Commentary on **Daniel Holender (1986) Semantic activation without conscious identification in dichotic listening, parafoveal vision, and visual masking: A survey and appraisal. BBS 9:1–66.**

Abstract of the original article: 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.

On not knowing the meanings of words we can detect: Crucial qualitative differences

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Holender (1986) claims that the pendulum has swung too far in favour of the semantic activation without conscious identification (SA/CI) position. If its position is to be changed a strong new force must be exerted. Although his target article and response are commendable for many reasons, there is little that is substantially new in it. In fact, in the quarter of a century since Brown's (1961) evaluation of alternative explanations of perceptual defence – response bias and the "partial cue" hypothesis – the only new opponent to emerge is the "absence" of light adaptation. Given the commentaries of Balota (1986) and of Evett et al. (1986), it seems unlikely this most recent adversary carries a lasting threat.

Dixon (1981) reviews evidence from 11 areas in which performance is analogous to what proponents of SA/CI theory would expect under stringent thresholding conditions. Holender restricts his overview to 3. In spite of this highly selective sampling of the relevant literature, he is occasionally still forced to express "puzzlement" when he encounters findings which run counter to his thesis (viz. parafoveal vision: Underwood & Thwaites 1982; visual masking: Humphreys et al. 1982). His sampling of research appears still more curious when one considers that very few active investigators of preconscious processing would consider that two of the three areas reviewed (dichotic listening, parafoveal vision) could ever produce conclusive evidence for the SA/CI hypothesis – because the priming stimuli can be brought to consciousness by an act of attention. Holender's review is remarkably scant in its reporting of studies, even in the two areas concerned, where such acts of attention would not allow a stimulus to be consciously identified. This is especially so with studies that involve auditory presentations.

Henley (1976), for example, carried out a study considerably more strong methodologically than many of those cited by Holender and found that responses to homophones presented to one ear at supraliminal intensities were influenced by subliminal cue words presented to the other ear. This study overcomes the difficulty Holender raises with dichotic studies regarding the levels at which stimuli are presented. Dixon (1986), in his commentary, points to several other investigations that support and extend these findings (e.g., Henley & Dixon 1974 and its successful replication by Mykel & Daves 1979). Holender overlooks these in his reply.

Dixon also referred to some work of mine, which has since been published (Groeger 1984; 1986). This work clearly shows effects, under visual and auditory conditions, of semantic activation occurring in the absence of conscious identification. More important, however, the work consistently shows evidence of qualitative differences between sub- and supra-threshold stimuli. Opponents of the SA/CI hypothesis, as it is usually framed, concede the importance of qualitative differences: There has to be "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" (Merikle & Cheesman's commentary, 1986, p. 42). Holender seriously undervalues the qualitative-difference criterion of Dixon (1981), and, one feels, he perhaps even misinterprets it since in his discussion he frequently treats qualitative and quantitative effects equivalently. Once again, important findings (e.g., Marcel 1983; Somekh & Wilding 1973) do not receive adequate discussion.

My own work consistently shows that after a detection threshold has been established for a message and that message is subsequently presented *above* this level, but at an intensity or signal-to-noise ratio which does not allow subjects to identify it correctly (even though they have no difficulty in reporting its presence), there is no evidence that the message is processed semantically; or, more correctly, semantic effects are not observed. However, when the same message is presented *below* the level at which it was previously detectable, there is clear evidence of semantic processing. Such findings cannot be reconciled with traditional (e.g., Eriksen 1960) and modern variants of the partial-cue hypothesis (e.g., Holender 1986; Merikle 1982).

Holender makes some curious comments (not peculiar to him) on the qualitative-difference criterion proposed by Dixon. He points out that qualitatively different effects can occur with "visible" (does he mean "detected but not identifiable" or "correctly identified"?) primes. This is almost certainly true, but almost certainly irrelevant. What is important to realise is that such qualitative effects arise when subjects can choose consciously how to interpret a particular stimulus; a conscious "choice" is impossible with undetectable stimuli. That qualitative differences still emerge under such circumstances is extremely important. Holender also points out that finding qualitative differences does not obviate the need to set thresholds carefully). This point is equally incontestable, but obtaining such effects must have implications for our views on the adequacy of the thresholding procedure used. Finally, Holender sometimes assumes that one corollary of the qualitative-difference criterion is that it requires different "mechanisms" to support it. What meeting the qualitative difference criterion implies (when the other two criteria are also met) is a dissociation between conscious and nonconscious processing, *not* that the nature of the processing actually carried out is necessarily different. Positing separate "mechanisms" is no more than a stylistic, but perhaps unnecessary, architectural embellishment.

