continuation of a story for a few words after each message was suddenly switched from one channel to the other. In a task in which the relevant and the irrelevant messages were identical but delayed in time, Treisman (1964) observed that subjects spontaneously noticed the sameness of the two messages when the delay was not too long (namely, a lag of one or two seconds) and the irrelevant message came first, even when it was a French translation of the relevant one. This evidence of access to the meaning of the irrelevant message came from incidental observations in situations in which subjects were asked to pay full attention to the relevant message and did not expect to be tested on the irrelevant one. There is therefore no a priori reason to expect voluntary shifts of attention toward the irrelevant message to occur; hence (provided attentional focusing can be consistently maintained) these data might well reflect semantic activation without attention.

At the other extreme, two studies showing substantial semantic analysis of the irrelevant message almost certainly reflect shifts of attention. Mowbray (1964) asked his subjects to shadow sequences of 50 words while attempting to remember a set of 1, 2, or 3 words presented in the secondary channel at various points during the trials. Shadowing of the relevant message had to be performed during the entire sequence, followed by recall of the set of words. Percentages of word recall ranged from 35% to 75% depending on the number of words in the set and on the duration of the interval between presentation and recall. However, the presentation of a word in the secondary channel, whether reported or not, was accompanied by a dramatic decrease in shadowing performance, indicating a shift of attention from one channel to the other. This is not surprising, because attention was attracted by the sudden presentation of the set of words in an otherwise silent secondary channel. Hence, the fair amount of semantic analysis of the secondary channel cannot be considered as occurring without attention. A similar conclusion applies to the data of Norman (1969), who also showed memory and shadowing decrements when items were discretely presented in the secondary channel.

Less easily interpretable are the results of a series of studies showing various amounts of semantic processing of the secondary channel that cannot be attributed to sudden shifts of attention induced by salient physical characteristics of irrelevant items. In the experiments of Treisman and Geffen (1967; 1968) targets appeared in both the primary and the secondary channels and subjects had to signal their detection by immediately tapping a ruler without ceasing to shadow efficiently. The detection rate for targets in the primary channel was 87% in the 1967 report and 91% in the 1968 report. The detection rate for targets in the secondary channel was only 8% in the first experiment and somewhat higher, 23%, in the second. In a further study in which subjects were required to stop shadowing as soon as they detected a target, Treisman and Riley (1969) found a still higher proportion of detections of targets in the secondary channel, 39%, compared with 70% (raw results, uncorrected for guessing) on the shadowed channel. In a similar condition Underwood and Moray (1971) reported 10% and 70% target detection in the irrelevant and relevant channels, respectively (pooled results of Experiments II and IIa in the no-noise condition). Still better performance is observed when the content of the primary channel need not be shadowed but only has to be monitored. Bookbinder and Osman (1979) reported 83% and 37% target detection in the primary and secondary channels, respectively. The corresponding results were 90% and 48% in the study of Underwood and Moray (1971), to be compared with the 70% and 10% just described for shadowing. Dennis (1977) also compared the detection rates for targets in the irrelevant message while subjects shadowed (36% detection) or monitored (57% detection) the relevant message. In the same vein, it has been shown that recall of the last item in the secondary channel (Peterson & Kroener 1964) or of the last item preceding a signal to stop shadowing (Glucksberg & Cowen 1970; Klapp & Lee 1974) ranged from 25% to 50%, which is quite similar to the detection levels in the experiments just reviewed. Recall performance rapidly declines for items presented earlier in the secondary message except in one case. There were considerably more recalls at points following unpredictable changes in the topic of the secondary message than at points without such changes (Yates & Thul 1979). This result held whether or not the new topic was related to the subject matter of the primary message.

One possible explanation for the semantic processing of the content of the secondary channel is that irrelevant items were in fact attended because subjects sometimes shift attention to sample the irrelevant message, or simply because they sometimes fail to maintain attentional focus. If this were the case, shadowing performance, which is itself resource-limited, should drop just before the appearance of reported irrelevant targets. Treisman and Geffen (1967; 1968) showed that this does not occur. In these experiments, an overall shadowing-error rate of close to 10% was observed. Shadowing errors were much more frequent (20% to 40%) within a range of three words before and five words after a target that was responded to, whereas targets that were not responded to did not exert any detrimental effect on performance. All these errors can be explained in terms of a conflict between the shadowing and the detection responses (including those related to the three words before a target, because the shadowing response always lags by about one second, which corresponds to two or three words). However, shadowing errors for words occurring in positions six to three before a target were not more frequent than anywhere else in the sequence, which clearly eliminates the possibility that momentary shifts of attention could account for target detection. The main point of interest lies in the fact that attention can be diverted from the primary channel even by events that have not been responded to. Dennis (1977) confirmed the analysis of Treisman and Geffen (1967; 1968) except that he observed concomitant impairments of shadowing performance with both targets that were and were not responded to, and Yates and Thul (1979) reported an impairment of shadowing contingent on topic changes in the irrelevant channel, which the subjects were not requested to report.

Three main conclusions can be drawn from the data reviewed so far. (1) Monitoring seems less resource-demanding than shadowing, releasing more resources for the processing of the irrelevant message (see also Kahneman 1970; 1973; 1975). (2) A salient physical event presented in the secondary channel can divert attention from the main task. (3) Semantic activation of irrelevant stimuli can divert attention in much the same way as attention-drawing physical events can. In all these conditions, semantic activation is often accompanied by conscious identification, as demonstrated by overt target detection or recall.

One question that cannot be answered on the basis of existing results is whether or not semantic analysis of the irrelevant message requires any attentional resource. The reason is that subjects are basically engaged in dual tasks in which they have to give a higher priority to one task than to the other. The high-priority task for which a high level of performance is required consists of shadowing or monitoring the relevant message. The low-priority task, which cannot be neglected completely, is detecting targets in the irrelevant message or recalling some material presented in it. Since both tasks are resource-limited, each performance depends on the particular trade-off between the quantity of resources allocated to each task. It is unlikely that no resources at all are allocated to the low-priority task. The only way to know how resources are allocated is to study the performance trade-off by varying the relative emphasis put on each task. This would indicate how much semantic processing of the irrelevant message is possible under the most extreme conditions of voluntary resource allocation to the processing of the relevant message. Unfortunately, no such study is available, so it is impossible to determine whether such a limit in the focusing of attention was indeed reached in the experiments just reviewed. The importance of this point can be seen from the results reported by Bookbinder and Osman (1979). The high-priority task was to monitor the primary channel for color words, and the low-priority task was to monitor both channels to detect a single target word. Overall, the responses for color detection on the primary channel were 82% correct and for target detection on the secondary channel 37% correct. Relative to these overall mean performance levels, subjects who scored high on the high-priority task scored low on the low-priority one, 90% and 13%, respectively; conversely, subjects who scored low on the high-priority task scored high on the low-priority one, 74% and 52%, respectively.

One way to avoid this trade-off in the allocation of resources between channels would be to eliminate the need to perform any task at all on the secondary channel. This is indeed what has been done in most of the studies that have claimed to show SA/CI: They have relied on indirect evidence of processing the meaning of irrelevant items without looking for direct evidence of it at the time of presentation. Hence, the converging operations needed to meet criterion 1 being unavailable, the proposed evidence for SA/CI relies on the absence of retrospective report for the content of the secondary channel (criterion 2), which is hardly convincing. Before discussing this point any further, however, let us analyze the relevant data to ascertain whether, with attention restricted to the primary channel, there is any reliable indirect evidence for semantic activation of irrelevant stimuli at all.

2.2. Semantic activation without conscious identification of the irrelevant message

Claims for the existence of SA/CI have been based on the results obtained in four different experimental situations. In three of them, indirect evidence for the processing of the meaning of the irrelevant message has been derived from its influence on (1) memory for relevant items; (2) shadowing latencies for relevant items; and (3) biasing of the interpretation of ambiguous relevant information. A fourth indirect test for the processing of the secondary message is provided by studies in which conditioned electrodermal responses to irrelevant items are monitored. In all these situations, subjects were required to pay full attention to the primary channel and, except in two cases (Corteen & Dunn 1974; Dawson & Schell 1982), they were never asked to monitor or signal anything about the content of the secondary channel. Only the last two lines of evidence call for detailed analysis, because they have often been accepted uncritically. First, the first two lines of evidence will be dealt with succinctly, since they have played a minor role and their interpretation is more straightforward.

Concerning the influence of irrelevant items on memory for relevant ones, four studies are based on the idea that if items in the secondary channel are in fact fully processed, they should be represented in primary memory to the same extent as relevant items. Hence, memory for an n-item list should show the characteristics of an n-item list if it is presented monaurally and those of a $2n$-item list if it is presented as a member of a dichotic pair of n-item lists. Davis and Smith (1972) used the probed-recall task devised by Waugh and Norman (1965) in which a list of words is followed by one of its members and the subject has to recall the word that followed the probe in the list. Smith and Burrows (1974), Smith and Groen (1974), and Traub and Geffen (1979) used the memory-scanning paradigm of Sternberg (1966) in which a probed word presented after a list required a speeded classification response as to whether or not it appeared in the list. The results were clear-cut: none implied that the items in the secondary channel were scanned together with the attended ones; all implied that some semantic processing of the unattended items did take place. This does not come as a surprise since the relevant message only had to be monitored, which is less resource-demanding than shadowing and often leaves enough spare resources for processing the irrelevant items (see Section 2.1). Given this, there is no logical reason to think that the meaning of these processed words was not available to consciousness.

As regards the influence of irrelevant stimuli on the latencies of the shadowing responses to the relevant stimuli, the study of Lewis (1970) is often cited as evidence for unconscious semantic activation. He showed that the latency of the shadowing response to a word in the primary channel was increased by the simultaneous presentation of a synonym in the secondary channel. However, in a follow-up study by Treisman, Squire, and Green (1974) it was shown that the effect occurs only at the beginning of a list of ten dichotic pairs (position 3), not at the end (position 7). Treisman et al. (1974) interpreted their results as showing that subjects were unable to reach a full state of focused attention right from the beginning of shadowing, which allows enough division of attention for irrelevant words occasionally to be processed. Later in the list, when attention focusing becomes more efficient, semantic processing of the irrelevant message no longer occurs. Again, the effect is explained by attentive processing, presumably leading to conscious identification of the stimulus.

A further positive result confirms that no firm conclusion can be reached unless attention deployment is carefully controlled and evaluated. Underwood (1977a) used dichotic messages from 5 to 11 words in length. One of the messages had to be shadowed, and the shadowing latency of the last word was measured. The baseline condition consisted of two strings of random words. In three experimental conditions a contextual sentence was presented in the primary channel, and in three other conditions the contextual sentence was presented in the secondary channel. The last word of this sentence was always presented in the primary channel, and its shadowing latency was measured. The amount of contextual information was varied by presenting the contextual sentence in its complete version or with the first third or the first two thirds of its words replaced by strings of random words. Shadowing latencies for the last word of the primary channel were found to be considerably shortened by presenting contextual sentences in either channel. The improvement was greater with the sentence in the primary than in the secondary channel, however, and there was increasing facilitation as a function of the amount of context in the primary channel. This last effect did not occur with context in the secondary channel. Hence, resource-allocation constraints were such that the full meaning of the irrelevant sentence could not be processed but the meaning of individual irrelevant words could sometimes be analyzed.

2.2.1. Effect of biasing information in the irrelevant message on the interpretation of ambiguous relevant messages. Three studies have examined the potential disambiguating role of information presented in the secondary channel for the interpretation of ambiguous sentences presented in the primary channel. The procedure always involved prior evaluation of the frequencies of each interpretation in the absence of context, so that any bias toward the interpretation suggested by the irrelevant information could be evaluated.

Lackner and Garrett (1972) presented their subjects with an ambiguous sentence in the primary channel and a simultaneous disambiguating sentence in the secondary channel. The subjects were instructed to pay attention only to the primary channel and to begin paraphrasing the ambiguous sentence before it ended. Four different kinds of ambiguity were studied: lexical, particle-prepositional, surface-structural, and deep-structural. Relative to a control condition in which the irrelevant sentences were neutral, the frequency of each interpretation of the ambiguous sentence was always increased in the direction suggested by the disambiguating sentence presented in the secondary channel, although not significantly so in the case of surface-structural ambiguity.

Although there was no measure of subjects' true deployment of attention in this study, there are at least three reasons for assuming that processing of the irrelevant message did not take place without attention. First, subjects started paraphrasing around the end of short sentences, which implies that, for nearly the entire duration of the competing sentences, they were engaged in a monitoring task without simultaneous emission of verbal responses. This is a situation in which selective attention to one message has typically been shown to be far from perfect (see Section 2.1), which may be the reason that one group of subjects used in a pilot study failed to ignore the irrelevant message. Second, disambiguation occurred even when the disambiguating portion of the irrelevant sentence followed the ambiguous portion of the relevant sentence, or when disambiguation was spread throughout the entire irrelevant sentence. This implies that complete syntactic and semantic analysis of the irrelevant sentence took place, an impossible achievement with attention more stringently locked to the primary channel, as demonstrated by Underwood (1977a). Moreover, the product of this analysis had to be integrated with the analysis of the relevant message in order to yield a particular interpretation of it. How could this be achieved without paying attention to both messages? Third, results reported by MacKay (1973) and Newstead and Dennis (1979) (to be analyzed below) shed more light on the problem of distribution of attention and are compatible with the present interpretation.

MacKay (1973) used a procedure similar to the one just described. Control of the allocation of attention was slightly better in the present case because subjects were required to shadow the ambiguous sentences presented through the primary channel. The secondary channel, which was to be ignored, was silent except for one or two disambiguating words presented simultaneously with the ambiguous portion of the sentence. Lists of up to 28 experimental sentences were followed by a recognition test. Corresponding to each experimental sentence, subjects had to choose which of two paraphrases was closer in meaning to the sentence heard originally. MacKay observed a strong bias toward the interpretation suggested by the disambiguating words presented in the secondary channel in the cases of lexical and surface-structural ambiguities and no effect in the case of deep-structural ambiguity. Unfortunately, these results are flawed by the isolated presentation of the disambiguating words in the secondary channel, a sudden physical event that is known to attract attention, as discussed in Section 2.1. That this was indeed what was happening was demonstrated by Newstead and Dennis (1979). They got the same results as MacKay in an exact replication of MacKay's procedure. However, when the disambiguating words presented on the secondary channel were embedded in a sentence the disambiguating effect no longer occurred.

Johnston and Wilson (1980) performed one of the few experiments in which one can compare the biasing effect of a nontarget in both a focused and a divided-attention version of the same basic task. In their Experiment 3, pairs, of simultaneous words were dichotically presented at a rate of 1.5 pairs per second. The task of the subjects was to detect target words belonging to a predesignated target category (e.g., body part). Each target was then a homonym, with only one of its meanings congruent with the target category (e.g., *head*). Each target was paired with either a neutral word (*time*), an appropriate nontarget word (*bald*), or an inappropriate nontarget word (*leader*). "Appropriate" thus means here that the nontarget word was related to the meaning of the homonym compatible with the target category, and "inappropriate" means that the nontarget word was related to a different meaningof the target word. In the divided-attention condition, target target words were distributed between the two channels, and so was the attention of the subject. In this condition, appropriate nontargets improved target-detection performance, relative to the control condition, and inappropriate nontargets worsened it. In the focused-attention condition, targets appeared only in the primary channel, and nontargets were confined to the secondary one. In this condition, the nature of the nontarget in no way affected detection performance for targets.

A somewhat similar procedure was used by Johnston and Dark

(1982). Subjects had to perform two tasks. The first task was designed to force subjects either to focus or to divide attention in a dichotic listening situation. It consisted of detecting predesignated target words embedded in a long sequence of dichotic pairs presented at a rate of 1.5 pairs per second. Subjects signaled detection by naming the target word. In the divided-attention condition, targets could appear in both messages, whereas in the focused-attention condition, targets were confined to the relevant message. Performance in this task was used to assess whether subjects were dividing or focusing attention as instructed. The second task was the main object of interest. Subjects had to watch a screen for the appearance of words that were polysemous homographs (e.g., *bank*). Homographs were discretely presented one at a time at irregular intervals while the dichotic sequence was continuously played. These homographs never occurred in close temporal proximity to targets in the first task and bore no relation to these targets. The task of the subject was to say a word semantically related to the homograph and then immediately resume monitoring for both tasks. The question was whether auditory priming words whose meaning was associated with one of the meanings of the homograph would bias the associative responses to the homographs toward that meaning. Two priming words, one 1 sec before and the other 0.5 sec after the homograph, were presented in each channel. A weak priming effect was observed in the divided-attention condition. That is, the free-associative response was more often related to the primed meaning than to the other meaning of the homograph. This effect was enhanced when priming words were presented in the primary channel in the focused-attention condition. In this condition, no trace of a priming effect was observed when the priming words were presented in the secondary channel. It should also be pointed out that the better priming effect observed in the focused as opposed to the divided-attention condition parallels the better target-detection performance generally observed in the former than in the latter condition (Moray & O'Brien 1967; Ninio & Kahneman 1974).

In conclusion, when the allocation of attention is loosely controlled (Lackner & Garrett 1972), or when attention can be attracted by events in the secondary channel (MacKay 1973; Newstead & Dennis 1979), there is clear evidence that the semantic content of the information presented in the secondary channel can bias the interpretation of ambiguous information presented in the primary channel. When attention deployment is better controlled, no such effect occurs (Johnston & Dark 1982; Johnston & Wilson 1980; Newstead & Dennis 1979).

2.2.2. Electrodermal responses to shock-associated words in an irrelevant message. Claims for the existence of SA/CI for the stimuli presented in the secondary channel have probably been raised with more vigor by experimenters using electrodermal responses (EDRs) to shock-associated words than by workers using any other paradigm. A first impetus for this kind of research came from a pilot study described by Moray (1969) in his book on attention. The technique consisted simply of a learning phase during which subjects were told that some words that had been indicated to them before the learning phase started were going to be paired with unpleasant but harmless electric shocks. For those subjects who became conditioned, very few trials were needed in order to obtain a conditioned EDR, which was relatively resistant to extinction when the shock was no longer associated with the word. During the subsequent experimental phase the shock-associated word was embedded in the message presented in the secondary channel in a dichotic listening task in which the relevant message was shadowed. The main point of interest is whether EDRs still occur in this condition and whether they will generalize to semantically related words that have never been associated with shocks. This last test is needed in order to assess whether the semantic properties, and not simply the physical properties, of the shock-associated word are the ones responsible for any EDRs evoked by words in the irrelevant message.

Before looking at the results, it is necessary to point out that an EDR is not necessarily an easy response to elicit. In the initial report by Moray (1969) only 12 subjects out of 30 showed conditioned EDRs. Corteen and Wood (1972) later confessed that one third of their subjects failed to show EDRs. Wardlaw and Kroll (1976) found only 18 subjects out of 75 who were properly conditioned. Conditioning was achieved by 10 subjects out of 12 and by 30 out of 44 in the Forster and Govier (1978) and Von Wright, Anderson, and Stenman (1975) studies, respectively. A second preliminary point is that semantic generalization of EDRs may also prove to be elusive (Feather 1965). Despite these facts, five studies out of six have shown EDRs to both shock-associated words and to some other words semantically associated with them.

Corteen (Corteen & Dunn 1974; Corteen & Wood 1972) found a substantial number of EDRs both to shock-associated words and to words belonging to the same semantic category when these items were embedded in the irrelevant message in a shadowing task. Wardlaw and Kroll (1976) completely failed to replicate these results using exactly the same design, but this failure could be due to an inappropriate learning phase. The reason Wardlaw and Kroll (1976) were unable to condition most of their subjects is unclear (see Forster & Govier, 1978, for a hint of an explanation), but, in view of this fact, and also in view of the rapid extinction of the responses in the few subjects who did finally achieve conditioned EDRs, these results are completely inconclusive. Two other reports showed EDRs elicited by a shock-associated word (Forster & Govier 1978; Von Wright et al. 1975) as well as by synonyms and homonyms of shock-associated words. These results all suggest that stimuli presented in the secondary channel are semantically processed.

It has also been claimed that this processing does not give rise to conscious identification of the targets. This belief is based on the fulfillment of criterion 2, that is, subjects reported virtually no awareness of the presence of shock-associated words in the secondary channel when interviewed after the end of the experiments (Corteen & Wood 1972; Forster & Govier 1978; Von Wright et al. 1975). In order to circumvent the weakness of criterion 2, Corteen and Dunn (1974) intended to fulfill criterion 1 by asking subjects to signal shock-associated words by pressing a buzzer. They further argue for the unconscious identification of these words because there was only 1 signaled word out of 114 opportunities. This is not a very convincing argument for lack of conscious identification, since subjects were engaged in a double-bind situation: they had to ignore the secondary message and also to demonstrate that they sometimes did not ignore it. In such circumstances lack of report probably says more about the way subjects resolve the conflict than about anything else.

Dawson and Schell (1982) have performed what is probably the best-controlled experiment in this group of studies. First they obtained very good conditioning of the EDR to two target stimuli. Then they observed marked EDRs both to shock-associated words and to semantically related words. The strongest effect was obtained with the group of subjects tested in the primary channel. Substantial effects were also observed with the two groups tested in the secondary channel. In addition, three different ways of assessing a subject's awareness of the presentation of the target were used. The first was a subsequent report of the presence of target words, the second was a key press similar to the one used by Corteen and Dunn (1974), and the third was a moment-to-moment monitoring of the shadowing latencies. Although the first two criteria could be considered unreliable when few verbal reports are made, the opposite is not true. Seventy percent of the subjects reported that they had heard at least one target word in the secondary channel; forth percent of the subjects in the key-pressing group pressed the key at least once when a target was presented. Finally, the last criterion, by far the most reliable, showed that the probability of a shadowing error occurring concurrently with or within two words following presentation of the shock-associated words was 0.19. This is quite substantial compared with a probability of shadowing error of only 0.05 with words also presented during the conditioning phase but never associated with shocks, and an overall shadowing error rate of only 0.025. Taken together, these three criteria allowed for a satisfactory assessment of trials during which subjects were aware of the presentation of the target words. Post hoc analysis made the important point that almost all EDRs occurred while subjects were aware of the presentation of a shock-associated word and virtually no EDR occurred without awareness of the presence of a shock-associated word in the secondary channel. However, a further analysis of trials in which no shift of attention was observed led to a somewhat puzzling but very interesting result. For these trials, a slight but significant number of EDRs were observed with subjects who heard the relevant message in the right ear and the irrelevant message in the left ear. There was no trace of EDR with subjects who got the opposite arrangement of relevant and irrelevant messages. At present, much caution is still needed in the evaluation of the implications of this incidental result because, as was mentioned by Walker and Ceci (1983), it might be an artifact of the recording of EDRs only from the left hand. Although this possibility has been dismissed by Dawson and Schell (1983), a replication of the results with balanced recording of EDRs over the left and right sides would be worthwhile in any case. Notice also that the left-side advantage for irrelevant stimuli is consistent with the results reported by Treisman and Geffen (1967) but not with two subsequent reports by Treisman (Treisman & Geffen 1968; Treisman & Riley 1969) in which a right-side advantage was found.

In summary, conditioned EDRs to shock-associated words presented in the secondary channel while subjects are shadowing the message presented in the primary channel have consistently been observed. Unfortunately, in most of the studies reviewed there is no satisfactory independent assessment of attention deployment (Corteen & Dunn 1974; Corteen & Wood 1972; Forster & Govier 1978; Von Wright et al. 1975). In the only study providing such an assessment (Dawson & Schell 1982), EDRs are found to be practically always confined to words that have received some attention. There is, however, a small subset of EDRs for which no shift of attention has been detected. This occurs only with subjects who have received the irrelevant message in the left ear and the relevant message in the right ear, not with subjects who got the opposite arrangement of messages. This observation deserves further investigation for at least three reasons. First, it is the only candidate instance of semantic processing without attention in the dichotic literature. Second, the stronger positive results reported by other investigators using EDRs were all observed when the irrelevant message was presented to the left ear of each subject (Corteen & Dunn 1974; Corteen & Wood 1972; Forster & Govier 1978). This point needs to be qualified because the possibility of confounding the results by recording EDRs only from the left side, raised by Walker and Ceci (1983) in the case of the study of Dawson and Schell (1982), applies to the results of Corteen (Corteen & Dunn 1974; Corteen & Wood 1972) as well but not to those of Forster and Govier (1978), who recorded the EDRs from the right side. If valid, this asymmetry offers interesting neuropsychological perspectives on the study of the linguistic capabilities of each hemisphere (see, e.g., Searleman, 1977, for a review) and even properties of awareness in each hemisphere (e.g., Puccetti 1981) in normal subjects.

2.3. Conclusions

Section 2.1 showed that conscious identification of targets presented in the irrelevant message determines concomitant decreases in shadowing performance. Whether semantic activation takes place because attention is shifted from the primary channel or attention is diverted because semantic activation occurs is immaterial to the present issue. What matters is that reallocation of attention allows conscious identification. Hence, to demonstrate that SA/CI of the content of the secondary channel is possible, a preliminary requirement would be to find evidence of SA/CI without attentional shift. This could be achieved by training subjects to shadow continuous messages at a high speed and by measuring the moment-to-moment variations in shadowing performance.

Except in one case (Dawson & Schell 1982), no such attempt has been made in the studies adduced as evidence for unconscious semantic activation of stimuli presented in the secondary channel. Dawson and Schell (1982) showed that evidence of semantic activation was almost confined to the cases where attention was diverted from the primary channel, with the possible exception of a small subset of attention-free effects of the meaning of irrelevant words presented in the left ear. As regards the other studies analyzed in Section 2.2, one can infer on the basis of the data reviewed in Section 2.1 that in most of the cases, either enough processing resources were available and indirect evidence of semantic activation was present, or resources were scarcer and influences from items in the secondary channel no longer appeared.

Should semantic activation without attentional shift be consistently observed (which is quite unlikely on the basis of what is generally found in dichotic listening), one should next attempt to implement the second requirement of criterion 1, namely, one should demonstrate that no conscious identification took place at the time of presentation of the critical stimulus. This cannot be accomplished without modifying the attentional characteristics of the situation unless one can find an index of conscious identification that is different from a purposive discriminative response of the subject. Requiring the content of the secondary channel to be reported surely directs some attention to it (i.e., allocates more resources), a design double bind when what is supposed to prevent conscious identification is precisely the scarcity of resources.

It can therefore be concluded that dichotic listening tasks are simply ill-suited for implementing the converging operations needed to fulfill criterion 1. Although indirect evidence for semantic activation of items in the irrelevant message could conceivably be obtained with extreme resource demands for the processing of the relevant message, whether or not these items are consciously identified would remain outside the range of investigation. A similar conclusion applies to a series of results showing that at the time of presentation of a polysemous word several of the meanings are momentarily activated (e.g., Conrad 1974) but that shortly after that only the meaning compatible with the context is still available, the other meanings being deactivated (Onifer & Swinney 1981; Pynte, Dô & Scampa 1984; Swinney 1979; 1982), Here, too, priming methods are used to provide indirect evidence of semantic activation. It is unwarranted, however, to infer from these results that the initial activation of multiple meanings is unconscious, because, as in dichotic listening, conscious awareness cannot be assessed without biasing the task in a confounding manner.

3. Parafoveal vision

3.1. General features and findings

This section will be concerned with a region of the visual field that extends up to 7 degrees from fixation. Although visual acuity is a continuously decreasing function of the distance from fixation (e.g., Anstis 1974), it has become traditional to distinguish three functional regions in the visual field: the foveal, parafoveal, and peripheral regions. The boundaries of the foveal and parafoveal regions are generally set at 1 and 5 degrees from fixation, respectively (e.g., Bouma 1978; Rayner 1978). In most of the experiments to be reviewed in this section, the relevant stimulus is presented at fixation, therefore falling in the foveal region. The irrelevant stimulus generally falls in the parafoveal region, or a little beyond the parafoveal boundary in a few cases. [*Ed. note*: The foregoing passage was revised slightly to incorporate a correction suggested by commentator K. Rayner.]

The main limitation in the processing of a parafoveal stimulus is decreasing visual acuity with distance from the fovea. The percentage of correct identifications of normally typewritten words (3 or 4 letters per degree of

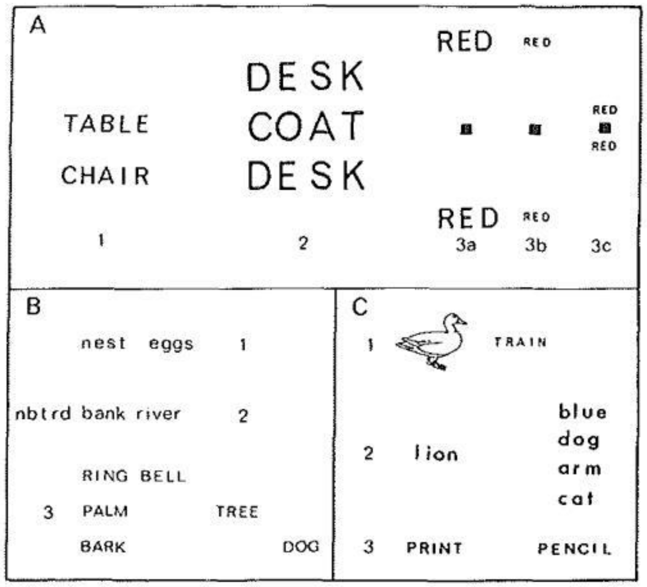

Figure 1. Approximate reproductions of displays used to investigate semantic activation from the parafoveal region of the visual field. A common scale has been used for all displays. At a distance of 23 cm, 0.40 cm subtends 1 degree of visual angle, which corresponds to three character spaces in the displays represented in panel B. Panel A: 1. Dallas and Merikle (1976); 2. Shaffer and LaBerge (1979); 3a, 3b, 3c. Merikle & Gorewich (1979). Panel B: 1. Paap & Newsome (1981); 2. Bradshaw (1974); 3. Inhoff (1982; Inhoff & Rayner 1980). Panel C: 1. Underwood (1976); 2. Underwood (1981); 3. Underwood and Thwaites (1982).

visual angle) drops from nearly 100% at fixation to less than 20% at a distance of 4 or 5 degrees from the fovea (Bouma 1973; Inhoff 1982; Inhoff & Rayner 1980). However, decreasing acuity can be compensated for by increasing the size of the stimuli (Anstis 1974). When stimuli are big enough to be discriminated almost perfectly, reduced parafoveal acuity can still be demonstrated in response latency. Response latencies have been shown to increase with eccentricity within a range of 5 degrees from the fovea for letter naming (Eriksen & Schultz 1978), letter comparison (Lefton & Haber 1974), word naming (Rayner & Morrison 1981; Schiepers 1980), and lexical decision (Rayner & Morrison 1981). It has even been shown that the time needed to name long words (7 to 11 letters) depends on the position of fixation within the words (O'Regan, Lévy-Schöen, Pynte & Brugaillère 1984). As far as complex stimuli such as words are concerned, there is no detailed study of the way different visual parameters interact to determine performance in parafoveal vision. Since size and eccentricity obviously play important roles, Figure 1 has been designed to give correct or approximate examples, according to the accuracy of available descriptions, of most of the displays used in studies relevant to the present discussion. The same scale is used in the reproduction of each display so that their relative sizes can be compared. The actual visual angles subtended by these displays in the experiments can be obtained by holding the book 23 cm from the eyes. In order to prevent eye movements from bringing parafoveal information to the fovea, exposure of the displays never exceeds 200 msec and is often much shorter than that. Display luminance is almost never reported but must be assumed to be well above threshold and to provide comfortable viewing conditions.

Figure 1A represents displays used in studies in which subjects were unable to avoid processing the irrelevant parafoveal stimuli while making a speeded response to the central stimuli. Figure 1A–1 shows one display used by Dallas and Merikle (1976) in which the word to be named was precued by two lines presented on each side of the spatial position that would be occupied by the relevant word. The cuing lines appeared 250 msec before the pair of words. Figure 1A–2 provides an example of the displays used by Shaffer and LaBerge (1979) in a binary classification of a central word belonging to one of four semantic categories, with two categories mapped into each response. Figure 1A–3 illustrates three of the displays used by Merikle and Gorewich (1979) in a modified version of the Stroop task in which the central color patch had to be named. The same task was used by Gatti and Egeth (1978) with words twice as big as those of Figure 1A–3a. Table 1 provides the mean reaction times to the foveal target as a function of the relationship between the parafoveal and foveal information for each study and also as a function of the eccentricity and the size of the parafoveal stimulus when they vary.

Two points should be stressed. First, subjects cannot help processing the irrelevant information along with the relevant information even if it hinders performance on the main task. This is clearly the case when stimuli are big enough to compensate for the reduced peripheral acuity (Dallas & Merikle 1976; Gatti & Egeth 1978; Shaffer & LaBerge 1979). Although weak, the 19- and 14-msec semantic relatedness effects reported by Dallas & Merikle (1976) in a naming task are in the same range as those obtained with successive foveal presentations of the words, namely, 30 and 19 msec in the experiments of Meyer et al. (1975, Experiment 3) and of Sperber, McCauley, Ragain, and Weil (1979, Experiment 2), respectively. Second, it is clear that increasing the distance of the parafoveal words can reduce or suppress the Stroop effect (Gatti & Egeth 1978; Merikle & Gorewich 1979) but that simultaneously increasing the size of these words can restore it (Merikle & Gorewich 1979). Similar failures of spatial selectivity have also been shown in letter-classification tasks (e.g., Eriksen & Eriksen 1974; Taylor 1977).

With displays providing visual conditions close to those encountered in reading normal text at a normal distance (Figure 1B–1) Paap and Newsome (1981) failed to observe any semantic priming effect from the unattended parafoveal word. In their first experiment, they established a baseline priming effect by presenting both the prime and the target foveally, each requiring a lexical decision. The prime was presented for 133 msec and preceded the target by 633 msec. A substantial semantic priming effect was found. In the second experiment the prime was displaced 1.2 degrees from fixation and presented along with a central unrelated word requiring a lexical decision; the target was then presented foveally 633 msec later as in the first experiment. No semantic priming effect was observed, suggesting that no semantic analysis of the prime had taken place. It is tempting to interpret this absence of a priming effect from parafoveal words, which

Table 1. *Mean reaction time (msec) to foveal targets as a function of the type of parafoveal information*

Authors	Study		Type of irrelevant stimulus: Same response — Identical	Same response — Same category	Same response — Diff. category	Diff. response	Neutral
Shaffer & LaBerge 1979	Exp. 1		613	626	666	669	—
	Exp. 2		572	575	595	618	587
	Study			Associate	Nonassociate	Nonword	
Dallas & Merikle 1976	Exp. 1			478	497	498	
	Exp. 2			545	559	—	
	Eccentricity[b]	Letter[c] height		Compatible	Neutral	Incompatible	
Gatti & Egeth 1978[a]	2.2°	1.2°		567	587	670	
	4.2°	1.2°		568	586	637	
	6.2°	1.2°		569	575	610	
Merikle & Gorewich 1979[a]	0.5°	0.24°		—	387	442	
	0.5°	0.57°		—	397	469	
	2.5°	0.24°		—	378	380	
	2.5°	0.57°		—	385	423	

[a]Data estimated from a graph. [b]Vertical center-to-center distance between the color patch and the color word in degrees of visual angle. [c]Visual angle in degrees.

is in sharp contrast with most of the results reported so far, as resulting from the difference in size between the parafoveal words used in each group of studies. In my opinion, the same factor can also account for the discrepancy between the results of two further studies concerned with parafoveal semantic information processing.

Underwood, Whitfield, and Winfield (1982) had their subjects listen to an incomplete sentence, the last word of which was visually presented at the fixation point. Another word was also displayed parafoveally, either to the left or to the right of the central word. Naming a central word congruent with the sentence context was facilitated by the presence of another congruent word to the right of the target. Naming a central word noncongruent with the sentence context was slowed down by the congruent right parafoveal word. The exact typography of the words was not reported, but the displays were probably close to that depicted in Figure 1C–3.

Stanovich and West (1983) used a somewhat similar procedure, except that their subjects had to read the sentence context instead of listening to it. The two end words of the sentence context were missing; they were displayed after the sentence had been switched off, when subjects read the last available context word aloud. The two end words consisted of a modifier and a common noun. The modifier was to be named as fast as possible. There was no evidence that the processing of the modifier was affected by the semantic content of the word presented to the right of it. The display size was very small, a three-letter word subtending 0.43 degrees of visual angle.

In the same vein, the idea that in reading the information processed during one fixation is integrated with the information (including semantic aspects) processed from the right periphery during the preceding fixation (e.g., Rayner 1978) has been challenged by McConkie, Zola, Blanchard, and Wolverton (1982). Furthermore, Rayner and Bertera (1979) made their subjects read from the parafovea by suppressing foveal information with a mask that moved in synchrony with eye movements. Reading performance was severely impaired even with a mask of only five character spaces. Taken together, all these results point to the conclusion that during the reading of text of normal size at normal reading distance, foveal semantic information processing is affected neither by the meaning of concurrent parafoveal words nor by the meaning of preceding parafoveal words (see McConkie, 1983, and Rayner, 1984, for recent reappraisals of this issue).

The picture that emerges from the data just reviewed is that within a region extending a few degrees around fixation characteristics of attention are opposite to those observed in dichotic listening. Concurrent identification of both the relevant and the irrelevant stimulus is easy and even unavoidable; selection is difficult and even impossible unless the discriminability of the irrelevant stimulus is very low. The task devised by Stroop (1935) and the derived forms discussed above once appeared to be prototypic demonstrations of automatic information processing, automaticity being defined as the lack of voluntary control and the resource-free aspects of processing (e.g., Keele 1973; Laberge & Samuels 1974; Posner & Snyder 1975a; Shiffrin & Schneider 1977). This view must now be revised because of new findings, among which the following two are good illustrations. (1) Kahneman & Chajczyk (1983) have shown a reduction in the influence of one parafoveal color name on the time to name a central color patch if another word is presented simultaneously on the other side of the patch. (2) The time to name a word is slowed down by the presence of other elements in the visual field (Kahneman, Treisman & Burkell 1983; Treisman, Kahneman & Burkell 1983). These results were observed with spatial positional uncertainty about the interfering or target words. These findings, and other related ones, have led to a revised view of automatic processing as involuntary but not necessarily resource-independent (see Kahneman & Treisman 1984; Laberge 1981; Schneider, Dumais & Shiffrin 1984). The new point worth emphasizing is that irrelevant stimuli can spontaneously and unavoidably divert a portion of the resources needed to process the relevant stimulus.

The extent to which visual semantic processing can be influenced by voluntary allocation of resources is much less well documented. For example, is it possible to suppress the processing of foveal information by paying more attention to parafoveal information? Some attentional suppression of foveal information has indeed been observed by Wolford and Morrison (1980) but not by Keren, O'Hara, and Skelton (1977). Sperling and Melchner (1978a; 1978b) have shown that subjects are able to trade off their digit-detection performance between two concentric squares of distracting letters. Kahneman and Henik (1981) briefly presented a circle and a square on either side of the fixation point. A colored word was printed in each shape. One word was a color name, and the other was neutral with respect to color. The relative positions of the circle and the square were randomly varied from trial to trial. Subjects were required to name the color of the word printed in one of the shapes, say, the circle. The Stroop effect was substantially greater when the distracting color name was in the circle than when it appeared in the square, even though the discriminability of the color name was the same in both conditions.

