CONSCIOUSNESS AND COGNITION 2, 169–193 (1993)

THEORETICAL FOCUS

The Mediation of Intentional Judgments by Unconscious Perceptions: The Influences of Task Strategy, Task Preference, Word Meaning, and Motivation

MICHAEL SNODGRASS,[1] HOWARD SHEVRIN, AND MICHAEL KOPKA

Department of Psychiatry, University of Michigan, Outpatient Clinic—Riverview Building, 900 Wall Street, Ann Arbor, Michigan 48109

In two experiments subjects attempted to identify words presented below the objective threshold using two task strategies emphasizing either allowing a word to pop into their heads (pop condition) or looking carefully at the stimulus field (look condition). Words were selected to represent both meaningful (pleasant vs unpleasant) and structural (long vs short) dimensions. We also asked subjects to indicate their strategy preference (pop vs look) and to rate their motivation to perform well. In the absence of conscious perception, both strategy preference and word meaning interacted with strategy condition, mediating the accuracy of subjects' direct word identification judgments. Motivation also mediated performance. Word structure had no effect. Unconscious perception manifested only in the pop condition, underscoring the importance of task strategy in determining whether subliminal effects are observed. A follow-up control experiment using sham flashes demonstrated that strategy preference and motivation effects were not artifacts resulting from performance feedback cues.

In recent years there has been renewed interest in the existence and nature of unconscious perception. Backward masking has been an influential experimental pardigm in the study of unconscious perception, as used in studies by Fowler, Wolford, Slade, and Tassinary (1981); Carr, McCauley, Sperber, and Parmelee (1982); and Marcel (1983). However, the positive findings of these and other investigators have been challenged by Merikle (1982) and Holender (1986), who raised a number of methodological criticisms. For example, both Merikle and Holender have stressed the importance of rigorous threshold-setting procedures to ensure that the priming stimuli are indeed truly unconscious. Merikle (1982) has emphasized the need for an adequate number of trials upon which to judge the attainment of an appropriate threshold. In the same vein, he urged that careful attention be given to making sure that subjects employ their response options with reasonably equal frequencies in threshold-setting tasks to minimize the confounding effects of response bias. Holender (1986) echoed these concerns, stressing that there must be adequate evidence that subjects are not consciously identifying stimuli. In this regard he suggested that the criterion of $d' = 0$ be employed, as well as examining subjects' performance at higher levels of d' for comparison.

[1] To whom reprint requests should be addressed.

1053-8100/93 $5.00

Cheesman and Merikle (1984), incorporating methodological improvements suggested by Merikle (1982) and using color names as primes, employed a forced-choice identity discrimination procedure in the threshold-setting portion of their investigation. They reasoned that by limiting subjects' response options to the four color words, potential response criterion problems in the usual detection threshold approach (such as a yes or no response set) could be minimized. The criterion for considering the masked primes as unconscious was correct identification of less than 30% in each of five consecutive blocks of 24 trials. Cheesman and Merikle found that masked primes presented at this "objective" threshold did not produce priming effects, contrary to the findings of previous investigators. At the same time they discovered that masked primes presented somewhat above the objective threshold could produce priming, even when subjects convincingly denied any phenomenal awareness of the primes. They further observed that priming decreased monotonically as exposure durations were shortened from supraliminal thresholds down through these "subjective" thresholds, finally reaching zero at the objective threshold. The authors concluded that the results of Marcel (1983), Fowler et al. (1981), and Balota (1983) could be understood as reflecting the effects of masked primes presented at subjective rather than objective thresholds. Their results have been interpreted by Holender (1986) as simply demonstrating that true unconscious perception does not occur when the objective threshold is adequately determined and $d' = 0$.

However, recent methodologically stringent studies by Dagenbach, Carr, and Wilhelmsen (1989) and Kemp-Wheeler and Hill (1988) have produced evidence for unconscious perception at the objective threshold. These two investigations are particularly relevant because they used procedures that bear on those employed in the current studies.

For example, Dagenbach et al. (1989) found that the relationship between priming and prime–mask SOA was nonmonotonic. Initially priming decreases as SOAs are shortened, eventually reaching zero (as reported by Cheesman & Merikle, 1984), but then reappears at shorter SOAs. In the current experiments, we use an extremely brief exposure duration (1 ms) that we believe is at or below typical detection threshold levels (cf. Wong, Shevrin, & Williams, in press).

Dagenbach et al. reported further that certain subjects showed *inhibitory* rather than facilitative priming when primes were presented at the detection threshold. Dagenbach et al. explained this inhibitory priming as the consequence of a *strategy* likely adopted by these subjects. In the current studies we also examine the effects of conscious task strategies. In contrast to Dagenbach et al., however, we have subjects directly use task strategies in the experiment proper.