Like many others who have read Holender's target article, I wonder what its intention really is. If its purpose is to convince all of the people that some of the studies cited as supporting SA/CI are flawed, then the review succeeds deservedly. If its purpose is to convince some of the people that all of the studies are flawed, then, since there have always been doubters of SA/CI, the paper is bound to find a measure of support. But to convince all of the people that all of the studies are flawed is perhaps overambitious, especially on the basis of a rather restricted sampling of the relevant literature.

Preconscious semantic processing: Why and how?

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I greatly appreciate Holender's (1986) target article in *BBS*. It deals with theoretical and methodological issues particularly relevant to experimental and cognitive psychology. The discussion may gain from considering some ideas from the domain of evolution and brain development. Brown (1977; 1982; 1983) presents a number of arguments to demonstrate the preconscious nature of semantic processing. He has tried to show why and how semantic (conceptual) processing can occur early in the microgenesis of a cognitive function.

According to the microgenetic approach to cognition, semantic processing is a more primitive function in phylogeny (and also in ontogeny) than structural processing (e.g., phonemic and syntactic analysis), the latter being a relatively new addition to the human cognitive system. Brown's (1977) structural model of cognition allows sufficient room for preconscious processing. This may be mediated at the brainstem level or at other lower-order brain structures.

Moscovitch (1983) has recently shown, using the stimulus onset asynchrony (SOA) technique, that asymmetric processing (e.g., structural processing, as seen in phonemic and syntactic analysis) is a second level function, higher order than semantic processing. With semantic processing performance differences between the cerebral hemispheres are smaller. The implication is that when a stimulus is degraded there is a predominant tendency to analyse it semantically, whereas specialized processing comes into play only when perception improves. Brown (1983) likewise suggests that lateral asymmetries occur according to whether the task in an experiment involves an early or late stage of cognitive processing.

According to Moscovitch (1983), the anatomical regions for higher-order processing are beyond the striate cortex. It is interesting to note that Holender makes a similar suggestion concerning the anatomical region for central masking.

The case for preconscious semantic processing (Dixon 1981), and semantic activation without conscious identification (SA/CI), can be strengthened in the light of these ideas and findings.

Why limit the availability of a prime-word in the study of automatic contextual facilitation?

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Holender (1986) has presented a comprehensive and complete overview of recent studies of dichotic listening, parafoveal vision, and visual pattern masking that are considered by their authors as providing evidence that a sensory input can give rise to semantic activation without concomitant conscious identification. We agree with Holender that the validity of the conclusions of the semantic activation studies depends partly on the adequacy of the procedures used to ensure that the prime-word is not consciously identified. In particular, better methodological conditions of threshold detection should be used and the procedures should be examined in detail.

Nevertheless, Holender's attempt to limit the criticisms to "procedural" aspects of the research under consideration has led him to omit some of the principal theoretical motivations underlying these studies.

The present commentary will examine the motivations underlying the use of masking procedures in the study of semantic priming. It is clear that, as noted by Holender himself, the development of priming research is directly related to the theoretical substrates of psycholinguistics and particularly to models of lexical access and word recognition, which assume the existence of mental lexical representations and processes that are not always available for conscious inspection.

Following Forster (1979), a number of recent models of word recognition assume that lexical access results from the operation of an autonomous lexical module and that contextual information cannot intervene in the operation of this module (Fodor 1983). [See also Fodor: "Précis of *The Modularity of Mind*" *BBS* 8(1) 1985.] In the framework of these models semantic priming can be viewed as a consequence of lexical organisation and therefore does not violate the autonomy hypothesis.

Posner and Snyder's (1975a; 1975b) two-process theory has been used to interpret the effects of intralexical semantic priming. According to this theory, automatic contextual facilitation must be distinguished from facilitation resulting from the use of strategic expectancies in that it operates rapidly and is obligatory. The most widely adopted interpretation of automatic priming effects is that presenting a prime-word causes the activation of related words in the mental lexicon. It is this preactivation of associated words that gives rise to the automatic facilitation effects.

If we accept this dichotomy between automatic and controlled processes, one important problem is to establish an experimental means of distinguishing them. The most common experimental procedure to assess this distinction has been to manipulate the temporal interval between the context-word and the test-word. However, it has become clear that controlling only this parameter is not sufficient to guarantee the automatic or strategic nature of the observed facilitation effect.