This analysis of the performance within a few degrees from the fovea suggests the following tentative description of concurrent foveal and parafoveal processing: Conditions are such that the identification of the parafoveal words is sometimes possible, sometimes not. Hence, conscious identification should be assessed on each trial. I assume that without spatial uncertainty, performance for both stimuli is generally data-limited. In such a case, there should be no mutual interference between the tasks used to assess direct and indirect evidence of semantic activation. Hence, the conditions needed to fulfill criterion 1 should be implemented. In certain circumstances, such as with spatial uncertainty about the stimuli or with poor discriminability of the foveal stimulus, the capacity of the processing system might become overloaded so that performance on one or both stimuli becomes resource-limited. In these cases, the assessment of criterion 1 faces the same kinds of difficulty encountered in dichotic listening, at least insofar as subjects are free to trade off the available resources between both tasks (which is probably not as evident as in dichotic listening). Separate and joint assessments of performance on each of the tasks used to implement criterion 1 should help clarify the issue. I shall return to this point after having reviewed the evidence for SA/CI of parafoveal words.

3.2. Semantic activation of parafoveal words not identified consciously

The first claim for SA/CI of unattended visual stimuli came from a visual parallel to auditory shadowing experiments. Willows (1974; Willows & MacKinnon 1973) asked her subjects to read relevant lines of text aloud while ignoring the content of irrelevant lines printed in between. The unattended lines contained information that, if processed, could have induced subjects to respond erroneously to some of the questions that followed the reading of a story. This was indeed what was observed, both when the unattended lines were printed in a color different from the main text and when they were printed in the same color preceded by a row of five *X*'s to differentiate them from the relevant lines. Hence, the extent to which irrelevant lines were processed was independent of their relative salience. Willows interpreted her results as evidence for the processing of the irrelevant lines without attention, because no slowing of overall reading speed was observed in the experimental condition compared to a control condition without irrelevant lines. It is doubtful for at least three reasons, however, that irrelevant information was ignored: (1) the situation is rather unusual, and skipping lines while reading might be a difficult task to perform; (2) overall reading speed is too gross a measure to be sensitive to occasional sampling of irrelevant information; and (3) factors other than attentional resources may limit the speed of reading aloud.

In the next three studies to be analyzed, physical characteristics of the displays also closely parallel those encountered in normal reading. Bradshaw's results (1974) are certainly the ones that have been the most often adduced as evidence for SA/CI of parafoveal information. As can be seen in Figure 1B–2, Bradshaw presented centrally a polysemous homograph like *bank* together with a random string of consonants on one side and a biasing word related to one of the possible meanings of the homograph (*river* or *money*) on the other side. Displays were presented for 125 msec, and subjects had to report the homograph as well as the biasing parafoveal word, if possible. Following the report, a second display was presented, consisting of two lists of defining words, one list per meaning of the polysemous word. Subjects had to choose which meaning they thought was the most appropriate for the foveal word. When the parafoveal word was reported (51% of the trials), 83% of the interpretations of the central word were related to the meaning suggested by the parafoveal word. When the parafoveal word was not reported, 53% of the interpretations of the central word were biased toward the meaning of the parafoveal word. The most surprising fact is that this last result, which is almost at the level of chance, was nevertheless found to be statistically significant.

However, in two further studies that replicated Bradshaw's procedure, Inhoff (Inhoff 1982; Inhoff & Rayner 1980) completely failed to confirm the result with unreported parafoveal words. Three eccentricities were used: 1, 3, and 5 degrees between the center of the homograph and the closest letter of the biasing word (see Figure 1B–3). For parafoveal words presented 1, 3, and 5 degrees from fixation, report rates were approximately 70%, 30%, and 10% respectively for right-biasing words, and 40%, 15%, and 5% respectively for left-biasing words. The number of interpretations of the central word that were biased toward the meaning of the parafoveal word closely paralleled the report performance for this word. With unreported parafoveal words, the probability that the forced choice would be biased in the direction suggested by them never exceeded chance. Bias was 0.52 in the experiment reported by Inhoff and Rayner (1980) and 0.51 and 0.53, respectively, in the first two experiments of Inhoff (1982). The conclusion that parafoveal information can be semantically processed without awareness is clearly not supported by the results. It should be stressed that the 0.53 bias observed by Bradshaw is not greater than the small nonsignificant biases reported by Inhoff. The only inconsistency lies in the fact that Bradshaw (1974) found this very weak bias to be statistically significant.

Table 2. *Picture-naming time (msec) in Underwood's experiments (1976; 1977b)*

Experimental condition	Picture left/word right				Picture right/word left			
	PN	PA	UW	RW	PN	PA	UW	RW
Focused attention: no report required[a]	—	581	593	617	—	—	—	—
Divided attention: words not reported[c]	—	—	665	700	—	—	661	654
Divided attention: no report required[b]	626	611	753	716	629	609	672	657
Divided attention: words reported[c]	—	—	860	711	—	—	775	745

Note: PN: picture name; PA: picture alone; UW: unrelated word; RW: related word.
[a]1976, Experiment 1. [b]1976, Experiment 2. [c]1977b.

In the last studies to be analyzed in this section, response latency to the foveal stimulus in Stroop-like tasks is used as the dependent variable. Regarding the salience of peripheral information, Figure 1C shows that, owing to the size and distance from fixation of parafoveal words, displays lie somewhere between those which lead to failure to spatial selectivity (Figure 1A) and those with which no influence from the meaning of the parafoveal word is found (Figure 1B).

Because of the similarities between both displays and procedure, it is worth considering together the results of the experiments reported by Underwood in two different papers (Underwood 1976; 1977b), even though the parafoveal word did not have to be reported in the first study. In these experiments, subjects had to name a picture that was presented simultaneously with an adjacent word for an exposure duration of 60 msec. As can be seen in Figure 1C–1, the discriminability of the parafoveal word was relatively high. This further depended on fixation location, which in turn depended on the experimental condition. In the focused-attention condition with the picture always appearing on the left (Underwood 1976, Experiment 1), subjects probably fixated at a point close to the center of the picture. This entailed that the lateral edge of the word start about 1.5 degrees from fixation, a condition close to that encountered in the Paap and Newsome (1981) study with comparable letter sizes (Figure 2B–1). The discriminability of the word is further increased in the divided-attention conditions in the other experiments (Underwood 1976, Experiment 2; Underwood 1977b): Here fixation probably fell somewhere between the word and the picture, that is, at a mean distance of 0.75 degrees from the edge of the word, a condition close to that encountered in Merikle and Gorewich's (1979) small-letter/small-separation condition (Figure 1A–3C).

Five results are relevant to the present discussion. They are best appreciated by concentrating the analysis on the cases where the picture was presented to the left and the word to the right, whereas most of the results, although weaker, are consistently the same with the opposite picture–word arrangement. Table 2 shows the following: (1) Compared with the picture-alone baseline condition, there is a mean 24-msec interference effect from the word in the focused-attention condition (pictures always on the left) of Experiment 1, which increased to 123 msec on the average in the divided-attention condition (pictures randomly on the left or on the right) of Experiment 2 (Underwood 1976). (2) In the experiment reported in 1977, the side of presentation of the pictures was blocked, but attention was probably divided between both fields because of the requirement to report the words. This explains why the absolute level of performance is similar to that observed in the divided-attention condition of the preceding study. (3) When the word is the picture name (Underwood 1976, Experiment 2), no facilitation relative to the picture-alone baseline is observed. (4) When words are reported (Underwood 1977b) or when words have more opportunity to be processed, as in the divided-attention condition (Underwood 1976, Experiment 2), words related to the pictures interfere less than words unrelated to them. (5) The opposite relationship is found when words are not reported (Underwood 1977b) or when words have less opportunity of being processed, as in the focused-attention condition (Underwood 1976, Experiment 1): words related to the pictures interfere more than words unrelated to the pictures.

At first sight, result 5, if restricted to the experiment reported in 1977, satisfies two of the criteria for concluding that parafoveal words can be semantically processed without being identified. Criterion 1 is fulfilled, since there is a strong interference effect (positive indirect evidence of semantic activation) caused by parafoveal words that cannot be reported at the time of presentation (negative direct evidence of semantic activation). Criterion 3 is also fulfilled, since the interference effects were qualitatively different with reported and unreported parafoveal words (result 4 vs. result 5).

It is nonetheless doubtful that semantic processing of unreported words really did take place without subjects' being aware of their identities. This claim rested upon the fact that the pattern of results found by Underwood is very similar to that observed with the standard picture–word interference paradigm in which both the picture and the word are foveally presented, a situation in which the word is always consciously identified. In this case, three general findings are worth comparing with the results analyzed here. First, relative to a baseline constituted by a picture-alone condition, latencies of picture naming are lengthened by the presence of any word different from the picture label. This interference varies between 70 and 120 msec in various experiments (Lupker 1979; Posnansky & Rayner 1978; Rayner & Posnansky 1978, Experiment 4). The 123-msec and 55-msec interference effects observed by Underwood (1976, Experiment 2) with words either on the right or on the left of the picture are thus very similar to those observed with foveal words. Second, the facilitation from the superimposed picture label is weaker than the interference from words different from the label. The facilitation amounted to 21 and 63 msec, respectively, in the Posnansky and Rayner (1978) and the Rayner and Posnansky (1978, Experiment 4) reports. The absence of facilitation in Underwood (1976, Experiment 2) is thus inconsistent with the usual finding, but the inconsistency is easily explained by the assumption that facilitation, which is the weakest effect, would probably suffer more from delays in processing as the result of parafoveal presentation than would the strongest interference effect. Third, interference effects are greater with words whose meanings are related rather than unrelated to the pictures (Lupker 1979; Rosinski 1977). The effects observed by Underwood (1977b) with reported parafoveal words – interference is stronger with words unrelated than with words related to the pictures – are thus the opposite of what is usually observed with reportable foveal words. There is no ready explanation for this observation, which certainly deserves further attention. On the other hand, effects observed with unreported parafoveal words (result 4) followed the general rule, as if these words were in fact reportable.

These inconsistencies should not rule out interpreting Underwood's results in the framework of the general picture–word interference task. Lupker and Katz (1981) have convincingly shown that interference and facilitation in picture naming stem from the fact that the name of the word is available simultaneously with the name of the picture, either at the stage of response selection or at the stage of response output or both. It is therefore precisely the availability of the word as a verbal response, which is of course an index of conscious identification, that is responsible for the effects observed in the picture–word interference task as long as the picture has to be named. The displacement of the word in the near parafovea would affect only the relative overlap between the time courses of automatic semantic analysis of both the relevant and the irrelevant stimuli. In the conditions used by Underwood (1976; 1977b), only a slight delay in the processing of parafoveal words is expected since the reduced discriminability due to distance from the fovea should be very moderate, as was argued above.

The trouble with the last conclusion is that it casts doubts on the reliability of verbal report as an index of conscious identification of the parafoveal word. This is the case when reporting the irrelevant word is a secondary requirement in a task involving speeded responses to the relevant stimulus; it sometimes underestimates the frequency with which subjects are aware of the meaning of the irrelevant word, as is suggested by an observation made by Underwood (1981). With displays like the one depicted in Figure 1C–2, exposed for 200 msec, report rate for parafoveal words was 27% when reporting all the elements in the display was the only task to be performed (Experiment 2). By comparison, report rate dropped to 3.5% when verbal report of the parafoveal words was secondary to a main task of speeded classification of the centrally fixated left-side word for each display (Experiment 1).

In the last-mentioned study, the low discriminability of the parafoveal items was due not only to their distance from the fovea but also to the presentation of a visual noise mask immediately following stimulus exposure. With 50-msec displays, there was less than 1% report of any parafoveal word, whatever the priority of the verbal report. However, the time taken to name the category to which the foveal word belonged was affected by the presence in the periphery of words belonging to the same category as the target (see Figure 1C–2). With 50-msec exposure, this effect was independent of the number of related words (from one to four), whereas with 200-msec exposure, the number of related words played a role in the amount of interaction with category naming. These results are thus compatible with the assumption of SA/CI, since there was indirect evidence that unreported words were processed (criterion 1). Furthermore, the effects were qualitatively different from those obtained when at least some of the parafoveal words could be reported (criterion 3).

Two further studies by Underwood (Underwood 1980; Underwood & Thwaites 1982) are worth mentioning, even if awareness cannot be assessed since no verbal report was required. The experimental conditions were very similar to those just described, and somewhat unexpected results were again reported. Underwood and Thwaites (1982) used 50-msec displays like that shown in Figure 1C–3. The parafoveal word was presented on the right and was immediately followed by a visual noise mask, a situation in which, according to Underwood (1981), identification of the parafoveal word should be precluded. However, latencies of the lexical decisions related to the foveal words were slowed down by the presence of semantically related parafoveal words. Under somewhat less stringent conditions, since no noise mask was involved, Underwood (1980) showed that latency of retrieval of a previously learned associative response to the foveal word is affected by the meaning of an unattended word located 3 degrees to the right. Typography was not reported, but one could hazard a guess that the displays were similar to the one shown in Figure 1C–3. This last result is far less unexpected than the one reported by Underwood and Thwaites (1982), because the discriminability of the parafoveal word was much less reduced and also because response latencies to the foveal word were very long, giving ample time for information about the peripheral word to accrue.

3.3. Conclusions

There are only five studies in which criteria for SA/CI have been assessed. In spite of Bradshaw's (1974) claim to the contrary, there is clearly no bias in the semantic interpretation of a foveal word when the parafoveal word is not reported (Inhoff 1982; Inhoff & Rayner 1980), a conclusion that is probably valid even for Bradshaw's results. In addition, two other reports in which both criterion 1 and criterion 3 were satisfied apparently demonstrate that SA/CI has taken place (Underwood 1977b; 1981). However, the possibility that Underwood's experiment (1977b) showed semantic access without identification has been tentatively (but in my opinion convincingly) dismissed. We are thus left with one tentative demonstration of SA/CI of parafoveal words (Underwood 1981) and possibly, by inference, with a second one (Underwood & Thwaites 1982). In the latter case, parafoveal words did not have to be reported, but discriminability conditions closely resembled those leading to almost no report in the study of Underwood (1981). These two investigations deserve further attention because their results are at odds with the main findings in parafoveal information processing. They are also unique in their use of a hybrid procedure that combines parafoveal presentation and masking (see next section) to achieve a sufficiently low level of discriminability. However, they are not immune to the specific difficulties in meeting criterion 1 elaborated below.

From a methodological point of view, the apparent satisfaction of criterion 1 – positive indirect evidence of semantic activation in the absence of reportable identification of the parafoveal stimulus at the time of presentation – cannot readily be interpreted unless several additional conditions are met.

(1) Visual conditions are generally such that conscious identification is neither always prevented nor always guaranteed. This implies that direct and indirect evidence of processing of the semantic content of parafoveal stimuli must be gathered on each trial. Subjects are thus engaged in a dual task in which a response, often a speeded response, to the foveal stimulus is followed by an attempt to report the parafoveal word. The problem with the dual task is that lack of report does not necessarily reflect lack of identification. This is at least one of the possible interpretations of the fact that report frequency can sometimes be much lower in a dual task than in a single report task in which the foveal word requires no response (Underwood 1981). In any case, the inclusion of a single report task could help considerably in disambiguating the results observed in the dual task.

(2) Comparison of report rates in the single and the dual task could lead to four different outcomes: (a) near-zero report rates in each condition; (b) equal nonzero report rates in each condition; (c) a higher report rate in the dual task; (d) a lower report rate in the dual task. Of these four outcomes, the first three favor an interpretation of criterion 1 in terms of unconscious activation of the meaning of parafoveal words. The first outcome was the one realized in the experiment of Underwood (1981) with displays exposed for 50 msec. Although it seems odd at first sight, the third outcome could occur if a stronger commitment to the foveal stimulus in the dual task determined an increase in priming of the parafoveal word processing. The fourth outcome is the only one that could compromise the interpretation of criterion 1. It was observed by Underwood (1981) with displays exposed for 200 msec.

(3) The reduced report rate in the dual task compared with the single report task could be accounted for either by a neglect hypothesis or by a reduced-resource hypothesis. In the neglect hypothesis it is assumed that conscious identification is equally frequent in both tasks but that the need to respond to the foveal stimulus sometimes prevents subjects from reporting the parafoveal word in the dual task. In such a case, absence of report does not

always reflect lack of identification. The reduced-resource hypothesis explains the lower report rate in the dual task by the fact that fewer resources are left for parafoveal processing than in the single report task. In such a case, lack of report might indeed reflect lack of identification. There is an urgent need to find a way to distinguish between the neglect and the reduced-resource hypotheses, because the fourth outcome might well be the most frequent one. Evidence for this assertion comes from the observation of much higher report rates for parafoveal words presented alone (Rayner & Morrison 1981; Schiepers 1980) than with concurrent relevant central words (Inhoff 1982; Inhoff & Rayner 1980).

(4) In order to distinguish between the neglect hypothesis and the reduced-resource hypothesis, an additional control condition is needed that would imply the same distribution of attention as the dual task without requiring a response to the central stimulus. A tentative control would be a mixed single task in which the semantic content of the central stimulus would cue the subjects as to which task to perform on each trial. Trials calling for a response to the central stimulus without report of the parafoveal word would be randomly mixed with trials calling for reporting the parafoveal word without response to the central stimulus. Comparable report rates in the control task and the dual task would favor the reduced-resource hypothesis. Comparable report rates in the control task and the single report task would favor the neglect hypothesis.

4. Visual masking

4.1. General features and findings

In backward pattern masking, the processing of a brief visual stimulus (the target stimulus, or simply the target) is impaired by the presentation, after a short delay, of a second visual stimulus (the masking stimulus, or simply the mask) that occupies the same location in the visual field. Three kinds of mask have been studied: bright flashes of light, random noises, and patterns. The extent to which target processing is impaired by patterned masks depends on the visual similarity between the two successive stimuli. When words or letters are used as targets, superimposed letters or pieces of letters in various orientations provide stronger masking fields than other visual configurations (e.g., Jacobson 1974). The time between offset of the target and onset of the mask also plays a role in determining how much masking occurs. This interval is generally called the interstimulus interval (ISI). Another way to describe the temporal relationship between the two visual events is in terms of stimulus onset asynchrony (SOA), which, in backward masking, refers to the time between the onset of the target and the onset of the mask. (For further details on masking terminology and classification see, for example, Breitmeyer & Ganz 1976; Felsten & Wasserman 1980; Kahneman 1968; Turvey 1973; 1978.)

Backward pattern masking is very widely used in information-processing research to limit the time during which processing of a visual event can take place. In using that procedure, investigators adhere more or less explicitly to two theoretical conceptions. One conception is that visual information processing is a two-stage process. The first stage is concerned with the buildup of a literal visual representation of the stimulus. The second stage consists of the extraction of information from this representation, which leads to identification of the stimulus. The other conception is that the role of the pattern mask is to interrupt the second stage of processing while leaving the buildup of the figural representation of the stimulus unaffected.

Seminal work by Sperling (1960; 1963; 1967) played a determining role in the elaboration of this theoretical framework, culminating in Neisser's very influential book (1967) and in the extensive empirical work of Turvey (1973). Neisser (1967) named the short-lived precategorical visual representation of the stimulus the "icon" or "iconic memory." Since then, the concept of iconic memory has been the object of various kinds of attack. Neisser (1976) himself was one of the first to raise doubts as to the ecological validity of the concept. He was followed by Turvey (1977), whose criticisms of the notion of icon are part of a more general plea for a paradigm shift in current conceptions of cognitive psychology (seeUllman 1980). More recently, Haber (1983a) has taken the debate further. It would be beyond the scope of the present paper to go deeper into these considerations. Suffice it to say that, although there is disagreement about the need to postulate an iconic representation to account for visual perception, there is general consensus on the fact that activation persists in the visual system after the termination of a brief stimulus. Eriksen (1980; Eriksen & Schultz 1978) argues that the icon is simply an epiphenomenon of a decaying trace left by the stimulus, not a constructed representation but simply a consequence of the slow course of information aggregation in the visual system.

For our purposes, it is of paramount importance to evaluate whether the interruption hypothesis is a credible explanation of the processing impairments observed under backward pattern masking. There is an alternative or complementary interpretation in terms of an integration hypothesis (Eriksen 1966; Kahneman 1968; Turvey 1973) that there is no initial undegraded representation of the stimulus available for processing, the icon being a composite representation that incorporates features of both target and mask. The descriptive distinction proposed by Turvey (1973) between central and peripheral masking is related to the distinction between integrative and interruptive conceptions of masking as outlined below.

The findings of Turvey (1973) concerning backward masking can be summarized as follows. With monocular or binocular presentation of the target and the mask, masking results from the interplay between two masking mechanisms, one peripheral and the other central, their relative influence depending on the relative energy of the target and the mask and the duration of the ISI. Peripheral masking obeys a multiplicative rule that relates the energy of the target to the minimum ISI needed to prevent masking. It can be obtained with any kind of mask – a flash of light, a random noise, or a pattern – but it cannot be obtained dichoptically, that is, by presenting the target to one eye and the mask to the other eye. Peripheral masking does not occur when the energy of the mask is less than the energy of the target. Central masking obeys an additive rule that relates target duration to the duration of the minimal ISI needed to prevent masking. Central masking occurs only with pattern masks, not with flashes of light. It does not occur with random noise except when the noise resembles a pattern. Central masking takes place both monocularly (or binocularly) and dichoptically, is unaffected by the energy of the target and the mask, and can be obtained when the energy of the mask is less than the energy of the target. Anatomically, peripheral masking can occur at the level of the retina, the lateral geniculate nucleus, and possibly the striate cortex, whereas central masking occurs only at the level of the striate cortex and beyond.

In view of the above-mentioned properties, it would of course be tempting to explain peripheral masking by an integration mechanism and central masking by an interruption mechanism. Turvey (1973) was certainly right in adopting a balanced position with regard to this equation, subsequent work having convincingly demonstrated that it is untenable.

The first integrative mechanism was proposed by Eriksen (1966). It consists of contrast reduction by luminance summation. Two supplementary integration mechanisms have been proposed by Breitmeyer and Ganz (1976; see also Breitmeyer 1980), who elegantly combine the behavioral data on masking with the growing neurophysiological knowledge about the mechanisms of visual perception. It is assumed that presenting both the target and the mask elicits activity in transient and sustained channels. Transient channels are thought to be involved in the detection of spatial location or in signaling changes in spatial location, whereas sustained channels are thought to be involved in the processing of the figural aspects of the stimulus, leading to recognition. Taken together, these integrative mechanisms provide a full account of backward pattern masking with short SOAs according to the following principles.

(1) Contrast reduction by luminance summation (Eriksen 1966) was originally postulated to explain peripheral masking by homogeneous flashes of light, but it evidently comes into play with any kind of mask since luminance is a property of any visual event. It has recently been demonstrated that this purely integrative mechanism can provide masking functions that obey either the additive or the multiplicative rule according to circumstances (Felsten & Wasserman 1980).

(2) An intrachannel integration mechanism (Breitmeyer & Ganz 1976) provides a way to account for peripheral backward pattern masking. At the level of the retina and the lateral geniculate nucleus, masking by integration results from the competition between the target and the mask for common spatial frequency-analyzing channels. At the cortical level, intrachannel inhibition consists of the addition of noise to a contour-synthetic process.

(3) An interchannel inhibition mechanism (Breitmeyer & Ganz 1976) is postulated to account for central masking. This kind of interference presumably arises from the inhibition of the activity of sustained channels corresponding to the analysis of the figural aspects of the target by the activity of transient channels elicited by the onset of the mask.

It can therefore be concluded that even with dichoptic backward pattern masking (which prevents luminance summation and the more peripheral intrachannel integrations between the mask and the target) the iconic representation amalgamates characteristics of both the mask and the target. The cortical intrachannel inhibition mechanism builds a composite representation that includes features of both stimuli. In addition, the interchannel inhibition mechanism removes some spatial frequencies from the target, leaving an impoverished composite representation.

This account of the way target and mask interact by integration is now incorporated by Turvey (1978; Michaels & Turvey 1979) in his view about iconic representations with short SOAs. For those who deny the need to postulate an iconic representation to account for visual information processing, the concept of integration has always been sufficient to explain masking at short SOAs (e.g., Eriksen 1980; Eriksen & Schultz 1978). In this view, masking is conceived as a consequence of the low temporal resolution of the visual system and the slow time course of information aggregation. The mask simply combines its energy and its features with the decaying trace of the target stimulus, leaving a new, undecipherable decaying trace.

If the multilevel integration theory of masking described above is correct, then this invalidates the tacit assumption underlying the use of masking as a technique to limit the time during which the processing of an initial intact representation of the visual information can take place. It is accordingly inappropriate to distinguish between the energy masking (peripheral masking) that is supposed to supply an initial composite degraded perceptual representation of the target, one not amenable to successful semantic processing, and the pattern masking (central masking) assumed to provide an undegraded initial representation of the target, which can, at least potentially, lead to semantic activation. Following this line of thought, the eventuality of the SA/CI of a masked word is a priori precluded because there is no place in the system where a legible representation of the word is available.

One classical challenge to integrative theories of masking is the phenomenon first reported by Robinson (1966) using flashes of light and further extended to stimuli closer to those used in information-processing investigations (e.g., Briscoe, Dember & Warm 1983; Dember & Purcell 1967; Dember, Schwartz & Kocak 1978; Kristofferson, Galloway & Hanson 1979; Turvey 1973). It has been shown that a masked target can be recovered if the first mask is followed by a second one that hinders its perception (i.e., if the mask is masked). According to one interpretation, the first mask interferes with the conscious identification of the target but does not affect an intact representation of the target, which remains retrievable in certain circumstances; that is, it can be unmasked by the second mask. An alternative interpretation, compatible with the integrative theories of masking, would be that the first mask is simply rendered ineffectual by the second, such that in the three-stimulus sequence the target is not masked at all.

Another challenge is the recent model proposed by McClelland and Rumelhart (1981; Rumelhart & McClelland 1982; see also Johnston 1981) to account for the word-superiority effect observed under marking (Reicher 1969; Wheeler 1970) in a paradigm that epitomizes the use of a pattern mask as an interruptive device. These authors propose that the mask inhibits the activation induced by the letter string at the level of feature and letter analysis, but that the simultaneously activated word representation is left relatively unaffected.

The most challenging theory of masking has been proposed by Marcel (1983b) as part of a broader conception in which he argues for the need to distinguish between the representations available to consciousness and those that are used in the processing of sensory inputs. His theory involves the following three propositions. (1) With peripheral masking the information is too impoverished to provide either representations useful for unconscious processing or representations amenable to conscious interpretation. (2) With central masking the representations used for unconscious processing of the input are unaffected by the mask. (3) With central masking, the conscious identification of the target is impaired because gestaltlike principles combine the representation of the target with the representation of the mask into a unitary percept. This amounts to a new integration mechanism that acts upon the representations used to generate conscious experience rather than on the representations used for unconscious analysis of the sensory input. Two points should be stressed. First, as noted above, it is extremely unlikely that any useful, semantically processable representation of the target is generated under pattern masking with short SOAs. Second, Marcel's new theoretical conception stemmed mainly from his demonstration of SA/CI for the pattern-masked priming words and from some related effects (Marcel 1978; 1980; 1983a; Marcel & Patterson 1978) of which the validity is questionable, as the rest of this section of the paper will attempt to show.

Priming with a masked prime is an ideal procedure for satisfying criterion 1. There is no concurrent task to perform, and it is assumed that subjects pay as much attention as possible in their attempt to identify the primes. Hence, performance is entirely data-limited. Whether the quality of the input is poor enough to prevent conscious identification is a problem of threshold determination. Before these data are considered, some related results will be reviewed, in which the availability of the meaning of the masked words has been assessed directly.

4.2. Direct evidence for semantic activation of masked words

The possibility that normal subjects might show a tendency to produce semantic paralexias – errors that resemble the correct answer in meaning rather than in sound or shape – under conditions of backward pattern masking has been raised by Marcel (1978), who happened to observe several errors of that kind in one of his experiments. Frequent semantic paralexias in the reading of isolated words constitute one of the defining symptoms of one form of acquired dyslexia, called "deep dyslexia" by Marshall and Newcombe (1973; see Coltheart, 1980a, for a review). It would be of considerable theoretical interest to find converging tendencies in normal subjects and in brain-damaged patients.

Allport (1977) has tried to document the phenomenon further. He observed that between 6% and 9% of the errors could be classified as semantically related to some of the target words he presented under conditions of pattern masking. This was actually observed in 6.1% of the cases in Experiment 1, which involved four-word displays masked to the point where subjects could identify only one word. There was also 7.8% semantic paralexia in Experiment 2, where only two words were displayed on each trial. Allport (1977) did not try to estimate the probability that these errors might have arisen by chance alone. He obviously took it for granted that such a relatively high proportion of semantic paralexias exceeded chance and reflected some kind of processing of the words that should have been provided as correct responses. The results reviewed below strongly suggest that his assumption was incorrect.

Both Ellis and Marshall (1978) and Williams and Parkin (1980) replicated the procedure used by Allport (1977) in his Experiment 1. They confirmed the initial observation by finding 8.9% and 12.7% of the errors that could be classified as semantic paralexias. Their criteria for judging the semantic relationship were admittedly somewhat intuitive. They then tried to estimate the probability that such errors could occur by chance alone. Their procedure was to randomly reallocate responses produced by each of the subjects to the different displays. When this was done, they observed proportions of semantic paralexias that were very close to those in the actual responses of the subjects. One can accordingly conclude that subjects are likely to produce from 6% to 9% semantic paralexias by chance alone.

Fowler, Wolford, Slade, and Tassinary (1981) confirmed this conclusion with a different procedure. In their Experiment 4, they presented 25 words to their experimental subjects interspersed randomly with 25 nonwords. The paired list of stimuli and responses was then presented to ten judges, but with the nonword stimuli replaced by randomly prechosen words. The judges were instructed to pick the ten pairs that were the most similar in meaning. In doing this, the judges failed to distinguish trials on which words were actually presented from trials on which nonwords were displayed.

A second line of evidence for unconscious activation of the meaning of unreportable words comes from another experiment of Marcel (1983a, Experiment 1), in which he reported the progressive disappearance of three aspects of word stimuli as a function of the progressive decrease in the duration of the SOA between the stimulus and the mask. A word or a blank field was presented before the mask. Subjects then either made a forced choice as to whether or not a word had preceded the mask or chose the member of a pair of words that was most similar, either visually or semantically, to the masked word. Only one kind of judgment was required on each trial. The first decision that reached a chance level of performance was presence versus absence; with further decreases in SOA durations, visual-similarity judgments could no longer be made with above-chance accuracy; at the shortest SOA, semantic characteristics became unavailable.

These observations are, of course, startling, because they are the opposite of what most current information-processing theories, in particular masking theories like that of Breitmeyer and Ganz (1976), would have predicted. Notice, however, that the greater availability of semantic compared to visual characteristics of words is not uncommon. McClelland (1976; see also Friedman 1980), in isolating trials for which all letters of pattern-masked words were correctly identified, nevertheless found that the probability of correctly reporting the case of these words (upper, lower, or mixed case) did not exceed 0.52.

The validity of the reported dissociation between detection and the processing of semantic or physical aspects of the undetectable words is critically dependent on how adequately the detection threshold is established. The SOA at which subjects first made erroneous presence–absence judgments was individually determined with the descending method of limits by Marcel (1983a). Six SOAs ranging from 20 msec below to 5 msec above threshold were then used to provide the results described above. The individual SOAs corresponding to 60% correct presence–absence judgments (chance being 50%) ranged from 20 to 110 msec.

These results were never successfully replicated. With SOAs yielding 70% correct detections, Fowler et al. (1981, Experiment 1) observed 68% correct semantic judgments and 54% correct phonetic judgments. In Experiment 2, semantic judgments amounted to 57% and word-shape judgments were at chance. Since only one judgment was required on each trial, one cannot tell whether or not correct semantic judgments always corresponded with correct detection. What is clear is that semantic-judgment performance was not better than detection performance. The most devastating result, however, came from Experiment 3 of Fowler et al. (1981), who never presented any stimuli before the mask but explained to their subjects what had been done in the other two experiments. The subjects then produced the same above-chance percentages of correct semantic judgments that had been observed before, 65% and 58% respectively for conditions corresponding to Experiments 1 and 2. So whatever the reasons for the biasing effect, it is clear that it can hardly be taken as evidence for semantic processing of the barely detectable words.

In another attempt to replicate Marcel's effect, Nolan and Caramazza (1982) made several procedural improvements, of which the most important was to ask subjects to make first a detection and then a similarity judgment on each trial. SOAs of 50, 69, 83, and 100 msec were used, and either a word or a blank field was presented before the mask on each trial. The results were very clear-cut: there was no trace of either visual or semantic information when detection performance was at chance. For those subjects who met Marcel's criterion of 60% correct detection performance, visual and semantic similarity judgments amounted respectively to 60% and 58%. Therefore, contrary to Marcel's claim, subjects were not better at making similarity judgments than at detecting the presence or absence of stimuli. Evett and Humphreys (1981) also mention a failure to replicate Marcel's procedure and results.

The reason for all these failures to replicate is that with SOAs ranging from 20 to 110 msec in the seminal study of Marcel (1983a, Experiment 1) it is extremely unlikely, at least in the case of the longest SOAs, that subjects were really unable to detect the words. By comparison, Jacobson (1974), who used a mask similar to that of Marcel, observed 100% correct identification performance with a mean SOA of 32 msec. Moreover, Fowler et al. (1981) used a 70% detection threshold because this performance level was reached with SOAs already too short to be further reduced. SOAs ranged between 10 and 25 msec, with mean durations of 15 and 18 msec, respectively, in their Experiments 1 and 2. Furthermore, close examination of the procedures shows that although the subjects of Fowler et al. (1981, Experiment 1) were dark-adapted, those of Marcel (1983a, Experiment 1) were certainly more light-adapted, since they looked at the forced choice outside the tachistoscope. This factor should have led to longer SOAs in the former than in the latter case, as will become evident in Section 4.3.1. For all these reasons, it is highly probable that detection performance was underestimated in Marcel's Experiment 1, perhaps because subjects adopted a very stringent criterion for responding positively during threshold determination, which they kept with respect to detection during the experimental phase for the sake of consistency.

To conclude, errors that can be classified as semantic paralexias have been consistently observed under severe conditions of pattern masking. However, current evidence suggests that this phenomenon can be entirely accounted for in terms of a nonzero correlation between subjects' response-selection strategies and experimenters' stimulus-selection bias. As regards the observation of a better forced-choice performance in semantic than in detection judgments, this proves to be elusive and almost certainly artifactual.

4.3. Fulfilling criterion 1: Pattern-masked primes

Most of the experiments to be reviewed in this last section used the same basic paradigm. A priming stimulus and a target stimulus are displayed successively. Priming effects are compared in two conditions, one with the prime left unmasked and the other with the prime masked backward by a pattern. In the unmasked condition, the prime can be consciously identified without difficulty. In the masked condition, two situations must be distinguished. In the less stringent one, the prime can no longer be identified but can still be detected. In the more extreme one, even detection of the presence of the prime is made impossible.

In order to achieve the desired masking condition, the priming phase of the experiments is always preceded by a threshold-determination phase. Starting at a relatively long SOA between the onset of the prime and that of the mask, SOA duration is progressively decreased with a descending method of limits until a subject can no longer identify the prime or even detect it. In the determination of an identification threshold, a prime always precedes the mask; in assessing a detection threshold, the mask is preceded either by a prime or by a blank field, and the subject has to say which event occurred.

4.3.1. Semantic priming without identification of the prime.

One of the first investigations devoted to semantic priming by unidentifiable, masked primes used pictures as both primes and targets (McCauley, Parmelee, Sperber & Carr 1980). This was then extended to include all the combinations of picture and word primes with picture and word targets (Carr, McCauley, Sperber & Parmelee 1982). The mask was composed of letters and letter pieces. Individual zero identification thresholds were determined for each of 10 pictures (McCauley et al. 1980) or for each of 12 pictures and 12 words (Carr et al. 1982) during a first session. These thresholds corresponded to the stimulus exposure durations (less 5 msec) at which the subject failed to identify each stimulus on six occasions. Full identification thresholds were determined by the ascending method of limits. Starting at the zero threshold exposure duration, stimulus exposure durations were progressively increased until the subject correctly identified each stimulus on six occasions. A speeded naming response to the target was required. Table 3 provides prime durations, priming effects, and prime report rates for the zero, full, and suprathreshold conditions of each experiment.

At this point, only results obtained when both prime and target were pictures will be considered. As can be seen in Table 3, there was no difference between the priming effects observed at the zero and full identification threshold conditions in the first experiment of McCauley et al. (1980). Two other intermediate exposure durations of the primes, 78 and 87 msec, yielded comparable priming effects of 46 and 54 msec, respectively. There was also no difference between the priming effects corresponding to zero, full, and suprathreshold exposure durations in the second experiment. A further reduction of the exposure duration of the primes to 37 msec still led to a 30-msec priming effect, but this finally vanished with primes exposed for 17 msec. Carr et al. (1982) obtained comparable results, except that the priming effect, already substantial in the zero and full identification threshold conditions, increased still further in the suprathreshold condition. Hence, pictures that subjects were unable to identify during the threshold-determination phase of the experiments nevertheless exerted a considerable priming effect in the second phase of the experiments.

In spite of the reliability of these results, I have always been very

Table 3. *Picture priming without identification*

Authors	Study	Prime	Target	Priming effect[c] Zero	Priming effect[c] Full	Priming effect[c] Supra	Prime duration[c] Zero	Prime duration[c] Full	Prime duration[c] Supra	Prime report rate[d] Zero	Prime report rate[d] Full	Prime report rate[d] Supra
McCauley et al. 1980	Exp. 1[a]	Picture	Picture	34	38	—	68	96	—	1	13	—
	Exp. 2[b]	Picture	Picture	46	33	39	53	90	250	2	45	95
Carr et al. 1982[b]		Picture	Picture	39	48	71	46	68	500	3	33	97
		Picture	Word	3	15	28						
		Word	Word	0	33	37	65	99	540			
		Word	Picture	8	40	49						
Purcell et al. 1983	Exp. 3	Picture	Picture	−26	—	44	25	—	250	0.5	—	97

[a]Results pooled on the two picture sizes. [b]Results pooled on the two ISIs between the prime and the target. [c]In msec. [d]In percent.

reluctant to consider them as evidence of SA/CI of the priming pictures. One reason was that I found it very doubtful that subjects were really unable to identify the priming pictures at mean exposure durations ranging from 48 to 68 msec. In this particular case, my skepticism was nourished by the fact that, with comparable masking conditions (the mask consisted of superimposed X's, O's, and I's), Rayner and Posnansky (1978) and Posnansky and Rayner (1978) reported full identification of pictures presented for mean durations ranging from 14 to 18 msec according to the experiment. In a way, I was wrong! Threshold durations were reliable in each case. Long exposure durations were provided by dark-adapted subjects (Carr et al. 1982; McCauley et al. 1980), whereas short durations were provided by light-adapted or less dark-adapted subjects (Posnansky & Rayner 1978; Rayner & Posnansky 1978). The influence of this factor was clearly demonstrated by Purcell, Stewart, and Stanovich (1983), who found a zero identification threshold for pictures exposed for 90 msec with dark-adapted subjects looking into a dark tachistoscope (Experiment 2); this threshold was reduced to 25 msec with light-adapted subjects looking into an illuminated tachistoscope (Experiment 3). Yet I was also right to be skeptical, even if for the wrong reason! Purcell et al. (1983) have pointed out a subtle artifact in the aforementioned studies.

The gist of the matter is that the levels of dark adaptation differed in the threshold-determination phase of the experiments and in the priming phase. During the threshold-determination phase, the room was dark (or dimly illuminated) and the tachistoscope was dark as well, except for the brief presentation of a priming stimulus and a 50-msec mask. In the priming phase, conditions were the same, except for the addition of a second stimulus, illuminated for several hundred msec (till the response was initiated). There was therefore much more light involved in the priming phase of the experiments than in the threshold-determination phase, which could have increased the level of light adaptation enough to allow the priming stimulus to be seen much more often than expected. Purcell et al. (1983) clearly demonstrated the validity of this argument. In their second experiment, they used dark-adapted subjects. They first determined the zero identification threshold for each subject in a way analogous to that described earlier (Carr et al. 1982; McCauley et al. 1980). They then recorded the picture report rate, which is expected to be almost nil, under conditions that mimicked the priming phase in earlier experiments. No second stimulus was presented, but a white field was exposed for 800 msec at the time corresponding to the presentation of the priming stimulus in the other experiments. They then observed 70% correct reports of the priming pictures instead of the expected 0% (or 20% by chance alone, since five different pictures were used).