Kemp-Wheeler and Hill (1988) also conducted a series of experiments in which detection rather than identification thresholds were assessed. They too found substantial priming with masked primes presented at the objective detection threshold. They noted that some of their subjects exhibited $d' > 0$ and hence could conceivably have been consciously processing the primes to some extent. In a reanalysis, they examined only those subjects who had obtained a $d' \leq 0$ and compared them with subjects whose $d' > 0$. Strikingly, both groups showed moderate priming effects of the same magnitude. They were thus able to demonstrate priming for subjects who were clearly at or below the objective threshold. They also showed that the magnitude of the priming effect did not increase as a function of d'. Subjects who obtained $d' > 0$ showed priming effects that were the same as, not greater than, those of subjects with $d' < 0$. Cheesman and Merikle (1984) have argued that when objective thresholds are adequately determined, obtained detection performance should vary randomly around zero, and thus there should be no correlation between d' and the size of the priming effect, exactly what Kemp-Wheeler and Hill (1988) found. These findings suggest that subjects with $d' > 0$ did not achieve this performance by virtue of conscious perception. In the current studies we adopt a similar line of reasoning.

Conscious versus Unconscious Perception: Implications for Direct and Indirect Measures

Interestingly, the criticisms of Merikle and Holender echo those voiced by Eriksen (1960) regarding the perceptual defense and vigilance literature. The most important of these criticisms, termed by Erdelyi (1974, pp. 8–10) the report suppression and partial cues response biases, focused on the possible effects of conscious processes in determining subjects' responses. Such problems in disentangling the effects of conscious versus unconscious processes regarding subjects' responses are even more accentuated when an "intentional" judgment, such as identifying a word, is made by the subject. In these circumstances, how can one tell if purportedly subliminal stimuli really are unconscious?

To circumvent these difficulties, Merikle (1982) and Holender (1986) have argued that even more stringent procedures must be followed when claims for absolute subliminality are made (e.g., rigorous determination of the objective threshold). Further, interest shifted to indirect rather than direct measures. This emphasis accounts for the appeal of the backward masking paradigm; it is designed to demonstrate that there is no conscious perception of the primes themselves while simultaneously showing indirect priming effects in the lexical decision task.

However, the notion that subjects' intentional responses reflect solely conscious processes (Holender, 1986) has recently been challenged by Reingold and Merikle (1988), who termed this the "exclusiveness" assumption. They pointed out (cf. Bowers, 1984) that it is perfectly plausible for intentional judgments to be affected by unconscious as well as conscious processes. Nonetheless, it could be argued that Holender's (1986) position is appropriately conservative in that it seems difficult to rule out conscious perception as an explanation for above chance levels of d' in intentional judgments. We believe, however, that we have developed a methodological approach that is capable of distinguishing unconscious from conscious influences on intentional judgments.

This paper presents three experiments that demonstrate that unconscious perception can be shown to affect conscious intentional judgments regarding stimulus identification. Further, we will show that the commonly unexplored influences of task strategy, task strategy preference, the affective valence of the stimuli, and subject motivation mediate the effects of unconsciously perceived stimulus information on intentional judgments.

ORIGIN OF THE EXPERIMENTS

The three experiments were designed to investigate the possible moderating effects of four factors on unconscious perception: (a) experimenter-determined task strategies, (b) subject-determined strategy preference, (c) semantic and structural differences among the stimuli, and (d) subject motivation. Unlike the backward masking approach which focuses on indirect indices of unconscious perception, the present experiments employ direct measures for this purpose.

The percentage correct identification achieved by subjects was the dependent measure. All relevant comparisons of the percentage correct are between equally sized groups of trials and therefore entail none of the difficulties found when percentages are used based on varying denominators.

Method of Presenting the Stimulus and Establishing Its Subliminality

In previous research (Shevrin, 1973, 1978), the second author developed a paradigm for establishing subliminality that departed in several respects from the backward masking paradigm: (a) Stimuli were presented uniformly under the same duration and luminance conditions for all subjects (1 ms, 10 ft-lamberts) and each flash was immediately followed by a reappearance of the fixation field at the same luminance level, thus producing what Turvey (1973) called energy masking; and (b) the absence of awareness of the stimulus was assessed at the end of the experiment by a discrimination procedure involving a relatively small number of trials (usually following an additional supraliminal procedure involving the same stimuli).

Although the published studies based on this paradigm have included exact and systematic replications (Shevrin, 1973, 1988; Dixon, 1981), both the energy masking approach and the method for establishing absence of awareness have come under criticism (Turvey, 1973; Cheesman & Merikle, 1984). Our aim in the current experiments is to show that the energy masking pardigm works, thus supporting previous work based on this paradigm as well.

We did not rely on threshold-setting procedures per se because there are serious statistical and methodological problems with the procedures usually employed by investigators for this purpose (see below). However, our procedure followed Cheesman and Merikle (1984, Experiment One) closely in several important respects. First, the intentional judgment was identical in format to that used by Cheesman and Merikle. Subjects were asked to identify which of four words were presented on each trial. Second, we used the same size and number of trial blocks employed by Cheesman and Merikle. We did this so that any conclusions we might make about the subliminality of the verbal stimuli would conform to the major criticisms of Merikle (1982) and Holender (1986) concerning adequate control of response bias and sufficient numbers of trials.