We would like to point out here that some good empirical indices do exist concerning the degree of automaticity of the procedures used. These indices, which may suggest the existence of automatic activation, can be described in terms of the *presence of a facilitation for related pairs and the absence of an inhibition for unrelated pairs*. Such dual information is an essential criterion for distinguishing between automatic and attentional processing as well as the preaccess or postaccess locus of this processing. Indeed, inhibitory effects can originate at a postaccess phase of lexical processing where the subject looks for a coherence between the meaning of the prime-word and that of the test-word (de Groot et al. 1982). The two word meanings must be available for such a coherence test to operate. It is here that the importance of masking procedures becomes apparent. The use of such procedures eliminates what may otherwise be a compulsory search for compatibility between the test-word and the prime-word.

Unfortunately, only a limited number of the priming experiments actually allow the application of this facilitation-without-inhibition criterion. In particular, in the research aimed at demonstrating the existence of unconscious activation of word meanings, the facilitation effects were estimated by comparing the data from related and unrelated word pairs (Fowler et al. 1981; Marcel 1983). On the other hand, the experiments conducted by de Groot (1983) and Balota (1983) do allow the use of this criterion in estimating facilitation from a neutral baseline. In both cases the authors report an absence of inhibition for unrelated word pairs with short stimulus onset asynchronies (SOAs), whereas with longer SOAs Balota observed a large inhibition effect for unrelated word pairs. This last result suggests that the subject was able to use some information about the prime-word in order to apply a coherence test procedure. When there is facilitation without inhibition the effects generally vary between 15 and 30 msec. It should be noted that the effects of comparable magnitude are observed with the naming task, which is in principle less sensitive to postaccess effects than the lexical decision task.

The approach developed by Holender in his critical review of research on semantic activation without awareness was centered on criticisms of methodological aspects of these studies. This appraoch is of no great heuristic value inasmuch as the criticisms put forward are formulated independently of the theoretical framework that encompasses them, a framework which may itself provide relevant new behavioral indices.

Semantic effects without awareness: Dichotic listening and dichoptic viewing

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The major points in Holender's (1986) argument against semantic activation without conscious identification have been dealt with at length by the commentators. I would, however, like to answer two of his criticisms of specific paradigms.

1. Dichotic listening. Holender is somewhat cavalier in his discussion, claiming that it is immaterial "whether semantic activation takes place because attention is shifted from the primary channel or attention is diverted because semantic activation occurs" (p. 9). On the contrary, this is the critical question and Holender should be asking whether or not target detection performance on an unattended channel is explicable in terms of random shifting of attention to that channel. This question may be more readily answerable than he admits.

Moray's (1959) results on name detection require a sampling of the unattended channel every three or four words in order to explain them, but target detection tasks using less personally significant targets imply much lower rates. Another approach is to compare detection of identical targets when target specification requires different amounts of semantic information. Treisman and Geffen (1967) argued that as homophones could not be distinguished in an unattended message, contextual information was unavailable. Wilding and Farrell (1970), however, criticized their argument and showed that subjects who were told the target was a word (e.g., "I" for one group, "eye" for another) did better than those told it was a sound ([aI]) (31% detections compared with 6.5%), even though the passages were identical. Taking the probability of sampling a word on the unattended channel as 0.065, the improvement due to using semantic context should be the product of this figure and the probability of predicting the target from an adjacent word. In a subsidiary experiment (unpublished) six subjects were recently given the four words preceding or the four succeeding each of the 24 targets and predicted the target correctly with a probability of only 0.07. Even assuming that sampling any *one* of these eight words would permit such predictions, the predicted improvement is a mere 0.04. Meaning is clearly being extracted more effectively than this, prior to any attention switching that may occur.

2. Masking. Dichoptic presentation provides a relatively easy way of achieving subliminal stimuli without drastic reduction in brightness or exposure. Holender briefly mentions experiments using this method (p. 21), noting that one obtained no priming from the suppressed eye (Zimba & Blake, 1983) and implying that the others (Philpott & Wilding 1979; Somekh & Wilding 1973) are vulnerable to doubts about reduced thresholds due to light adaptation during the experiment. However, Zimba and Blake themselves point out that their method involved true binocular rivalry and the other experiments probably involved simultaneous pattern masking, so differences in method can explain the conflict in the results. Second, it is unclear how changes in light adaptation (if they occur) in both eyes should reduce masking. Philpott & Wilding's two experiments (though not Somekh and Wilding's) were run throughout in a lighted room with a lighted adaptation field in the tachistoscope to each eye and both produced marked effects of the meaning of subliminal stimuli. If Holender wishes to rule out these results because phenomenal report was used as the criterion for subliminality, then he must explain why his preferred criterion of signal detection measures-does not itself involve perception below the threshold of awareness.

Author's Response

Semantic activation without conscious identification: Can progress be made?