This observation by Purcell et al. (1983) adds credence to the assumption that when priming effects were observed with exposure durations corresponding to zero identification thresholds, or even lower, subjects were in fact able to identify the priming stimulus much more often than expected on the basis of threshold determination. Therefore priming effects can hardly be attributed to SA/CI of the prime. This conclusion is further strengthened by the negative results of a third experiment by Purcell et al. (1983). They resorted to an obvious method of equalizing the levels of light adaptation between the two conditions. They simply worked in a well-lit room with light-adapted subjects and used a bright background field in the tachistoscope. Under such conditions, the addition of a second stimulus during the priming phase of the experiment adds nothing to the level of light adaptation already reached. As can be seen in Table 3, when this was done, priming pictures exposed for a duration corresponding to a zero identification threshold (which is now much shorter, 25 msec, as has already been stressed) exerted no priming effect at all on the naming of a second associated picture (the reverse effect was not significant).

Regarding word primes and word targets, Carr et al. (1982) observed no priming effect with word prime exposed for 65 msec (zero identification threshold; see Table 3). In this case the increase in the level of light adaptation during the priming phase of the experiment was ineffective. This does not of course invalidate the conclusion already reached. The figural properties of a mask consisting of letters and pieces of letters made it more potent for words than for pictures, as indicated by the higher exposure durations needed for zero or full identification threshold with words than with pictures. It just happened that, with Carr et al.'s (1982) particular conditions, the increase in the level of light adaptation was strong enough to overcome the less efficient masking with pictures but not the more efficient one with words.

At this point one might wonder why things are so complicated. Would not the difference in visibility of the prime between conditions have been revealed by simply asking subjects to name the target first and then to try to recall the prime, on each trial? The answer is negative because that was what was actually done. As can be seen in Table 3, report of the priming stimulus was almost perfect in the suprathreshold condition, almost nil in the zero threshold condition, as expected, but considerably below the expected 100% under the full threshold condition. In the latter case, report rate obviously does not reflect subjects' true identification possibilities, which also casts doubt on the reliability of the absence of report under the zero threshold condition. This problem has already been touched on earlier in this paper (see Section 3.2.; Underwood 1981). There are at least two reasons for the unreliability of report rate when report is a secondary task. One is that subjects could simply adopt a laxer report criterion in the double task than in the single task used in threshold determination. The second is that they might neglect to report an actually identified prime in order to avoid interference with the speeded response to the target.

In any case, recall of the primes interferes less with the naming task when primes are clearly visible (suprathreshold conditions) than when they are barely visible (full threshold condition). Interference is also less in the zero threshold condition, presumably because subjects do not even try to recall primes (Carr et al. 1982; McCauley et al. 1980). Purcell et al. (1983, Experiment 3) observed an overall level of performance that was nearly 200 msec slower with subjects required to recall the primes than with subjects not required to do so. Nevertheless, equivalent priming effects were observed in both groups.

Under conditions similar to those discussed here, Fischler and Goodman (1978) presented a priming word for 40 msec, then a pattern mask for 50 msec, and finally an item calling for a lexical decision. There was no attempt to make the priming word unidentifiable, and subjects were simply asked to try to recall it after having made the response to the target. An overall 28-msec semantic priming effect was observed, and 48% of the primes were recalled. After the data were partitioned as a function of prime report, two rather puzzling effects were observed. First, a large 66-msec priming effect was confined to those trials for which the priming word was not recalled. With recalled, identified primes, the priming effect was simply nil. Second, the overall level of performance was much lower for trials without recall of the prime than for trials with recall of the prime (885 vs. 780 msec). Both results were contradicted by Carr et al. (1982) in the analysis of their full identification threshold data. There was no difference between the priming effects observed with and without prime recall, and overall performance was worse with than without prime recall (732 vs. 667 msec). No firm conclusions can be reached, however, because neither the initial observation of Fischler and Goodman (1978) nor the failure to replicate it by Carr et al. (1982) can be considered very informative. Overall individual mean priming effects were estimated on the basis of 15 and 12 trials (minus about 5% of errors), respectively, in these two studies. Therefore, after the partitioning of the data, only a small unequal number of trials were left to provide the estimates of the individual mean reaction times used to determine the priming effects with and without prime recall. It follows that much instability is expected in these data and little credence can be given to any particular pattern of results obtained under such conditions.

With a modified version of the paradigm described so far, presenting four fields successively, instead of two, Evett and Humphreys (1981) and Humphreys, Evett, and Taylor (1982) observed various kinds of priming effects without explicit identification of the primes. Each trial comprised the successive presentations of a first pattern mask, a lowercase prime word, an uppercase target word, and a second pattern mask. There were no ISIs between fields, and their exposure durations were equal. Subjects knew that two words were presented, and they were asked to report any word they could identify on each trial. Threshold trials were used to establish an exposure duration at which about 40% of the targets were identified. This procedure regularly yielded mean exposure durations of 35 to 40 msec, with individual values ranging from 25 to 55 msec. During the main part of the experiment, subjects were also required to report any word they could on each trial. Typically, only target words (second words) were reported, whereas report rates of the primes (first word) were almost nil, amounting to 1% or 2%. Despite this, target report rates were strongly influenced by the nature of the primes. Evett and Humphreys observed semantic (Experiment 1) and graphemic (Experiments 2 and 3) priming effects, and Humphreys et al. (1982) added phonological priming effects to the series. Similar effects with very short SOAs between the primes and the targets were also observed in categorization tasks in which word–nonword classification time (Pickering 1976) or letter–digit classification accuracy (Humphreys 1978; 1981) was influenced by the category of the primes.

Two points should be stressed about the experiments using the four-field paradigm (Evett & Humphreys 1981; Humphreys et al. 1982). First, there is no doubt that the method is very successful in demonstrating the existence of presumably automatic priming effects, that is, priming effects that are not determined by subjects' anticipation of the targets. Very short SOAs between the prime and the target have also provided reliable priming effects with the usual two-field paradigm. This was the case when the target replaced the prime at the same location after 40 msec (Beauvillain & Segui 1983; Fischler & Goodman 1978, Experiment 2), a value close to the exposure duration of each field in the four-field paradigm. Second, authors who have used the four-field paradigm have mainly been concerned with the automatic aspects of priming rather than with demonstrating unawareness of the identity of the primes. There is no doubt that subjects reported only one word per trial (actually the second word), but this might simply reflect their reluctance to provide two responses in what is in any case a strained situation. They might then report only the easier letter string. We have no information about the number of prime reports that could have been observed had the subjects been required to report the first word only. But we know that a pattern of reports of the first and second words that is opposed to the one described here can be obtained by slightly modifying the procedure used by Evett and Humphreys (1981) and Humphreys et al. (1982). That is, with a sequence consisting of black field, first word for 30 msec, second word for 20 msec, and pattern mask, Naish (1980) observed a large number of reports of the first word and a small number of reports of the second. Of course, minute procedural differences might explain the difference in results, which implies that there is a real need to find reliable methods to assess subject's true identification capability in these paradigms. The unavailability for report of the first letter string in the four-field paradigm is now better evaluated by the introduction of a condition in which the second letter string was a row of *X*'s rather than a word (Humphreys & Evett, personal communication, 16 January 1984). Again, only around 1% report of the first word was observed, which casts some doubts on my hypothesis that subjects reported only the easier second string in the standard situation.

One can conclude that some initial demonstrations of semantic priming without conscious identification of the priming stimulus have clearly been flawed by inadequate controls for the relative levels of dark adaptation reached during the threshold and the priming parts of the experiments (Carr et al. 1982; McCauley et al. 1980). When there are such controls the effect is no longer obtained (Purcell et al. 1983). The observation of priming effects confined to trials without prime recall (Fischler & Goodman 1978) has not been replicated (Carr et al. 1982) and should be considered inherently unreliable unless the number of trials is considerably increased. Finally, the four-field paradigm developed by Evett and Humphreys (1981) does not suffer from the artifact of differential light adaptation, since conditions are the same during threshold determination and priming. However, in the experiments reported so far, the ability to identify the priming word has not yet been fully assessed.

4.3.2. Semantic priming without detection of the prime. In the last experiments to be analyzed in this paper, the prime was so severely masked as to be undetectable. The SOA at which detection rate fell just below 60% was determined with the procedure described in the introduction to this section; then 5 msec was subtracted from that value to provide the threshold SOA to be used in the priming part of the experiment. This part of the experiment is generally preceded and followed by a set of threshold trials to verify that the prime cannot be detected at a rate above chance.

One of the first experiments reported by Marcel using this procedure is Experiment 3 in his recently published paper (1983a). It was based on a modified version of the Stroop task in which color patches were preceded by color words. The main finding was that the pattern of interference and facilitation was the same whether or not the color word was masked. These results could not be replicated by Cheesman & Merikle (1984), who found no Stroop effect when detection of the color word was at chance. For higher SOAs, the amount of Stroop effect was an increasing function of SOA duration.

A second experiment based on this procedure was described by Marcel (1980) in the context of the study of priming by polysemous words. Primes were presented either normally for 500 msec or for 10 msec followed by a pattern or noise mask after the predetermined ISI, which corresponded to the level of 50% correct detection in this particular experiment. As was mentioned in Section 2.3, there is now good evidence that several meanings of a polysemous word are automatically accessed at the time of presentation. There is also evidence that one particular meaning is promptly selected on the basis of the context, the others being deactivated (Pynte, Dô & Scampa 1984; Swinney 1979; 1982). If this is so, only the selected meaning would be efficient for priming, provided the time elapsing between the prime and the word was not too short. A selective priming effect was indeed observed by Schvaneveldt, Meyer, and Becker (1976), whose supraliminal conditions were replicated by Marcel (1980). Marcel and Schvaneveldt et al. used a paradigm in which three items were presented successively, with the polysemous word always occupying the second position. Selective priming is demonstrated by the fact that, compared with appropriate controls, the response to a word like *money* is facilitated in a sequence like *save–bank–money*, but the response to the word *river* is not facilitated in a sequence like *save–bank–river*. When Marcel masked the polysemous word with a bright field (peripheral masking) the priming effect was no longer observed. When the polysemous word was pattern-masked (central masking) to a point where detection of the prime was at chance, unselective priming effects were observed. This time, both *money* and *river* were primed when they ended a sequence the first and second members of which were *save* and *bank*, *bank* being unavailable to awareness. This demonstration looks very compelling since both criterion 1 and criterion 3 were met, that is, the prime was unreportable and the effects observed without conscious identification were qualitatively different from those obtained with conscious identification of the prime.

The next four experiments all used dichoptic rather than binocular presentations of the prime and the target. Marcel's Experiment 4 (1983a) was partially prompted by the contradiction between the Stroop effects observed in his Experiment 3, described above, and the absence of such effects with subliminally presented words reported by Severance and Dyer (1973). Procedural differences between the two experiments corresponded roughly to conditions leading to peripheral and central masking as defined in Section 4.1. This led to the use of an experimental design in which monocular energy masking (appropriate for pure peripheral masking) and dichoptic pattern masking (appropriate for pure central masking) were contrasted. The pattern-masked part of the experiment was replicated by Balota (1983) and Fowler et al. (1981).

The experiment is based on the paradigm of Meyer et al. (1975) in which lexical-decision priming by semantically related words is evaluated. The prime was masked, and the target appeared after an SOA of 2000 msec. SOAs of 200 and 350 msec were also included by Fowler et al. (1981, Experiment 6) and Balota (1983), respectively. Table 4 shows that priming effects were reliably observed under dichoptic pattern-masking conditions, provided the time elapsing between the prime and the target was long. With a 2000-msec SOA, there was no difference between the priming effect due to unmasked and pattern-masked primes in the study of Marcel (1983a), whereas the priming effects were weaker with pattern masking than without masking in the Fowler et al. (1981) and Balota (1983) experiments, the difference reaching statistical significance only in the latter case. Under pattern-masking conditions,

Table 4. *Priming without detection in dichoptic masking*

Authors	Study	SOA[a]	Masking condition	
			No mask	Pattern
Marcel 1983a	Exp. 4	2000	62[b]	54
Fowler et al. 1981	Exp. 5	2000	38	29
	Exp. 6	2000	—	32
	Exp. 6	200	—	−3
Balota 1983		2000	51	35
		350	57	12

[a]Stimulus onset asynchrony between the prime and the target in msec. [b]Priming effect expressed in msec.

the priming effect did not show up when the SOA was short, being either 200 msec (Fowler et al. 1981) or 350 msec (Balota 1983). In addition, no priming effect was observed by Marcel (1983a) when the prime was masked by noise, which confirms the initial prediction; that is, the difference in priming effects between the pattern- and noise-mask conditions or the difference between the results of Marcel's Experiment 3 (1983a) and those of Severance and Dyer (1973) is attributed to the fact that different mechanisms of masking are at work in each case. In short, semantic priming without conscious detection of the prime is observed under central masking conditions but not under peripheral masking conditions.

Further evidence of semantic analysis without conscious identification of the prime can be found in the fact that subjects whose performance improved between the threshold control trials given before and after the priming part of the experiment did not show stronger priming effects than subjects whose performance did not improve (Fowler et al. 1981). Balota also reports that subjects whose detection threshold was reached with a very short ISI between the prime and the mask showed a stronger priming effect (41 msec) than subjects requiring a longer ISI to reach the threshold (29 msec). This observation was confined to the long interval between the prime and the target. Both observations have been taken as further indications that subjects really did not see the priming word during the main part of the experiment.

However, some results were also at odds with this conclusion. First, unconscious priming cannot be attentional. Automatic priming is known to occur with very short SOAs (Beauvillain & Segui 1983; Fischler & Goodman 1978; Neely 1976; 1977). Priming can even occur with a spatially nonoverlapping prime presented shortly after the target (Kiger & Glass 1983). Hence, the absence of priming effects with short SOAs (Balota 1983; Fowler et al. 1981) is inconsistent with the rest of the results. Also, in Balota's data, a cost–benefit analysis (Posner & Snyder 1975a; 1975b) revealed a tendency to obtain both facilitation and inhibition from pattern-masked primes, a fact more compatible with intentional, therefore conscious, than with automatic priming.

In spite of these inconsistencies dichoptic pattern masking has until recently proved to be one of the most reliable procedures in the masking literature for tentatively demonstrating semantic activation without awareness. It was also tempting to bridge the gap between this topic and the related field of binocular rivalry (see Walker, 1978, for a review), in which claims for the demonstration of the same phenomenon have also been made (e.g., Philpott & Wilding 1979; Somekh & Wilding 1973; but see Zimba & Blake, 1983, for a failure to get priming effects from the suppressed eye). Unfortunately, such conclusions must now be completely reassessed in light of the artifact of modified levels of light adaptation between the threshold determination and priming phases of the experiments as discovered by Purcell et al. (1983). The possibility that this artifact flawed most of the undetectable prime experiments is just as compelling as in the case of unidentifiable primes (see Section 4.3.1).

The danger was particularly strong in the experiments of Balota (1983) and Fowler et al. (1981, Experiments 5 and 6) because subjects were dark-adapted and viewed a dimly illuminated tachistoscope. Hence the addition of a target stimulus in the priming trials compared with the threshold trials contributed to increasing the level of light adaptation of the subjects, which in turn implies that the discriminability of the stimuli was better in the former than in the latter type of trial. One might even be tempted to use this factor to interpret subtle aspects of the results. First, there was no priming effect with the shortest SOA between the prime and the target. This might readily be explained by analyzing Balota's experiment. A new trial was initiated every 10 sec. It follows that the dark intertrial interval that preceded the next prime lasted 6 sec when SOA duration was 2 sec and 7.65 sec when SOA duration was 0.35 sec. Hence subjects had more time to readapt to dark after the preceding target and before the next prime when the SOA was short than when it was long, which implies that they had less chance to see the prime in the former than in the latter case. Although intertrial intervals were more variable in the experiments reported by Fowler et al. (1981; Experiments 5 and 6), the same reasoning could nevertheless apply because a difference of 1.8 sec is expected in the mean intertrial intervals corresponding to long and short SOAs. Second, one can go even further in the interpretation of these results. Balota (1983) found a small, nonsignificant priming effect with his short SOA, whereas Fowler et al. (1981, Experiment 6) observed no effect at all. This might be explained by the fact that the increase in the level of light adaptation during priming compared with threshold trials was greater in the experiment of Balota because he presented the target four times as long as Fowler et al. did. As regards Marcel's Experiment 4, nothing is said about the lighting conditions of the experimental room, but the inside of the tachistoscope was dark. If, as subjects often do, Marcel's subjects kept their faces pressed against the eyepieces throughout a long run of trials, they could well have been exposed to different levels of dark adaptation between threshold trials and priming trials, even if the room was actually lit.

On the basis of this alternative interpretation, priming effects can be attributed to the fact that subjects were able to see the prime more often than expected on the basis of threshold determination. Then it is simply irrelevant that subjects who improved performance on control threshold trials after the priming part of the experiments did not show greater priming effects than subjects who did not improve (Fowler et al. 1981, Experiment 5). The fact that subjects who had very short ISIs showed a greater priming effect than those who needed longer ISIs (Balota 1983) could simply mean that the subjects who were the best at coping with the masking situation were also more likely to benefit from an increase in light adaptation. In spite of the increase in light adaptation, conditions of visibility are still poor. One might therefore not expect the prime to be clearly seen on each trial, which could account for the weaker priming effect in the masked than in the unmasked conditions (Balota 1983; Fowler et al. 1981) and also for the fact that volitional aspects of priming are less prominent under masking (Balota 1983). Aside from the interpretation proposed above, the absence of priming effects with short SOAs might simply reflect the slow information aggregation of barely visible primes.

To sum up, the hypothesis that priming effects observed under very severe conditions of pattern masking reflect SA/CI of the prime cannot be considered as supported by the data reported so far. An alternative explanation in terms of conscious identification of the primes is at least as reasonable in the face of present evidence.

4.4. Conclusions

In Section 4.2 it was shown that semantic paralexias do occur with pattern masking (Allport 1977; Fowler et al. 1981; Ellis & Marshall 1978; Williams & Parkin 1980). However, this can be accounted for without postulating semantic analysis of the masked words. Section 4.2 also showed that the initial observation (Marcel 1983a, Experiment 1) of better performance in visual or semantic similarity judgments than in presence–absence judgments has never been replicated (Evett & Humphreys 1981; Fowler et al. 1981; Nolan & Caramazza 1982). The inconsistency among results might be explained by the rough assessment of presence–absence performance in the initial study.

Section 4.3 reviewed evidence of semantic priming from pattern-masked primes, a condition eminently suited to implementing criterion 1. One can in principle determine a threshold SOA at which subjects are unable to identify or even detect the priming stimulus and then use it to check whether or not the prime has retained its potency. This procedure has been used successfully several times (Balota 1983; Carr et al. 1982; Fowler et al. 1981, Experiments 5 and 6; Marcel 1980; 1983a, Experiments 3, 4, and 5; McCauley et al. 1980). Unfortunately, these results offer either no evidence at all or, at best, equivocal evidence in favor of SA/CI. An alternative interpretation is simply that the visibility of the primes has often been much better in the priming trials than indicated by the threshold trials of these experiments.

With respect to priming by picture, this alternative interpretation has been clearly demonstrated by Purcell et al. (1983), who showed that Carr et al.'s (1982) and McCauley et al.'s (1980) subjects, because of their higher level of light adaptation in the priming than in the threshold trials, were able to consciously identify the prime more often in the former than in the latter case. In Section 4.3.2, I speculated about the possibility that other results (Balota 1983; Fowler et al. 1981, Experiments 5 and 6; Marcel 1983a, Experiment 4) might have been flawed in the same way. It should be borne in mind, however, that Carr et al. (1982) failed to show any priming effect with words under the very same conditions in which increased light adaptation had led to picture unmasking in the priming trials. It is an empirical question whether prime identification during the priming trials was indeed increased by the higher level of light adaptation compared with threshold trials in the experiments using undetectable priming words. Even if this did not occur, the fundamental issue would remain unaffected. There is a more general reason why primes could have been more often consciously identified than threshold trials indicate, namely, threshold establishment might have been inadequate.

Most of the problems that have plagued the classical psychophysics of threshold determination reappear in the experiments described in this section of the paper as if the better solutions proposed in the framework of signal-detection theory (e.g., Swets, Tanner & Birdsall 1961) were nonexistent. The major issue concerns the interpretation of the negative responses of the subjects. For example, the aim of the descending method of limits is obvious for the subjects who might adopt a very stringent criterion of detection or identification. Merikle (1982) has also criticized the fact that there were too few trials at threshold SOAs to allow for reliable evaluation of unidentifiability or undetectability of the priming stimulus. When more reliable methods of threshold determination are used, semantic judgments were no better than presence–absence judgments (Nolan & Caramazza 1982), and there was no Stroop effect (Cheesman & Merikle, in press). Moreover, in this latter case, the amount of priming was found to increase with the detection frequency of the color names. It is tempting to infer from this result that the reduced priming effect observed by Balota (1983) and Fowler et al. (1981, Experiment 5), but not by Marcel (1983a, Experiment 4), simply reflects lower proportions of identified primes in the masked than in the unmasked condition. There is a similar interpretation for the stronger facilitation effect found by De Groot (1983) with subjects who claim to be able to identify some primes during the priming trials compared with the weaker effect of subjects who claim to identify no prime.

At first sight, two results reported by Marcel do not seem open to the criticisms made above. He observed that there were qualitatively different priming effects with masked and unmasked primes (Marcel 1980) and that an increased repetition of a masked prime increased the amount of priming but not the number of prime recalls (Marcel 1983a, Experiment 5). It would be very incautious, however, to conclude from the pattern of results that the procedure used to make the prime unavailable to consciousness had in fact been adequate. It should be remembered that qualitatively different priming effects can be observed with conscious primes as well (see Section 1).

One can therefore conclude that the issue of SA/CI of pattern-masked stimuli is still unsettled in the face of current evidence. An alternative explanation in terms of conscious identification is at least as plausible, if not more compelling, in accounting for available data. The standard integration theory of pattern masking (see Section 4.1), which gives a coherent explanation of a large body of data and according to which priming by undetectable pattern-masked primes is hardly conceivable, is certainly not challenged by the findings reported so far. Whether it could be challenged by results obtained with the aid of improved methodology is still an open issue.

The criticisms raised in this part of the paper are mainly directed at the procedures used in existing work on semantic activation without identification or detection of the primes. However, the ability of backward pattern-masking experiments to address that issue remains unaffected by these criticisms. Better conditions of assessment of criterion 1 can readily be achieved by improving the methodology; some suggestions in that direction follow.

(1) Detection or identification thresholds of the masked primes should be determined by modern psychophysical methods. Priming effects should be measured at zero or near-zero values of d' but also at higher values of d', in order to give a fuller picture of the phenomenon (see Merikle 1982; Cheesman & Merikle, 1984).

(2) Light adaptation conditions should be equalized between threshold and priming trials. This amounts to saying that the structure of each kind of trial should be the same with respect to the temporal and energy characteristics of the visual events involved. This requirement is almost automatically met when subjects are light-adapted and the background field of the tachistoscope is bright. With dark-adapted subjects looking into a dark tachistoscope, threshold trials should involve a bright field of the same duration and intensity occurring at the same time as the target field in priming trials.

(3) Controls for the availability of the primes, or aspects of the primes, should be included in the priming part of the experiments. This can be achieved by asking subjects to recall the prime on each trial of the priming task. Mutual interference between prime recall and target response has been observed, however. Hence a better procedure would be to mix priming and control threshold

trials and to cue subjects concerning which task to perform on each trial.

5. General conclusions

This paper has proposed an analysis of the data relevant to the issue of SA/CI in three different fields of inquiry. In order to demonstrate the existence of such a phenomenon, a twofold condition, referred to as criterion 1, must be met. At the time of presentation of the critical stimulus, (1) there must be indirect measurable effects of semantic activation, and (2) the identity of the stimulus must be unavailable to the subject's consciousness, that is, he must be unable to give direct evidence of identification (e.g., through verbal report or any kind of voluntary discriminative response).

Conclusions specific to each topic can be found at the end of each of the three main sections of the paper (see 2.3, 3.3, and 4.4). A brief summary that provides answers to the three questions asked in the introduction of the paper is given below.

The first question was whether the experimental paradigms adequately satisfied criterion 1. The main problem is that the availability of the semantic content of the critical stimulus to awareness should be the same in the task devoted to the measurement of indirect evidence of semantic activation as in that devoted to the assessment of the extent to which the identity of the stimulus is reportable. The adequacy of the paradigm in implementing this requirement depends on how report performance is limited. At one extreme, dichotic listening tasks provide conditions in which performance is resource-limited. Whether the semantic content of the critical stimulus reaches consciousness depends on how much of the available resources are allocated to its processing, a factor that is partly under the subject's voluntary control through attention deployment. There is empirical evidence that subjects' distribution of attention is affected differently by the instructions corresponding to each task required by the assessment of criterion 1. It therefore follows that dichotic listening cannot provide conditions that satisfy criterion 1. At the other extreme, backward pattern masking (in which performance on the masked critical stimulus is data-limited) offers excellent opportunities for satisfying criterion 1. In this case one need not be preoccupied with eventual changes in the availability of the semantic content of the critical stimulus in each task, since increasing attentional focus or effort does not change the processing conditions. Parafoveal vision occupies an intermediate, but favorable, position since resource limitation plays only a moderate role compared to data limitation in the processing of the parafoveal stimulus.

The second question concerned the existence of data that satisfy criterion 1. No such data can be found in the field of dichotic listening for the reason just given. Positive data are very scanty in parafoveal vision, but several tentative positive answers have been put forward in pattern-masking investigations. In any case, it is premature to consider these results as evidence of SA/CI unless they are replicated under improved conditions.

The third question, concerned with the possibility of improving methodology, is therefore crucial. As suggested at the end of Section 4.4, the answer is definitely positive as regards pattern masking. The answer is much more elusive in the field of parafoveal vision, although in Section 3.3 some tentative improvements are suggested.

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Unconscious semantic processing: The pendulum keeps on swinging

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Holender suggests that because the theoretical framework is now available to interpret semantic activation without conscious identification (SA/CI) the pendulum has now swung too far in the direction of uncritical acceptance of this research. However, there is evidence that his review of the literature may reflect a swing in the opposite direction since there are (1) misinterpretations of certain studies on pattern masking and (2) failures to acknowledge highly relevant data.

Holender highlights a series of dichoptic masking experiments involving semantic priming by Marcel (1983a), Balota (1983), and Fowler, Wolford, Slade, and Tassinary (1981) that have purportedly demonstrated SA/CI. According to Holender, the latter two studies simply fall prey to a problem noted by Purcell, Stewart, and Stanovich (1983) concerning light adaptation because the target stimulus appears in the priming trials but not in the threshold-setting trials. This is clearly a valid point concerning the McCauley, Parmelee, Sperber, and Carr (1980) experiments. However, there are important differences between the McCauley et al. and the Balota and Fowler et al. studies that must be recognized if Holender's claims are to be evaluated.

First Holender states that there was a "dark intertrial interval" in the Balota and Fowler et al. studies; however, in both of these studies the intertrial interval was a return to a lighted fixation field that was adjusted to a lower luminance level but was clearly not dark. (Although Balota reported only that the fixation field was adjusted to lower luminance, the actual luminances followed those reported in Fowler et al.) McCauley et al. were restricted to presenting a dark fixation field because there were only three fields in their tachistoscopes. However, both Balota and Fowler et al. used four-channel tachistoscopes with one field used for a fixation field. Thus, subjects in both the threshold-setting and priming trials were looking at a lighted fixation field. This is an important issue because the dark adaptation occurring in the threshold-setting trials in the Balota and Fowler et al. studies was considerably less than in the McCauley et al. study.

Second, the discrepancy in luminance between the fixation field and the other fields was approximately .25 as large in the Balota and Fowler et al. studies as in the McCauley et al. study. Thus, the likelihood of the large impact of light adaptation by the targets was considerably lessened in the Balota and Fowler et al. studies.

Third, because of the Polaroid filters used to obtain dichoptic masking, the luminance levels of all fields except the fixation field were considerably lower in the Balota and Fowler et al. studies than in the McCauley et al. study. This is an especially important issue when one considers that the influence of a light-adapting stimulus not only is eliminated but can be reversed at lower luminance levels (Scharf & Fuld 1972). Thus, although Balota and Fowler et al. did not totally equate the luminance levels across the threshold and testing sessions, the potential for light adaptation's occurring in the priming trials is relatively minimal compared to that in the McCauley et al. study. In fact, even Purcell et al. did not cite this as a problem with the Fowler et al. study. (Balota's paper had not yet been published.) Finally, it is worth noting that Dagenbach & Carr (see their accompanying commentary) have recently conducted two experiments that replicate and extend Balota's and Fowler et al.'s semantic priming effects while strictly controlling the light environment during threshold-setting and test trials.

For the sake of this discussion, however, let us assume that the priming effects observed by Balota and Fowler et al. were due to subjects' consciously recognizing the primes, as Holender suggests. One would expect such conscious recognitions to have implications for other aspects of the data in these studies. There are two relevant aspects from the Balota study that Holender failed even to acknowledge.

First, consider the nonword data. In the suprathreshold condition, when the primes were words, response latency to the nonwords was 40 msec faster than for the neutral nonword primes (e.g., *xxxxx*). However, in the threshold prime condition, there was only a 4-msec difference between word primes and nonword primes for nonword targets. Neely (1977) has interpreted such suprathreshold word prime nonword target facilitation as reflecting a conscious semantic matching strategy between the prime and the target. One obvious reason that this matching strategy would not work in the threshold conditions is that subjects did not have the primes available for such a conscious match.

A second aspect of the Balota study also suggests that the primes were not being consciously recognized. That is, Balota included a later context-recognition test to further address the impact of the prime items. In this recognition test the targets were presented in a context either the same as or different from the one that earlier occurred during the priming trials (i.e., the primes). Since Holender suggests that the observed priming effects were due to some proportion of the primes being identified, let us consider the condition where the largest priming effect was found, that is, the condition in which most of the primes were presumably recognized. This condition is the long stimulus onset asynchrony (SOA) condition for the homographic targets. For these items the priming effect was 47 msec for the threshold prime condition and 31 msec for the corresponding suprathreshold condition. If subjects were consistently recognizing the prime items, as Holender suggests, one would expect some impact of these recognized items on a later context-recognition memory test. However, this recognition test yielded a 31% influence of same-versus-different context in the suprathreshold condition and a 0% influence of same-versus-different context in the threshold condition. Thus, if subjects were indeed recognizing the prime *inch* and this facilitated the lexical decisions to the word *yard*, it is unclear why this recognized word, *inch*, had no impact on how *yard* was stored in memory. That is, subjects were apparently just as likely to store in memory the meaning of *yard* referring to "measuring instrument" as the meaning referring to "back yard." Again, it is unclear why this would occur if subjects were consciously recognizing the prime word *inch*. In fact, one might argue that a degraded stimulus might produce quite a large impact on memory performance since the subject may be forced into capacity-demanding constructive processing of the degraded stimulus. However, there was no evidence of such an impact in this study, even though in all conditions where the subjects could see the prime word there were highly significant episodic context effects.

This commentary had two goals. The first goal was to point out differences between the McCauley et al. study and the Balota and Fowler et al. studies. These differences appear to be substantial enough so that light adaptation was not a crucial issue in producing the observed threshold priming effects. Obviously, the final answer must await futher empirical investiation. The second point is that in the Balota study, the threshold-setting task was only one of at least three converging lines of evidence (along with the nonword data and the recognition memory data) that appear to indicate that subjects were not consciously identifying the primes during the priming task. In this light, instead of prematurely accepting the research on SA/CI, Holender may be prematurely rejecting this research.

Through the looking-glass and what cognitive psychology found there

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Let us first examine what cognitive psychology has found in the laboratory and focus on a small portion of the considerable data scrupulously surveyed by Holender.

A number of investigators claim to have shown priming effects without the identification or even the detection of the pattern-masked prime. Criteria for (the absence of) identification or detection were of two kinds: verbal reports obtained from independent threshold determinations and verbal reports from the priming phase of the experiments, respectively. I will not discuss the factors that, as Holender suggests, might flaw reliance on independent threshold determinations, which would indeed appear to be open to criticism. Rather, I will consider whether it is true that the subjects' failure to exhibit verbal indications of identification or detection of primes should not be taken at face value as bona fide evidence of . . . *what?*

Obviously, absence of verbal identification or detection does not imply that events precipitated by the prime have come to be substantially extinguished; quite the contrary, semantic priming from unreported primes seems to constitute undeniable evidence for the persistence of such events and for their high-level integration, specific structure, and effectiveness. What may cause surprise, being counterintuitive, is the fact that these events, so close to the level of our pretheoretical conception of thought process, should not exhibit the whole complex of properties that shape the observable consequences of "supraliminal" stimulations, notably, accessibility to verbal report.

It would exceed the scope of this commentary to speculate as to whether and to what extent surprise is justified if one considers clinical syndromes such as conduction aphasia or deep dyslexia. Likewise, it would be beside the point here to consider possible explanations for the dissociation between semantic priming and verbal reportability. So let us turn to what seems to be the focus of Holender's worries.

The studies that constitute the field of his survey are a good illustration of the truism that the meaning of words such as *identification* or *detection* is relative: Verbally unreported primes *must* have been in some way identified in order to engender semantic priming. Holender, however, is not so much concerned with the verbal-versus-nonverbal aspects of identification as with a (nonparaphrastic) "conscious"-versus-"nonconscious" dichotomy. He rejects verbal reports as criteria for consciousness of priming stimuli without actually suggesting other criteria to support his strong contention that "on the basis of current evidence it is most likely that these stimuli were indeed consciously identified." To put it more precisely, he criticizes the criteria that have heretofore been adopted by several investigators and suggests a number of methodological improvements that might indeed contribute to a better insight of the subject. However, the disentanglement of d' and *beta*, which he rightly recommends, would not provide an answer to his question.

Suppose it were shown that verbal denial of the occurrence of a priming stimulus was due to a very conservative response criterion and that relaxation of the latter reveals some residual detection (which would not necessarily imply on this index rather than on the former one, for a judgment about the conscious quality of the events at issue?

To take an extreme case, who would seriously claim that Weiskrantz, Warrington, Sanders, and Marshall (1974), in finding that their patient could make correct guesses as to the location of targets he denied having seen, have shown that these targets were, after all, consciously detected? What we can get from d's in experiments with pattern-masked primes is an indirect measure of the strength of a nervous process; a measure that might bear interesting relations to priming effects but that cannot be offered for the investigation of consciousness as if it were the speculum par excellence.

The real problem is that, in its renewed interest in consciousness, psychology seems far from having satisfactorily defined its referent and from having developed a strategy for its scientific investigation. I am inclined to suppose that this scarce realized state of affairs is in part due to the circumstance that the strongest challenges to our intuitive concept of consciousness so far have come from the study of brain-lesioned people. So the investigation of semantic activation in conditions of more or less impoverished input, as well as Holender's critique, are most welcome.

It is all too natural to muse over the phenomenal experiences of subjects undergoing semantic activation experiments, and one wonders to what extent Holender is in fact referring to these experiences throughout the target article. However, I have tried elsewhere (Bisiach 1985) to argue that phenomenal experiences in themselves are unknowable and, in spite of possible postbehavioristic hopes, cannot be part of scientific explanations. Phenomenal experiences, indeed, differ from hypothetical constructs in at least two important respects. First, as far as we ourselves are concerned, they are not at all hypothetical. Second, the ascription of phenomenal experiences to other people is a mere guess we make by analogy with ourselves, since there seems to be no independent reason for their postulation. That is tantamount to saying that they are not a construct either. Thus, speculation over other people's phenomenal experiences seems to be question-begging projective introspectionism over something that stands beyond our reach and that has no scientific import. Unlike Alice, psychology can only see the smoke of the looking-glass house fireplace, not the fire itself.

Theories of visual masking

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Of the several methods of evaluating semantic activation without conscious identification, Holender identifies visual masking as the only valid one. It is therefore important to evaluate Holender's interpretation of masking. Unfortunately, several of his assumptions are questionable.

A fundamental distinction in visual masking is Kahneman's (1968) differentiation between type A, most effective for simultaneous target and mask, and type B, which is strongest at a stimulus onset asynchrony (SOA) of 60–100 msec and typifies metacontrast. There is little or no type B masking when SOA = 0. The differentiation of type A and type B functions is related to Turvey's (1973) classification of peripheral and central masking, which Holender accepts, but is distinct from it; the A-B contrast is defined in terms of psychophysically observed masking functions rather than a presumed anatomical locus of action. Holender does not differentiate between type A and type B functions and therefore overrates the importance of integration theoreis. Further problems come from weaknesses of the theories themselves.

Holender relies on the sustained-transient hypothesis of Breitmeyer and Ganz (1976), which has become a dominant theory of visual masking (Turvey 1980; Michaels & Turvey 1979; Breitmeyer 1984). Holender's Section 4.1, principle 2 uses the Breitmeyer and Ganz conception of intrachannel integration to account for peripheral backward pattern masking. These authors in turn rely on data from Fiorentini and Maffei (1970) to show that neurons with transient receptive fields are not involved in intrachannel masking. Fiorentini and Maffei measured the psychophysical threshold for a disc sinusoidally modulated in brightness, while a surrounding ring was modulated at the same frequency but with varying phases. At low temporal frequencies masking was optimal at a phase lead of about 45 degrees (annulua leads disc), while at high temporal frequencies optimal masking shifted to phase lags (disc leads annulus). Breitmeyer and Ganz (1976) interpreted this as a shift from a sustained-cell intrachannel mechanism at low frequencies to a sustained-transient interaction at higher frequencies.

When organized in terms of the latencies at which target and mask interact, however, the apparent shift in mechanisms looks quite different. The optimal phase angle at the low frequency of 1.5 Hz, for instance, was +45 degrees, corresponding to a latency (disc leads annulus) of 83 msec. At the high frequency of 8 Hz optimal masking occurred at a phase angle of −90 degrees, implying a shift from paracontrast to metacontrast mechanisms. The negative phase angle of 90 degrees can be equally well interpreted as a positive phase angle of 270 degrees, however, corresponding to a latency of 94 msec (disc leads annulus). The 11-msec difference in latencies is within the sampling error, for Fiorentini and Maffei sampled every 45 degrees, corresponding to every 83 msec at 1.5 Hz. Thus there was no shift in masking properties and no need to postulate a shift in masking mechanisms in the middle of the range of temporal frequencies studied. The Breitmeyer and Ganz hypothesis is called into question.

The Breitmeyer and Ganz interchannel inhibition mechanism (Holender's Section 4.1, principle 3) encounters both theoretical and empirical objections. The theoretical problems begin with the fact that the transient neurons' burst is too brief and the latency difference between transient and sustained neurons is too small to account for type B functions. Recognizing this problem, Breitmeyer and Ganz postulate two stages of transient-on-sustained inhibition, one at the lateral geniculate level and one at the cortical level. The two-stage interaction, coupled with the added complication of the transfer characteristics of two kinds of local inhibitory interneurons, makes it impossible to predict the behavior of the model by simple superimposition of histograms, as Breitmeyer and Ganz attempt to do. The two inhibitory mechanisms have different temporal characteristics and will be expected to interfere with each other under some conditions and resonate under others. The only way to determine the behavior of the theory is to model it mathematically and simulate it on a computer. This has never been done, so the Breitmeyer and Ganz proposal remains a hypothesis rather than a model.

Another way to evaluate the interchannel theory is to test its predictions empirically by recording from single neurons during metacontrast. The model predicts that optimal backward masking whould yield silence or near silence from cortical sustained-type neurons, because their firing is inhibited by transient neurons. Recordings from the monkey's primary visual cortex have shown, however, that a cell that responds to a target stimulus will always respond to a disc-ring sequence at the optimal latency for metacontrast (Bridgeman 1980). Monkeys were trained to respond to the brighter of two flashed discs and then were presented test trials consisting of two equally bright discs, one surrounded by a simultaneous ring and the other by a delayed ring. At the proper disc-ring latency, the disc surrounded by the delayed ring should appear dimmer owing to metacontrast. The percentage of behavioral responses to the simultaneous-ring side was a measure of metacontrast. When the same stimulus array was presented during single-unit recording, two bursts of firing were obtained. The first was unaffected by the mask, but the later burst, peaking at a latency of about 300 msec, was reduced in trials where simultaneously recorded psychophysical data indicated that the disc at the location of the cell's receptive field was masked by metacontrast. This pattern of results occurred in nearly all cortical neurons recorded. The prediction of the sustained-transient theory was contradicted, for the earliest peak of single-cell activity was not reduced by interchannel inhibition.

Thus the interchannel type B masking hypothesis of Breitmeyer and Ganz (1976), while supported by the data available at the time of its publication, fails when the hypothesized cortical responses are measured directly. Another neurophysiologically based model, which has been simulated, predicts unchanged initial firing under metacontrast conditions (Bridgeman 1971; 1978). Like other masking models at the neurophysiological level, however, it cannot handle practice effects and other cognitive influences on masking.

We can conclude that Holender begins his analysis of the masking literature in semantic activation with a flawed interpretation of a model that is itself flawed. Holender notes that if his interpretation of masking is correct, then the assumptions behind the use of masking to limit the processing time of visual information are invalid. Since his interpretation is not entirely correct, the validity of the masking paradigm remains an open question. To progress further, we need a better theory of masking.

Now you see it, now you don't: Relations between semantic activation and awareness

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Holender's analysis achieves two valuable ends. First, it identifies some important theoretical and methodological standards that ought to be applied in studying the relationship between semantic activation and awareness. Second, it effectively defends a skepticism that seems quite appropriate regarding extant claims for semantic activation without conscious identification.

The proper reply to Holender's analysis would be to produce data that meet the standards. We believe that we have done so. More importantly, we believe that the data, which are currently under review for publication, suggest a substantial reconceptualization of what the phenomena of "perception without awareness" or "unconscious perception" are revealing about the human information-processing system.

Recently, such phenomena – or such putative phenomena – have been taken as evidence that automatic perceptual encoding mechanisms can be dissociated from the conscious attentional mechanisms responsible for strategic control of information processing, and it has been assumed that subconscious priming reflects the autonomous operation of these perceptual mechanisms uncontaminated by the goals, intentions, or conscious ruminations of the perceiver (Dixon 1971; 1981; Fowler, Wolford, Slade & Tassinary 1981; Marcel 1980; 1983a; 1983b). Perception without awareness, then, has become a tool for building theories about automatic information processing carried out in the "cognitive unconscious" (Rozin 1976). We argue here that this view is wrong.

Consider the semantic priming paradigms that go farthest toward meeting Holender's first criterion: indirect evidence for semantic activation accompanied by direct evidence against conscious processing. Investigations using such paradigms – and analyses of them, including Holender's – have treated the judgment tasks that perceivers engage in during the threshold-setting portions of such experiments as if they varied in a relevant way along only a single dimension. That dimension is their stringency under the conditions of the priming portions of the experiments. Thus the use of detection as a threshold-setting task has been viewed as an attempt to attain a more stringent or preclusive state of unawareness that can be attained using whole report or forced choice recognition or some other judgment requiring content knowledge of the stimulus.

We agree that threshold-setting tasks can vary in stringency, but they can also vary in something else. Our results suggest that engagement in the threshold task constitutes training in how to go about trying to perceive the prime and that this training leaves its mark on prime processing and hence on how the prime influences processing of the target.

More specifically, while it appears to be true that subconscious words can produce semantic priming, important characteristics of the phenomenon are determined by the type of information about the prime toward which perceivers attempt to direct their attention. That is, the threshold-setting task is an experience that affects perceivers' strategies for attempting to gain information from the prime. These information-acquisition strategies appear to interact with and modify the operating characteristics of encoding mechanisms. The result is that different threshold-setting tasks, by inducing different information-acquisition strategies, cause different patterns of semantic activation and priming – even though the stimuli that do the priming do in fact remain outside awareness. There are conscious effects on unconscious perception.

We infer these effects of information-acquisition strategies from experiments that used several different types of threshold-setting tasks, partly as converging operations on perceivers' unawareness of the primes and partly in order to discover whether the nature of the threshold-setting task carried any consequences for priming aside from determining the prime-presentation conditions. In one experiment, we set two thresholds for subjects who had shown suprathreshold priming in an initial block of lexical-decision trials with primes presented unmasked for 500 msec each. The first threshold was always set using a detection task that required presence–absence judgments in which the identity of the word was unrestricted ("Was there a word of any kind or a blank field before the mask?"). The second threshold was either another type of detection task that restricted the identity of the word that could occur ("Was 'doctor' or a blank field present before the mask?"), a forced-choice recognition task ("Was 'doctor' or 'bread' presented before the mask?"), or a semantic similarity judgment ("Was the word before the mask more similar in meaning to 'nurse' or 'bread'?").

The unrestricted presence–absence threshold proved to be the more stringent of the two thresholds for the large majority of subjects. Among these subjects, those who had engaged in the two types of detection tasks enjoyed facilitative priming from semantically related primes relative to unrelated primes in a subsequent lexical-decision task when the primes were presented at the unrestricted detection threshold. This replicates the findings of Fowler et al. (1981) and Marcel (1983a). Those subjects who had engaged in the detection task followed by the forced-choice recognition task produced the same result. However, subjects who had engaged in the detection task and then the semantic similarity judgment showed *inhibitory* priming when primes were presented at the unrestricted detection threshold. Semantically related primes resulted in longer, not shorter, lexical-decision latencies than did unrelated primes.

When primes were presented at the second, less stringent threshold that had been established for each subject, all of these priming effects disappeared and none of the primes had any significant influence on target processing. The latter outcome, that somewhat longer exposure times reduce rather than increase priming when the primes are near threshold, is consistent with arguments made by Dixon (1971), and the specific finding that primes presented at a forced-choice recognition threshold fail to produce priming replicates the recent results of Cheesman and Merikle (1984; in press).

In a second experiment, we found that the same people could exhibit facilitative priming in one block of lexical-decision trials and inhibitory priming in the next block, if the first block was preceded by a threshold-setting task requiring detection judgments and the second was preceded by a task requiring semantic similarity judgments. This shift from facilitation to inhibition as a function of the immediately preceding task experience occurred despite the fact that in both blocks of lexical-decision trials, primes were presented under exactly the same conditions, at the detection threshold established initially.

Our interpretation of this result is that the detection task trains perceivers to attend mainly to visual information about the prime, whereas the semantic similarity judgments retrain them to try to attend mainly to semantic information. When attention is directed toward prime information *other than* the semantic code that must do the work of priming, activation occurs in the semantic code and spreads to related codes. This facilitates subsequent target processing. It might be that in this case the operation of semantic encoding mechanisms really is unaffected by the perceiver's attempts to retrieve prime information, since those attempts are directed at a different encoding system, and that the resultant priming does indeed reflect rather purely the automatic activational processes within the semantic system. This is an empirical question that awaits further study.[1]

In contrast, when attention is directed instead toward the semantic system, as perceivers learn to do in order to carry out semantic similarity judgments, quite different consequences accrue from failing to retrieve into consciousness the sought-for information about the prime. In this case, retrieval failure is accompanied by inhibition. Such inhibition does not occur when semantic strategies are applied to suprathreshold primes and hence succeed (see, e.g., Becker 1980; Neely 1977; Posner & Snyder 1975a; Tweedy, Lapinski & Schvaneveldt 1977), nor does it occur with subthreshold primes when perceivers have been previously trained to make nonsemantic judgments, as our other threshold-setting tasks indicate. Therefore the inhibitory semantic priming seems to depend in particular on the occurrence of a *failed* attempt to retrieve *semantic* information from nascent but not yet accessible perceptual codes. It would appear, then, that trying to introspect the meaning of a near-threshold word and failing makes it temporarily more difficult to gain access to the region of semantic memory in which that word and related words are represented. Thus perceptual encoding mechanisms may be subject to a content-specific refractory period after an unsuccessful retrieval attempt.

Such is our argument against the notion that subconscious priming isolates perceptual encoding mechanisms and exposes their automatic operation in pure form. For this argument to carry any weight, we must defend our belief that the results we have described were obtained under conditions in which the primes were actually subthreshold. There were several checks on this in our experiments. The threshold itself was the stimulus onset asynchrony (SOA) between the prime (typed in lowercase letters and presented for 10 msec via projection tachistoscope in a dimly lit room) and a pattern mask made of randomly jumbled letters and presented for 20 msec at an illumination slightly greater than that of the prime. In order to ensure that conditions of light adaptation were equated between the threshold-setting and priming tasks, the prime–mask combination was followed at an SOA of 1,500 msec by a 500-msec flash of light, corresponding to presentation of the target, just as it would be in the priming session (see Purcell, Stewart & Stanovich 1983).

Threshold was considered to be reached when the subject fell below 12 correct decision in a block of 20 trials in whatever judgment task was being used. In the detection task subjects knew in advance that words would be present on 50% of the trials and distributed their responses approximately accordingly. In all the various threshold-setting tasks, all subjects individually and each group of subjects as a whole produced d' values at threshold that were not different from zero.

After the threshold was set but before the priming session, subjects were presented with a series of 20 words at the threshold SOA and asked for a whole report. A response was required on every trial. No subject in any condition ever identified any of these words correctly.

Once the priming session commenced, two more precautions were taken. Subjects were instructed to report, after responding to the target, any primes they thought they might have recognized or could make a guess about. Occasionally a related prime was reported correctly. However, subjects were slightly but not significantly more likely to report a related prime on an *unrelated* trial than on a related one. This indicates that subjects were probably free-associating to the target rather than reporting actual introspections about the prime. Finally, at the end of the priming session subjects were once again presented with a series of 20 trials of threshold judgments in order to discover whether their thresholds had changed over the course of the experiment. The data on which we base our conclusions come from subjects who remained at chance in this threshold recheck (Dagenbach & Carr 1985).

NOTE

1. As Holender points out, the characteristics of subthreshold priming in studies that have used detection tasks to set thresholds are not identical to the characteristics of suprahtreshold priming that has been labeled automatic, especially with respect to the prime–target stimulus onset asynchrony needed to observe the effect. It remains to be seen whether the difference arises because (a) as Holender concludes, the subthreshold priming is not really subthreshold, (b) the subthreshold priming is in fact subthreshold but is subject to as yet unanalyzed strategic influences, (c) the subthreshold priming is automatic, but given the very small amount of stimulus information that gets into the system, it takes longer for priming to build up than with suprathreshold presentation, or (d) the subthreshold priming is automatic, but the suprathreshold priming heretofore thought to be automatic is subject to as yet unanalyzed strategic influences.

Electrodermal responses to words in an irrelevant message: A partial reappraisal

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Holender's overall case seems reasonably convincing, although there are places where he does tend toward splitting hairs, particularly in Section 4.3. Despite this, his insistence that criterion 1 be met unequivocally is harsh but justified. The idea of unconscious identification and processing, while hardly as extreme or potentially revolutionary as claims for extrasensory perception, is sufficiently counterintuitive to demand rigorous evidence, and I think Holender does demonstrate (though only just) that such evidence does not yet exist.

My main concern is with Holender's treatment of the dichotic listening evidence, and although he has some fairly harsh things to say about the Corteen and Wood (1972) and, particularly, the Corteen and Dunn (1974) study, I think his criticisms are fully justified. I am convinced that the subjects in the Corteen and Wood study did not remember much about the irrelevant channel after the procedure was completed, but I have never been sure that they did not have some momentary awareness of the critical stimuli at the time of presentation. The Corteen and Dunn study was designed to overcome that problem, but, as Holender points out, the situational demands meant that it, or any other similar experiment, was or would be poorly designed to resolve the question. In the experimental context strong emphasis was placed upon fast and accurate shadowing, and it was clear to the subjects that their shadowing performance was being recorded. In retrospect it seems obvious that almost all subjects would choose to continue with the shadowing, which, to them, must have appeared to be the more important part of the experiment. It is not outside the bounds of possibility that, among the many hypotheses intelligent subjects invariably generate during experimental sessions, some subjects came to the conclusion that the key-pressing instruction was merely a ruse to divert them from the serious business of shadowing. Different instructions, or even a different experimental atmosphere, might have dramatically changed the results obtained and made them closer to those of Dawson and Schell (1982). There seems to be no question that the dichotic listening paradigm is ill-suited to the study of unconscious processing, no matter how promising it may have appeared in the early 1970s.

This commentary provides a suitable opportunity to mention some little-known evidence relevant to the laterality effects referred to by Holender. Wood (1973), in a replication of Corteen and Wood's earlier study, presented the irrelevant material to one group of subjects in the left ear, as before, and to a second group in the right ear. The left ear group reproduced the earlier finding, but the right ear group produced no responses to the shock-associated words whatsoever. This finding bewildered us at the time, but ten years of intermittent speculation have suggested two alternative interpretations. Both are fairly simplistic but may bear some consideration.

The first relates to possible capacity demands. In an unpublished study I have established that right ear shadowing is significantly easier than left ear shadowing, using both errors and speed of presentation of the shadowed message as criteria of ease. This is probably because the right ear message projects more directly to the left hemisphere, which is dominant in linguistic processing. This would suggest that right ear shadowing might take up less attentional capacity than left ear shadowing, leaving more capacity for the irrelevant message. Shadowing in the left ear, being more difficult, may take up almost all the available capacity so that the irrelevant message is disregarded. This interpretation argues against unconscious processing of the irrelevant channel, because attentional capacity should be unimportant if unconscious processes are involved.

The second interpretation involves speculation about the source of electrodermal activity. If electrodermal responses are activated contralaterally then perhaps recording from the left hand would detect responses only when the irrelevant message, containing the shock-associated words, was projected primarily to the right hemisphere. Unfortunately this interpretation is difficult to evaluate because of the considerable confusion over the source of electrodermal responses (Hugdahl 1984). Currently there is evidence to support almost any pattern of laterality effects, and further speculation in this direction awaits clarification of the psychophysiological findings.

A history of subliminal perception in autobiography

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In the fall of 1957, I was a University of Michigan freshman attending an introductory seminar in psychology led by Robert Hefner. A book called *The Hidden Persuaders* had just appeared, and we spent more than one afternoon session arguing about the possibility of subliminal perception and perceptual defense. The really heavy evidence bearing on the argument was an alleged demonstration in which the word *popcorn* had been flashed briefly on a movie-theater screen (subjects were certainly dark-adapted, by the way), causing a mindless stampede to the candy counter. Personally, I resisted the notion from the start. Even if I had actually read the book we were discussing, I would probably not have been any more sympathetic. My objection to subliminal perception probably did not come from my Midwestern upbringing either, but rather from my enduring style as a knee-jerk skeptic and general intellectual spoilsport. It sounded ridiculous to me that stimuli below the level necessary for detection could affect our behavior.

Nothing in my subsequent undergraduate or graduate training at Michigan shook that attitude. There was talk, in a perception course taught by Max Schoeffler, of signal detection theory, but although I thought I understood how the model worked I did not appreciate that if you outlawed the concept of the threshold you changed fundamentally the ground rules for talking about subliminal perception. In a few years Robert Zajonc published his perceptual-defense study on the tachistoscopic recognition of dirty and clean words, he showed that the reliable effects were all on the response-emission side rather than the stimulus-registration side (Zajonc 1962). I think I managed to work this experiment into lectures in every single class I taught thereafter for 15 years. Still later, Zajonc (1980) made it clear that he himself had actually been pulling for the other outcome. Whereas I had consistently ignored a statistically unreliable trend for an effect on the stimulus side in the 1960 experiment, he once told me he had always believed it.

Soon after I arrived at Yale in 1965, I met Matthew Erdelyi, whose biases about perception and science were opposite to mine. We began a series of arguments that we have both enjoyed, sporadically, ever since. I was delighted, and he dismayed, when his dissertation research (Erdelyi 1970) on perceptual recovery – a problem related to unconscious perception – turned up a clear criterion effect with no sensitivity effect. Like Zajonc, he was unconvinced, and he published an important review subsequently (Erdelyi 1974) affirming the reliability of various "new look" phenomena.

Meanwhile, in about 1967, I used to eat lunch frequently at a local pub with Wendell Garner and John Morton. Being neither Midwesterners nor obsessively skeptical, they patiently convinced me that to claim subliminal perception is *impossible* is simply stupid. Their main reason was not that they trusted any particular piece of evidence for it. Instead, they stressed how the enormous power and complexity of perceptual models then emerging – such as Morton's logogen model – could easily supply machinery for making it happen. My response was the equivalent of, "Well, OK, it's not impossible; but there still isn't any good evidence for it."

Why, then, did I undergo a thorough conversion in the early 1980s? I like to think it was not a change in intellectual style resulting from age and mellowness. The publication of some apparently unimpeachable experiments in our toughest journals was one factor. (I never thought of the problem of dark adaptation. Even the best of these recent articles clearly stands impeached by Holender's review, though we should remember that, as in federal procedures, impeachment does not constitute conviction. I hope people are busy doing the *correct* comparisons, which seem extraordinarily straightforward, eliminating confoundings with the visual adaptive state.) A second factor was my finally coming to terms with the model of Rumelhart and McClelland (1982). Their assumptions clearly allowed lower levels of perception to be terminated, with higher levels spared, a property they used to good effect in explaining the word superiority effect found after pattern masking. It seemed only a small concession to carry their interpretation of this "respectable" phenomenon up a level higher in order to have meaning spared with the lexical level damaged. A third factor was my sympathy with Marcel's (1983b) general theoretical position separating in a radical way our conscious experience from the initial mechanisms of perception. Again, as a teenager in Ann Arbor, I had embraced the view that people have no privileged access to their own mental processes. I like to use the example of kidney function as another bodily process, like cognition, about which we have no direct access. Try as we may, we cannot gain conscious access to a formant, in the perception of real speech, for another example.

Thus, I changed my mind. Everyone knows that converts are the most zealous of partisans. Accordingly, I inspect the evidence and arguments adduced by Holender with even more narrowly squinted eyes than I did the original experiments that initiated this whole episode. Biases toward perception aside, we can all be grateful for the empirical focus Holender has brought to these issues. It almost makes one believe in crucial experiments again.

On private events and brain events

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It is a great pity that Holender's target article did not appear in time for inclusion in those chapters of my two books (Dixon 1971; 1981) which advanced possible explanations for the intense, prolonged, and sometimes ridiculous controversy over whether or not people can be affected by stimuli of which they are unaware. For whatever else it does, his critique exemplifies the quite extraordinary efforts that some people will make to disprove the claim that brains may discriminate events of which minds remain oblivious. For *his* efforts in this direction, Holender should be congratulated.

Considering all this hard work, however, the results are sadly disappointing. Even though he draws the most curious conclusions from some of the research he mentions, even though he omits mention of research which produced results inimical to his thesis (e.g., Henley & Dixon 1974, and the successful replication of this by Mykel & Daves, 1979) and even though he uses the most ingenious special pleadings, the general conclusion Holender draws – that we cannot be sure from any of the three research areas he reviews that subjects were in fact unaware of the stimuli which were evidently analysed semantically – is neither earth-shattering nor terribly convincing.

Let us look at this a little more closely. Of course Holender is in one sense absolutely right: We cannot be *certain* that the subject was unaware. There is no possible experiment, there are no possible paradigms that can *prove* unawareness for the very simple reason that awareness and unawareness are private events.

Being a truism, therefore, this conclusion does not depend for its acceptance upon Holender's painstaking analysis. However, as a goad to proponents of unconscious perception his monograph is not without value, if only because it invites discussion of some rather basic issues. For example, accepting Holender's truism, how *should* we proceed when judging the validity of the research he castigates? At best we can only end up with a probabilistic judgement based on answers to such questions as: Which is more likely, that the subject was telling the truth or that he was lying when he said he was unaware of certain preceding events? Similarly, if a subject says he was not conscious of a particular stimulus, which is more likely, that he is speaking the truth or that he *was* aware of the stimulus but immediately (or within a short time) forgot that he had been aware?

Now bearing in mind the truism that the truth or falsity of the assertion "I was *not* aware" can only be known to the subject (because awareness is a private event), how are we to judge the likelihood of negative answers (i.e., "I was unaware") being correct? To arrive at the best possible probabilistic judgement regarding this matter, the following considerations are particularly relevant:

1. Most people most of the time probably prefer telling the truth to telling lies. Our belief in this assumption is mandatory for most psychological experiments. If, for example, we present a complex array of unrelated items and then ask, Did you notice the letter *X*?" (Say) and the subject replies "No", are we not right to assume that he is telling the truth? Unless we assume veracity on the part of our subjects, none of those psychological experiments which rely upon verbal indicators are worth carrying out.

2. It would be very difficult, if not impossible, to *prove* that people may have a conscious experience which they may then immediately forget. Devotees of Freudian theory might claim that this could happen owing to repression of some traumatic experience, but it seems inherently unlikely (though not of course impossible) that in the experiments cited by Holender, the apparently unheard material in the other ear was so traumatic that the subject immediately had to repress this horrific experience. [See also *BBS* multiple book review of Grünbaum *Foundations of Psychoanalysis, BBS*, 1986.]

In the study by Corteen and Dunn (1974) in which subjects were encouraged to signal awareness whenever they consciously heard anything, the possibility of forgetting over time was, of course, ruled out. Holender copes with this difficulty by suggesting that this was a "double bind" situation that put the subject in a "conflict" and that it was this conflict that prevented him from reporting stimuli on the "other" ear. Now we know that double binds can be unpleasant and even, so it has been suggested (Bateson, Jackson, Haley & Weakland 1956), a possible factor in the aetiology of schizophrenia; but is it really likely that Corteen and Dunn and their subjects were simulating interactions between schizophrenogenic mothers and their offspring? I hardly think so. Personally, I find Holender's use of the terms *double bind* and *conflict* in this context sensational, gratuitous, and basically misleading, if for no other reason than that, had Corteen and Dunn's subjects been in sufficient conflict to produce the behavioural effects on which Holender pins his argument, the autonomic consequences would almost certainly have impaired any chances of picking up discrete electrodermal responses to individual words (i.e., the galvanic skin response base would have been inordinately high). There was no evidence of this.

3. One of the criteria for assuming that unconscious perception has occurred is the emission of responses that, though semantically linked to the stimulus, differ qualitatively from those that the subject would give if the stimulus had been supraliminal.

Holender disputes the force of this criterion by citing priming studies (Neely 1977; Posner & Snyder 1975a; 1975b) in which "qualitatively" different effects can be produced in the processing of consciously identifiable stimuli. Unfortunately, the "qualitative" differences to which he refers were that sometimes the prime was inhibitory, at other times facilitatory. Such differences to which he refers were that sometimes the prime was inhibitory, at other times facilitatory. Such differences are along a single continuum of effects and as such irrelevant to *my* criterion of qualitative differences (see Dixon 1971; 1981). In case this is anything other than crystal clear, consider the analogous case of two taps, one of which emits water that is sometimes hot and sometimes cold, while the other sometimes emits petrol but at other times emits tomato ketchup. While the behaviour of the first tap is presumably due to the state of the boiler, that of the second, which shows real qualitative differences in its performance, implies the operation of some far more complex underlying mechanism. The argue from one to the other is, to say the least, misleading.

It is, incidentally, most unfortunate that Holender was unable to deal with the recent experiments on semantic activation without conscious identification by Groeger (1984a; 1984b). These studies, involving both masking and dichotic listening, not only illustrate but satisfy the criterion of qualitative differences. In a typical study the subject is asked to complete the following sentence, arriving at supraliminal intensities in one ear: "She looked —— in her new fur coat." For completion he has to choose between the two words *smug* and *cosy*. Simultaneous with the incomplete sentence in one ear he is presented with the cue word *snug* in the other ear. The crucial finding from this research is that if the cue is presented at threshold intensities the subject tends to select *smug* as his completion (i.e., he is influenced by the consciously heard *structure* of the cue), but if the cue is presented well below threshold he selects *cosy* as his completion. In other words, not only does he show qualitative differences as a function of unconscious versus conscious reception but also, in the latter case, shows evidence of semantic analysis without awareness.

4. The last point I would like to make in connection with the probabilistic judgement as to whether or not subjects were unaware in the experiments impugned by Holender concerns the prior likelihood of unconscious perception having occurred in these studies. For a detailed assessment of this issue, the reader is refered to the extensive literature reviews of relevant researchers (Carr & Bacharach 1976; Dixon 1971; 1981; Dixon & Henley 1980; Erdelyi 1974; Smith & Westerlundh 1980). From looking over the hundreds of experiments covered in these and comparable collections, the unprejudiced reader may well be influenced by two facts. The first is that no less than 11 different and relatively unrelated areas of research (several of which involved experiments who were not interested in proving unconscious perception) have as their common denominator the finding that the brain can be affected by stimuli of which the mind is unaware. The second is that the occurrence of unconscious perception is not only physiologically possible but is actually predictable from what is now known about relationships between cortical processes, reticular activation, and awareness.

My final point, which has been made many times before, is really a matter of common sense. Given the enormous discrepancy in information-processing capacity between that of the brain and that of the very limited channel of conscious awareness, would it not be surprising (if not actually astonishing) to find that we can never respond to the meaning of stimuli of which we are unaware?

Anyway, armed with these various considerations, the unprejudiced reader of Holender's target article, which states that proponents of semantic analysis without awareness probably drew the wrong conclusions from their studies, may perhaps come away with two impressions. The first is that it is not investigators of dichotic listening, parafoveal vision, and masking who have drawn the wrong conclusions. The second is that the most interesting phenomenon to which Holender's paper draws attention is the extraordinary antipathy some people still have toward the idea that we might be influenced by things of which we are unaware. Would it be putting it too strongly to say it reminds one of the scepticism of "flat earth theorists" when confronted with the alarming theory that the world is round? It is probably true to say that such resistance does not spring from questioning such tiresome observations as that if you keep going west, you will eventually arrive back at where you started from, but rather from a deep-seated anxiety that if, despite common sense and the evidence from viewing only very limited extents of observable terrain, one were to accept the "rotundity" hypothesis then, in the event of being wrong, one would have only oneself to blame for toppling over the edge!

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Experimental indeterminacies in the dissociation paradigm of subliminal perception

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In the belief that consensus on the subliminal perception hypothesis has swung from the over skeptical to the over enthusiastic, Holender undertakes a detailed methodological examination of three contemporary literatures on the topic (a limited, if important, sample). He shows these to have more or less serious methodological problems and concludes that our newfound enthusiasm for subliminal perception "may be premature" though not, perhaps, beyond redemption. Holender's critique, which is incisive as well as fair, is a much needed contribution, which will doubtless help shape the methodological agenda in this area over the coming years.

In this commentary I wish to focus on an aspect of methodology that shades over into theory: the problem of experimental indeterminacy.

The three lines of research on subliminal perception that Holender deals with all fall within the *dissociation paradigm* of the unconscious, one of two basic classes of evidence for the existence of unconscious processes (Erdelyi 1985). The dissociation paradigm involves the situation where the subject is (purportedly) unaware of some stimulus input but nevertheless evidences availability of the input. More formally, the paradigm involves an observed discrepancy between two concurrent indicators of information,

$$\varepsilon > a$$

where a is an indicator of information accessible to awareness or consciousness and ε an indicator of information available to the subject. The discrepancy between indicators is taken to index a mental dissociation such that information that is available to part of the mind is inaccessible to (and is therefore dissociated from) consciousness. Since the metric of availability/accessibility is not typically the same for the two indicators and is therefore not directly contrastable, the dissociation paradigm is usually realized in the special case where the indicator of consciousness, ε, is some positive value. Thus:

$$\varepsilon > 0 | a = 0.$$

This version of the dissociation paradigm subsumes Holender's criterion 1 and 2 for subliminal perception (from Dixon 1971; see also Eriksen 1958; Goldiamond 1958).

It is extremely easy to generate experimental data that superficially satisfy the dissociation paradigm. For example, in what may be the first modern subliminal perception experiment, Pötzl (1917/1960) demonstrated that stimulus features (presumably) undetected by the subject ($a = 0$) nevertheless show up in the content of the subject's dreams ($\varepsilon > 0$), a finding that has been extensively replicated with dreams, daydreams, free associations, free imagery, and the like (see Dixon 1971; Haber & Erdelyi 1967). The methodological problems of the paradigm always center on the adequacy of the indicators, especially the crucial $a = 0$ part. Thus, is it the case in the $\varepsilon > 0 \mid a = 0$ situation that the observed $a = 0$ actually represents null awareness, or has the experimenter merely failed to index awareness? It will be noted that this is the great thematic issue coursing through Holender's critique – and, indeed, through most critiques of subliminal perception.

I was surprised at Holender's concluding suggestion, which seems to run against the thrust of his review, that it might be acceptable to obtain "zero or near-zero values of d'" for a. With near-zero a levels it is an easy matter to get hefty $\varepsilon > 0$ effects (see Erdelyi 1970; 1972; Haber & Erdelyi 1967). Indeed, if the "limen" of consciousness is defined in the usual statistical sense (e.g., as, the 50% detection level), then "subliminal" perception exists *by definition*. For the phenomenon to be surprising, subliminality must be defined in an absolute sense, as in $d' = 0$, where detection is at chance level. Hence the problem with any subliminal perception effect based on the $\varepsilon > 0 \mid a = 0$ condition ultimately hinges on the $a = 0$ component. Perhaps a more practical approach would be to transpose to the general $\varepsilon > a$ case by using a common metric for both a and ε (e.g., d'), which could show, even where $a > 0$, that more information is discriminated by the perceiver than is discriminated merely consciously.

Unfortunately, the problem is not only quantitative but qualitative as well. We rely on *indicators* of registration or awareness because, obviously, we cannot directly assess contents of mental subsystems. Indicators, however, are not only more or less sensitive but also more or less appropriate. There has been remarkably little discussion of this obvious problem, perhaps because our attention has thus far been riveted on the merely quantitative issue. It is well known that recognition indicators of memory typically yield information estimates greater than those of recall indicators. Yet recognition – recall discrepancies have not usually been treated as instances of subliminal perception or memory. Why not? Probably because at an intuitive or common-sense level both indicators of memory are taken to indicate information accessible to consciousness, a strange situation since now we seem to have a dissociation paradigm operating within consciousness ($a \neq a'$). It becomes evident that we must worry not only about making sure that $a = 0$ but also determining *which* $a = 0$. Put differently, how do we decide that a particular indicator of availability, ε (whether it involves galvanic skin respones, primed biases, free associations, dreams, affect discriminations, perceptual fluencies, or whatever), is not simply another indicator of consciousness, a? And what is one to make of the case where the "same" indicator, such as word-completion (Graf, Squire & Mandler 1984) yields substantially different values depending on whether subjects conceive of the task as a memory test (which results in lower scores among amnesics) or as a fragment completion exercise? I suggest that consciousness is no clearer a construct than, say, intelligence and that any given indicator of awareness is no more incontrovertible than a particular IQ index of intelligence.

Even if we wished to sidestep the construct validity issue of indicators of awareness, another experimental indeterminacy arises, as Holender makes clear, from the fact of forgetting. In dichotic listening, the subject may give evidence of semantic registration of information in the rejected channel ($\varepsilon > 0$) but be unable in a subsequent test of memory to register any conscious access to this information ($\alpha = 0$). This null memory, however, need not mean that the information was not accessible at the time of the input; it may simply have been forgotten in the interim. For this reason Holender concludes that the dichotic listening procedure cannot in principle provide the data that satisfy the paradigmatic condition for the demonstration of subliminal perception. What is interesting is that Holender does not dismiss the remaining procedures on the same ground, since, as he himself points out, no detection report can be simultaneous with the stimulus and so the same possible objection may be raised for all of them. Thus, even if all the other issues concerning the Marcel (1980; 1983a; 1983b; Marcel & Patterson 1978) approach, the most promising of the three, could be resolved, it would still not necessarily follow that unconscious priming was being demonstrated. One could advance the story that the pattern mask does not stop the information from being accessed to consciousness but merely obliterates information already (fleetingly) in consciousness, and that *it is these fleeting blips of consciousness* (psychological versions of physics's "virtual particles") that initiate the processes of priming. This same type of argument could be made (though it would strike many as sophistical) for a variety of cognate phenomena, including perceptual defense, defense processes in general, the ability of free associations to reveal currently inaccessible information, and so forth.

Forgetting is not the only memory problem that must be taken into account. It is now well documented that with processing effort, accessibility may actually *increase* with time, so that the course of memory with time may be hypermnesic as well as amnesic (Erdelyi 1984). Thus, it cannot be assumed that an indicator of awareness that is initially null ($d' = 0$) may not be positive with further effort. Indeed, since both a and ε might differentially increase or decrease over successive tests, any particular $\varepsilon > a$ inequality might change and even conceivably flip-flop over time. It no longer seem's sufficient to be concerned merely with controlling response bias effects; the amount of processing time and effort invested in each indicator – the subject's "processing bias" – is also a likely determinant of a and ε.

I have underscored some of these experimental indeterminacies to suggest that methodological refinements are not in themselves likely to resolve the basic conceptual issues that operate in the dissociation paradigm of subliminal perception, except possibly in the negative sense. If more careful measures of subliminality (where $d' = 0$ for a) undermine activation effects, then we need not, of course, be concerned with these imponderables. If, however, as seems likely, it will still be possible to show absolute subliminal effects ($\varepsilon > 0 \mid a = 0$), we must be prepared to encounter ultimate limits in the conceptual resolution that our current methodologies afford us.

Identification, masking, and priming: Clarifying the issues

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Holender has provided a broad review of work on semantic activation effects under conditions where subjects often cannot explicitly identify stimuli. He concludes that although other methods (e.g., dichotic listening, parafoveal visual presentations) may be capable of meeting his criteria for unconscious semantic activation, it is backward masking that offers the best conditions for so doing. In discussing results produced using masking techniques, he argues that threshold procedures have been inadequate, that identifiability of primes has not been fully assessed, and that in some cases the results are open to criticism that changing levels of dark adaptation of subjects between the threshold and priming phases of the experiment may have rendered primes more easily available than would have been expected from the threshold trials. All these criticisms carry weight, and the methodological improvements suggested by the author must be taken seriously.

However, we feel that it is also the case that Holender's arguments about masking rest on a number of theoretical preconceptions that are not necessarily valid and that in fact tend to obscure the main issue. For instance, consider Holender's assertion that semantic activation without conscious identification (SA/CI) is contingent upon masking operating by a process of interruption rather than integration. This is surely irrelevant. Even if the "multilevel integration theory" of masking is correct, and even if there is therefore "no place in the system where a legible representation of the word is available," it remains possible for semantic effects to occur due to stimuli partially activating their stored representations. Holender is also obscure on the issue of what constitutes an interruption as opposed to an integration theory of masking. He cites McClelland and Rumelhart (1981) as providing an interruption account, presumably because they assert the involvement of inhibitory processes (though see also Breitmeyer & Ganz 1976). However, as we understand it, masking in the McClelland and Rumelhart model acts by adding noise to selective levels of representation. This could as easily be classed as integration theory as an interruption theory.

Holender considers masking to an appropriate tool for investigating SA/CI because it produces solely data-limited conditions. Again, we feel that this misunderstands masking effects. There are now several good pieces of evidence indicating that the report of briefly presented stimuli under masking conditions is resource-limited, in the sense that it is constrained to operate on only one perceptual object at a time (e.g., Duncan 1980; see Humphreys, in press). In fact, it may be that evidence for semantic activation without explicit identification can be found under masking conditions precisely because masking precludes the identification of more than one of two briefly presented stimuli. As Holender notes, we have produced some data relevant to the issue of whether priming effects occur under masking conditions where subjects fail to explicitly identify primes. For instance, using a four-field masking procedure, primes and targets could both be letter strings, the prime could be a letter string and the target a row of *X*'s, or the target could be a letter string and the prime a row of *X*'s. On each trial subjects were asked, prior to reporting any letters that they could, to discriminate whether two letter strings or one letter string plus a row of *X*'s were presented. Under conditions where subjects could not report primes at any better than chance (when the target was a row of *X*'s) and where subjects could not discriminate whether the prime was a letter string or a row of *X*'s, we found reliable repetition and orthographic priming effects (Humphreys, Evett & Quinlan, in preparation).

These data clearly suggest that priming can occur without explicit identification of primes. But how do the data relate to Holender's criteria for SA/CI? Unfortunately, we are not sure. The most relevant criteria (i.e., 1 and 2) are distinguished on the basis of whether direct evidence of stimulus identification is obtained at the time of presentation or retrospectively. Although our data are obviously more concerned with immediate stimulus identification than with, for instance, reports of the semantic content of a nonshadowed message following a primary shadowing task, the distinction between retrospective and immediate report is difficult to uphold since all reports of masked displays will be to some extent retrospective. Also, we do not know whether subjects could detect the presence of primes if that were their primary task. The important point here is that to ask subjects to detect the presence of primes would be to change the nature of the task. Thus any differences between a detection task and the identification task would not tell us how subjects are performing in the identification procedure. Indeed, it may be that Holender's criterion 1 can never be fulfilled using a procedure in which the effect of the masked stimulus is assessed indirectly while at the same time subjects are asked to base detection responses on primes. In such a case the criterion cannot be refuted even when subjects fail to detect primes, since it can always be argued that the prime was rapidly forgotten.

With regard to the four-field masking procedure, we would also like to note that it is not surprising that small modifications in the procedure generate reports of the first letter string. Altering the sequence and durations of the stimuli will obviously change the parameters of the masking function; there is nothing mysterious in this.

Given our arguments about the difficulty in using direct measures of conscious identification, the best criterion for SA/CI would seem to be that of qualitative differences in performance with conscious and unconscious stimuli. There are indications that priming effects with masked stimuli are qualitatively different from those with unmasked stimuli. For example, Cheesman and Merikle (in press) have shown differential effects of the proportion of trials on which primes and targets are related; Marcel (1980) has shown different effects of using an ambiguous word prime; while Forster and Davis (1984) and Humphreys, Quinlan, and Besner (1983; see Humphreys, in press) have shown differences in the time course of repetition effects. It can be pointed out that different patterns of performance may obtain with unmasked stimuli; however, the point is to show qualitative differences due solely to the introduction of a mask.

As a final issue, we suggest that it is possible to make a distinction between being aware of the presence of a stimulus and being aware of its identity when subjects must choose from a wide set of possible responses. Our own results indicate that priming effects can occur when subjects are not aware of the identity of the prime. Furthermore, these priming effects are qualitatively different from those found when primes can be identified, indicating the influence of different representations and processes. Such differences are of theoretical interest in their own right. The question of whether priming effects occur when subjects are not aware of the presence of the prime is quite separate and bears on other theoretical issues. These questions should be considered independently.

Knowing and knowing you know: Better methods or better models?

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One need not be a cognitive psychologist to find the notion of unconscious mental activity compelling. In my undergraduate classes, students inevitably raise the "subliminal perception" issue, providing their own anecdotes, as soon as pattern recognition and attention are discussed. I point out that although the evidence is controversial, there is nothing *in principle* wrong with the notion of such dissociations, which are in fact very much a part of contemporary psychological theory.