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Aside from the potentially important new results reported by Groeger, this additional set of commentaries does not seem to bring much fresh material to the debate that appeared in the initial *BBS* treatment (Holender 1986). The commentators misunderstand my position to various degrees; this could easily have been alleviated by careful reading of the target article and of my response to the first series of commentaries. In any case, as I believe in the virtue of repetition, I welcome this opportunity to rehammer some nails; this may help to clarify further my position regarding semantic activation without conscious identification (SA/CI).

The problem is not lateralized: A reply to Kurian. The commentary of **Kurian** provides us with some entries into the literature on differential processing by the two cerebral hemispheres and its relation to phylogeny. This is, of course, a fascinating topic but I am not sure it is directly relevant to the problems dealt with in the target article. Perhaps part of the confusion arises from the fact that Kurian does not seem to distinguish between the concept of preconscious processing and the concept of SA/CI. I myself made this confusion until I came across Dixon's (1981) second book. Dixon took the opportunity of the updating of his work, formerly entitled *Subliminal Perception* (Dixon 1971), to change the title to *Preconscious Processing*. This has turned out to be extremely misleading because, though dealing basically with the same topic as before, Dixon shifted the emphasis from a controversial, specific research area to an uncontroversial, general domain of investigation. Realizing this prompted me to change the initial title of my own review from "Semantic processing without awareness" to the present "Semantic activation without conscious identification" in the hope of specifying more clearly what is at issue.

Currently, few cognitive scientists seem inclined to doubt the existence of unconscious or preconscious mental representations and processes. On the empirical side, one of the most difficult methodological problems facing investigators lies in the quest for indirect, reliable means of studying these unconscious representations and processes. On the theoretical side, the idea of the existence of processes impenetrable to consciousness has received its most extreme, challenging formulation in Fodor's (1983; 1985) book *The Modularity of Mind*.

What was at issue in the *BBS* treatment of my target article was whether or not there are ways to deliver stimuli to the sensory organs that ensure that the preconscious processes can be called into play without, at some unspecified stage, generating (as products or as byproducts) representations that are available to conscious awareness. It is not clear to me that this issue is affected by the fact that the research mentioned by **Kurian** implies that we have to postulate different kinds of unconscious representations and processes in each hemisphere or by the additional fact that there are different kinds of conscious representations and processes, as implied by work on commissurotomized (e.g., Gazzaniga 1983) and hemispherectomized (e.g., Dennis 1980a; 1980b; Dennis et al. 1981) patients.

Be theoretical, Segui and Beauvillain suggest. I tend to be theoretical, as much as possible, but not exclusively. **Segui & Beauvillain** seem to be paying much more attention to the small than to the large print in their reading of the target article. (The detailed, methodological analysis of the experiments was set in small print.) Those reading the large print, and also my response to the commentaries, will realize that I have been theoretical all along, even too theoretically biased according to some commentators (e.g., Groeger, see below).

As a matter of fact, in dealing with the three research areas analyzed in the target article – dichotic listening, parafoveal vision, and pattern masking – I always divided my analysis into three parts. The first part was devoted to my best possible theoretical integration of the main findings of the field as assessed independently from the data usually judged relevant to the issue of SA/CI. The second part was a review of the findings relevant to the issue of SA/CI and of the methodological criticisms and difficulties associated with each experiment. The third part was a summary appraisal of the main findings based on the first and the second part and some suggestions for methodological improvements, where possible.

The bulk of my personal contribution lies in my theoretical appraisal of each field and the way it construes the problem of SA/CI. With respect to the methodological problems raised by each experiment and the potential inconclusiveness of the results, almost everything was scattered in the literature waiting for somebody to do the review. Granted that theoretical options are controversial by their nature, the methodological part of my paper can still be taken as an invitation to improve the empirical approach in such a way as to generate more conclusive and more replicable results. This, in passing, should clarify to Groeger that my intentions were quite different from those he assumed at the end of his commentary in assessing which of my presumed goals had been achieved.

Let us now turn to Segui & Beauvillain's advice not to formulate the criticisms independently of the theoretical framework that encompasses them. This is hardly disputable, but being theoretical does not have the same implications in every context. We must at least distinguish between theorizing about phenomena that are well established and easily replicable and theorizing about phenomena that are more elusive. To be theoretical in the sense advocated by Segui and Beauvillain is perfectly valid in the former but not in the latter case. It is appropriate, for example, in the three following situations: Assessing the relevance to reading research of the word superiority effect observed with Reicher's (1969) procedure; pondering whether the easily replicable effects observed in the lexical decision task tell us anything relevant about lexical access (e.g., Balota & Chumbley 1984); and discussing the conscious or unconscious nature of the mental representations tapped by the syllable-monitoring task investigated by Cutler et al. (1986). It seems to me, however, that this is the wrong strategy when the phenomenon is elusive and indeterminate. Why bother with the potential theoretical implications of the putative demonstrations of SA/CI with masked primes for the analysis of the components of the priming effect or for the modularity issue before we even know for sure which kind of threshold to measure and how to measure it (see the subsection *Threshold for what?* in the *BBS* response and the commentaries addressed therein). This itself constitutes an antecedent theoretical problem.