The notion of an unconscious but active component of mental life predates Freud and psychoanalysis in formal psychological thinking (see Klein, 1977, for a nice historical perspective), and the tradition of interest in subliminal perception and perceptual defense certainly predates the "cognitive revolution." But as Holender points out, the componential nature of most recent cognitive theory, and in particular the popularity of automatic-process, late-selection theories of attention, has made the search for the dissociation between semantic activation and conscious identification more alluring, and acceptance of evidence favoring the hypothesis apparently less critical. The emergence of the "modularity" metaphor in cognition, with autonomous, "informationally encapsulated" units for analysis of various aspects and levels of sensory events and for orchestrating responses to them will hardly slow this trend (e.g., Fodor 1983; Forster 1979; cf. Marshall 1984). It is easy to imagine such a system producing not only lexical activation but more complex semantic representations without concomitant awareness. [See BBS multiple book review of Fodor 1983 in *BBS* 8(1) 1985.]

Since Holender presents a critique more of the effort to discover evidence for semantic activation without conscious identification (SA/CI) than of the rationale for expecting such a dissociation, his paper is likely to stimulate even more experimental work along similar, if better controlled, lines rather than a rethinking of the model of mind from which it follows. So after a few comments on methodology, I will consider an alternative "dissociation" that may be empirically more accessible, and theoretically more fruitful, than SA/CI.

At the methodological level, Holender has provided a thorough and sobering demonstration of both the insufficiency of presently available evidence for SA/CI and the frequent overwillingness to accept this evidence. One of the most important themes of this critique was the confusion between and the ultimate inseparability of criterion 1 and 2 – that operationally we must equate consciousness of an event with subsequent discriminative responses such as recall (see Section 1). It was the realization of this that led Goodman and me (Fischler & Goodman 1978) to interpret our evidence for priming without prime report in our Experiments 2 and 3 (which was at least internally reliable, despite the small sample of trials; see Section 4.3.1) in the more modest sense of automatic activation: Since the prime was not sufficiently attended to establish prime report, it was unlikely to be the source of voluntary expectations about the target word. In any event, Holender's point seems unassailable.

A second comment about the adequacy of methods concerns the use of physiological measures to explore SA/CI. The use of electrodermal response (EDR) as a marker of activation during dichotic listening is criticized on several grounds (see Section 2.2.2), including the uncertainty of establishing and maintaining the conditioned EDR itself. Other physiological measures of cognitive activity, in particular event-related brain potentials (ERPs) and pupillary dilation (PD), may provide better means of demonstrating SA/CI. ERPs seem ideally suited for investigating the amount of semantic processing of and resource allocation to "unattended" or briefly presented words. Several studies already reported provide largely negative evidence for semantic processing of stimuli on unattended channels, including the presence of a PD to nontargets in an attended but not in an unattended ear (Beatty 1982, p. 284) and the presence of an enhanced late positivity in the ERPs to targets versus nontargets in the attended but not in the unattended ear (e.g., Hink, Hillyard & Benson 1978).

The ERP in particular has the advantage of far greater temporal resolution than the EDR and does not rely on a conditioned emotional response to produce differences that reflect semantic processing. In one case, for example, we have found differences in ERPs to true and false sentences that were independent of whether subjects judged the truth value of the sentences veridically, intentionally misresponded to some sentences, or made no response at all (Fischler, Childers, Achariyapaopan & Perry, in press). Moreover, the ERP may be analyzed into components that are independently affected by different aspects of an event, making it a more "diagnostic" (see Wickens 1984b, chap. 8) measure of concurrent but dissociable processes. One drawback is the need in most cases for averaging across trials to obtain acceptable signal strength, which makes it more difficult to use the ERP to study events on discrete trials. Still, it seems promising as an unobtrusive measure of (a) whether a stimulus event was attended and (b) the extent to which its processing is similar to that of consciously identified stimuli (see criterion 3, Section 1).

Suppose that even more careful work with improved methods still fails to show the predicted dissociation. What are the implications for cognitive theory? We should already be suspicious that the SA/CI effect is not exactly overwhelming and, if it exists at all, may depend on subtle experimental conditions to demonstrate it. We might simply say that the link between the output of the lexical analyzer and the General Problem Solver, say, in Forster's (1979) model – which seems most closely identifiable with the ability to make conscious discriminations – is automatic and inevitable. Clearly, this removes at least some of the force of the original distinction.

It is possible, however, that we are looking for the wrong dissociation in these tasks. An alternative path is suggested by an analogy between perceptual and memory tasks. The reduced report rate for the critical but low-priority words in a variety of dual tasks described by Holender (see Section 3.3) appears to be a perceptual version of output interference (Tulving & Arbuckle 1963) in retrieval of information from memory. In considering other possible ties between perception and memory, we might consider the dissociation between knowing, or remembering, and feelings of knowing, or the confidence that remembering has occurred. This dissociation was clearly described by William James and is easily documented in the laboratory. With regard to perception, our intuitions at least about what constitutes conscious experience are closely tied to the sense of coherence of objects and events to particular places in space and time. It appears that this subjective experience is dissociable collectively from the semantic analysis and accuracy of report that is determined by this analysis.

A personal anecdote may or may not help clarify my point. My own interest in componential analysis began as a subject in a fellow student's study of word recognition. With a threshold set at 50% by a poststimulus pattern mask, my report – and the activation producing it – was of course right about half the time. What was startling was the dissociation between accuracy and my experience of what I had seen. On some trials, I was reasonably sure of my report and was wrong. More unsettling were trials where I was convinced I had seen nothing and shortly thereafter was aware of a word, reported it, and was right. The dissociation was not between activation and report but between both of those and the experience of the event.

I was later able to produce a similar dissociation under conditions of rapid serial presentation of a series of words one of which was a capitalized target word. At higher presentation rates, subjects often reported with some confidence the word following the capitalized word in the list (see Fischler 1975). More recently, Treisman and Schmidt (1982) have described similar "illusory conjunctions" when subjects report what attributes go with what stimuli in a multielement display, although there the effect seems to depend on the reports being a secondary task.

At the conclusion of their recent reassessment of attention and automaticity, Kahneman and Treisman (1984) outline a model of mind that I think captures the distinction being made here. What constitutes experience is the integration of semantic and episodic elements of events and objects in what they term "object files"; it is to these tokens of experience that attention can be directed or withheld, not to nodes in a semantic network. A similar speculation regarding the "purpose" of iconic representations was presented by Coltheart (1980b). The broader implications of this sort of "metamodel" for information-processing theory are not at all clear to me, and the dissociation I have described could be dismissed as nothing more than another case of rapid "forgetting" following a perceptual event. But it may at least shift the study of how subjective experience is related to and dissociable from the mechanisms of perception and memory to phenomena whose existence we can at least have some confidence in.

An operational definition of conscious awareness must be responsible to subjective experience

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I will confine my remarks largely to Holender's discussion of the masking literature and, within that topic, to his operationalization of conscious awareness.

In 1960, Eriksen reviewed research investigating the possibility that discrimination and learning may occur without awareness. The bulk of the research under review used verbal report as an index of awareness. Eriksen found no evidence for discrimination or learning without awareness when awareness was defined as verbal report (in practice, verbal report obtained under conditions of maximum sensitivity). Holender evaluates findings from the masking literature using essentially the same definition of awareness, and he draws the same conclusion.

However, Eriksen also noted a number of difficulties with the operational definition of awareness in terms of verbal report. An important one is exemplified by a study he cites (Adams 1957) in which subjects provided two verbal reports on each trial, one a psychophysical judgment and the second a rating of confidence in the judgment. Subjects showed better-than-chance discrimination on judgments that they rated as guesses. Because the psychophysical judgments were made verbally, the experimental outcome must be classified as showing discrimination *with* awareness even though that classification is in conflict with the subjects' own (also verbal) assessments. Eriksen went on from there to review the literature on perception and learning without awareness, using this admittedly defective definition of awareness as verbal report "as a beginning point" (p. 281). However, in my view, the difficulties he raises with the definition eliminate it as a valid operational definition of awareness.[1] I accordingly find Holender's criticisms concerning whether subjects were in fact unaware of ostensibly unconsciously perceived masked stimuli to miss the mark when they focus on whether stimuli were in fact presented at subjects' detection or identification thresholds.

If an operational definition of conscious awareness in these terms is not valid, what is a valid definition? There may be no fully satisfactory one. Although consciousness is a fundamental fact of human existence that psychologists must attempt to understand, it appears somewhat refractory to experimental study. However, the definition in terms of verbal report can be improved on. Certainly, a definition should be more responsible than discriminated verbal reports are to the perceivers' subjective experiences. A better operational definition, suggested by Adams's (1957) findings and recently adopted by Cheesman and Merikle (in press), is in terms of perceivers' confidence that they have perceived a difficult-to-perceive stimulus.[2] Cheesman and Merikle have found that, at target-mask stimulus onset asynchronies (SOAs) at which subjects' confidence ratings suggest random guessing (as to which of four color-words had been presented before the mask), their guesses are better than chance, and they show priming effects of the color word in time to name a subsequently presented color patch. This outcome replicates an earlier outcome by Marcel (1983a). Cheesman and Merikle call the target-mask SOAs at which subjects have no confidence in their identifications of the masked words their "subjective thresholds." In contrast, when SOAs are set instead at subjects' "objective thresholds" – that is, so that their color-word identifications are at chance – priming by the masked color words is absent.

Of the two thresholds, the subjective threshold is the interesting one for the study of perception without awareness, because it reveals the provocative discrepancy between what perceivers know and "what they know they know" (cf. Turvey 1974). One finding at the objective threshold is interesting but less related to the issue of perception without awareness. That is the finding confirming Eriksen's conclusion that discriminated verbal reports are as sensitive to perceptual products as are other response measures.

Experiments by Marcel (1983a), Balota (1983), and Fowler, Wolford, Slade, and Tassinary (1981), among others, were flawed by using clumsy procedures to set the subjective threshold. (In our study, we set the target-mask SOA to a level at which subjects were just better than chance at detecting the presence or absence of a word or nonword before the mask.) Moreover, our conclusion that subjects can show effects of unconsciously perceived stimuli by indirect measures of perception such as priming but not by direct measures such as identification of the masked stimulus may have been mistaken. Despite their flaws, however, the experiments of Marcel, replicated by Balota and by Fowler et al., do appear to have established the phenomenon of perception without awareness using masking procedures if awareness is defined in terms of viewers' subjective thresholds. Using that threshold, Cheesman and Merikle have replicated the findings of these studies, including the important finding by Marcel (1980) that responses to stimuli perceived with and without awareness may pattern differently.[3]

Other sources of information confirm the conclusion just drawn from the masking studies that perception can take place without awareness. Research on blindsight (e.g., Weiskrantz, Warrington, Sanders & Marshall 1974) suggests that neurological damage can create a chronic condition in which perceiver/actors act appropriately toward certain visually specified properties of environmental objects but experience blindness. (However, see Campion, Latto, and Smith, 1983, for a skeptical view of the phenomenon of blindsight.) Compatibly, in cases of "hemineglect," patients may fail to recognize their limbs on one side as belonging to them and may leave them out of their self drawings. Nevertheless, if the patients are ambulatory, they walk on the neglected leg and do so without knocking into objects or other people with the limbs they neglect (Friedland & Weinstein 1977).

Perception without awareness is evident in everyday experience, too. One example occurs when perceivers habituate to familiar stimulation. In the "Bowery el" phenomenon described by Pribram (1969), residents of New York City who were accustomed to sleeping through the noisy passage of the elevated trains during the night were awakened by the *failure* of the trains to pass through after they had been dismantled. A second example occurs when behaviors are "automatized." People who drive the same route frequently, for example between work and home, report that they may emerge from a daydream halfway home without any recollection of having traversed the first half of the trip. Other examples are attested of perceptions (and actions with reference to them) achieved outside of awareness in "actions not as planned" (Reason 1979; Norman 1981). For example (from a collection of action slips described by Reason, 1979): "I went up to my bedroom to change into something comfortable for the evening. I stood by my bed and started to take off my jacket and tie. The next thing I knew I was getting into my pajama trousers (p. 72)."

These two additional sources of information from the clinical literature and from everyday experience indicate that the general observation of perception without awareness is established. The research reviewed in Holender's target article is of interest for the possibilities it offers for subjecting the phenomenon to careful experimental study and, in the case of the masking studies, for the light it sheds on the phenomenon of masking itself.

NOTES

1. The same conclusion can be reached in another way. If perceivers show evidence of having perceived a difficult-to-perceive stimulus by one response they make (for example, a button-press response to a target primed by a briefly presented, masked stimulus), it is not at all unlikely that coverging evidence can be obtained using a variety of other response measures. That is, the influence of a stimulus is unlikely to be restricted to an influence on just one type of response (such as a button press), although the various responses may differ in their sensitivity or responsiveness to the stimulation. In particular, the vocal tract should not be immune to effects of any unconsciously perceived stimuli if the fingers are not immune.

2. This operational definition is obviously not perfect either. If some responses may be influenced by unconsciously perceived stimuli, then, possibly, confidence judgments may be so influenced as well (see note 1). However, the measure is an improvement over those of greater (indeed, excessive) sensitivity that are less responsible to the perceiver's own experience.

3. In my view, Holender dismisses his third proposed criterion for unconscious perception (from Dixon 1971; 1981) – that unconsciously

and consciously perceived stimuli have qualitatively different effects on experience – too hastily. His grounds are that some consciously perceived stimuli have qualitatively different mutual effects on performance. However, qualitative differences in performance are found in the masking literature (Marcel 1980; replicated by Cheesman and Merikle, in press) when the only difference in the conditions giving rise to the differences in performance is the presence or absence of a mask.

Attentional orienting precedes conscious identification

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As Holender points out, most studies that show semantic activation without conscious identification suffer from a common methodological deficiency: the lack of an objective criterion that differentiates between conscious and preconscious word processing. Specifically, Underwood (1981; Underwood & Thwaites 1982) reported effects of a parafoveally available word on lexical-decision reaction times (RTs) to a foveally available word even when the conscious identification of the parafoveal word was unlikely because of its short visual availability. However, conscious identification may have occurred and mediated the observed effects. Parafoveal words were presented only to the right visual field (VF); this absence of spatial uncertainty may have led to a highly efficient shift of attention toward the parafoveal word or induced a repositioning of the eyes. Even the low report performance may not conclusively support the claim that the effects were a result of preconscious semantic processing of parafoveal words. Some parafoveal words may have been consciously identified at the time of stimulus presentation but may not have been retrievable in the report task.

A different and potentially more objective approach is to compare the effects of conscious and preconscious word processing in subjects whose attentional orienting is selectively impaired, so that the visual presentation of a stimulus will either be conscious or remain completely unnoticed. Posner, Walker, Friedrich, and Rafal (1984) showed that patients who suffer from unilateral lesions to the parietal lobe completely fail to notice a parafoveal stimulus if attention is engaged at some spatial position and the parafoveal stimulus is presented contralateral to the attended position. In contrast, identical parafoveal stimuli are immediately noticed if attention is ipsilateral to the parafoveal stimulus. Posner et al. concluded that injury to the parietal lobe is precludes the reorienting of attention to the visual field that is contralateral to the site of the lesion.

Recently, we (Inhoff & Posner 1985) instructed patients with unilateral parietal injury to perform a variety of tasks, each of which assessed the effects of a parafoveal distractor on a foveal category decision task. If semantic processing can occur without conscious identification, these patients should show semantic effects of a parafoveally available stimulus on a foveal target regardless of whether the parafoveal stimulus (distractor) is presented ipsilaterally or contralaterally to the foveal (attended) target.

In the first series of experiments the patients viewed a foveal letter or digit while a parafoveal letter or digit (distractor) was presented for 150 msec next to the foveal display in the ipsi- or contralateral visual field. The target and distractor subtended approximately 1 degree of visual angle each, and they were separated by about 1 degree. The distractors were either identical to the foveal target, from the same category (e.g., both letters or both digits), or from conflicting categories (e.g., foveal letter and parafoveal digit). In Experiment 1, the foveal and ipsi- or contralateral distractor were presented simultaneously, in Experiment 2 the ipsi- or contralateral distractor preceded the target by 60 msec.

The results showed that there were no effects of the distractors on the foveal category choice when the target and distractor appeared simultaneously (Experiment 1). Experiment 2 showed effects of the distractor on foveal category choice only when the distractor appeared in the ipsilateral VF. The shortest RTs occurred when the foveal target and ipsilateral distractor were identical, and the longest RTs occurred when they were of conflicting categories. No systematic effects were observed for contralateral distractor presentations. This finding conflicts with Underwood's (1981) and Underwood and Thwaites's (1982) assertion that semantic processing in the parafovea occurs without the orienting of attention to the parafovea. Control subjects without neurological disorder replicated the patients' ipsilateral pattern of results for distractor presentations to the right and left VF. Thus, effective semantic processing of the parafoveal display can occur provided the parafoveal stimulus is relatively easy to identify and precedes the foveal target and, more crucial to the present argument, if the subject is able to orient attention to the parafovea.

Patients also performed a lexical-decision task that more closely replicates Underwood and Thwaites's procedure except that the parafoveal letter string could occur in either the right or the left VF for 150 msec. A string of letters constituting a word or a pronounceable nonword was presented foveally while a parafoveal distractor was present. Three types of distractors were used: words related to the foveal word, words unrelated to the foveal word, and a homogeneous brightness field created by a string of *X*'s. The target and distractor were 4 to 6 characters in length, and each subtended between 1.5 and 2 degrees of visual angle. On half of the trials, the onset of the target and of the distractor occurred simultaneously; on the remaining trials the parafoveal distractor preceded the foveal target by 120 msec. The results showed longer RTs for ipsilateral than for contralateral distractor presentations; yet, in contrast to the findings of Underwood and Thwaites, there was no difference between the effects of related and unrelated parafoveal words on the foveal lexical-decision task. There were, however, significantly shorter lexical-decision times when a homogeneous brightness field was parafoveally available than when words were presented to the ipsilateral or to the contralateral VF, suggesting that some visual discrimination was performed in the attended ipsilateral and unattended contralateral visual field. Control subjects, who performed in the same experiment with the additional instruction of reporting the parafoveal word on half of the trials replicated this pattern of results. Longer lexical-decision times were found if the parafoveal stimulus was a word than if it was a row of *X*'s. More important, there was virtually no difference between the effects of related and unrelated parafoveal words on the foveal lexical decision when subjects had been unable to report the parafoveal distractor.

Again, these results are in empirical disagreement with Underwood's (1981) and Underwood and Thwaites's (1982) conclusion that parafoveal word processing can be performed at a semantic level without the orienting of attention to the parafovea and without the conscious identification of the parafoveal item. In fact, the effective orienting of attention to the parafovea appears to be a prerequisite for the semantic analysis of parafoveal input (Experiment 2). Only low-level figure–ground segmentation, as it may be implicated in the differentiation of a homogeneous parafoveal brightness field from a string of letters, may be performed preattentively.

Semantic activation, consciousness, and attention

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The assumption that a stimulus can undergo semantic activation (processing) but not attain conscious identification (consciousness or controlled processing) is a defining feature of dual-process theories of information processing (e.g., Posner & Synder 1975a; Shiffrin & Schneider 1977). It is central to late-selection theories of attention (e.g., Deutsch & Deutsch 1963) and is adopted even by early-selection theories that allow irrelevant stimuli to undergo at least a modicum of semantic processing (e.g., Johnston & Heinz 1978). The assumption that semantic processing is accompanied necessarily by consciousness compels an extreme early-selection view of attention in which stimuli can be prevented from attaining consciousness only by being blocked from semantic processing, a view that may no longer have a major advocate (e.g., Broadbent 1971). Holender develops a strong case that semantic processing may indeed be undissociable from consciousness. Thus, Holender's review constitutes a serious challenge to most contemporary theories of information processing in general and attention in particular. I agree with Holender that the processing dissociation in question has not been clearly demonstrated. However, I disagree with some of Holender's methodological recommendations. In what follows I examine the problem of testing for a dissociation between semantic processing and consciousness.

The methodology one employs to research the issue depends on the theoretical framework from which one views it. To illustrate this point, let us examine the issue from the perspective of spotlight theories of attention (e.g., LaBerge 1983; Posner, Snyder & Davidson 1980). Assume that an attention mechanism can be focused on an area of the visual field such that conscious identification is possible only for stimuli that fall within this area. Four versions of this spotlight view of attention can be differentiated in terms of (1) whether or not semantic processing is limited to stimuli that fall inside the spotlight and (2) whether or not the results of the semantic processing of stimuli that do fall inside the spotlight necessarily attain conscious representation. Each version has different implications with respect to both the dissociability of semantic processing from consciousness and how to test for it.

Version A assumes that the spotlight is a necessary precondition for semantic processing and that the semantic records of spotlighted stimuli necessarily attain (or are translated into) consciousness. This view easily accommodates the apparent lack of evidence for a dissociation of semantic processing from consciousness; semantic processing in the absence of consciousness is deemed impossible even in principle. Backward masking of a stimulus to which the spotlight is directed (e.g., a foveal stimulus) can prevent conscious identification of the stimulus *because* it prevents semantic processing. A stimulus presented outside the spotlight (e.g., a parafoveal stimulus) is processed neither semantically nor consciously.

Version B agrees that only spotlighted stimuli undergo semantic processing but argues that this semantic processing need not be represented in consciousness. This view receives some support from studies suggesting that people can selectively attend to just one of two objects when one is superimposed over the other (e.g., Duncan 1984; Rock & Gutman 1981). Version B suggests the following procedure for demonstrating semantic processing without conscious identification: Limit exposure duration of superimposed objects so that only the relevant one can be identified, and then test the semantic-priming potency of the irrelevant object. To the extent that semantic priming does not require conscious processing of the prime stimulus, it should be demonstrable even for the irrelevant and unidentified object. This view can attribute the apparent lack of evidence for a dissociation of semantic processing from consciousness to the lack of an appropriate test for the dissociation.

Version C agrees with Version A that semantic processing is not dissociable from consciousness for stimuli that fall inside the beam of attention but argues that semantic processing in the absence of conscious identification can be engendered, at least to some extent, by stimuli that fall outside the beam. This view suggests the following kind of procedure for demonstrating a dissociation between semantic processing and consciousness: Present two stimuli to different sides of the fovea, have observers focus their attentional spotlights on just one side while keeping their eyes stationary, set exposure duration so that just the spotlighted stimulus can be identified (i.e., there is not enough time to move the spotlight to the other stimulus), and measure semantic priming for the other stimulus. This procedure ensures that the relevant stimulus does not enjoy any data-driven or bottom-up advantage over the irrelevant stimulus. The apparent lack of evidence for semantic processing without conscious identification is potentially attributable to the apparent fact that this procedure has not been precisely followed. However, as Holender shows, none of the studies using similar procedures of parafoveal presentation has yielded clear-cut support for Version C. On the other hand, some of the findings lend sufficient support to this view that it would be premature to reject it at this time (e.g., Underwood 1976).

Version D combines the features of Versions B and C and allows for a dissociation of semantic processing from consciousness for stimuli either inside the spotlight or outside it. Thus, in this version, the dissociation could be tested by the use of either superimposed stimuli as suggested by Version B or parafoveal stimuli as suggested by Version C.

The main points to be drawn from this analysis of spotlight theories are that (a) different theories of the relationship between semantic processing and consciousness call for different ways to test for a dissociation and that (b) none of the spotlight theories outlined above calls for the kind of test recommended by Holender. All of these theories call for a strict lockstep relation between semantic processing and consciousness of relevant, spotlighted stimuli. To the extent that a dissociation is possible at all (Versions B–D), it is limited to irrelevant stimuli inside the spotlight (Version B), outside the spotlight (Version C), or in either location (Version D). From the perspective of all of these theories, the failure to identify a relevant spotlighted stimulus logically implies, the failure to identify a relevant spotlighted stimulus logically implies the failure of semantic processing; the backward masking of such a stimulus can prevent conscious identification only to the extent that it prevents semantic processing. Thus, the methodology recommended by Holender may be the least suitable one with which to test for semantic activation without conscious identification.

Approaches to consciousness: Psychophysics or philosophy?

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It has been generally accepted at least since Freud that much of what goes on in our brains does not reach conscious awareness. More recently, psychologists working in several different areas have suggested that our behaviour can be affected by sensory stimuli of which we are not aware. As a general statement, few would argue with this either. For example, there are many homeostatic processes, such as the modulation of respiration rate by changes in atmospheric oxygen concentration, in which we are *never* aware of the physical stimulus initiating the change. The difficulties arise when the initiating stimulus can *sometimes* reach consciousness, perhaps at higher intensities or when tested under different conditions. Holender's case of semantic priming by subthreshold stimuli is an important example of this. Blindsight, discriminative behaviour elicited by stimuli of which the patient is unaware as the result of damage to the visual cortex, is another (Campion, Latto & Smith 1983; Campion & Latto 1985). Dissociations of awareness and behaviour have also been claimed for sleep learning (e.g., Cooper & Hoskovec 1972), posthypnotic suggestion and other hypnotic phenomena (reviewed in Wagstaff 1981), perceptual defence

(Brown 1961), some of the preattentive mechanisms formulated by cognitive psychologists (e.g., Neisser 1967), and indeed aspects of all skilled behaviour (Economos 1983).

These are all, to a greater or lesser extent, acceptable as theoretical possibilities, but, as Holender so effectively shows for semantic priming and as we feel have shown for blindsight, it has never been possible to demonstrate them empirically. The central problem in trying to design experiments to determine whether the subject is aware of stimuli that are affecting his behaviour is that we have no adequate operational definition of conscious awareness (*conscious identification* in Holender's terminology). Holender addresses this in Section 1. We agree that conscious identification cannot ultimately be fully characterised and requires an operational definition, but his suggestion of "an operational definition that equates conscious identification with the ability to resoond discriminatively to a stimulus at the time of presentation" is not adequate for two reasons. First, it is not operationally precise enough. (A photocell responds discriminatively to light without, presumably, *conscious* identification.) An adequately precise definition, derived from Holender's, would be the ability to make a verbal identification of a word – a process that would normally be accepted as necessarily conscious. Second, Holender fails to appreciate fully the logical status of such a definition. Since it does not include all conscious processes, it can be a sufficient but not a necessary condition for conscious identification. Although the presence of such an ability may demonstrate the presence of consciousness, it does not follow, contrary to Holender's claim, that the absence of the ability demonstrates the absence of conscious identification.

In addition to the problem of producing an adequate operational definition, there is also the difficulty of establishing that the subject is using equivalent criteria in equivalent conditions when determining discriminative performance (i.e., verbal identification) and semantic activation. Holender demonstrates convincingly in his review of the literature that in no case have these criteria been adequately equated. Coming to the present paper from research into blindsight, we have an extraordinary feeling of déjà vu. (Is déjà vu, if veridical, itself a form of semantic priming without conscious awareness?) In the potentially most powerful backward masking paradigm (Section 4), just as in blindsight, all the experiments contain one or both of two methodological errors: They fail to match stimulus parameters in the two conditions, in this case the measurement of the threshold for presence/identity of the prime and the measurement of the priming effect itself; and they fail to ensure that the subject is using the same decision criterion in the two conditions, either by using signal detection theory methodology or, more economically, by using criterion-free forced-choice procedures.

Holender leaves us with the conclusion (Section 5) that, for the pattern-masking paradigm at least, tightening up the methodology would finally allow us to test empirically the hypothesis that semantic activation can occur without conscious awareness of the priming stimulus. But, to return to the central problem outlined in our second paragraph, supposing an experiment were done that met all Holender's criteria (Section 4.4), what could we conclude?

As we showed at the beginning, it is a relatively trivial observation that we do many things unconsciously, and this includes the fact that priming words affect later words, whether conscious or not, since we know that this occurs in reading ordinary text. Our minds clearly construct semantic models of the world and retain these for a long time. Such constructions with verbal material are highly trained and therefore rapid. The reporting of the acoustic or physical properties of words is not so highly skilled, simply because it is not useful in our culture to be able to make such reports. The fact that you can recall the meaning but not the words of the last sentence is a commonplace observation. As you read this sentence, it is actually very difficult to describe just what you are conscious of, although you are clearly conscious of something. Without specifying precisely what is going on in reading text, self-examination is quite sufficient to establish that semantic activation without conscious identification in terms of verbal report not only is not controversial but is in fact the very basis of everyday manipulation of language.

To return to the empirical question, we could conclude, if experiments with the appropriate technical competence were done, that the verbal identification of words can be affected by the semantic attributes of previously presented words that cannot be verbally identified. But as we have already pointed out, because of the necessarily loose operational nature of our definition, the *absence* of the ability to verbalise has nothing to say about consciousness per se (though the fact that semantic summation can occur between two stimuli remains an important finding, presumably related to normal processes occurring during reading). Changing the operational definition of conscious identification to the verbal *detection* of the prime, as in Marcel's experiments (Marcel 1983a), does not help since detection is not a sufficient condition for awareness either. If also adds a new problem not discussed by Holender. For now all we can conclude is that the sensitivity for verbal detection is less than the sensitivity for the semantic discrimination of the two words taken together, and, as Haber (1983b) points out in the context of blindsight, differential sensitivities on different kinds of tasks do not necessarily imply different underlying processes. It is meaningless to compare sensitivities for different tasks in a single situation. The difference in sensitivity is entirely arbitrary and depends on relative task difficulty. But if task difficulty is matched, the sensitivities will by definition be the same. It is possible to look at the differential effect on the two sensitivities of a particular treatment (as Marcel, 1983a, attempts to do in his Experiment 1), but it has to be done over a wide range of stimulus parameters (i.e., over a range of task difficulties from easy detection/difficult semantic identification to difficult detection/easy semantic identification) before it is possible to conclude that differential effects on sensitivity are due to a genuine independence between the two functions rather than to arbitrary differences in task difficulty.

So even with methodologically perfect experimentation, definitional difficulties make it impossible to draw empirical conclusions about consciousness. But we can and should continue to speculate about consciousness at a theoretical level. Considering the functions of consciousness in relation to the reading of text, for example, it is clear that we can be conscious or not conscious of the surface or the semantic attributes of a word depending on the degree of our competence, the nature of the text, and the demands of the task, and that the actual functions of consciousness in reading text are of psychological importance. It is therefore useful to incorporate consciousness into models of information processing as Marcel (1983b) does; but if we use an ecologically more valid domain, such as that of actual text reading, then consciousness would be formulated as a process subserving the manipulation of mental representation rather than as some kind of end process in itself as Marcel suggests. By keeping our discussion of consciousness on a theoretical level and reserving experimentation for investigating definable cognitive processes such as verbal identification and semantic activation, we shall both avoid many of the insurmountable problems and learn a great deal about the nature of those cognitive processes.

Conscious identification: Where do you draw the line?

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What constitutes conscious identification? What is perhaps the central argument in Holender's target article is that previous researchers attempting to show semantic activation without conscious identification have done an inadequate job of measuring conscious identification. This point is well taken. However, the problem in measuring conscious identification is actually more than just one of measurement. What has to be dealt with first is the issue of how to define the concept being measured.

If an observer can accurately report a word that is presented, everyone would agree that a conscious identification has taken place. On the other hand, if the observer claims to know nothing of the nature of the word and shows no indication of knowledge on any objective measure, a conscious identification clearly has not occurred. Unfortunately, these two possibilities are not exhaustive. Do we wish to argue that the observer has or has not made a conscious identification if (a) the identity of the word is known, but because the observer is not certain and wishes to maintain a high report criterion, it is not reported; (b) the observer has the correct word in mind but does not believe it was the word that was presented; (c) the word is among two or three possibilities the observer is considering, but one of the others is reported; (d) the observer knows the identity of the word momentarily, but the knowledge was so fragile that it is quicly forgotten, like the inappropriate meanings of polysemous words; or (e) the observer knows some of the physical features of the word and could, if forced, make a reasonably accurate guess? As this list of possible scenarios makes clear, knowledge about the identity of a word is not an all-or-none thing but is, in fact, better represented as a continuum (the reader can undoubtedly conceive of other levels on this continuum). Recognizing, then, the essentially continuous nature of knowledge about a word, one's first problem with "conscious identification" becomes where to draw the line.

Holender, who is rightly attempting to be as conservative as possible in evaluating the evidence for semantic activation without conscious identification, appears to have divided this continuum somewhere around level d. Others, more favorably disposed toward the notion of semantic activation without conscious identification, may choose to divide the continuum somewhere around level b. The problem this creates for different camps of people talking to one another is obvious. If an experimental procedure could be devised so that, for example, level c was consistently achieved and semantic activation was noted, the liberals would accept it as a demonstration of semantic activation without conscious identification whereas the conservatives would not. Unfortunately both (or neither) would be right.

The potential definitional problems created by the use of the term *conscious identification* appear to be numerous. However, for the sake of argument let us assume that we can divide the knowledge continuum into a consciousness part and an unconsciousness part at some spot almost everyone feels comfortable with, perhaps somewhere around level d. The question then becomes how to determine which stimuli are above that criterion and are hence "consciously identified" and which are not. Clearly, simply asking for a report is insufficient, a point Holender makes many times. He suggests instead that more modern psychophysical techniques, particularly signal detection techniques, are now available to help answer this question.

In theory, the idea of using signal detection techniques sounds reasonable. Unfortunately, with the continuum divided as suggested in the last paragraph, the implementation will be somewhat problematic. In the first part of the experiment, in which the conditions for preventing a conscious identification are established, the observer would presumably view words followed by masks in an appropriately light-adapted environment. In the typical signal detection procedure, the observer would then be asked either to choose between two alternatives, one of which is correct, or to make a yes–no decision about a single alternative. Now consider the fate of those words which fall below the criterion on the knowledge continuum that is, those words from which the observer can recognize only a letter or two. Even in the most controlled situations, this small amount of information should allow the observer to show a d' greater than zero for these words. In fact, only the words that the observer completely fails to perceive (i.e., none of whose properties the observer detects) would produce a d' of zero. Consequently, only those words could be classified as not being consciously identified. Thus, the effect of this situation would be to force the criterion to a lower position on the knowledge continuum, making a fair test of the semantic activation without conscious identification hypothesis impossible.

In essence, the semantic activation without conscious identification hypothesis, as stated, does not appear to be testable. The reason is that the term *conscious identification* is too vague to lend itself to empirical investigation. Furthermore, operationalizing it in a way that would be workable appears to require the adoption of a polarized position. That is, either conscious identification would have to be defined as the ability to report the presented word accurately, or the lack of conscious identification would have to be defined as the total absence of any indication of knowledge about the word on any objective measure. To anyone willing to adopt either of these positions, the target article appears to offer some useful insights. To those who feel that both positions do violence to their definition of conscious identification, the implications are also clear. The best solution would be to avoid the use of the term altogether.

The psychophysics of subliminal perception

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Holender provides a strong methodological critique of current research into subliminal, or "below-threshold," perception. In these remarks, I ask how "below-threshold" could be defined so as to be consistent with modern psychophysics. Threshold theory does not lead to a useful translation, but two quite different detection-theoretic interpretations can be stated.

Threshold-theory interpretations. Thresholds were once considered the building blocks of perception but have been eroded by psychophysical progress. Current understanding may be summarized as follows (see Luce 1963; Green & Swets 1974): (1) No fixed *stimulus* threshold divides detection from failure to detect; rather, the probability of giving a "yes" response in a detection experiment increases gradually with level. (2) it may be that a fixed *sensory* threshold divides a sensory continuum into discrete *detect* and *nondetect* states. If so, all stimuli, including the null stimulus, lead to the detect state with probability greater than zero. (3) Observers do not necessarily map the detect and nondetect states into "yes" and "no" responses but can choose to say "yes" to some but not all detect states or "no" to some but not all nondetect states.

A key requirement in the research surveyed by Holender is that *stimuli* be below threshold. A subthreshold sensation is defined, in threshold theory, as one that falls in the nondetect region, but what is a subthreshold stimulus? It is not enough to say that (definition a) any stimulus that leads to a nondetect state is subliminal, since statement (2) above implies that the same stimulus may lead to the detect state on another presentation. It is too much to say that (definition b) a stimulus that *never* leads to a detect state is subliminal, since, by statement (2), no such stimuli exist. One possible definition is that (c) a subliminal stimulus is one that leads to the detect state just as often as does a null stimulus.

Notice that definition (c) does not imply a threshold theory of detection, since it is equivalent to defining a subliminal stimulus as one for which the hit rate (probability of saying "yes" to a stimulus) equals the false alarm rate (probability of saying "yes" in the absence of a stimulus). I discuss this definition further below, in the equally applicable context of detection theory.

The paradoxical nature of some subliminal perception results arises from the implicit adoption of definition (b), combined with an assumption about the sensation–response relation that is inconsistent with statement (3) above. In general, threshold theory does not provide an instructive analysis of subliminal perception; the rest of my remarks are cast in terms of detection theory.

Detection-theory interpretations. Signal detection theory (SDT) shares with threshold theory two key ideas: that the relation between stimuli and sensory states is probabilistic, and that the relation between sensory states and responses is under the observer's control. SDT differs from threshold theory in assuming that there is a continuum of sensory states on which the observer places a *criterion* to divide "yes" from "no" responses.

Two measures of sensitivity are commonly used within the context of SDT. One is d', the normalized distance between the means of the underlying stimulus and no-stimulus distributions. This criterion-free measure has no counterpart in threshold theory. The second sensitivity measure is called, confusingly, the "threshold"; by this is meant the stimulus level for which d' equals some fixed value, often 1.0. I will refer to this measure as an *empirical threshold*. (The fixed performance level is sometimes given in percent correct, a similar definition that, since percent correct is not criterion-free, confounds sensitivity with bias.)

Within the context of SDT, a "subliminal" stimulus could be (d) one that is below the empirical threshold, or (e) one that leads, on the average, to a below-criterion sensation. I also consider definition (c), which can be simply restated in SDT terms: $d' = 0$.

Definition (d) is often used in the research summarized by Holender. Stimuli satisfying this definition may be detectable, but with $d' < 1.0$. Evidence that these stimuli have produced sensory effects is unsurprising.

Definition (e) translates "threshold" into "criterion." Although the criterion of SDT and the threshold of threshold theory are in some ways parallel constructs, they are critically different: That a sensory event falls below criterion has no implications for the location of that event relative to either the stimulus or the no-stimulus distribution. SDT takes no stand on whether below-criterion stimuli are consciously perceived.

Consider, however, the following argument: If a stimulus is subliminal in sense (e), so that its presence is not reported, then SDT predicts that no information about the characteristics of the stimulus can be reported either (unless the observer shifts the criterion, as Holender proposes at several points). That is, a positive detection response is logically required for above-chance *recognition* performance. Such an argument was implicitly made by Marcel (1983a, Experiment 1); he considered his data, which failed to show this pattern, to provide support for subliminal perception.

To demonstrate the fallacy in this argument, let me describe a psychophysical detection/recognition experiment reported by Lindner (1968). On each trial, Lindner presented either a 500-Hz tone, an 1100-Hz tone, or the null stimulus. Listeners were required to respond "yes" or "no", and in any case, to report whether the frequency was "high" or "low." Recognition of tone frequency was above chance, even on trials when the detection response was "no."

Figure 1 shows a possible internal decision space for Lindner's experiment. The space is defined by activity in two independent frequency channels; pure tones falling in different critical bands, as these do, are commonly thought to be independent (Scharf 1970). Repeated presentation of the same stimulus leads to a distribution in this space; such distributions are here represented by circles one standard deviation from the mean. The listener makes both a detection and a recognition response on each trial by comparing the sensory observation with two adjustable criterion curves. The detection criterion divides "yes" responses (concave region) from "no" responses

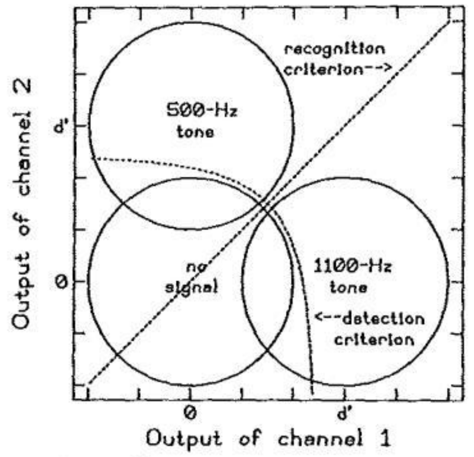

Figure 1. (Macmillan). A decision space for the experiment of Lindner (1968), in which listeners were required both to detect the presence of and to recognize the frequency of a pure tone. See text for details.

(convex region); the recognition criterion divides "low" responses (left of line) from "high" responses (right of line).

Reliable recognition is clearly possible for observations below the detection criterion. This result appears to satisfy Holender's criterion 1 and is completely consistent with SDT. The essential assumption that the stimuli being recognized differ from the null stimulus in an uncorrelated manner seems intuitively reasonable for the word and picture stimuli used in most subliminal perception experiments.

Finally, what about definition (c)? Above-chance recognition performance (or other evidence for activation) when detection $d' = 0$ would be, for almost everyone, persuasive evidence for subliminal perception.

This approach is sometimes assayed (e.g., by Marcel 1983, Experiments 3 and 4), but it entails some practical difficulties. An accurate estimate of d' cannot be made quickly. To distinguish the hypotheses $d' = 0$ and $d' = 0.5$ at the .05, one-tailed level, assuming symmetric criterion placement, requires about 140 trials (see Gourevitch & Galanter 1967). Investigators who attempt to fulfill the condition of $d' = 0$ have typically used far fewer trials, as Merikle (1982) has pointed out, and thus cannot have demonstrated zero sensitivity.

Conclusion. Five psychophysical interpretations of subliminal perception have been considered. Interpretations that take the threshold concept literally (definitions a and b) are based on discredited models of the threshold. An empirical interpretation (definition d) is too weak to express the subliminal perception hypothesis. According to the two remaining translations, *subliminal* might mean (c) indistinguishable from the null stimulus or (e) below the response criterion. Either definition might be acceptable to Holender, who presents "an operational definition that equates conscious identification with the ability to respond discriminatively to a stimulus at the time of presentation."

Whether definition (c) could ever lead to fulfillment of Holender's criterion 1 is uncertain, and satisfying the definition would be at best onerous. Definition (e), on the other hand, can satisfy the criterion whenever the stimuli in an experiment are statistically independent. If this definition is a satisfactory paraphrase of subliminal perception, then that phenomenon may be more common and less mysterious than is usually imagined.

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Consciousness and processing: Choosing and testing a null hypothesis

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Holender's target article appears to be methodological, yet it assumes a particular null hypothesis without stating clearly what it is or why it is assumed. This makes commentary difficult.

A peculiar state of affairs seems to exist in cognitive psychology. Most current accounts of perception, cognition, and task execution have no place for consciousness. Phenomenal experience and subjectivity are apparently unnecessary for models of cognition, and there is certainly no evidence of them in the behaviour of artificial intelligence programs and automata, from which much information processing is derived. Yet information-processing theorists react with scepticism when models and data are offered that explore the idea that phenomenal experience is dissociable from or not a prerequisite for the processing of sensory data. What should be the null hypothesis?

Precisely what is at issue in Holender's paper changes from section to section. At different points it is semantic analysis without awareness of the stimulus, without awareness of its meaning, and without identification. Whatever it is of which Holender is sceptical, no theoretical reason is given that motivates his scepticism. In the 1950s and 1960s the disbelief in perceptual defence and nonconscious perception was largely due to the failure to distinguish knowing and perceiving *consciously* from *nonconsciously*. To deny the existence of nonconscious mental processes is to adopt the position of John Locke or of the phenomenologists, a view rejected by the very techniques of twentieth-century psychology, in eschewing reliance on introspection.

Holender's methodological review begs certain theoretical questions. What is so special about *semantic* analysis? After all, the kind of semantic analysis at issue hardly involves semantic primitives, meaning postulates, reference, or any form of signification; it can be conceived of as merely based on associations between orthographic, phonological, or pictorial descriptions of a certain level. Is Holender questioning whether any sensory analysis goes on without awareness or only the level of such analysis? He does not question Humphreys, Evett, and Taylor's (1982) finding that a nonconscious visual letter string can produce phonological priming. So if it is only the level of nonconscious analysis that is in question, what sort of theory says that awareness is necessary to or a necessary concomitant of a particular level of analysis?

If Holender concludes that there is little evidence for semantic processing without awareness (and implicitly, that it does not exist?), what does he suppose is the relationship between phenomenal awareness of an aspect of an event and the nonconscious mental processing of that aspect? Does he suppose that meaning (as he defines it) is only a property of awareness? Does he deny the distinction between conscious and nonconscious states and processes? If not, the following issues are raised. Clearly awareness of an aspect of an event cannot *precede* all processing of that aspect. If it did, one would regress to asking what underlies the awareness. So either (a) nonconscious processing/representation of an aspect may precede awareness of it, or (b) it is necessarily synchronous with it. If nonconscious representation of an aspect of an event can precede its conscious representation, the latter can be prevented and the former can exist without the latter. If awareness of an aspect were identical to or necessarily synchronous with any processing yielding a representation (of whatever kind) of that aspect, how could we deal with all the functional processing that undoubtedly does go on without awareness? (e.g., what precedes a new thought?) It therefore seems that awareness of an aspect of sensory or cognitive processing is at least theoretically dissociable from the processing itself.

If this position is acceptable, then Holender and the rest of us are faced with the question of why, given his inferences about the data, there is *so little* evidence of processing without awareness. There are several answers to this question. One is that on the whole psychologists do not look for such evidence: In general, they set tasks and examine how people deal with the intentional aspects of these tasks, and in perception they are concerned with conscious percepts. Another reason is that subjects are unwilling to allow mental events for which they cannot account to enter into those aspects of their actions over which they have control. In addition, perhaps Holender feels that there are psychological and logical limits to what certain investigative procedures can demonstrate. But of course this does not bear on the issue of the existence of the demonstranda.

However, is there really so little evidence? First, besides the evidence of our everyday experience and the issue of the status of knowledge and memories when they are not present in consciousness, there are several areas of research completely ignored by Holender, which seem to provide such evidence. These include binocular rivalry, stabilized images, cortical evoked potentials, signal discrimination without detection, the Pötzl phenomenon, cortical blindness, sensory suppression, hypnotic phenomena, amnesia, effects of verbal stimulation during anaesthesia (see Dixon, 1981, for review). There are doubtless many faults to be found in these areas of research. But taking research in these other areas into account shifts the weight of evidence against the null hypothesis.

Second, Holender's method of argument gives a distorted picture of the evidence he has reviewed. In almost every procedure considered, experiments are mentioned that he admits he cannot fault. These studies are described as "at odds with the rest of the literature," i.e., with those studies with which fault can be found. This is unacceptable: Studies showing a positive effect are not "at odds" with studies failing to disprove the null hypothesis. Even worse, Holender's conclusions are based on an inappropriately "statistical" inference. In essence he is arguing that the bulk of the evidence goes against the phenomenon in question. This is equivalent to seeking evidence of black swans in ten samples of swans, finding them in two of the samples, and then concluding that the bulk of the evidence goes against their existence! Finally, in several cases Holender argues by fiat or prejudges what is in question. As one example, he dismisses parafoveal studies on the basis of retinal characteristics. But these characteristics are derived from studies using conscious discrimination and therefore cannot tell us about aspects of retinal sensitivity where phenomenal experience is not involved.

I would like to turn now to a few comments on Holender's treatment of pattern masking, since this is the only technique reviewed to which strong criteria of awareness can be applied and since I have some association with it. First, some time is spent reviewing the potential mechanisms of masking, and in the concluding section it is stated that "the standard integration theory of pattern masking . . . according to which priming by undetectable pattern-masked primes is hardly conceivable, is certainly not challenged by the findings reported so far." There is a misconception here. The mechanism by which masking occurs has little to do with where or at what level in processing such a mechanism is operating. Indeed, in my theoretical paper (Marcel 1983a; 1983b) an integration account of masking was proposed. Thus it is quite possible to accept an integration (or an interruption) theory of masking that operates at the level of the achievement of conscious percepts without in the slightest affecting the theoretical possibility of priming by masked primes. Throughout, Holender confuses availability to consciousness with physical stimulus quality.

Holender says in Section 4.1. that according to the multilevel integration theory of masking "the eventuality of the SA/CI of a masked word is a priori precluded because there is no place in the system where a legible representation of the word is available." (1) The issue of legibility to whom or to what is ignored, as is the possibility of parallel representations; (2) the time course of interchannel inhibition or feature integration is not considered (it could permit fast read-out, or even extraction of possible contour groupings from an integrated image, as proposed by Marcel (1983b); (3) to preclude the possibility of empirical phenomena by a fiat based on an accepted theory is to *reinstate* the treatment of Galileo by the Inquisition.

Second, Holender makes much of whether subjects are light- or dark-adapted to the same extent during the priming phase of an experiment as they are when thresholds are determined. This point seems valid but subsidiary to the issue of whether subjects are *aware* of the masked word in the critical phase of the experiment. In my own experiments (and, I understand, in those of Fowler et al.), subjects were asked if they had been aware of any stimuli other than the fixation, mask, and target word, and none who answered affirmatively were used. To cope with this more stringently, Holender's final suggestion for methodological improvement is to mix priming and detection trials and to cue subjects which task to perform on each trial. This is exactly what was done in Experiment 5 of my 1983a paper. In a procedure where light adaptation could not have been altered for the two types of trials, and where subjects could not detect primes (as assessed by the procedure Holender recommends), reliable priming was obtained.

I would like to conclude with two general comments. First, even if I were to take all Holender's criticisms seriously and evaluate each experiment as he does, I would conclude that the faulted experiments do not constitute evidence of anything, that the few experiments failing to find an effect (if not faulted themselves) merely fail to disprove the null hypothesis, and that the half dozen or so experiments that remain unfaulted constitute a valid disproof of the null hypothesis, that is, that semantic activation without awareness exists.

Second, to return to my opening comments, it is commonly inferred from recent demonstrations of processing without awareness that the role of consciousness is being deemphasised and that I myself emphasise the importance of nonconscious processes. Quite the opposite is the case. Cortically blind patients who have no phenomenal experience of an object in the blind field will nonetheless preadjust their hands appropriately to the size, shape, orientation, and 3-D location of that object in the blind field when forced to attempt to grasp it (Marcel 1982). Yet such patients will make no spontaneous attempt to grasp a glass of water in their blind field even when thirsty. Voluntary actions often depend upon conscious perception. Indeed, when we talk about mental life, however much it depends on nonconscious processes, we are talking about consciousness.

Semantic activation and reading

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The issues concerning semantic activation that are discussed by Holender are important in the development of a theory of reading. My comments try to describe this relationship.

Reading is accomplished through making a series of eye fixations. The average distance the eyes travel between fixations is about eight or nine letter positions for skilled readers, but with considerable within-subject variability. Fixations average about 200–250 msec in duration, again with great variance. By steadily fixating a letter and attending to different words, one finds that 10 or more words can be identified without moving the eyes.

Although sufficient visual information is available to permit identification of a number of words on each fixation, the average saccade only advances the eyes by a word or two along the line of text being read. This discrepancy raises a basic question about the nature of visual perception during reading which can be characterized by contrasting two extreme models, one based on wide-ranging automatic semantic activation and the other on strong attentional selectivity. These models parallel Holender's dichotomy of semantic arousal with or without conscious identification.

The first model assumes that processing during a fixation is characterized by the type of automatic identification that is demonstrated in Stroop-like experiments: Identification is automatic for all words that can be resolved. Thus, on each fixation there is semantic arousal of a number of words, each activating associated words. The time of this activation may vary, depending on the visual eccentricities of the words. But the general view is of a number of words being activated on each fixation, with waves of activation spreading from each of them to associated words, thus providing a dynamic and active basis underlying further processing.

This view of perception during fixations has several implications. First, a critical task of the higher processes must be to coordinate the response to the wide-ranging semantic activation so that constraints provided by syntax and other word-order-based characteristics of the language are not lost. Second, each word is in an aroused state for some time, being activated on several fixations. These multiple activations may reinforce each other to facilitate reading (Bouma 1978; Smith 1971). Third, foveal perception of each word may be facilitated by prior peripheral activations (Haber & Hershenson 1980). Fourth, this model raises the question of how it is that words actually enter into further processing. Is the teeming lexicon simply an unconscious basis that influences the serial selection of words to consider in reading, or does the combination of words aroused somehow push language processing along?

The second model assumes that semantic activation results from attending words and that reading involves extreme attentional selectivity. Even though a number of words could potentially be identified during a fixation, only one or two are typically attended and activated, thus becoming involved in advancing the reader's understanding of the meaning of the text. There is no semantic activation of unattended words, whether peripheral or foveal, and there is no spreading activation from any words other than those that are attended during a fixation. The information-management problems are greatly reduced in this model. It is necessary on each fixation to determine which word or words should be attended, but the system then deals with semantic information concerning only those words.

Thus, different theories of reading reflect different assumptions concerning the conditions under which semantic activation occurs, which in turn influence the ways in which processing beyond the semantic activation level is conceptualized.

Some may believe that this issue has already been settled. There are many studies that indicate that the processing of words presented in the visual field is automatic and cannot be withheld. How could processing be otherwise during reading? A response to that argument would indicate that there are critical differences between the task characteristics of normal reading and of studies in which such automatic processing has been demonstrated. These studies have involved tachistoscopic presentation. The subject is typically poised, ready to grasp the stimulus to be presented on the screen. Visual attention is given to the display area. The onset of the stimulus pattern is marked by a sharp change in the visual array, something that is known to be attention-grabbing. Within these circumstances it may well be impossible, at least without a great deal of training, to resist responding to one prominent part of the stimulus pattern while maintaining the ability to respond to another. In reading, on the other hand, a fixation is simply one in a long series of exposures to the text, with ongoing processing occupying the mind both before and after that exposure. The stimulus field is filled with many elements, which are permanently part of the display, not appearing suddenly in an attention-grabbing manner. Furthermore, skilled readers have previously had millions upon millions of similar exposures and may have learned visual selection strategies that are rather specific to this situation. Given these differences, it is quite possible that the results of studies involving tachistoscopic presentations will not generalize to the normal reading situation.

There are studies that, though not conclusive, provide evidence against a wide-ranging semantic activation model. It might be expected that replacing one word in the text with another during a saccadic movement in reading (McConkie, Zola, Blanchard & Wolverton 1982) or after a brief mask in the middle of a fixation (Blanchard, McConkie, Zola & Wolverton 1984) would be disruptive since a single word position is associated with the activation of two different words. Such disruption does not necessarily occur. Neither is there evidence that the lack of preactivation of a word during reading, due to its not being present in the text on prior fixations, slows the processing of that word. If words are being unconsciously activated from prior peripheral exposure during reading, it is not clear that this has any facilitative effect on the reading process itself.

Consciousness is a "subjective" state

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Holender has provided a useful summary and critique of the evidence often cited to support the perception-without-awareness hypothesis. We agree with many of his conclusions, and, in particular, we heartily endorse his conclusion that no reported study involving dichotic listening, parafoveal vision, or visual masking provides strong, unambiguous evidence that semantic activation occurs under conditions in which observers are unable to respond discriminatively. Furthermore, on the basis of our own research (Cheesman & Merikle 1983; 1984; in press), we feel that Holender's conclusion can be taken even one step further; as long as it is assumed that the boundary between conscious and unconscious perceptual processes is defined by the threshold for discriminative responding, no evidence for unconscious perceptual processes will be found when precautions are taken to ensure accurate measurement of discriminative responding.

Although we agree with many of Holender's conclusions concerning previously reported studies, we strongly disagree with a critical assumption underlying his review. Holender accepts without question the widely held assumption that perceptual awareness or consciousness is best defined as better than chance-level discriminative responding. In fact, Holender states that discriminative responding provides the only essential criterion for establishing perceptual awareness or consciousness. Thus, if an observer can respond discriminatively to a stimulus, then, by definition, the observer is aware of the stimulus; and conversely, if an observer cannot respond discriminatively to a stimulus, then, by definition, the observer is unaware of the stimulus. In contrast to Holender's position, we feel that perceptual awareness or consciousness is a subjective state that is not adequately defined by the threshold for discriminative responding.

To illustrate our position, it is helpful to consider the characteristic reactions of observers performing difficult detection tasks. One cannot help noticing that observers often claim that they are not aware of the stimuli and that their responses are only guesses. Despite these claims, objective detection performance is usually considerably better than chance, and the observers typically express surprise when they are given feedback concerning their actual level of performance. Thus, in difficult detection tasks, subjects often claim that they are unaware of the perceptual information, even though their objective detection performance may indicate a considerable ability to respond discriminatively to the stimuli.

These observations lead to a distinction that we feel provides a basis for resolving the controversies that have continually plagued research investigating the relationship between perceptual processing and awareness (Cheesman & Merikle, in press). In our opinion, a distinction must be made between the *subjective threshold,* the level of discriminative responding at which observers claim not to be able to detect perceptual information at better than a chance level of performance, and the *objective threshold,* the level of discriminative responding corresponding to chance-level performance. Our research indicates that a somewhat higher stimulus energy level is associated with the subjective threshold than with the objective threshold and that considerable perceptual processing occurs when information is presented at energy levels between the two (Cheesman & Merikle 1984; in press). We feel that the subjective threshold, or the threshold for claimed awareness, better captures the phenomenological distinction between conscious and unconscious perceptual experiences and in our opinion provides a better definition of the boundary between conscious and unconscious perceptual processes than is provided by the objective threshold.

The obvious criticism that can be directed against our suggested definition of awareness is that the subjective threshold simply measures subjective confidence. It is therefore equivalent to asking observers whether they are conscious of a stimulus. As noted previously (Merikle 1983; 1984), when awareness is defined in this manner, an investigator transfers the responsibility for defining awareness to an observer so that, in effect, each observer is asked to provide his own definition of awareness. For this reason, before the subjective threshold can be used effectively to define awareness, at least one additional criterion or converging operation is needed.

The converging operation that we have adopted in our research program is similar to Dixon's (1971) third criterion, which Holender discusses and then dismisses as only an incidental corollary. This criterion stipulates that conscious and unconscious perceptual processes can be distinguished only if it can be demonstrated that particular independent variables have qualitatively different effects when the same perceptual information is presented at conscious and unconscious levels of stimulation. In our opinion, a demonstration of qualitative differences provides much stronger support for a distinction between conscious and unconscious perceptual processes than can ever be provided by any approach based solely on evidence indicating that perceptual information is processed both above and below a particular threshold. In fact, we feel that general agreement concerning the role of awareness in perceptual processing will never be reached unless qualitative differences between conscious and unconscious perceptual processes are demonstrated.

In summary, we feel that a twofold approach, based on subjective thresholds and qualitative differences, is needed to distinguish conscious from unconscious perceptual processes. Our research indicates that no evidence for any perceptual processing whatsoever is found when perceptual awareness is equated with an objective threshold and care is taken to establish this threshold by proper psychophysical procedures (Cheesman & Merikle 1984; in press). Thus, if perceptual awareness or consciousness is measured adequately by an objective threshold, as Holender assumes, then our research indicates that unconscious perceptual processing does not occur. On the other hand, our research also indicates that perceptual information presented above and below a subjectively defined threshold has qualitatively different behavioral effects (Cheesman & Merikle 1983; in press). We feel that such findings support the twofold approach we advocate. More important, demonstrations of qualitative differences provide an indication of how conscious and unconscious perceptual processes differ, and it is only by establishing qualitative differences that it will ever be possible to specify the critical differences that distinguish these processes.

What do you mean by conscious?

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What is happening with BBS and word recognition? First Humphreys and Evett (1985) and now Holender require unrealistic standards of proof before one is allowed to adopt particular theoretical positions. Humphreys and Evett were at least pushing a default theory, but it isn't clear at all what contribution is being made by the target article to the development of our ideas. Methodological surveys are important. We would, of course, need to be told if there were an artifact in, say, one of the Marcel (1983a) experiments, but that Holender is allowed to make such claims outside the context of, say, the Marcel (1983b) theoretical paper is a commentary in itself. Whether there are such artifacts we will no doubt be told by other contributors, but, in any case, it should not be lost sight of that what counts as an artifact depends upon the definitions of the terms in the theoretical description, which, in turn, depend upon the model one is using. Holender eschews theory, and so *conscious* and *semantic* are defined only in terms of particular procedures and by covert appeal to the everyday use of related terms such as *aware*.

In fact, consciousness can be regarded as epiphenomenal, or as a set of constructive processes equivalent in kind to the rest of the information-processing equipment or as a device that monitors the operation of other processes. The question of whether a subject displays conscious identification of a stimulus would be answered differently depending on which one of these theoretical positions one adopted. In all cases one would want to relate the theoretical use to our common experience of consciousness. After all, we would not have such a term available if we did not have experience in need of labelling. But Holender expects us to discuss consciousness while explicitly prohibiting phenomenology from the discussion and from the experiments. Being a subject in an experiment is an experience that too many experimenters deny themselves. The interest of the Marcel demonstrations (and Dixon's [1971; 1981] work, etc.) lies in the fact that as a subject one has not the slightest idea, strive as one may, of what is going on. That Marcel, at any rate, has had to adopt particular procedures to make the point (or, indeed, to get published) is a pity. But whether the presence or absence judgements happen to be 60% or 70% correct is really of little importance if one is actually talking about unconscious processes that, on some definitions, can be operating just as well with detection tasks as with any other.

If the debate is not about consciousness but is instead about the processing of verbal and pictorial stimuli then Holender equally fails to meet the demands. Again, this is because his discussion is couched in model-free terms. Thus, Marcel's experiments could be seen as attacking-stage theories of processing. With a stage theory, there would have to be an accessible outcome at the first stage of a multistage process before there could be any processing with accessible outcome at the second stage. If you can get semantic information without identification then that could be seen as supporting, rather, a cascade theory of processing. A theoretical issue of this kind can be decided without bringing "consciousness" into the discussion at all. However, it would require the use of a proper task analysis of the experimental paradigms being used in terms of the theoretical framework one has adopted. What one cannot do is look for (sophisticated) artifacts atheoretically.

As a climax I would like to challenge Holender's assumption that the null hypothesis should be that semantic activation requires consciousness. Why shouldn't the null hypothesis be the contrary? After all, the claims are not at all outrageous theoretically. Even very early versions of the logogen model (e.g., Morton 1968) accounted quite explicitly (albeit rather cavalierly) for unconscious processing of verbal stimuli. All it needs is two thresholds on the logogen – a very respectable, nonmystical information-processing account that would be entirely suitable for these lean economic times. Indeed, given my own theoretical history I feel perfectly justified in throwing out Holender's entire position, instead challenging him to *prove* that a semantic analysis requires consciousness to be engaged. My ancient theory (and its offspring) asserts that such is not the case – indeed, requires that it not be. Whose is the burden of proof?

Processing of the unattended message during selective dichotic listening

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Holender reviews literature from three paradigms that have yielded most of the results giving raise to claims for semantic activation without conscious identification of the stimulus. Holender is successful in casting considerable doubt on these claims. In the present commentary, I will focus on one of these paradigms, that involving dichotic listening, particularly the analysis of the unattended message during focused or selective dichotic listening, one of the key issues in Holender's paper. Recent research using event-related brain potentials (ERPs) appears to be of considerable relevance here.

Holender's main claim here was that there is no valid evidence for semantic activation of (any of) the irrelevant message when attention was stringently focused on the message delivered to the opposite ear, that is, when no conscious identification of irrelevant stimuli occurred. If no semantic activation indeed occurs in these conditions, it would be important to ascertain whether this is due to imperfect processing of physical stimulus features or to lack of semantic analysis of fully processed sensory data. The latter alternative is suggested by the ERP data. This evidence is provided by the ERP component called the "mismatch negativity" (MMN), isolated from the N2 wave (Ford et al. 1973; Squires et al. 1975) by Näätänen, Gaillard, and Mäntysalo (1978; 1980). MMN is an attention-independent, automatic brain response to a physically deviant stimulus in a sequence of repetitive homogeneous stimuli. MMN is sensitive to slight stimulus changes, even to those approaching the discrimination threshold (Sams et al. 1985). When tone pips are delivered, MMN occurs in response to pitch and intensity deviations in both directions (for a review, see Näätänen et al., in press). MMN is generated by a neuronal-mismatch process between the sensory input from a deviant stimulus and the neuronal representation of the physical features of the repetitive, "standard" stimulus (Näätänen, in press). This process, at least in response to a pitch change, can be localized to the primary auditory cortex by magnetoencephalographic methods (Hari et al. 1984). It is important to note that MMN is similar for the attended and unattended inputs in selective dichotic listening (Näätänen et al. 1978; 1980), which implies that the neuronal representations of the standard stimuli in both inputs must contain fully processed sensory information and that the comparison process occurring within each input is an equally elaborated (task-unrelated; see Näätänen, in press) processing of their physical stimulus features. The short-duration neuronal stimulus representations mentioned probably form the neurophysiological basis of the precategorical store or sensory register, at least in the auditory modality in which this is called echoic memory (see Näätänen 1984; Näätänen et al., in press). Even the time course of decay (a few seconds) of the neuronal representations is attention-independent (see Näätänen & Gaillard 1983).

On the other hand, no available ERP data appear to clarify whether semantic activation occurs to the unattended message of selective dichotic listening, although in principle the N400 wave of semantic incongruency or mismatch (Kutas & Hillyard

1980) might be used for this purpose (Hillyard, pers. comm. July 1984).

So far I have dealt with the case of a presumed "full" focusing of attention in selective dichotic listening. In laboratory reality, however, this focusing is often far from perfect, as becomes clear from Holender's review. The task may simply be too easy (require too little processing capacity). When it is demanding enough, the subject may trade off between the two inputs, and in any case appears to need some time to get his attention fully focused (Treisman et al. 1974). Moreover, when the subject has reached a sharp attentional focus, every now and then his attention is momentarily caught by certain physical aspects of the irrelevant input, most notably either *qualitative* (such as a tone in the middle of a spoken sentence) or *quantitative* (such as a word delivered after a break in the to-be-ignored input) changes. ERPs suggest some cerebral mechanisms of these involuntary attention switchings. In case of a qualitative physical change, the automatic neuronal-mismatch process described above appears to provide a central "interrupt" signal that causes attention switching to this change when the momentary threshold that varies as a function of a number of factors is exceeded (Näätänen, Simpson & Loveless 1982). As the neuronal-mismatch process is elicited even by slight changes, the system controlling the focus of consciousness is, apparently, frequently bombarded with these signals in any selective-listening situation. The resulting attention switching is, presumably, responded to by the controlling system by redirecting attention, but the focus is on the "wrong" side for a short while each time. Even such momentary attention switchings can be associated with abundant transfer of sensory information. This is due to the highly elaborate nature of attention-independent sensory processing: The "glimpse" to the "wrong" side reaches high-quality outputs of the ongoing sensory processes and precise representations of physical features of the stimuli of the immediate past. Hence attention switching, triggered by qualitative physical changes in the unattended input, provide, momentary contacts between these sensory data and the long-term memory system; this is where I see semantic activation by the to-be-ignored input as taking place.

A similar stimulus-driven attention-switching mechanism seems to be controlled by quantitative changes in sensory input. The most effective of such changes involve abrupt stimuli presented after a silent period: It is impossible not to become aware of discrete stimuli delivered to the otherwise silent "unattended" ear (Newstead & Dennis 1979). This attention-switching mechanism can probably be identified with some of the generator processes of the N1 wave of the ERP, particularly of its nonspecific component (see Näätänen & Gaillard 1983). The latter is very large after a period of a few seconds with no abrupt stimuli but is completely habituated when the stimulus is repeated with short interstimulus intervals.

In sum, ERP studies seem to reveal the rich realm of precise automatic sensory analysis and storing occurring outside our conscious awareness. Moreover, ERPs also suggest some mechanisms of involuntary, stimulus-driven attention switching by which these sensory data can reach our conscious experience (and cause semantic activation).

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On determining what is unconscious and what is perception

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Holender's thorough review is timely and penetrating, but what are we to make of it? It seems to me that its main asset lies in the fact that it serves as an excellent illustration of the kind of tail-chasing we cognitive psychologists can get involved in when we overlook the conflict between our perpetual interest in issues that are undecidable by publicly observable data and our deeply rooted commitment, inherited from the positivistic tradition, to observable data as the only means of testing theoretical claims in psychology. To practice empirical science properly, we need tests whose validity does not depend on the fate of the issues being tested. Is this the case here?

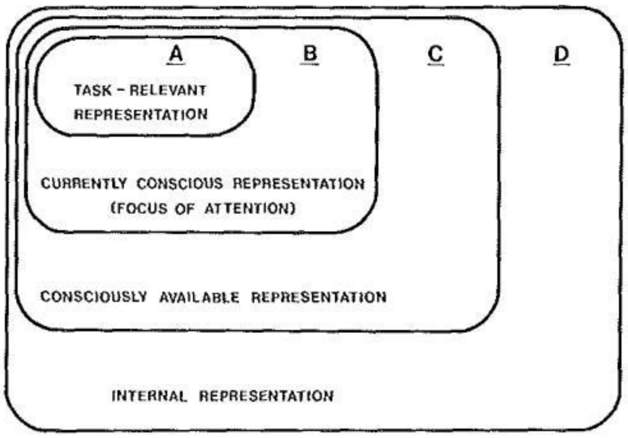

Figure 1. A hypothetical classification of the output of perceptual processing.

What is the touchstone for conscious identification? The issues addressed in various discussions of unconscious perception concern the relationship among the concepts depicted in Figure 1: (a) Does B & $\bar{A}$ exist, or is the perceptual process totally subordinated to intention? (b) Does C & $\bar{B}$ exist, or does perception require or entail focal attention? (c) Does D & $\bar{C}$ exist, or is all the information yielded by perceptual processes *potentially* available for conscious inspection? The rules of the positivistic game demand that to test these issues, any evidence for the availability of processed information should be coupled with a demonstration of the failure of perceptual tasks that are *known* to require the property that is hypothesized to be unnecessary, such as availability to consciousness. This is, for example, the assumption implied by Dixon's (1971) criterion 1, at least when the latter is taken as a necessary and sufficient condition, as it is by Holender. However, do we really know of such tasks, or do we simply delude ourselves into thinking that we know?

More specifically, criterion 1 postulates that had unconscious perception existed, it could not have been manifested in a "direct" test. Is this so evident? Suppose it had been indicated that people can perform at a better than chance level in a direct test even with no awareness of whatever guides the choice of responses? Could that not be taken as evidence par excellence for unconscious perception? Consider, for example, phenomena such as blind sight [see Campion & Latto: "Is Blindsight an Effect of Scattered Light, Spared Cortex and Near-Threshold Vision?" *BBS* 6(3) 1984], performance controlled by the right hemisphere in split-brain patients [see Puccetti: "The Case for Mental Duality *BBS* 4(1) 1981], or unconscious performance of tasks under hypnosis. Regardless of how relevant those phenomena are for the behavior of normal subjects in a normal state of awareness, they certainly challenge the conception of perception as a process the output of which is always consciously available.

Hence the existence of unconscious perception need not (and perhaps cannot) be determined by the dissociation between the results of two types of operational test, however interesting such a finding is in itself. To test whether a given item of perceptual information the presence of which is indicated by indirect methods is actually unavailable to consciousness, we simply have to judge whether it has any reflection in phenomenal experience. If experience is not acceptable as a legitimate source of data, then the issue itself must not have been scientifically legitimate in the first place. Since consciousness is a label for a class of experiences rather than a theoretical term, when we ask questions about it we invite phenomenal observations as pertinent evidence. And indeed it usually goes without saying that a hypothetical process of which we have no phenomenal indication is to be treated as unconscious (e.g., unconscious inference, unconscious memory search, unconscious activation of all meanings of a polysemous word). Substantiating such predications empirically is impossible.

So why bother attempting it in the case of identification: It seems to me that the reason we do so is that issues (a) through (c) enumerated above are central to major controversies in cognitive psychology: (a) is relevant to the determination of the source of initiative for perceptual processes; (b) is relevant to the debate about the locus of selection, and (c) is relevant to the question of whether the encoding of stimulus information is necessarily mediated by some storage device that is accessible to consciousness.

Different paradigms for different issues. Since these issues are deemed so important, what can be done to address them empirically? Holender does not do them justice by lumping them together. Different paradigms suit different issues.

The dichotic listening paradigm is used to study whether focal attention is necessary for perception, namely, issue (b). If it were not necessary, that would constitute support for late-selection theories, even if the product of that nonfocal perception were *potentially* accessible to awareness. In fact, the paradigm is designed to ensure that the only factor that could ex hypothesi preclude perception is the lack of attention, so that the locus of selection should be ascertainable.

Granted that it is usually just as futile to investigate the processing of the rejected channel by means of a direct test as it is to explore the properties of darkness by aiming a spotlight at it. Thus, to ascertain that the to-be-ignored channel is really unattended, we have to trust the manipulation itself (one that has repeatedly been shown actually to manipulate the allocation of attention) as well as indirect indices such as momentary fluctuation in the quality of performance on the to-be-attended channel. This is an inherent weakness in the study of attention-free processing, yet it is a weakness that whoever is nonetheless committed to study this subject matter has to put up with.

However, when the issue under investigation is rather whether some products of perception are inaccessible to awareness even under intense scrutiny of the attentional beam, namely, issue (c), then it becomes mandatory to secure full attention to the stimuli: hence the only way to deny them access to awareness is by degrading data quality or by curtailing the period of processing. Thus, the masking paradigm is suitable for studying *this* issue.

But here we return to the question of how the issue can be decided. As claimed above, criterion 1 may be sufficient (if the problem of response bias is met), but it is clearly not necessary. Criterion 3 would be appropriate only if it were surmised that representations that are and are not available to awareness arise from two different types of perceptual processes. A more economical conjecture is that there is only one type of process and that its products are not equally available for various subsystems of the mind (cf. Erdelyi 1974). Criterion 2 obviously seems quite weak in the eyes of orthodox positivists, but since awareness has the elusive characteristic that it often changes when inspected too closely, unobtrusive methods may be indispensable.

Thus, it is unlikely that even if Holender's methodological recommendations are followed the issue will be resolved. Converging evidence from all criteria will be required. Furthermore, no amount of positive experimental evidence for conscious identification is likely to convince thousands of investigators and subjects if their phenomenal experience points to the contrary. And as noted above, when the question concerns the contents of consciousness, phenomenal experience is after all the ultimate criterion. On the other hand, when the question is whether subliminal perception exists, and the limen is interpreted in the pure operational sense, then the disparity between various indices for perception becomes crucial, and Holender's methodological caveats become most relevant.

Semantic activation – process or product? So much for the criteria for deciding whether unconscious perception is indeed unconscious. Now, how do we establish that it is indeed perception? We must distinguish between a relatively stable representation within which certain characteristics of the stimulus are explicated (Marr 1982) and the process whereby such a representation is generated. The term *perception*, at least in the present context, should be reserved for the former. Yet, during the processes of analysis, a number of perceptual hypotheses may be examined, and numerous memory nodes may be temporarily activated (e.g., consider the word recognition model of McClelland & Rumelhart, 1981). Those transient events may leave no trace in the representation that emerges as the final outcome of the process, yet they may induce by-products such as changes in the level of activity of many related memory nodes that may in turn produce observable effects such as priming, changes in galvanic skin response, and so on. In the literal sense, that would be semantic activation, but I doubt this is the sense that most of us have in mind when we think of unconscious identification.

Summary. In sum, dichotic listening tasks are appropriate for testing whether perception requires attention. The separate issue of the possible presence of perception that can never be brought under the spotlight of awareness should indeed be investigated by paradigms such as masking. However, the ultimate criterion for availability to awareness must be phenomenal experience. The discrepancy between thresholds of different perceptual indices is an important empirical finding, but its theoretical interpretation is not straightforward. In addition, it is suggested that we worry about the possibility that so-called indirect evidence reflects side effects of perceptual processing rather than the contents of its final product. In that case, what are being observed are vestiges of the processing of stimuli that do not make it to awareness. Whether those stimuli are below the threshold for overt response is an open question.

The pilfering of awareness and guilt by association

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Holender's target article and similar arguments advanced by Purcell, Steward, and Stanovich (1983) and Cheesman and Merikle (1984) have excised subjective experience and awareness from the body of consciousness. No wonder there's nothing left. To equate "conscious identification with the ability to respond discriminatively to a stimulus at the time of presentation" is to impose a criterion that ignores an essential aspect of consciousness. An operational definition of consciousness must appeal to the notion of awareness. It simply does not make sense to say that the thermostat in my house is conscious of New Mexico's hot days and cool evenings.

Holender's definition of conscious identification seems to abuse the concept of identification as well as that of consciousness. In everyday use *identify* is reserved for occasions when we can name a unique instance ("The suspect was identified as Tony Marcel") or assign an instance to a category ("Marcel was identified as one of the troublemakers"). It is never used to imply discrimination in the absence of the ability to categorize. For example, one might say, "They look different, but I can't tell you which is Marcel." But in the same situation it would be bizarre and misleading to say: "I can identify them, but I can't tell you which is Marcel." Psychologists speaking their jargon also tend to treat identification as more than simply discrimination. For example, the controversy concerning the categorical perception of speech sounds hinges on the difference between identification and discrimination performance (Liberman, Harris, Hoffman & Griffin 1957; Paap 1975).

The discriminability definition of consciousness seems to eliminate, or at least obfuscate, an interesting psychological state. The patients described by Marcel (1983b) who suffer from blindsight lack a phenomenal visual experience. Despite the fact that they are not consciously aware of the identity of a stimulus presented to their blind side, they can make successful forced choices at least in gross shape discriminations. Does this mean that we should conclude that these patients can "really" consciously identify the stimulus? Does this explain why words presented to the blind side can bias the meaning of auditorily presented polysemous words? Does this mean that blindsight is functionally like a bad case of myopia? [See also Latto & Campion, this issue.]

Similarly, I think there is something interesting to explain in the case of pattern masking. At sufficiently short stimulus onset asynchronies (SOAs) observers exhibit blindsight in the sense that they have no visual experience of the target word (chance-level detection threshold?) and are not aware of its identity. When in this state most observers would find it very surprising (and perhaps disconcerting as well) to learn that these words were producing semantic priming. You would not remove the mystery by telling them that, although they couldn't see the words, their forced-choice guesses were above chance. This statement would probably only add fuel to their burning skepticism. Any plausible explanation of this phenomenon seems to fall back on the notion that stimuli can be analyzed at a deeper level than that which reaches conscious awareness. Restating this conclusion somewhat more specifically, the evidence seems strong that semantic activation can occur without a concomitant visual experience and without an awareness of the word's identity, that is, under conditions when observers say that they can't see it and don't know what it is. Why is this not an interesting question? Why would it become interesting only if this subjective experience (subjective lack of experience actually) were accompanied by an objective failure to discriminate from among a set of forced choices?

If we want to study the relationship between conscious awareness and semantic priming then we should select values of the independent variable (SOA) in the case of the masking paradigm) that correspond to changes in conscious awareness. To complain when this is done and to advocate a procedure that appeals slightly (or not at all) to the observer'sphenomenal experience seems to argue that conscious awareness lies outside the scope of our theories and that we should really study a different issue. If we wanted to, we could legislate the possibility of semantic activation without conscious identification (SA/CI) out of existence by simply eliminating the requirement that the discriminative response occur at the time of stimulus presentation. Thus, we could conclude that Marcel's subjects were obviously aware of the masked primes since the response times to the targets indicate a clear ability to discriminate related from unrelated primes.

I see no evidence that scientific virtue is being compromised by an uncritical acceptance of SA/CI. The evidence using visual masking was examined with great care and two subtle types of confounding were detected. Fowler et al. (1981, Experiment 3) have shown that the stimulus pairs used in semantic similarity judgments can have "chance" baselines well above 50% and Purcell et al. (1983) have shown that sensitivity during priming trials can be greater than that during threshold trials if priming trials involve an increase in light adaptation.