It seems noteworthy that neither Fodor (1983) nor his commentators (except for Kinsbourne 1985) on the *BBS* "Précis of *The modularity of mind*" (Fodor 1985) used the putative results from the SA/CI literature to argue for the existence of informationally encapsulated input systems. This neglect stands in sharp contrast to the widespread concern about the stumbling block for the investigation of modular input systems posed by the discovery of experimental procedures that bypass the central processors. This is extremely telling about the current attitude of the majority of the scientific community toward the phenomenon of SA/CI. Even Marcel, Balota, and Fowler do not seem to be engaged in any systematic research program stemming from their initial apparent successes (Balota 1983; Fowler et al. 1981; Marcel 1983); nor, for that matter, are Segui & Beauvillain engaged in such an enterprise, despite the potential theoretical interest they see in it.

I must end my reply to Segui & Beauvillain by urging them to pay more attention to the data and to the methodology before engaging in theoretical speculations. This may have prevented them from drawing what seem to be partially unwarranted conclusions from the results of de Groot (1983). Using unmasked primes and a short 240-msec interval between prime onset and target onset de Groot did indeed find a small inhibitory effect from unrelated words (Experiment 1) that vanished with masked primes (Experiments 5 and 6). What Segui & Beauvillain overlooked, however, is that this inhibitory effect disappeared both in the subjects retrospectively claiming they never read the primes and in those claiming they sometimes read the primes. Moreover, the facilitative effect was 20 ms for the former and 40 ms for the latter subjects, whereas it was 26 ms with unmasked primes in Experiment 1. Furthermore, no individual masking threshold was established and retrospective reports after the session almost certainly underestimated the actual frequency with which each subject read the primes.

Taken together, all these facts hardly constitute evidence that the masked primes were never consciously identified. They are much more compatible with the following interpretation. Subjects claiming to see no primes did see a considerable number of them and those claiming to see only some primes must have seen most of them, as they had an even larger facilitative effect than the subjects in Experiment 1. It should be clear, however, that the assumption that the masking conditions were not thorough enough to prevent conscious identification of the primes does not imply that the mask exerts no detrimental effect at all. It certainly constitutes a very distracting event that can interfere with the subject's anticipation of the presentation of some words, or with checking for coherence between the prime and the target, or both. An obvious, easily testable prediction stemming from this interpretation is that any distracting event coming shortly after the prime – such as a mask presented, say, after 100 ms (too late for preventing identification in most trials), or a loud noise, or even a tap on the shoulder – would have suppressed the inhibitory effect from unrelated words. With a longer interval between the prime and the target, such as the two seconds in Balota's (1983) experiment, subjects could simply have recovered from their distraction, which would account for their large inhibitory effects. In any case, Segui & Beauvillain seem to agree with me about the assumption that in this condition Balota's subjects were probably able to identify a non-negligible number of primes.

Don't be theoretical, Groeger suggests. I tend to be atheoretical as long as no theory is needed. Unlike Segui & Beauvillain, Groeger seems to be much more data oriented. At the end of his commentary, he criticizes the target article for being both restricted in the sampling of the data and theory driven in the analysis of the selected data.

One must distinguish two very different lines of argument in Groeger's commentary. On the one hand, he is providing new, apparently reliable data showing that the meaning of undetected spoken or written words is processed. This is certainly the most appropriate response to my criticisms of the prior evidence. On the other hand, Groeger's arguments often seem inconsistent and even spurious. For example, he seems to agree with my analysis of the problems raised by the data from dichotic listening and parafoveal vision; he goes so far as to claim that very few active investigators of SA/CI believe that conclusive evidence can ever stem from these two areas. I am very pleased with this claim, but why does Groeger allow proponents of subliminal perception (e.g., Dixon 1981) to use the data from these two lines of investigation as converging positive evidence for SA/CI while he denies the right of opponents to claim that they are not relevant to the issue: This seems both inconsistent and unfair. In the same vein, Groeger claims that the eleven areas reviewed by Dixon (1981) satisfy stringent thresholding conditions. Yet, in concluding his first paper he wrote about his own data: "These results also provide what is, to the author's knowledge, *the first adequate evidence* in support of the view that awareness is not required for the semantic priming effect to be observed" (Groeger 1984, p. 312, emphasis added).