The discovery that some of the studies purporting to demonstrate SA/CI were flawed seems to have led to a rush to condemn them all ("none of these studies has included the requisite controls to ensure that semantic activation was not accompanied by conscious identification of the stimulus of the time of presentation." I will briefly appeal one case, although other defendants could be found for a class action suit. Marcel's (1983a) Experiment 5 was a semantic priming study that randomly mixed detection and lexical-decision trials. There was no opportunity for differential light adaptation. After each detection trial subjects were asked to guess a word that might have appeared before the mask. The prime-mask sequence was repeated up to 20 times per trial. Priming increased as a function of number of repetitions, whereas the probability of correct detection did not. The masked word was guessed correctly 3 times out of 1,400 opportunities.

A final conjecture is that the Stroop paradigm may be a red herring. Marcel's (1983b) discussion of inhibition as a concomitant of conscious intention provides a compelling explanation for why unconsciously activated polysemous words facilitate all associated meanings, but this assumption seems bankrupt (or is it flooded?) in the face of the interference generated by unconsciously activated color words. Perhaps strong Stroop interference will occur only with conscious identification.

Against semantic preprocessing in parafoveal vision

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The issue of semantic activation without conscious identification (SA/CI) is highly controversial, and Holender has taken a very definite stand with respect to it. Proponents of the apparent phenomenon believe that it has a great deal of theoretical importance and explanatory power. Those who are opposed to the concept feel that it seems too mystical, relying on almost magical capabilities of the human information-processing system. Holender argues that enthusiasm for the phenomenon is premature, that many of the results are artifactual, and that the data can probably be explained by the subjects' having consciously identified the stimuli.

I have never found the dichotic listening experiments purporting to show the phenomenon very convincing. I find the pattern-masking experiments very intriguing, but, like Holender, I have never been sure that subjects were not sometimes identifying the masked prime. Holender's suggestion that experimenters should check on each trial to determine whether subjects knew what the prime was seems very good. Inhoff and I (Inhoff & Rayner 1980; see also Inhoff 1982) used such a procedure with parafoveally presented words and found no evidence of SA/CI, as Holender noted.

With respect to SA/CI for parafoveally presented words, Holender points out that the evidence supporting the idea is scanty. Some relevant evidence supporting Holender's thesis has been reported by Rayner, McConkie, and Ehrlich (1978) and Rayner, McConkie, and Zola (1980). The studies of parafoveal vision reviewed by Holender primarily dealt with the effect of a parafoveally presented word on a simultaneously presented foveal word. He concluded that there is little evidence for any type of effect. Another way in which SA/CI could occur is through a process in which semantic preprocessing occurs for parafoveal words. Such a process has occasionally been suggested for reading, in which SA/CI occurs before the reader fixates on the word. The experiments by Rayner et al. (and McClelland & O'Regan 1981) demonstrated facilitation in naming a word from prior parafoveal preview. In the task, subjects fixate on a target cross in foveal vision. In parafoveal vision a word or letter string is presented, and when the subject makes an eye movement to its location, it is replaced by a word, which must be named. In the most critical condition for present purposes, the word *chair* is presented parafoveally and is replaced by *table* during the saccade. Prime–target relationships of this type presented foveally are known to result in significant priming effects when both prime and target are consciously identified. However, when the semantically related prime is presented parafoveally, no facilitation occurs (Rayner et al. 1980).

The information that is primarily facilitative from the parafoveal preview has been shown to be the first few letters of the parafoveal word; if the first and second (the word that is named) stimuli share the first two or three letters, there is facilitation (Balota and Rayner 1983; Rayner et al. 1980). None of our studies (be they with this naming task or in a reading situation) have ever found any evidence of semantic preprocessing of parafoveal words. Indeed, the challenge offered by the findings reported by McConkie et al. (1982) for our views has little to do with semantic preprocessing. McConkie et al. argued that no information from a parafoveal word is useful in processing that word when it is later brought into foveal vision by an eye movement. (A recent study of ours [Balota, Pollatsek & Rayner, in press] indicates that the amount of contextual constraint and the particular dependent variables one chooses to analyze account for the discrepancy, but this is beyond the scope of the present commentary.) The important point is that we have not found any evidence for semantic preprocessing (which is another form of SA/CI) of parafoveal words or pictures (Pollatsek, Rayner & Collins 1984).

I end this commentary on a cautionary note. The enthusiasm with which SA/CI via the pattern-masking paradigm has been embraced by some researchers has sometimes let to unwarranted generalizations. For example, on the basis of his well-known pattern-mask studies, Marcel (1978) has suggested that in reading meaning is simultaneously available from a number of places on a page. As I have noted here and elsewhere (Rayner 1984), there is no evidence for such an assertion. Indeed, it is precarious at best to generalize from a brief, foveally presented, pattern-masked word to anything beyond that situation. Words presented in such a manner are not analogous to parafoveal words in reading. Both types of words are degraded, but they are degraded in different ways. I became most convinced of this a few years ago when we conducted an experiment similar to that reported by Pickering (1976). Pickering presented a word or nonword foveally for about 30 msec followed immediately by a word or nonword in the same spatial location; the subjects then made a lexical decision on the second stimulus. If the target was a word and had been preceded by another word, the response was faster than if it had been preceded by a nonword. Conversely, a nonword target was responded to faster when preceded by another nonword than when preceded by a word. There were a number of conditions in the experiment we carried out that are too complicated to discuss here (unpublished data). The basic point is that the pattern of results was very different when the prime was briefly presented to the fovea (and followed immediately by the target) compared to when the prime was presented parafoveally (for 175 msec) followed by the target in foveal vision.

In summary, it is my opinion that there is no good evidence for SA/CI in parafoveal vision. Subjects will, of course, sometimes be able to consciously identify parafoveal stimuli, and identification may well influence the data pattern. While the pattern-masking studies are intriguing, inappropriate generalizations should not be made from them. Holender's arguments against SA/CI in general and pattern-masking studies specifically present an interesting challenge for those doing work of this type.

Priming without awareness: What was all the fuss about?

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Holender's target article is very timely. Actually, it is more than timely. It is close to being overdue.

There will probably be wide agreement with two of Holender's conclusions: that the dichotic listening paradigm cannot provide the conditions necessary to demonstrate the phenomenon under consideration and that the parafoveal vision experiments, while somewhat less problematic, provide no strong evidence for it. It is the third major conclusion – that the results from masking studies do not warrant the conclusion that semantic priming without awareness has been demonstrated – that will be the center of dispute.

We would argue, however, that Holender's summary of the masking literature is accurate and that his conclusions are justified. Semantic paralexias are easily disposed of as an artifact. In addition, Marcel's (1983a) observation that better-than-chance semantic judgments could be made at stimulus onset asynchronies (SOAs) at which detection judgments were at chance has failed to replicate several times. The literature on semantic priming without greater than chance detection or identification of the prime is more ambiguous. Nevertheless, we believe that Holender's target article successfully demonstrates that all of the existing positive findings are subject to at least one of the methodological criticisms discussed by Cheesman and Merikle (1984), Merikle (1982), and Purcell, Stewart, and Stanovich (1983) or to one of Holender's own variants of these arguments. Indeed, there are other possible confounds in these experiments beyond those discussed by Holender and previous critics. For example, most researchers fail to consider the effective contrast level of their stimuli. The contrast level of the prime is a function of the adaptation state of the subject's eye. In a completely dark-adapted eye a brief, intense stimulus will provide much less effective contrast than would the same stimulus presented to a more light-adapted eye (i.e., an eye adapted by the target stimulus). This is particularly true when stimuli are presented against a dark or dim background, as was the case in many of the experiments under discussion (e.g., Balota 1983). The net effect of this change in effective contrast is an increase in the visibility of the primes from threshold to priming sessions. In additon, one should not lose sight of Merikle's (1982) original criticisms merely because we and Holender have seemed to emphasize adaptation-level explanations of previous results. If the basic threshold-setting procedure is not adequate, other criticisms are in some sense superfluous. This is relevant because many of the experiments that are subject to adaptation-level confounds are also plagued by inadequate threshold-setting procedures.

When one looks at the literature in its entirety, as Holender has done, there appear to be two things that preclude accepting the conclusion that semantic priming without identification of the prime has been demonstrated. The first is that that there have been enough clear replication failures (e.g., Cheesman & Merikle 1984; Experiment 3 of Purcell et al. 1983) to raise suspicions about the original findings. The second is that the positive demonstrations are virtually all subject to at least one of several demonstrated design criticisms (e.g., the differential masking demonstrations of Purcell et al., Experiments 1 and 2; Merikle's points about the reliability of threshold determination), and this makes it possible to attribute any observed priming to the fact that subjects could see the prime more often than would be expected on the basis of threshold determination.

Of course, the state of the evidence in this particular research area is not uncommon in cognitive psychology. Many of our effects are elusive. Hunting down artifacts in unlikely findings that have prematurely entered the textbooks is a major sport of doctoral candidates in search of a thesis topic. But given that the state of the evidence is as muddied as Holender has said it is, what in the world was all the hullabaloo about? Why was a reasonably large seminar room at the 1981 Society meeting Psychonomic bursting at the seams with people trying to cram into a seminar on priming without awareness? Surely the strong response to this line of research occurred because of how "awareness" was operationalized in terms of thresholds. However, as Cheesman and Merikle (1984) have noted, the issues have been confused because some investigators have inadvertently measured a subjective rather than an objective threshold, and secondary reports have been unclear about just what conclusions were to be drawn from this research.

Cheesman and Merikle (1984) define the subjective threshold as the level at which subjects *claim* not to be able to discriminate the stimulus and the objective threshold as the level at which discrimination is actually *at* chance. The latter threshold is, of course, lower than the former. There is little question that experimentation has demonstrated that semantic priming can occur below the subjective threshold. Subjects' responses are affected by stimuli they claim not to see. This is a manifestly important finding deserving of more attention than cognitive psychologists have given it. But it is not a revolutionary finding as long as the subjective threshold is above the objective threshold. Indeed, it is common in psychophysical experiments for subjects to be identifying stimuli at 75% accuracy and at the same time claiming to be just guessing. The Cheesman and Merikle (1984) paper contains a simple demonstration of this phenomenon. Their experiments also show that an amount of priming occurs that is commensurate with the amount of sensory processing of the prime (this is also implicit in Holender's discussion of the results in Table 3), but there is a whole range of sensory processing of the prime that the subject is unaware of. Again, fascinating – but quite different from the claim that priming occurs when the subject is at *objective* threshold, when he cannot identify the prime with greater than chance accuracy. This claim is indeed revolutionary, but, as Holender shows, there is no demonstration of it that is free from serious methodological problems and there exist several replication failures.

The priming without awareness literature will provide an interesting case study for the intellectual historian of the year 2000 who decides to analyze the development of cognitive psychology. Perhaps it will be said that by 1985 the field had come of age. With the newfound maturity, however, came a worry that cognitive psychology was getting just a little too fat and contented – getting a little soft, shall we say. There was a growing feeling that the standard paradigms and assumptions were being taken for granted, that they were being given a free ride without earning their way. What they needed was a little toughening up – a little more honing with the blade of critical science. Contributing to this general feeling was the fact that, like most social scientists, cognitive psychologists were uncritical readers of Kuhn. Clearly, when surveying the landscape in search of the anomaly that would trigger the new paradigm shift, what better candidate than the claim that a stimulus that is invisible even when the subject is fully intent on perceiving it can have as strong an effect on a behavior as a stimulus that is clearly seen? But, alas, what Holender has shown us is that the uncritical acceptance of anomalies can be as dangerous as the tendency to cling to the standard paradigms.

Facilitation or inhibition from parafoveal words?

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One of the problems described by Holender concerns the direction of the effect caused by parafoveal words. With some tasks they facilitate the response, and with others they cause inhibition. The following comments address the question of why parafoveal primes should have inconsistent effects. To uncover the various influences of primes upon targets in these tasks it is necessary to go beyong Holender's analysis of the visual characteristics of the displays and to specify the stages of processing involved in each of the effects.

Investigations of picture naming have, in general, found inhibition from a priming word that is a semantic associate of the picture. This is the case for words superimposed upon the picture (e.g., Lupker 1979; Rosinski 1977; Smith & Magee 1980) and for words appearing to one side of a fixated picture (Underwood 1976, Experiment 1). Under divided-attention conditions, however, picture-naming facilitation can be observed for associated words relative to nonassociated words (Underwood 1976, Experiment 2). When words are used as the target items, the direction of influence of associated primes is also dependent upon the task performed by the subject. The lexical-decision task produces an inhibition effect (Underwood & Thwaites 1982; Underwood, Rusted & Thwaites 1983), unless attention is divided (Dallas & Merikle 1976), and a category-decision task produces facilitation (Shaffer & LaBerge 1979; Underwood 1981). The items used in the control condition emerge as being of critical importance in semantic categorisation tasks, however, for whereas associates facilitate in relation to nonassociates, the nonassociates used in these experiments have actually been members of competing categories. These inhibition effects with nonassociates therefore reflect competition during category selection. When a word is to be reported under difficult viewing conditions then an associated prime can also produce facilitation (Allport 1977), as it can if the target is to be named and disambiguated (Bradshaw 1974) or used to provide additional information in the selection of a paired-associate response (Underwood 1980).

Picture-naming tasks. Presumably the stages involved in naming a line drawing of a familiar object include feature extraction, object recognition, name generation, and name output, and any of them are candidate stages for the locus of inhibition. It is difficult to imagine how an associate of a picture could inhibit feature extraction, however, for if the prime were to operate at this stage it would be to direct a search for picture features, and this would result in facilitation. When a name is to be generated for the object then inhibition can arise because the associate has a name that is a competitor to the picture name in the sense that the two lexical units share semantic features. In addition, the picture may augment the activation of the lexical representation of the associated prime by providing information about the prime. This lexical activation may then serve to provide a semantic distraction effect at the stage of picture-name generation.

Whereas most of the investigations of picture naming have reported inhibition effects, the pattern can be changed by making picture processing more difficult. Subjects had to divide their attention between two possible spatial locations in Experiment 2 of Underwood (1976), and this had the effect of producing associative facilitation, in contrast to a picture accompanied by an unrelated word. This same pattern (of associates producing facilitation relative to nonassociates) was found in an experiment in which subjects were required to report not only the picture name but also the identity of the prime (Underwood 1977b). However, nonassociates produced inhibition only when they could be reported. In the cases when the primes could not be reported, the associates provided the slower naming latencies. It may have been the case, then, that the earlier experiment resulted in nonassociative inhibition as a result of words being available for report. It is not clear from Holender's remarks on this experiment why it does not satisfy his criteria for semantic processing without awareness. The parafoveal words are effective and unreportable, and they produce an effect different from that produced when they are reportable.

Lexical-decision tasks. Only when the to-be-reported and not-reported words have spatial uncertainty is associative facilitation observed (Dallas & Merikle 1976), and the more time allowed for focusing the smaller is the facilitation effect. This pattern of effects is similar to those introduced by spatial uncertainty in the picture-naming task (Underwood 1976, Experiment 1 vs. Experiment 2). With attention focused upon the spatial location of the target, inhibition emerges in the lexical-decision task (Underwood & Thwaites 1982; Underwood et al. 1983). The stages involved in this task include letter-string identification (featural analysis), a search of the lexicon for an entry for the identified letter string, and, when an entry is found or when a deadline has expired, decision and output of the decision. As with picture processing, identification could be facilitated only by the provision of associative information, and so the effects are located after lexical access. A nonassociated prime arriving earlier than the target may provide inhibition if the interstimulus interval is long enough for the subject to generate an incorrect expectancy with Posner and Snyder's (1975a) conscious-attentional mechanism. However, in the experiments described here the primes were masked and presented as distractors that were to be ignored. The prime should have an effect by the fast-acting automatic activation mechanism rather than the slower attentional mechanism, and this allows for facilitation only. An associated prime may aid identification of the letter string (in which case a facilitation effect should be observed), but its recognition also creates a second source of lexical activation. When this activation shares semantic features with the target then difficulty may occur in deciding which item *is* the target. If isolation of the target activation is made difficult by the presence of competing activation in a nearby memory location, then only associates should produce inhibition. Slower lexical-decision responses may result from the necessity of an additional process of activation-identification. The items that cause the greatest inhibition are therefore those that are most like the item requiring the response, as in the Stroop effect.

Word-naming tasks. There is no reason to suspect that a parafoveal prime should influence the encoding of a fixated target. The target gains the advantage of being processed by a more sensitive area of the retina, and in many of the experiments described so far it is described to the subject as the to-be-attended or primary stimulus, with the prime described as a distractor that is to be ignored. If encoding were to be delayed by some manipulation such as visual degradation or even later presentation of the target, then encoding should be facilitated by an associated prime. The associative facilitation effect reported by Balota and Rayner (1983) is an example of prepresentation influencing encoding.

When verbal report of a fixated word is required the pattern changes and facilitation is observed: Allport (1977) took accuracy of report as his dependent measure and found that an associated prime increased the likelihood of successful report. In this situation the encoding of the target is difficult and so might be expected to be open to influence from any extra information available at the time of encoding. An associated prime makes available additional information about the target and thereby increments the lexical activation caused by the target and facilitates report. It is this provision of additional lexical information that results in faster paired-associate naming (Underwood 1980) and disambiguation of polysemous targets (Bradshaw 1974). With good encoding of a target that is to be named, a parafoveal associate would be expected to inhibit naming for the same reason that inhibition occurs with picture naming. Lexical distraction would require that the two sources of activation be separated before the correct name could be selected.

Word-categorisation tasks. Rather than have subjects name words, a number of experiments have used semantic categorisation tasks. The advantage over word naming is that the meaning of the word must be processed, and so effects of associated primes are more likely. Shaffer and LaBerge (1979) and Underwood (1981) have found that associated primes facilitate categorisation responses to fixated targets, relative to nonassociated primes, and in a slightly different paradigm Rosch (1975) found that a prime spoken at the same time as two words facilitated a same/different category response. Loftus (1975) concluded that the simultaneous spoken prime could influence only decision processes occurring after lexical access, and this may also be the case for the category responses with parafoveal primes. Facilitation results from associated activation incrementing the information collected about a particular category name. If a potential category response is "animal," then presenting two animal names rather than one will simply provide more information about the category "animal." Primes that are members of competing categories should, by this model, inhibit the categorisation response relative to primes that are neutral, and this is what happens (Shaffer & LaBerge 1979; Underwood 1981). The "nonassociated" words used here were in fact members of nontarget categories used in the experiment, and relative to nonwords and associated primes they inhibited processing.

However, the explanation is not quite straightforward, because associated primes inhibit processing relative to nonword primes (Underwood 1981). Because the primes did not affect encoding, the candidate stages for inhibition in the task are abstract categorisation of the encoded object name, selection of the category name, and output of the selected name. Shaffer and LaBerge used a manual response rather than category naming and found no difference between associative primes and nonword controls. They reported a small facilitation effect in fact, but the difference was not reliable. Associative inhibition could arise from competition at any of these stages except output, whereas inhibition from nonassociates is restricted to category selection. Given the difference between these two categorisation experiments and the difference in associative inhibition observed, this inhibition effect appears to be located at the stage of selecting a category name. If two associated words generate lexical activation at the same time, then it may be necessary to select the word that requires further processing. It is this selection process that incurs a time penalty in the categorisation task and in other tasks.

A final point concerns the nature of the failed replications that Holender cites without any cautionary note about the problem of the null hypothesis. Almost all the experiments reporting positive effects of parafoveal words employed high-contrast displays generated by card tachistoscopes and slide projectors (e.g., Bradshaw 1974; Dallas & Merikle 1976; Underwood 1976; Underwood & Thwaites 1982). Almost all the experiments reporting failures to replicate these effects used the low-contrast displays generated by cathode ray tube (CRT) screens (e.g., Paap & Newsome 1981; Inhoff 1982; Inhoff & Rayner 1980). In general, displays that project high stimulus energy to the parafovea of the retina tend to be associated with effective stimuli. The low stimulus energy projected by CRT displays, including our own in unpublished experiments, tends to be associated with the absence of any influence. Given the sensitivity of the parafovea this is perhaps not surprising, and it may be that effects of parafoveally presented words are apparent only under high-contrast viewing conditions.

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A review of the literature with and without awareness

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I believe that Holender's target article is misguided in several respects. I will examine two problems with it. The first is a slight misconstrual of our work on priming, and the second concerns a paradox in the definition of awareness.

Holender dismisses much of the work on priming without awareness on the basis of improperly set thresholds. He expresses two reasons for concern about thresholds. The first is that there were different degrees of dark adaptation in the threshold-setting sessions and the priming sessions. Purcell, Stewart, and Stanovich (1983) found that identification thresholds obtained with dark-adapted subjects were significantly higher than thresholds obtained with less dark-adapted subjects. Furthermore, they showed that in some experiments (e.g., McCauley, Parmelee, Sperber & Carr 1980) the subjects were less dark-adapted in the priming phase of the experiment than in the threshold-setting phase. The subjects in those experiments might therefore have been aware of the primes during the crucial phase of the experiment.

Holender dismisses our research (Fowler, Wolford, Slade & Tassinary 1981) on similar grounds. This dismissal is inappropriate. Our lighting conditions were not the same as those used by McCauley et al. or by Purcell et al. Our subjects were dark-adapted prior to the start of both the threshold-setting and the priming phase of the experiment, but lighting conditions during the experiment probably reduced the degree of dark adaptation considerably during the course of a session. During the threshold-setting sessions in our experiments, the illuminated fixation field was in view at all times when the stimulus or mask was not in view except for the interstimulus interval between the target and mask (a period ranging from 10 to 70 msec). During the priming phase, the lighting conditions were similar except that the tachistoscope was also dark during the interval between the offset of the mask and the onset of the prime (an interval of about 1,950 msec). Although it is true that the target field (500 msec in duration) was brighter than the fixation field, the tachistoscope was lit a higher percentage of the time during the threshold-setting sessions. The total amount of light available, then, was roughly similar in both phases of the experiment, and in neither phase were the subjects very dark-adapted. In support of this contention, the average threshold stimulus onset asynchronies (SOAs) achieved by our subjects were quite low. In fact, they were among the lowest of any related research. Holender argues that McCauley and others obtained very high thresholds due to dark adaptation during the threshold sessions. (The SOAs in McCauley et al. with dark-adapted subjects in threshold sessions ranged from 53 to 68 msec on average. When Purcell et al. controlled lighting conditions across the two phases of the experiment, they achieved an average SOA of 25.89 msec. Our SOAs averaged 19 and 22 msec in the two priming experiments.)

Holender's second concern about thresholds is that most investigators used too few trials or used insensitive procedures in arriving at the thresholds. This might or might not be true in our case, but the low-threshold SOAs provide some evidence that our procedures were reasonably sensitive.

My primary concern with the target article is the definition of awareness or consciousness and the procedures recommended for establishing the lack of awareness. Holender defines priming without awareness as "positive indirect evidence of activation (the semantic priming effect) together with negative direct evidence of identification (inability to make a voluntary discriminative response to the prime)." The latter response can take several operational forms but often involves a forced-choice judgment concerning the identity of the prime. This forced-choice judgment is supposedly a direct measure of awareness even though there are experiments in which subjects are above chance on the forced-choice judgment but completely deny seeing the prime (Cheesman & Merikle 1984). What does it mean to claim that subjects are aware of stimuli that they deny seeing? Furthermore, is it reasonable to assume that forced-choice judgments are unaffected by information that we are not aware of? For instance, assume that we *can* have semantic priming from information that is below our awareness threshold. The experimenter now asks us to make a forced-choice judgment concerning some of those primes presented below the awareness threshold. Given our assumption, the correct alternative should have a higher than average level of semantic activation. If the subjects were sensitive to the level of semantic activation, they could use that information to respond correctly on some of the forced-choice trials. Greater-than-chance performance on those forced-choice trials, then, would not necessarily indicate awareness. Furthermore, it is possible to imagine that there is more than one threshold: one that must be exceeded to achieve awareness and a lower one that must be exceeded to achieve any sort of semantic activation. Based on the previous analysis of the possible relationship between priming and forced choice, a procedure such as that used by Cheesman and Merikle (1984) or by Purcell et al. might arrive at SOAs below the latter threshold, eliminating the possibility of observing semantic priming. This would depend in part on the relative sensitivity of semantic priming and forced-choice judgments to semantic activation.

My belief, then, is that the procedures suggested by Holender for determining awareness are inherently flawed. Although I recognize numerous problems such as differential criteria, I still believe that our best test for awareness is to ask the subjects. Using this criterion, there are numerous demonstrations of semantic activation without awareness, including demonstrations by many of the critics on the topic (e.g., Cheesman & Merikle 1984).

Author's Response

Conceptual, experimental, and theoretical indeterminacies in research on semantic activation without conscious identification

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There are a priori conceptual problems associated with demonstrating SA/CI (semantic activation without conscious identification), problems that arise before any experiment is set up. Two commentaries in particular provide valuable clarifications that help organize this discussion: **Erdelyi** gives us a concise but complete analysis of the indeterminacies in what he calls "the dissociation paradigm of the unconscious," and **Macmillan** provides some clear definitions of the concept of subliminality in current psychophysics.

It will be useful to reformulate the problem of SA/CI in terms of **Erdelyi**'s dissociation paradigm, in which one attempts to make the subject unaware of the stimulus or of certain aspects of it. In the experiments reviewed this was achieved by impoverishing stimulus quality (masking), diverting the subject's attention from the processing of the stimulus (dichotic listening), or both (parafoveal vision). Assuming that the information about some aspects of the stimulus is represented somewhere in the subject's brain (and accessible by a process called "semantic activation" because the review concerns stimulus meaning), one looks for a discrepancy between two indicators of the availability of information: One is an indicator of availability to awareness (direct evidence of semantic activation, which I equated with "conscious identification"), the other of availability to the processing system ("indirect evidence"). Logically there are two possible dissociations between indicators, but empirically there is only one because a consciously identified stimulus necessarily has its semantic representations activated (this corresponds to criteria 1 and 2; Section 1).

Indicators of awareness. There is some confusion among commentators about what was considered an indicator of conscious identification in the target article. **Fowler** incorrectly concludes that *only* verbal report was accepted as an indicator, whereas **Latto & Campion** explicitly advocate relying on verbal report because they are dissatisfied with a definition equating awareness with the ability to respond discriminatively to a stimulus.

Because of the heterogeneity of the experimental situations under review, one must in practice consider a range of possible responses as indicators of awareness. In studies concerned with dichotic listening (Section 2), parafoveal vision (Section 3), and unidentifiable masked primes (Section 4.3.1) subjects are required to demonstrate their conscious identification of the stimulus by naming it immediately upon presentation or by recalling it somewhat later. This corresponds to unrestricted verbal report because stimuli are drawn from a large pool. In the more stringent masking studies (Sections 4.2 and 4.3.2) subjects must perform either a forced-choice discrimination (often a choice between two stimuli, sometimes four) or a forced-choice detection (a presence–absence judgment).

Paap is right that including under the heading of "conscious identification" responses recorded under any of these conditions involves stretching the meaning of the concept of identification somewhat. Forced-choice detection or discrimination does not imply identification. Moreover, verbal labeling is not the only indicator of true identification. Consider a commissurotomized patient with an object presented exclusively to his right hemisphere. If this patient can point to an object in the same semantic category, choose it among a series of objects by touch only, or mime its use, he is certainly demonstrating a form of conscious identification, even though he cannot name it.

One property common to all these indicators of awareness is that subjects make their responses intentionally. It is fundamental that an indicator of awareness must be intentional. This is stressed in the first two paragraphs of the target article, where direct evidence of semantic activation is defined as "any voluntary discriminative response that can be elicited on the basis of the meaning of a stimulus" and direct evidence of semantic activation is in itself taken as evidence for conscious identification. This hardly endows thermostats, even **Paap's** thermostat (or **Latto & Campion's** photocell), with consciousness.

Implicitly or explicitly, almost all the commentators seem to agree that masking is the only method among the three reviewed that can be used to implement the dissociation paradigm (criterion 1). Hence conscious identification is generally operationalized by above-chance performance in a forced-choice detection or discrimination task. Many commentators are dissatisfied with such a criterion (e.g., **Latto & Campion, Merikle & Cheesman, Stanovich & Purcell, Wolford**). Their criticism takes two forms. First, both **Navon** and **Paap** claim that subjects who have no subjective experience of the stimulus would be astonished to learn that their forced-choice performance was above chance or that any semantic priming had occurred. Second, if after tightening the methodology to reach a true $d' = 0$ in forced-choice detection we still find evidence of semantic activation, this will tell us nothing about consciousness or the lack of it (**Bisiach, Fowler, Morton, Navon, Paap**). Hence, operationalizing conscious identification in terms of forced-choice detection or discrimination serves to excise "subjective experience and awareness from the body of consciousness" (**Paap**). It follows that either subjective phenomenal experience is taken into account and studying SA/CI under pattern masking makes sense, or subjective experience cannot be operationalized and the question lies outside the scope of scientific inquiry.

Threshold for what? The threshold question will be discussed exclusively with respect to pattern masking. Threshold setting consists of finding an appropriate stimulus onset asynchrony (SOA) between the beginning of a brief presentation of a high-contrast stimulus and that of a high-contrast pattern mask. **Paap** claims that if we want to study the relation between awareness and priming we should select an SOA value corresponding to a change in conscious awareness. **Navon** and **Wolford** make a similar case in suggesting that the best way to probe a subject's conscious identification is simply to ask him to comment on his subjective experience. Also, in being able to name a stimulus (verbal report) while no restriction on the number of possible responses is imposed, a subject certainly communicates an aspect of the contents of his consciousness (**Latto & Campion**).

Lupker's commentary is pertinent here (see also the recent debate between Henley, 1984, and Merikle, 1984). The problem is that conscious identification is simply not an all-or-none affair. At a certain SOA a stimulus can sometimes be seen vividly and sometimes almost not at all. There is a whole range of SOAs for which subjects have partial information about the stimulus. Partial cues can be used to guess the identity of the stimulus, especially in a forced-choice discrimination situation (**Lupker's** level e).

A small digression here may be helpful in assessing the validity of many experimental results. This problem could be called the *episodic memory trace fallacy*, but because of its close relation to the validity of criterion 2 for demonstrating SA/CI, it will henceforth be called the *criterion 2 fallacy*.

Consider three states leading to a correct response in a forced-choice discrimination. In state 1 the subject sees the stimulus clearly; he could have reported it even without restrictions on the number of possible choices. In state 2 the subject makes a correct guess on the basis of a combination of his knowledge of the stimulus possibilities and his conscious perception of partial visual information (sophisticated guessing). In state 3 the subject makes a correct response by learning to respond discriminatively on the basis of partial visual cues (perceptual learning). I assume that even if at the time of responding the subject could justify his response in each state (providing a different reason in each case, of course), the situation would rapidly change as time went on. In other words, the episodic trace of the correct choice will be different in each state, being good in state 1, perhaps even better in state 2 (see Jacoby 1983), and considerably poorer in state 3 (see Smith, Theodor & Franklin, 1983, for tentative supporting evidence.) As long as there are no clear empirical data about the time course of episodic traces left by correct responses in each of these states (and many other states as well) one cannot use criterion 2 (alone, or in combination with criteria 1 and 3) to draw noncircular conclusions about SA/CI at stimulus presentation time.

To return to the main problem: **Lupker** analyzes the situation correctly in claiming that we will find ourselves trapped in unending controversies concerning where we should set a threshold on the continuum of knowledge in order to separate semantic activation with and without conscious identification. Anticipating that this would be the case, Marcel (1983a) decided to choose a detectability threshold to separate conscious and unconscious perception. It is clear that what he intended to show was that with a $d' = 0$ (or close to 0) for detection (an objective threshold) semantic priming still occurs. In his commentary **Marcel** again stresses that pattern masking "is the only technique reviewed to which strong criteria of awareness can be applied."

We are back where we started. Some commentators take it for granted that at the level of a true $d' = 0$ for detection, the likelihood of finding evidence of semantic activation is very small (**Johnston, Lupker**). For others, finding priming under such conditions would indeed be startling (**Fowler, Macmillan, Navon, Paap**), but it would not tell us anything interesting about consciousness because phenomenal awareness would not be taken into account.

The last stage in the development of this controversy is that, happily enough, threshold-setting methods have been so clumsy that a very stringent, objective detection threshold has never been attained. What has inadvertently been attained is instead a subjective threshold (Cheesman & Merikle 1984, in press). This is close to what we are looking for if we want to take a subject's awareness into account (**Fowler, Merikle & Cheesman, Stanovich & Purcell, Wolford**).

An objective threshold is defined in terms of stringent criteria such as the SOA at which $d' = 0$ for detection or forced-choice distrimination. A subjective threshold is defined in terms of a subject's rating of his performance; it corresponds to the SOA at which he still claims to perform at a chance level in his forced choice while already being above chance objectively. Adams, in reviewing the evidence for this notion in 1957 (at which date there existed no study more recent than 1939), wrote that "there seem to have been no published studies of learning to judge one's uncertainties more accurately (i.e., to eliminate the discrepancy between confidence judgment and performance) or to manipulate the discrepancies through practice, knowledge of results, reward, or punishment" (Adams 1957, p. 388). Almost three decades later, even with signal detection theory's intervening influence, the situation appears to be unchanged.

Objective threshold definition. An extremely useful analysis of what "below threshold" could mean in the framework of current psychophysics is provided by **Macmillan**. No one would disagree with him that his definitions a and b are both unsatisfactory and that classical threshold theory should be abandoned. Only his definition c is acceptable. In this case a subliminal stimulus is one that leads to the detect state as often as the null stimulus, a condition implemented by $d' = 0$ in signal detection theory.

Macmillan also stresses that signal detection theory takes no stand as to whether or not a below-criterion stimulus is consciously perceived. **Bisiach** makes a similar case (see also Haber 1983a). If one decides to adopt Macmillan's definition c, however, in which below threshold is equated with below criterion, an interesting property emerges, as Macmillan demonstrates in his analysis of the results of Lindner (1968). If one asks subjects for forced-choice detection and a forced-choice identification on each trial, above-chance identification can occur even on trials for which the detection response is negative. This signal detection analysis removes much of the mystery from the results reported by Lindner (1968) and Rollman and Nachmias (1972), which are often taken as evidence for subliminal perception. Notice that a similar observation made by Marcel (1983a, Experiment 1) has never been replicated (see Section 4.2 and also the results of Dagenbach & Carr, 1982).

Erdelyi wonders about the adoption of a near-zero value for d'. This criterion appears to reflect the general attitude in the field. For example, **Fowler** finds that arguing that forced-choice detection and identification were not exactly at chance misses the mark because subjects were clearly unaware of the stimuli. **Morton** claims that it does not matter if presence–absence judgments are 60% or 70%. **Marcel**, in dealing with the possibility that subjects are slightly better light-adapted during the priming than during the threshold phase, writes that it is "subsidiary to the issue of whether subjects are *aware* of the masked word in the critical phase of the experiment."

It is hence evidence that no one has really been attempting to reach a $d' = 0$. Instead, a rather stringent criterion such as forced-choice detection is adopted and then measured leniently. The objective is to reach a point at which the subject is unaware (has no phenomenal experience) of the identity of the stimulus. In other words, investigators have been looking for a subjective threshold without knowing exactly how to get it but believing that there exists a whole range of SOAs for which subjects simply cannot identify the stimulus even if they can detect it.

The claim in the target article that prime availability in the priming phase could actually be better than one would believe on the basis of the threshold phase was intended to imply that many primes can actually be seen (i.e., consciously identified). The point is conjectural and the solution empirical, but in the experiments of Purcell, Stewart & Stanovich (1983) a small increase in light adaptation did cause a considerable increase in prime reports.

Macmillan's clear account of the rationale behind the development of signal detection theory provides a partial answer to **Dixon**'s points 1 and 2. Obviously, the assumption that subjects could lie played only a minor role in the development of this theory. Within a certain range of sensory states subjects must make decisions in partial uncertainty. Under such circumstances there is a conflict between contradictory demands; it is impossible to win both ways (e.g., increasing hits without increasing false alarms, or increasing speed without increasing errors). The word *conflict* was in the dictionary well before the existence of psychoanalysis and its offspring.

Criterion 3: Qualitatively and quantitatively different effects. This point concerns the indicators of availability of information to the processing system. The commonly used indicator is a semantic priming effect measured in a subthreshold and a suprathreshold condition. There could be not only a quantitative difference between these priming effects but also a qualitative one. It is generally held (**Dixon, Evett, Humphreys & Quinlan, Fowler, Merikle & Cheesman**) that finding a qualitative difference between the effects in the two conditions implies that different underlying processes mediate the effects; that is, criterion 3 is met.

One should be very cautious not to confound the logical status of possible outcomes with their probabilistic status. There is a logical independence between the existence of distinct processes and the observation of qualitatively different effects. We may nevertheless have reason to think that one is more likely to find qualitatively different effects when different underlying mechanisms are involved in each condition than when only one mechanism is (a possibility envisaged by **Navon**). It is on the basis of such an assumption that **Dixon** (point 3) insists that the effects should really be qualitatively different.

It follows that if we satisfy criterion 1 (i.e., if we set an appropriate threshold for an indicator of awareness and find a priming effect) we can conclude that SA/CI exists. If, in addition, we are lucky enough to get a priming effect qualitatively different from that of the suprathreshold condition, our conclusion is further reinforced.

Three points should be kept in mind to avoid confusion. First, because of the logical status of the relation between effects and processes, meeting criterion 3 is incidental, not necessary. Second, the quest for qualitatively different effects does not exempt us from measuring a threshold, a point Dixon has stressed forcefully (Dixon 1971, p. 18); in other words, we are still dealing with a dissociation paradigm. Third, we may get qualitatively different effects from visible primes.

It should be clear from these three points that it is fallacious to make the two-step deduction that observing qualitatively different effects in two conditions implies that different processes are involved in each condition, and that this in turn implies that primes are available to awareness in one condition and not in the other. Such an inference will henceforth be called a *criterion 3 fallacy*. This should clarify my position regarding criterion 3 for **Evett et al., Fowler, Merikle & Cheesman**, and **Underwood**.

To reply to **Dixon**'s point 3 more completely, it appears that he may be misunderstanding Posner and Snyder's (1975a; 1975b) approach in the cost–benefit analysis. This analysis is indeed based on finding qualitatively different priming effects. From observing a benefit (facilitation) without a cost (interference) in the priming effect Posner and Snyder would infer the existence of an automatic, unconscious, spreading-activation process; from observing a benefit with a cost in the priming effect, they would infer that a conscious preparatory process was involved.

Merikle & Cheesman's distinction between objective and subjective thresholds. Cheesman and Merikle (1984, in press) proposed to resolve the controversy about which criterion to use for SA/CI with a two-level approach in which awareness is defined in terms of a subjective threshold and criterion 3 is met. The extent to which this approach is successful can only be assessed by analyzing their procedure in some detail. Experiment 2 (Cheesman & Merikle 1984) and Experiment 4 (Cheesman & Merikle, in press) will be considered.

Merikle & Cheesman measured an objective discrimination threshold by requiring a forced choice between four color words (*red, blue, green*, and *yellow*) masked by a pattern. The subjective threshold was determined by asking subjects to rate their performance after each block of 48 trials (Experiment 2) or 96 trials (Experiment 4). At the SOA corresponding to the objective threshold (25% correct forced choices) it was shown that the color words produced no Stroop effect at all on the naming of a surrounding color patch. The SOA at which subjects rated their performance to be at chance was longer than that corresponding to the objective threshold. At this subjective threshold SOA, identification performance was much better than chance (66% correct forced choices in both experiments) and a substantial Stroop effect was found, although this effect was still larger in the suprathreshold condition.

Merikle & Cheesman's interpretation is probably flawed by the criterion 2 fallacy (defined above). Even if subjects were perfectly able to justify their response on each trial, asking them to rate their global performance retrospectively after 48 or 96 trials should lead to the observed discrepancy between claimed performance and real performance.

Merikle & Cheesman as well as **Fowler** would probably reject this criticism by claiming that criterion 3 was met. But was it? In Experiment 4, the authors randomly mixed suprathreshold and subjective threshold trials and compared a condition in which each type of trial (congruent, incongruent, neutral) was equally likely with a condition in which congruent trials were presented two-thirds of the time. In the suprathreshold condition, increasing the frequency of congruent trials increased both the facilitation and the interference effects, thereby enhancing the total Stroop effect. No such frequency effect was observed in the subjective threshold condition. This is taken by the authors an indication that criterion 3 has been met. But finding either no modification or an increase in the magnitude of the Stroop effect can hardly be considered qualitatively different effects of congruent-trial frequency; these are purely quantitative effects. Hence, criterion 3 is not met. Even if the congruent-trial frequency effects were qualitatively different in the suprathreshold and the subjective threshold conditions, this should be considered a criterion 3 fallacy because threshold measuring was itself flawed by the criterion 2 fallacy as explained above.

In the section "Threshold for what?" state 1 is defined as one in which subjects see stimuli perfectly well and state 3 as one in which subjects have learned to discriminate between stimuli on the basis of partial cues. With suprathreshold presentations subjects should almost always be in state 1, whereas with a subjective threshold

SOA (66% correct forced choices), they should be in state 3 very often, because they have had hundreds of trials involving four words differing in length, first letter, last letter, and so on. We know that state 1 provides good episodic memory and a good frequency effect. Unfortunately, we do not have the same information about state 3.

Until we have this information the enthusiasm of **Merikle & Cheesman** for their method, shared by **Fowler**, **Stanovich & Purcell**, and **Wolford**, seems unwarranted. Moreover, there is the more general problem of distinguishing between objective and subjective thresholds, raised by Adams (1957). Although performance for the discriminative judgment has been manipulated in almost every imaginable way (instruction, payoff, etc.), nothing similar has been done with respect to a subject's confidence in his judgment.

Summarizing indeterminacies and preamble to a research program. Automatic priming by prior presentation of a stimulus is the major indicator of the availability of information to the processing system that does not depend on a voluntary decision by the subject. Physiological indices could in principle play the same role, but those so far known are less sensitive than automatic priming to various aspects of stimulation (see **Fischler** and **Näätänen.**)

All the other indicators depend on a voluntary decision by the subject. These are, for example forced-choice detection, forced-choice discrimination, naming, semantic categorization, and semantic association. At the time of making such decisions a subject will have different conscious contents according to what task he performs. Later, the episodic traces left by the decision itself and by the associated content of consciousness will also be different, depending on the task.

In addition, both the immediate conscious contents and the episodic traces will be affected by the the quality of the stimulation and the amount of attention the subject can devote to its processing. In particular, near detection or discrimination threshold a subject may make a correct response without knowing it is correct (e.g., **Merikle & Cheesman, Stanovich & Purcell**), or he may make a wrong response being convinced it is correct (**Fischler**).

Because all these indicators differ in their sensitivity to stimulation (**Erdelyi, Fowler, Latto & Campion**), finding a particular dissociation between a pair of them is not especially informative. Several such indicators should therefore be measured across a whole range of stimulus parameters (from threshold values to well above threshold) and attentional parameters (Latto & Campion). One should also try to find a common metric for all these indicators, such as d' or derived measures (as suggested by Erdelyi). Moreover, possibilities such as hypermnesia (Erdelyi) show that Latto & Campion and **Navon** are right in claiming that criterion 1 is only a sufficient condition. Converging evidence from criterion 2 is necessary too.

One should realize, however, that at the present level of conceptualization the original question of SA/CI in the dissociation paradigm ceases to make sense. We are no longer reasoning in terms of a threshold approach; criteria 1, 2, and 3 have disappeared from the formulation of the problem, and the question of finding qualitatively different effects is now amplified well beyond the original intention. We are now dealing with a whole multidimensional set of qualitatively and quantitatively different effects. Whether it would be possible to partition this set into two subsets of effects corresponding to our current intuitive notions of semantic activation with and without conscious identification is an open issue. Whether the proposal constitutes a realistic research program will be dealt with at the end of the third section of this Response.

Experimental indeterminacies

Masking. An excellent summary of the experimental problems associated with masking studies can be found at the beginning of the commentary of **Stanovich & Purcell.** We could hardly be in closer agreement.

Both **Balota** and **Wolford** disagree with my suggestion that differential dark-adaptation conditions in the threshold and priming phases of the experiments of Fowler, Wolford, Slade & Tassinary (1981, Experiments 5 and 6) and Balota (1983) could have flawed their results. Balota and Wolford argue that (1) their subjects were less dark-adapted than those of McCauley, Parmelee, Sperber & Carr (1980) because their fixation field was dim (luminance of 7.5 cd/m^2) rather than dark as in McCauley et al.'s study and (2) the luminances(23 cd/m^2) of all fields except the fixation field were lower than in the experiments of McCauley et al., in which field luminances were set at 55 cd/m^2. Hence, the total amount of light was roughly equivalent in both phases of the experiments of Balota (1983) and Fowler et al. (1981) and overall dark adaptation was less pronounced than in the experiments that proved to be flawed by the artifact of differential dark adaptation. There is, however, one additional aspect of the situation that is not taken into account by Balota and Wolford: The difference in temporal distribution of light during a threshold and a priming trial should also play an important role (as suggested in Section 4.3.2 in tentatively accounting for the absence of priming effects with short SOAs.)

Taking Balota's (1983) 2,000-msec SOA condition as an example, the question is whether a subject's visual system is in a better, equivalent, or worse state to process the next priming word after a priming trial compared to a threshold trial. During a threshold trial, the fixation field was dimly illuminated all the time except for a very short period during which three events took place: A word or a blank field was presented for 15 msec followed by a variable interstimulus interval (mean duration of 32 msec) and a 30-msec mask. Hence this whole episode had a mean duration of 77 msec. Nearly 10 sec later the next word or blank field was presented. In a priming trial the fixation field was dimly illuminated all the time except for an episode similar to that just described followed by a period of complete darkness of nearly 2 sec and then a target word illuminated for 2 sec. Six seconds later the next priming word was presented. Both Balota and Wolford are optimistic in thinking that the state of the visual system is more or less the same in priming and threshold trials, whereas I am more inclined to think that subjects are better able to identify the primes in priming than in threshold trials. In addition to the arguments already mentioned, Balota and Wolford provide two further justifications for their position.

First, **Wolford** argues on the basis of his very-low-threshold SOA that there should have been no artifact. Notice, however, that mask efficiency depends not only on energy and contrast, the level of dark adaptation of the subject, and SOA, but also on the similarity between target and mask features. Of course, we lack a precise quantitative description of the effect of target–mask feature similarity. One should therefore be cautious in using a difference between levels of dark adaptation in different experiments to explain a difference in threshold SOA durations, as Wolford does in his commentary (or the other way round, as in Section 4.3.1 of the target article.)

Second, **Balota** argues against the artifactual interpretation on the basis of Scharf and Fuld's (1972) report. He claims that the influence of a light-adapting stimulus not only is eliminated but can be reversed at lower luminance levels. This point is made to emphasize the difference between McCauley et al.'s study and those of Balota (1983) and Fowler et al. (1981). Scharf and Fuld studied the influence of a priming light flash on the duration threshold for identifying four letters masked by a pattern. Subjects were dark-adapted and viewed a dark fixation field (not a dim one), and the priming flash was presented immediately before the target letters (not several seconds earlier). The duration of the priming flash was varied between 0 and 500 msec. With all fields (except fixation) set at a very low luminance (0.33 cd/m^2), threshold duration increased slightly with very short durations of the priming flash and remained at that level with longer durations. What could these results imply about the state of the visual system in trials involving events with the temporal and energy characteristics I described above? The reader must judge.

If, as **Balota** concludes, the final answer must await further empirical investigation, what was the point of all this discussion? Obviously, no compelling argument can be made about whether or not the results of Balota (1983) and Fowler et al. (1981, Experiments 5 and 6) were actually flawed by the greater-than-expected availability to consciousness of the primes during the priming phase. Nor can we expect to get the psychophysical data allowing quantitative predictions in the near future. Hence, the only appropriate response is to perform experiments in which the lighting conditions are equated during both the priming and threshold phases as suggested in Section 4.4. **Carr & Dagenbach** obviously reached the same conclusion in designing their experiments.

In view of this uncertainty, Balota's results remain ambiguous; there is evidence both for and against SA/CI. This was reflected in the discussion in Section 4.3.2, but Balota is right in saying that this was incomplete. (The nonword data could indeed be considered further evidence for SA/CI, a point I failed to notice in reading the paper.) The incidental memory data were not cited because of lack of space (editorial pressures to reduce the length of the target article were strong) and because these data did not appear to carry much weight, their interpretation being flawed by the criterion 2 fallacy.

Carr & Dagenbach certainly go a long way toward meeting the methodological criticisms raised by similar experiments. We should be confident that the light adaptation problem is adequately eliminated. This procedure yielded a priming effect that was considerably smaller at the detection threshold SOA than with suprathreshold presentation, being 18 msec and 74 msec (averages on three groups of subjects), respectively. Notice, however, that Carr and Dagenbach's procedure is not strictly comparable to that of the other experiments. The main difference lies in the interpolation of various threshold-setting tasks between the usual detection threshold-setting phase and the priming phase of the experiment. The important result is, of course, that the magnitude of the priming effect at detection threshold depends on which particular interpolated task was performed. In the most favorable condition the mean priming effect amounted to 30 msec (see Dagenbach & Carr 1982). Unfortunately, the corresponding suprathreshold priming effect of this group was not reported. It is unlikely, however, that with an overall suprathreshold priming effect of 74 msec, we should revise the conclusion that the improved methodology of Carr and Dagenbach produces less priming at detection threshold than at suprathreshold. Strictly speaking, these results (and those of Balota, 1983) do not replicate Marcel's (1983a, Experiment 4) observation of equal priming effects in both conditions.

The commentary of **Evett et al.** is very helpful conceptually. The fundamental distinction is between masking by undetectable primes and masking under less stringent conditions. Within the range of SOAs used to reach undetectability, the hierarchy of masking mechanisms described in Section 4.1 is probably responsible for the buildup of a visual representation amalgamating the target and the mask into an indivisible whole. In the passage stating that there is no place in the system where a legible representation of the word is available, "legible" should have been disambiguated by "no place" (see also **Marcel**'s commentary where he asks "legible to whom"). What was meant was that neither an individualized processing representation of the word nor a conscious representation of the word exists. In such conditions there is pure data limitation in processing.

With longer SOAs part or even all of the stimulus begins to become available, indicating that the hierarchy of integrative mechanisms progressively loses its influence; but this does not mean that other masking mechanisms, namely attentional masking, are not limiting performance (see Breitmeyer 1984, Chapter 8; Michaels & Turvey 1979). In this case processing is resource-limited.

Evett et al. seem to have successfully developed a masking paradigm in which processing of the words is resource-limited. Hence, since they claim they cannot implement criterion 1 by looking for direct evidence of processing of the first word for the very same reasons that it cannot be implemented in dichotic listening (see Section 2.3), all they can do is resort to subtle control conditions in the hope of getting indirect evidence of awareness of the first word. Even so, it would be interesting to know whether subjects could report the first word only if they were urged to do so.

Parafoveal vision. In the review of SA/CI from the parafovea it was assumed that the processing of the parafoveal stimulus was essentially data-limited. The degree of data limitation is determined by the interaction between the sensitivity of the parafoveal region and the physical characteristics of the stimuli. Although there was a focus on physical stimulus size (see Section 3.1), it is obvious that other visual parameters such as contrast are important as well (as pointed out by **Underwood** at the end of his commentary). It is less obvious that processing of the parafoveal stimuli could depend on how much attention the subjects pay to it. This position is justified at the end of Section 3.1. **Inhoff** and **McConkie** are more ready to admit that attention is indeed an important determinant of parafoveal information processing. The crucial point is that increasing the size or the contrast of the parafoveal stimulus or paying more attention to it will simply increase the frequency and speed with which it is consciously identified; but how could we implement the converging operations of criterion 1?

The problem is that all studies have been concerned with parafoveal stimuli that are sometimes identifiable and sometimes not. Visibility is therefore less stringently restricted than in masking with undetectable primes. Hence, as pointed out by **Rayner**, pattern-masked words and parafoveal words are degraded in different ways and, one should add, to different degrees. This is why Rayner finds different patterns of results in Pickering's (1976) task according to whether the prime was foveal or parafoveal. Also, in reading without foveal information (Rayner & Bertera 1979), because partial visual cues are available, subjects produce visual confusion errors, not semantic paralexias. Rayner is therefore perfectly right in pointing out that it is unwarranted to generalize from putative demonstrations of SA/CI with the pattern-masking paradigm to parafoveal vision.

In principle, **Underwood** is right in proposing a stage analysis of facilitation and interference effects in Stroop-like tasks. However, he does not sufficiently stress the crucial role of the relative time courses for processing both the relevant and the irrelevant information. In addition, his commentary contains some inaccuracies; for example, Dallas and Merikle (1976) used a naming task, not a lexical-decision task as Underwood incorrectly reports. Also, Underwood uses misleading terminology for describing some effects in his introduction and in his section on picture-naming tasks.

The fact is that one cannot describe the relation between the results of two conditions in terms of facilitation and interference without referring to a baseline condition. The appropriate baseline is of course provided by a picture-alone condition. As can be seen in the third line of Table 2 of the target article, in Experiment 2 of Underwood (1976) words both related and unrelated to the picture produced interference, but the interference was smaller with related than with unrelated words. It is therefore inappropriate to describe the results as Underwood does in his commentary, saying that dividing attention "had the effect of producing associative facilitation, in contrast to a picture accompanied by an unrelated word." Even though Underwood (1977b) did not use a picture-alone condition, the experiment was quite similar to that of 1976. In accepting the 1976 baseline as a reasonable approximation, it is clear that related and unrelated words interfere with picture naming in this experiment as well (Table 2, lines 2 and 4.)

What is crucial for determining the pattern of interference and facilitation is the time at which the irrelevant information reaches the appropriate stage, which in turn depends on the retinal location of this information (and on size, contrast, etc.). This time-course problem can generate qualitatively different effects not reflecting different underlying processes.

As explained in Section 3.3, implementing the dissociation paradigm by partitioning the trials into those for which the parafoveal item is reported and those for which it is not poses a number of nontrivial problems. Nor is it guaranteed that the addition of the control conditions I suggested will solve these problems. These difficulties could have been stressed a priori, casting doubts on **Underwood's** apparent meeting of criterion 3 in two different experiments (Underwood 1977b, 1981), which could be instead interpreted as instances of criterion 3 fallacy. In Section 3.2 too much space was devoted to speculating about the possibility that some unreported words were indeed identified in the experiment of Underwood (1977b). It is useless to go further into these considerations because the condition crucial to test these hypothesis (picture with superimposed word foveally displayed) is lacking.

In addition to reporting several failures to replicate some of **Underwood's** data **Inhoff** makes some criticisms similar to my own about inconclusive demonstrations of SA/CI in two reports of Underwood (1981; Underwood & Thwaites 1982). Both **McConkie** and **Rayner** claim forcefully, on the basis of their own extensive study of reading processes, that they have no data at all suggesting that SA/CI in the parafovea exists. Rayner clearly reiterates that he never got any evidence for semantic preprocessing from parafoveal preview, a point somewhat misrepresented in the brief allusion to these data in Section 3.1.

In the second part of his commentary **McConkie** argues that there are critical differences between normal reading and Stroop-like tasks. The point is well taken, but notice that it is hard to find supporting evidence for this claim in the existing data because there is confounding between tasks and physical parameters. Most of the Stroop-like tasks showing parafoveal processing involved large, high-contrast parafoveal words (displayed on cards in a tachistoscope or projected on a screen), whereas most reading-like tasks showing no such processing used small, lower-contrast parafoveal words (displayed on a cathode ray tube).

The first part of **McConkie's** commentary is very useful in showing what would be the consequences for reading models of adopting a wide-ranging semantic activation view (one apparently adopted by **Latto & Campion**) or a strong attentional selectivity view. It is a pity that McConkie does not cite references when he says that with gaze fixation maintained in one position, more than 10 surrounding words can be identified provided the subject pays attention to them. Section 3.1 required just such information, but almost none was found, apart from the papers of Sperling and Melchner (1978a; 1978b).

Inhoff provides us with very interesting new data concerning attentional factors in parafoveal processing, more or less explicitly interpreting them as showing that his patients are unable to pay voluntary attention to a contralateral parafoveal stimulus. But why should a subject pay voluntary attention to an irrelevant stimulus? It may be, rather, that with random left or right presentation an irrelevant parafoveal stimulus can automatically divert part of the attentional resources of the subjects only if the contralateral parietal cortex is intact. This view stems from the new conception of automaticity discussed at the end of Section 3.1.

Dichotic listening. The only commentators to devote their entire commentary to dichotic listening are **Corteen** and **Näätänen** (**Fischler** also devotes part of his). I am delighted at Corteen's basic agreement with the criticisms of his data in Section 2.2.2 (Corteen & Dunn 1974; Corteen & Wood 1972). It should be noted that Corteen never made strong claims about SA/CI in the irrelevant channel; he drew only tentative conclusions from his data. The explicit position he now adopts in his commentary is extremely courageous. It is in sharp contrast with a decade of uncritical overinterpretation and overgeneralization stemming from his data in so many reports by other authors. Corteen's commentary makes it unnecessary for me to make any further reply to **Dixon**'s point 2; how could I find a better answer?

As to event-related potentials (**Näätänen, Fischler**), though they seem promising for monitoring attentional focusing, they seem much less likely to provide valid indices of semantic activation with or without conscious identification. One cannot anticipate the future development of this research area, but for the time being the precision of the physiological indicators of semantic processing and of awareness is far from what is needed to provide answers to our psychological questions.

Theoretical indeterminancies and general scientific options

Converging evidence. Some commentators mention that aside from the three topics reviewed there are other sources of evidence for perception without awareness. **Fowler** cites some everyday experience, and **Latto & Campion** draw on their views of the reading process to make a similar case. **Fischler** provides some personal experience to argue that the dissociation is not between activation and report but between both of these and subjective experience. As pointed out by **Bisiach**, however, it is the study of brain-lesioned people that has provided the most challenging data about fragmentation of consciousness. Among the neuropsychological syndromes cited by the commentators are conduction aphasia and deep dyslexia (**Bisiach**), hemineglect (**Fowler**), right-hemisphere performance in commissurotomized patients (**Navon**), and, above all, blindsight (**Bisiach, Fowler, Latto & Campion, Marcel, Navon, Paap**).

Taking a converging-evidence point of view, Dixon (1971) first subsumed eight research areas under the heading "subliminal perception." In increasing these lines of evidence to 11, Dixon (1981) now integrates everything into the notion of "preconscious processes." In his point 4, **Dixon** reiterates that all these "relatively unrelated areas of research" constitute evidence for the same underlying phenomenon. Notice that blindsight (seriously shaken by the review of Campion, Latto & Smith 1983) and the three lines of evidence reviewed in the present paper are among Dixon's 11.

No doubt each of these sources of evidence deserves detailed analysis and criticism in its own right. Probabilistic judgments as to where the weight of evidence is (**Dixon**, point 4; **Marcel**) – or, in my terms, where the pendulum is – can indeed be used to distinguish between those who are pessimistic and those who are optimistic about the issue. Nobody wants to legislate on possible sources of scientific curiosity and creativity. If, on the other hand, we want to use converging evidence to build a theory, each of the lines of evidence taken individually must be strong enough to warrant the enterprise; **Morton** calls this "unrealistic standards of proof." Perhaps we should think of converging evidence as two different (not always coinciding) things, being either a "subjective state" for generating new, nonspecific pretheoretical ideas or an "objective basis" for theorizing and modeling.

I close this discussion of converging evidence with three remarks about some neuropsychological data. First, the occurrence of semantic paralexias (Marshall & Newcombe 1966) in some reading disorders (not yet called "deep dyslexia" at the time) was one of the arguments used by **Morton** at the end of the sixties for positing two thresholds on the logogen. After passing the first threshold the word is available in the processing system without being available to awareness; after passing the second one, the word becomes available as a possible response and is therefore consciously identified (**Wolford** likes this idea). This is the way Morton "accounted quite explicitly (albeit rather cavalierly) for unconscious processing of verbal stimuli" (see the end of his commentary). Notice, however, that what was overlooked at that time was that deep dyslexics are often aware of making semantic paralexias. Recently, Morton and Patterson (1980) attempted to deal with this fact in a current version of the logogen model, but it is not guaranteed that everyone will be satisfied with their solution.

Second, the most startling neuropsychological evidence for unconscious perception certainly came from blindsight. Even if one does not agree with all the criticisms made by Campion et al. (1983), the status quo as regards the interpretation of blindsight can hardly be considered the same following the publication of this BBS treatment. This is overlooked by **Bisiach, Marcel, Navon**, and **Paap** but not by **Fowler**.

Third, **Navon** is right in claiming that direct tests of unconscious perception can be used, but they seem to be much more successful as sources of evidence with brain-lesioned patients than with normal subjects viewing stimuli they are not aware of (see the discussion of Experiments 1 and 2 of Marcel 1983a).

Choosing a null hypothesis. In pondering what my null hypothesis might be **Marcel** wonders whether I deny the existence of nonconscious mental processes. **Morton** makes a similar point. Some clarification is needed.

I of course share the position of most modern cognitive psychologists that much of the processing of a clearly visible stimulus (strictly speaking, a stimulus that is going to become clearly visible through processing) is unconscious and that the processing of many of its aspects, including semantic aspects, precedes awareness of the subset of aspects that do become consciously represented. [See also Libet: "Unconscious Cerebral Initiative" *BBS* 8(4) 1985.] I also agree with **Marcel** that awareness of an aspect of processing is *theoretically dissociable* from processing itself, and that awareness is not a prerequisite of processing. (This position is similar to that reached by **Crowder** in about 1967.)

None of these general theoretical ideas implies that if the visual stimulus is presented at a very low energy level or masked by a pattern dissociation between semantic processing and availability to awareness should occur. The null hypothesis implicitly adopted in the target article was that *SA/CI in the dissociation paradigm does not exist,* not that unconscious processing does not exist (or, put another way, not that SA/CI simpliciter does not exist). This was apparently obvious to all the commentators except **Marcel** and **Morton.** Let us take the implementation of the dissociation paradigm in priming by masked primes as an example. Assume we reach threshold for a satisfactory indicator of awareness. There are two possible outcomes: Either we get a priming effect significantly different from zero or we do not. Since the absence of an effect is uninformative, there is only one viable null hypothesis, namely, that "SA/CI by masked primes does not exist." The proposition "SA/CI by masked prime exists" cannot be taken as a null hypothesis because it can only be confirmed (by finding significant priming effects), but it cannot be rejected.

The preceding considerations are critical for assessing some of Marcel's claims based on the radical qualitative distinction he has made between mental representations used in processing and the constructed representations available to awareness (Marcel 1983b). If one claims that a subject is completely unable to identify, say, a small word presented 15 degrees away from the fovea, **Marcel** will reply, as he does in his commentary, that this does not tell you anything about retinal characteristics because the evidence concerns conscious representations, not processing representations. Has Marcel changed the null hypothesis? Is the null hypothesis now that parafoveal presentations prevent access to consciousness but not to the processing system, in other words, that "SA/CI in parafoveal vision exists"? If so, then the model is always confirmed by finding priming effects but can never be refuted by finding no priming.

The null hypothesis cannot be changed. The only psychological model that would be refuted by finding subliminal effects is one in which there is a perfect identity between processing representations and conscious representations. As **Marcel** points out, such a model is rejected by twentieth-century psychology. The reasons for this rejection are various, but none involve the existence of subliminal perception of one form or another. It follows that the issue remains as empirical as it ever was. Both **Marcel's** model and **Morton's** logogen model can accommodate SA/CI under, say, severe pattern-masking conditions. These models are of course unaffected by failures to demonstrate the phenomenon, but they are also unaffected by finding positive evidence for it. Hence, it appears that Morton seriously miscasts the argument when he claims that one cannot look for artifacts atheoretically (**Fischler** also points out that the implications of the issue of SA/CI for cognitive psychology are not strong).

For SA/CI in the dissociation paradigm to be an interesting phenomenon, we need a model of the processing system that rules out the existence of SA/CI under impoverished sensory stimulation; that is, one implying a null hypothesis of the form "SA/CI does not exist." It is not clear whether it is possible to formulate such a model at an exclusively psychological level of description. However, if we have only hypothetical constructs such as integrative or interruptive mechanisms of masking, **Marcel** can rightly claim that this distinction has nothing to do with the level of processing at which these mechanisms are operating. If, however, we hypothesize that these mechanisms are implemented physiologically at the transducer level, the situation changes.

Such a leap poses some epistemological problems. Can we integrate a physiological level of description into a psychological model? Morton (1982) has given an interesting answer to this question. He wrote that "there is no physiological or anatomical fact that could in principle falsify or verify a purely psychological theory" (Morton 1982, p 90). He went on to claim that he finds attempts to link the two levels premature and unsuccessful "apart from the extreme of sensory and motor considerations" (Morton 1982, p 90). Hence, the processing of stimuli impoverished at the sensory input level involves exactly the conditions in which we can draw on physiological evidence to formulate psychological hypotheses.

It is in this context that the masking theory of Breitmeyer and Ganz (1976) is appealing, because it explains masking phenomena on the basis of mechanisms implemented at the transducer level. The hierarchical integrative theory of masking (see Section 4.1) based on the mechanisms proposed by Breitmeyer and Ganz (1976) would be falsified by the discovery of SA/CI by undetectable pattern-masked primes (with short SOAs). This is because such a theory implies a null hypothesis of the form "SA/CI does not exist." This theory would be refuted by rejecting this null hypothesis. (**Marcel** interprets raising SA/CI under pattern masking to the level of a potential falsifier of so powerful a theory as Breitmeyer and Ganz's as reinstating "the treatment of Galileo by the Inquisition.")

One final point in this section concerns failures to replicate effects. **Marcel** is of course right in claiming that an experiment showing no effect demonstrates nothing. If this absence of an effect is a failure to replicate an initial positive result, however, it casts doubt on the validity of the initial finding even if the discrepancy cannot be explained (perhaps it was a swan painted black, or a black nonswan). Conversely, a single positive finding that is at odds with a large body of evidence should not automatically cast doubt upon this evidence (as is exemplified by the review of evidence for SA/CI in parafoveal vision). Considering the difficulties associated with experimentation in cognitive psychology, one is always amazed to find any single study considered a crucial experiment. Who doubts that *nurse* primes *doctor* in a lexical-decision task? No one. It has been replicated dozens of times. Who doubts that a masked polysemous word unselectively primes all its meanings (Marcel 1980)? I do; unless you give me at least two or three replications (the best would be to have some replications following Marcel's procedure as closely as possible and others using an improved methodology). My position is the same with respect to Experiment 5 of Marcel (1983a); and also, it goes without saying, with respect to the still sketchy, undetailed description of Marcel's blindsight data (see **Marcel's** and **Paap's** commentaries for a more optimistic assessment of these data.)

Breitmeyer's masking theory and McClelland and Rumelhart's model. Breitmeyer (1984; Breitmeyer & Ganz 1976) provides us with an integrative approach to masking that is successful in accounting for almost every masking phenomenon so far known. Aside from its scope, its main asset is that it is not an autonomous psychological theory but draws on neurophysiological and neuroanatomical data. The proposed mechanisms of masking start playing their role right from the retina, in the pathways and relays to the visual cortex, and up to the visual cortical level.

Within the range of short SOAs, such as those used in experiments dealing with undetectable primes, it is likely that all that is available for processing is an input amalgamating the target and the mask into an undecipherable representation (both for the processing system and for conscious awareness). This would result from the operation of the hierarchy of integrative processes mentioned in Section 4.1. It is all too easy to overlook Breitmeyer's theory almost completely, or to discuss it only vaguely (Marcel 1983b). If one takes Breitmeyer's theory seriously, one cannot argue,especially with short SOAs, that there is complete independence between masking mechanisms and the processing level at which they operate. Similarly, however successful McClelland and Rumelhart (1981) are in simulating some of the effects found with the paradigm of Reicher (1969), there is no serious attempt to evaluate whether the hypotheses are plausible in the light of Breitmeyer's contribution.

Bridgeman provides us with a radical criticism of the underpinnings of the two mechanisms of masking proposed by Breitmeyer, namely, intrachannel integration and interchannel inhibition. If he is right, we would lose our first modern possibility of making SA/CI under pattern masking a potential falsifier of a theory. I am not competent to discuss the validity of the core of Bridgeman's criticism of Breitmeyer's theory, but I have three replies to Bridgeman's commentary.

First, even if we reject Breitmeyer's masking mechanisms a theory of masking may still need to take anatomical and physiological aspects of vision into account. Second, it is disappointing that **Bridgeman** (and Breitmeyer in his book) takes no position regarding the plausibility of SA/CI with severe pattern masking. Does this mean that, aside from evaluating threshold-setting procedures, nothing can really be said about the problem? Third, in his introduction Bridgeman erroneously claims that I overrate the importance of integration theory because I do not differentiate between type A and type B functions. There has always been a very strong tendency to equate type A function with an integrative mechanism and type B function with an interruptive one. A superficial reading of Turvey (1973) could suggest that he did this, but in fact he rightly hesitated to draw this conclusion. Later, in integrating most of Breitmeyer and Ganz's ideas, Turvey (1978; Michaels & Turvey 1979) accepted that the type B function in backward pattern masking is caused by interchannel inhibition. For reasons given in Section 4.1 of the target article, this mechanism should be considered integrative (in a subtractive way). Conversely, a blatantly integrative mechanism such as contrast reduction by luminance summation can generate superb type B functions, as has been shown by Felsten and Wasserman (1980). It is therefore unwarranted to equate masking functions with masking mechanisms.

The following quote from McClelland and Rumelhart (1981, p. 39) showing that theirs is an interruptive concept of masking can serve as a reply to one misunderstanding by **Evett et al.**: "In the bright-target/patterned-mask condition, the primary limitation on performance is the amount of time that the information is available to the system in relatively legible form rather than the quality of the information presented." It is nevertheless true that in looking at different mechanisms in the hierarchical model we can find elements compatible with either an integrative or an interruptive conception of masking. McClelland and Rumelhart take no stand as to whether SA/CI should occur with pattern masking, and they avoid the issue of consciousness of partially activated representations. **Navon** suggests that transient activation of candidate words in the model could be conceived of as a form of SA/CI. Notice that if these activated candidates could influence subsequent decisions, there should be graphemic and phonological priming but little or no semantic priming. This is because candidate words are activated on the basis of shared letters and, eventually, shared phonology (Glushko 1979). It follows that if, as **Morton** points out, Marcel's masked prime results support a cascade rather than a stage theory of information processing, it is not the cascade theory implemented in McClelland and Rumelhart's (1981) model but rather the one dreamed of by **Crowder.**

The status of consciousness in cognitive psychology and perspective on a research program. It is pointed out by **Bisiach** and by **Navon** that subjective experience, being not a hypothetical construct but a personal, private phenomenon, lies outside the scope of science. **Marcel** and **Morton** stress that many cognitive psychologists consider consciousness to be unnecessary for their models of cognition. In Fodor's (1983) recent formulation, consciousness should be associated with a central processor whose nonmodular organization prevents scientific investigation. In what follows, I shall adopt Fodor's (1983) distinction between three types of psychological processes: transducers, input systems, and central processors. [See multiple book review of Fodor's *Modularity of Mind*, *BBS* 8(1) 1985.]

Yet it seems to be going too far to assert as **Dixon** does that *both* awareness and unawareness are private events. Even in eschewing awareness in theorizing, cognitive psychology often relies on a subject's voluntary response to a stimulus to infer something about information processing in the cognitive system. This response (e.g., naming or categorizing a stimulus) reveals a small portion of the content of the subject's consciousness. **Latto & Campion** rightly stress that it is the *absence* of response that cannot be equated with unawareness – but that is just a particular example of the general principle that a negative response is simply uninformative (see also **Navon**).

There is something peculiar in attempting to investigate unconscious processes by rendering a stimulus subliminal. In older studies of subliminal vision no systematic distinction was made between reaching threshold by reducing stimulus energy and reaching it by masking an otherwise identifiable stimulus. It was hypothesized that in both cases the stimulus would be registered in the processing system (actually, what Fodor calls the input system) and would undergo deep processing. **Marcel's** recent theory denies that a stimulus which is too weak or is peripherally masked by a bright flash could reach the input system in a form preserving useful informational content. The reason is simply that the processes occurring at the transducer level cannot deliver an adequate output to higher levels. Marcel argues that this is not the case when subliminality is achieved through pattern masking (ideally in the dichoptic situation, to ensure that only central masking is involved). For the theoretical reasons developed in Section 4.1, pattern masking with short SOAs is probably not less of a problem at the transducer level than brightness masking is.

It is remarkable that Fodor (1983) avoids drawing on any kind of subliminal data in discussing empirical evidence compatible with his conjectures about the properties of input systems. What is fascinating in his theoretical formulation is precisely that a good (supraliminal) sensory input (1) can undergo a fair amount of processing into a modular input system, including lexical access, without necessitating any intervention from the central processor and (2) that the central processor can be ignorant of the fact that such processing has occurred.

Of course, these two points are just the dissociation between conscious and unconscious mental representations we have been discussing all along. The advantage of Fodor's formulation over **Morton**'s and **Marcel**'s models, however, is that it puts a heavier stress on epistemological problems, allowing us to see more clearly some research paradoxes in cognitive psychology.

From an empirical point of view, attempts to demonstrate the existence of informationally encapsulated input systems draw heavily on tasks suggesting automaticity in information processing, including many Stroop-like experiments. One cannot avoid bringing attention into the debate, however, because it constitutes the other side of the coin (see Kahneman & Treisman, 1984). If there were no such thing as attentive processing, we would not need the concept of automatic processing. In the present context, the term *attention* is used exclusively in the sense of an intentional act of selection, not in the sense of a capacity limitation (the resource metaphor; see Navon, 1984), as in the target article.

The study of attention, as a faculty of the central processor, is as legitimate as it ever was, whatever the epistemological status of such a notion (in Fodor's formulation attention is considered a horizontal faculty of the nonmodular central processor, which precludes its scientific investigation). If the input systems cannot avoid processing concurrent inputs (that are not impoverished at the transducer level), or cannot even avoid accessing different meanings of a single polysemous word (Swinney 1979; see end of Section 2.3), it is nonetheless through an act of selection that the central processor chooses which competing information is going to be brought to awareness. Another question is whether the central processor can choose not to have any knowledge at all about at least one of the concurrent stimuli or concurrent stimulus meanings; whether the mind can be thus shut like the eyes is a much more controversial issue.

By way of clarifying for **Fischler, Johnston, Marcel, Morton,** and **Navon** the view advocated in the target article: Attention is not a prerequisite for semantic processing (of supraliminal stimuli) at the level of the input systems; there is an inevitable link (see **Fischler**) between at least the content of one of the activated unit and consciousness (central processor). Under competing stimulation, the purpose of voluntary attention is to enhance one particular link at the expense of the others (or equivalently, the subject's task determines what is attended).

One paradox in the study of attention (see Section 2.3) is that a selective attentional theory posits that unattended stimuli are not consciously identified, but this assumption cannot be tested directly. The reason is well captured by **Navon**'s metaphor of attempting to study the properties of darkness by pointing light at it. In other words, the dissociation paradigm of the unconscious (as embodied in the converging operations of criterion 1) cannot be implemented in situations involving selective attention. In such situations, all we can hope to study is the dissociation between attention and processing, not between consciousness and processing (see also Kahneman & Treisman 1984).

This conclusion was already reached by the target article and does not appear to require modification. By contrast, the conclusion that reaching an appropriate threshold in a masking situation may be able to provide the desired dissociation should be revised in the light of the commentaries and of the present Response. The problem is that operationalizing conscious awareness in terms of a threshold fails to make sense in two respects. First, it forces us to use indicators of awareness (such as detection) in which subjective experience is almost completely eliminated. Second, once we reach such a level there is probably not enough useful processable information delivered at the output of the transducer systems to activate meaning representations of the stimulus in the input systems and in the central processor.

Johnston's methodological choice hence leads to an important local research program in which we should engage more fully (version B of Johnston's spotlight view of attention is now beautifully illustrated by a series of experiments by Allport, Tipper & Chmiel, in press). One can disagree, however, about what would be achieved by such research. Johnston hopes that a dissociation between consciousness and processing could be demonstrated, though with some difficulties, whereas all that may emerge is a dissociation between processing and attention (**Navon** appears to share the latter view). What if we never find any priming from unattended stimuli? That would force us to adopt an extreme early-selection view of attention only to the extent to which we were right in locating automatic priming effects at the level of input systems, which is a theoretical choice, not a demonstrated fact. Kahneman and Treisman (1984) also point out that the early–late description of selection makes no sense without reference to the kind of activity in which the subject is engaged.

The problem does not concern merely the relation between attention and perception (or semantic processing), but also, as pointed out by **Fischler**, the relation between perception and memory (see Tulving 1984). **Erdelyi** is right in suggesting that the distinction between criteria 1 and 2 is sophistical because there is no way of operationalizing it (Fischler). There is, of course, no task involving only perception, attention, or memory, only tasks we consider better suited for investigating one aspect or other of information processing. Areas of research also become more and more integrated. There is some blurring of the notion of semantic and episodic aspects of a stimulus at the time of presentation in Kahneman and Treisman's (1984) recent account of the role of attention in perception (see Fischler). There is an equivalent trend in the study of the long-term memory effects of such an event on the subsequent perception or recognition of the same stimulus (Jacoby 1983).

The research program proposed at the end of the first section of the Response stems from such considerations. It is not at all unrealistic; people are actually busy working on it (e.g., Balota 1983; Jacoby 1983; Smith et al. 1983), but the range of stimulus and task parameters studied is still too narrow. There is a whole series of questions in cognitive psychology whose only appropriate answer is empirical. Yet there is also a tendency not to do justice to the complexity of the questions we ask in being too modest in the experimental enterprise. Exploring more widely the parameter space of the tasks we are interested in would probably avoid a lot of pointless discussion about what are and what are not contradictory or converging results.

One would thereby achieve better models of information processing in which the notion of consciousness did not need to play any functional role. We should not forget, however, that most of our conclusions would still be based on the subject's performing the tasks according to the instructions, which sometimes entails revealing part of the content of his consciousness. The dissociation between knowing and the feeling that one knows alluded to by **Fischler** and **Fowler** should be further investigated. The present discussion suggests, however, that such a notion cannot be conceptualized in threshold terms, which casts doubt on the validity of any kind of subliminal approach.

Epilogue

Newell (1973, p. 298) writes: "We never seem in the experimental literature to put the results of all the experiments together. . . . One picks and chooses among the qualitative summaries of a given experiment what to bring forward and juxtapose with the concern of the present treatment." In other words, it often looks as if (in what is generally conceived as an empirical endeavor) one can manage to avoid data that would limit the rhetorical justification of one's speculations. The somewhat tiresome review of the data in the target article represents a modest attempt to meet this criticism while engaging a group of experts in debate about the conceptual and theoretical aspects of SA/CI in the context of the experimental results.

I am extremely indebted to all the commentators for helping to broaden the scope of this account well beyond the limits of the initial narrow analysis. The reader now has many of the conceptual, empirical, and theoretical elements (and some less formal ones as well) he needs to set his own pendulum where he wants it to be with respect to the issue of SA/CI in dichotic listening, parafoveal vision, and visual masking.

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