There is a striking similarity – not only in the arguments that are actually the same, but even in style – among the commentaries of Groeger, Dixon (1986), and Marcel (1986). They all seem to pay much more attention than deserved to the results of a handful of experiments. Most of these experiments are not designed well enough to be conclusive; their results are thus susceptible to various interpretations. Also, for most of them, there is either no known attempt to replicate, or even partial or full failure to replicate, crucial aspects of the results. I have already discussed such arguments in my replies to Dixon and Marcel (see especially the "Converging evidence" and the "Choosing a null hypothesis" sections of my response, pp. 57–58, and also Stanovich & Purcell's [1986] commentary).

On the positive side, Groeger (1984; 1986) has shown that after being presented with undetectable spoken or written words subjects' forced choices were biased in favor of words related semantically rather than structurally (phonologically or orthographically) to the undetected words. The method used in threshold-setting is probably not stringent enough to guarantee that a true $d' = 0$ has been reached for detection. This criticism seems to be alleviated, however, by the fact that semantic processing of the undetected words cannot be accounted for in terms of the partial-cue hypothesis. This is because biases toward the structurally similar choice rather than toward the semantically related choice – biases that were common with detectable but unidentifiable words – disappeared when the words were presented at an intensity below the presumed detection threshold. Hence, Groeger seems to have discovered a procedure in which he can argue that the qualitatively different effects observed with the unidentifiable and undetectable words imply that the detection threshold was indeed reached.

With respect to qualitatively different effects, three points should be clarified. The first is that the qualitative–quantitative distinction is not as clearcut as Groeger seems to believe. Both descriptions often apply and emphasizing one at the expense of the other has different theoretical implications. For example, I have argued that the different frequency effects found in the supra- and the subthreshold conditions of Cheesman and Merikle (1985) are more appropriately described in quantitative than in qualitative terms (see my response, p. 53). Conversely, it is usual to emphasize the qualitatively different aspects of the three patterns of results that can be observed in the cost-benefit analysis of the priming effect, but this formulation can easily be translated into quantitative terms too. Similarly, it is probably possible to formulate a quantitative description of the qualitative differences found by Groeger, though, clearly, the qualitative description makes more sense in this context.

The second point is that qualitatively different patterns of priming effects can indeed be observed with consciously identifiable primes ("visible" primes, as I have called them.) Even when these qualitatively different effects correspond to conditions with masked and unmasked primes, it does not automatically entail that the primes are identifiable in one condition and not in the other. An example is the above interpretation of the results of de Groot (1983). Hence, in at least some circumstances, using the qualitative difference between effects as an a posteriori argument for appropriate threshold-setting can amount to begging the question of unconsciousness (I called this risk the *criterion 3 fallacy* in my response).

The third point is that I agree with Groeger that postulating different mechanisms to account for qualitatively different effects may be unnecessary. Actually, I only did it once, when I inadvertently adopted Dixon's (1986) formulation in responding to one of his claims.

The complete *BBS* treatment provides a framework in which both proponents and opponents of the SA/CI hypothesis can find their share of conceptual, experimental, and theoretical questions that must be raised in assessing the conclusiveness of existing investigations and in setting up new experiments. My review in the target article was indeed selective and intentionally so – although at the time it was probably nearly exhaustive with respect to the existence of SA/CI within the three lines of investigation under scrutiny. Those who think that the data I have been overlooking (Henley 1976; Henley & Dixon 1974; Mykel & Daves 1979) or just mentioning in passing (Philpott & Wilding 1979; Somekh & Wilding 1973) should have been given more weight may concede that, considering the different requirements that have been discussed in the *BBS* treatment, these data are at least not fully conclusive. Why not try to replicate these results and to build on them? The results I omitted seem too inconclusive to justify lengthy discussion. To reply to Wilding's commentary, however, I must analyze his procedure (see below).

The new data of Groeger (1984, 1986) are admittedly much more challenging than those just mentioned because the experimental procedure looks more appropriate and the results seem more clearcut. Notice that it would not be the first time that apparently solid results turned out to be flawed for subtle reasons. It is, therefore, too soon to assess whether my conclusions need to be revised. Even if they must be modified in the light of these new data, it does not render the criticisms of past evidence obsolete, a point with which Groeger should agree, as he sometimes claims that his own data provide the first adequate evidence for SA/CI.

I am certainly very eager to see some replications of Groeger's experiments stemming from independent laboratories and a systematic exploration of the boundary conditions of the phenomenon. We need both exact replications of Groeger's procedure and improved procedures, if possible. Notice that if after a modification is introduced into Groeger's procedure the results happen to be different from his, then an exact replication of his procedure is also due in order for the results to be really conclusive.

Be more critical with both theories and data: A reply to Wilding. I believe that a more careful reading of Sections 2.1 and 2.3 of the target article might have made Wilding's first point of his commentary dealing with dichotic listening unnecessary. A truncated quotation out of context can be misleading. The full assertion was, "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*" (Holender 1986, p. 8, emphasis added). This issue concerns finding evidence for SA/CI in the secondary message, which requires demonstrating that (1) semantic activation can occur without attention being paid to the secondary message and that (2) there is no concomitant conscious identification. The first issue is testable and the outcome is, of course, crucial for attentional theories. The existence of SA/CI in dichotic listening is not testable because simultaneously probing for (1) and (2) is impossible without inducing the subject to redeploy his attentional resources in a way that contradicts requirement (1). If semantic activation of the content of the secondary message can indeed be carried out without attention being paid to this message, but once activated the semantic content of this message attracts attention and becomes reportable, then requirement (2) is contradicted. Wilding and Farrel's (1970) experiment deals only with the processing-without-attention side of the question. In providing no continuous monitoring of how much attention is paid to the primary message, this experiment suffers from the same defficiencies as those discussed at the end of Section 2.1 of the target article (pp. 5–6).

Before dealing with Wilding's experiments with dichoptic presentation, it is necessary to remove some ambiguities from my previous claims about this research. I considered Wilding's procedure best suited for generating binocular rivalry rather than any other binocular phenomenon, such as dichoptic masking (as did Walker 1978, who took Somekh and Wilding's [1973] results as among the best evidence for SA/CI in binocular rivalry). Hence, I took Zimba and Blake's (1983) results of a very well designed experiment as a convincing refutation of claims based on unclear results stemming from less well designed experiments. I never intended to suggest that Wilding's procedure could have been flawed by changes in light adaptation. I concede, however, that the paragraph in which I alluded to Wilding's experiments (Holender 1986, p. 21) is written in a way that can eventually be interpreted as if I were entertaining this possibility.

Let us now examine the theoretical aspects of the problem. Wilding accepts the suggestion made by Zimba and Blake (1983) that his procedure is best suited for generating simultaneous pattern masking rather than binocular rivalry. Assuming that this is so, it would still seem to be an inadequately designed dichoptic masking experiment for one or both of the following two reasons. First, simultaneous presentation of the mask and the target is far from providing optimal masking conditions with dichoptic presentations because central masking is generally stronger with a mask following the target by a certain amount of time (Breitmeyer 1984; Turvey 1973). This criticism applies equally to the experiments of Somekh and Wilding (1973) and of Philpott and Wilding (1979). Second, the defocused schematic face (see Smith et al. 1959, p. 168) used by Somekh and Wilding (1973) is certainly far from being an appropriate pattern mask for a word. Unless the authors have been extremely lucky in accidentally getting the appropriate spatial relations between some features of the face and the word, the face is not better suited for causing metacontrast either. Hence, only the brightness of the face can exert a masking effect. Unfortunately, making by light is generally not observed dichoptically, and when it is, it is absent precisely in the cases in which patterns such as words serves as targets (Green & Odom 1984). The situation is even worse in Philpott and Wilding's (1979) experiments, except in one condition in which different letter strings are presented to each eye (appropriate for dichoptic pattern masking, but suboptimal because of the null stimulus onset asynchrony SOA). In the other conditions, an outline of a shape cannot be a pattern mask for a different shape or for a word, nor can a color patch be a mask for a word or a word be a mask for a shape.

Wilding's procedure is thus hardly appropriate for generating dichoptic pattern masking. Moreover, the claim made by Zimba and Blake (1983) that the 300 to 800 ms exposure durations used by Somekh and Wilding (1973) are too short for binocular rivalry to occur is almost certainly incorrect. What is probably true is that these exposure durations are too short for the subject's experiencing rivalry during any one of the trials, but this does not preclude that one of the monocular stimulis becomes dominant while the other is suppressed at the end of each trial as suggested by the work of Wolfe (1986). It has been convincingly demonstrated with more complex procedures that "rivalry occurs inevitably whether visual stimuli are presented for more than 150 ms" (Wolfe 1986, p. 279). During the initial 150 msec of visual stimulation, some kind of abnormal fusion between the two stimuli occurs. If this is so, the fused image in Wilding's experiments should (for the reasons just given, i.e., because simultaneous pattern-masking conditions are not well implemented) contain a fair number of features from each of the monocular stimuli. Hence, even if one eye becomes dominant after 150 msec and remains so until the end of each trial, there is ample time at the beginning of stimulation for processing both stimuli.

The only difference between the supraliminal and the subliminal conditions of Somekh and Wilding lies in the density of the masking filter covering the word side. Notice, however, that the stronger filter did not prevent reading of the word if the face was not concurrently presented to the other eye. The lighter filter did not prevent reading of the word at all. The best characterization of the situation is therefore the following: During the initial 150 msec of exposure the word in the abnormal fused composite image should be harder to read in the subliminal than in the supraliminal conditions because of the difference in strength of the filters covering it. If after 150 msec rivalry is initiated, the proportion of trials during which the face and the word are dominant should be different in the two conditions because the relative saliency of the contours of each monocular stimulus affects the rate of alternation (Levelt 1966). The contrast of the face was constant across conditions but the contrast of the word was sharper in the supraliminal than in the subliminal condition.

In this complicated situation the crucial question accordingly concerns whether the stronger filter presented on the word side was strong enough to prevent reading of these words during the whole session. Aside from retrospective report, no check for the availability of the word was interspersed between the experimental trials. Moreover, the initial threshold setting was based on only four trials, which is hardly enough for subliminality to be reliably assessed.

Somekh and Wilding's (1973) experiments comprised one supraliminal and two subliminal (as defined above) series of 20 trials in which subjects had to judge whether a blurred face presented to one eye (always the same schematic face but in two versions: facing left or facing right) looked "cheerful," "neutral," or "miserable." The words *happy*, *sad*, or a word orthographically similar but semantically unrelated to one of these was concurrently displayed to the other eye. Phillpot and Wilding's (1979) experiments included only supposedly subliminal trials. Subjects had to name a word, a shape, or a color presented to one eye while being presented with conflicting or nonconflicting information to the other eye (word or shape).

In Somekh and Wilding's (1973) experiments, the first series of subliminal trials shows no effect. Only one of the two critical words was effective in biasing judgments in each experiment, and the biasing effect was exactly the same in the supraliminal and the subliminal condition. In Experiment 1, nothing contradicted the obvious conclusion that the words were simply equally readable in both conditions. In Experiment 2, however, the qualitative differences between the influences of the words *sad* and *sap* in the supraliminal and subliminal conditions are generally taken to mean that subliminality was indeed attained. The rationale underlying this inference is exactly the same as the one systematically exploited by Groeger (1984; 1986) in his experiments. In the present context, however, the danger of drawing an erroneous conclusion from this subset of results observed in only one quarter of the data (considering both experiments together) is enormous in view of the awkwardness of the threshold-setting phase of the experiments. The situation is even worse in Philpott and Wilding's (1979) experiments, in which we are forced to interpret the anomalous results of an interfering effect stemming from both conflicting and nonconflicting semantic information as if the only possibility were the unavailability of this information to consciousness. This time, there is not even a supraliminal control condition allowing for comparison.

To conclude, Wilding's procedure is inappropriate for generating simultaneous pattern masking as suggested by Zimba and Blake (1983). It is probably appropriate for generating binocular rivalry, but according to the foregoing analysis this phenomenon contributes little to the establishment of subliminality. Hence, Wilding's procedure, whether successful or not, cannot be used to argue for (Walker 1978) or against (as I did in the target article) the existence of SA/CI in binocular rivalry. For that matter, the only conclusive data we have are those of Zimba and Blake (1983). These do not show the slightest evidence of SA/CI from the phenomenally suppressed stimuli in binocular rivalry. Wilding's complex procedure thus amounts to attempting to achieve subliminality by reducing the signal-to-noise ratio of visually displayed stimuli by covering them with filters of increasing density.

Conclusion. Many of the indeterminacies associated with research on SA/CI that were pointed out by the commentators in the initial *BBS* treatment suggest that progress will be slow. Moreover, it will probably depend as much on the evolution of our general conceptions about the relations between conscious and unconscious mental representations and processes as on the empirical demonstration of SA/CI. At the theoretical and conceptual level, one should already be prepared to abandon the idea that these relations can be construed in terms of a threshold framework, as was discussed at the end of my response.

For those who still entertain the opposite view, especially those attempting to demonstrate the existence of SA/CI from stimuli presented below an objective threshold (d′ = 0), considerable effort should be devoted to establishing this threshold in a way compatible with current psycholophysical theories (Erdelyi 1986; Macmillan 1986). In this framework, results will always be more conclusive if it is possible to set up conditions in which qualitatively different effects with suprathreshold and subthreshold stimulations can be unambiguously interpreted as converging evidence that the threshold is reached. It is too soon to tell for sure whether Groeger's procedure is a real breakthrough in this direction, but there is no doubt that his results are potentially important enough to warrant attempts to confirm and extend them. It would be extremely encouraging if the present discussion could promote progress in such an empirical enterprise.

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