Task Strategy

Two strategies were employed. In the first, subjects were instructed to attend carefully to the visual field and look hard for any trace of the stimuli (the *look* condition). This strategy emphasized the straightforwardly perceptual aspects of word identification, thus encouraging subjects to attend to partial cues that some have held can account for subliminal effects. This strategy is probably adopted by many subjects in subliminal perception studies. Clearly, the nature of the look strategy provides conscious perception with every opportunity to manifest itself.

The second strategy condition urged subjects to allow one of the four stimulus words to pop into their heads—to say whichever of the four words came to mind (the *pop* condition). The idea for this strategy came from investigations reported by Dixon (1981, pp. 93–94), suggesting that opening oneself up to spontaneous associations combined with a relaxation of perceptual vigilance could maximize subliminal perception. Evidence for unconscious perception in this strategy condition would be more difficult to explain by an alternative hypothesis of conscious perception.

Task Preference and Motivation

We reasoned that subjects' attitudes toward the task strategies might mediate the strategies' effects on their performance. Following the completion of the two strategy conditions, we asked subjects which of the two conditions they preferred or found more suitable to them. After the first experiment the third author (who administered the first two experiments) noted that some subjects appeared interested in the task and motivated to do well, whereas others appeared uninterested, nonmotivated. We decided to examine this as a possible moderator variable as well. For Experiments Two and Three we asked subjects to rate their own motivation (described in detail below).

Inferential Approach and Use of Statistics

In the present experiments, only a direct index of subliminal perception is employed, namely the percentage correct achieved by each subject on a stimulus identification task. Although it is perhaps more difficult to demonstrate the presence of unconscious perception without the simultaneous use of indirect indices, we believe we have developed an inferential approach and method of statistical analysis that allow such inferences to be made. Given the novelty of our approach and its centrality for our conclusions, a detailed discussion of its structure is in order. Further, some explanation of what is wrong with typical threshold-setting procedures is necessary in order to lay the conceptual groundwork for our approach, which seeks to show adherence to the objective threshold and evidence for subliminal effects simultaneously.

When investigators have sought to demonstrate the existence of unconscious perception, an essential assumption of the standard approach is that conscious perception will serve only to elevate performance above chance on the threshold identification task. This assumption is basic to any psychophysiological attempt to determine thresholds. The standard approach also assumes without justification the converse of this assumption, that is, that above chance performance is due *only* to conscious perception. Thus, in an attempt to guarantee that subjects are not consciously aware of putatively subliminal stimuli, most investigators (e.g., Cheesman & Merikle, 1984; Dagenbach et al., 1989) have adopted threshold determination procedures in which subjects are either eliminated altogether or their exposure durations reduced if they perform above chance as defined by some arbitrary criteria. In Cheesman and Merikle's first experiment, they required that subjects complete five consecutive blocks of 24 trials each with less than 30% correct identification (25% being pure chance) before they judged that the objective threshold had been truly reached. In the same vein, Dagenbach et al. administered a threshold recheck after the lexical decision phase and eliminated subjects who achieved more than 60% correct of 20 trials in a two-choice format. These sorts of procedures have two basic flaws.

First, the finding that a given subject exceeds chance performance is not unusual but rather to be expected given that in any sample there is random variation around the mean. Here the appropriate question is, Does the number of subjects in the upper tail of the distribution fit with what one would expect from random variation? If so, there is no reason to postulate conscious perception as a casual factor. Nonetheless, the standard approach either routinely eliminates such subjects or further shortens their stimulus durations in an attempt to make them "behave."[2]

The second, more fundamental problem with the standard approaches to threshold estimation concerns the common practice of considering individual subject performance in isolation from the sample. The real question is: What evidence do we have concerning whether the sample in question belongs to the population of unconscious perceivers versus conscious perceivers? The most important data here are sample characteristics, not individual performance levels. No evidence for conscious perception in the sample exists if the performance distribution is normal with a mean around chance (as in our experiments). The proper question regarding individual subject performance then becomes: Is that

[2] For example, Dagenbach et al. (1989, Experiment One) eliminated subjects if they made greater than 23 correct judgments out of 40 in a two-choice format. They incorrectly stated (page 421) that the probability of this by chance alone was about .26. Actually, it was about .35, because their procedure used two blocks of 20 trials, not one block of 40. In fact, 29% of their subjects exceeded this criterion, below the proportion expected by chance alone. Nevertheless, they were erroneously eliminated as suspect. Similarly, by requiring that subjects' performance be less than 30% in each of five consecutive blocks, Cheesman and Merikle (1984, Experiment One) unintentionally demanded that subjects systematically perform below chance. Given their sample mean of 26.8% correct and a *SD* of 2.7, there is a probability of 88% by chance alone that a given subject will complete a block of 24 trials at less than 30% correct performance. The probability by chance of performing consistently below 30% for five consecutive blocks is then about 53%. In turn, the probability by chance of their sample of eight subjects all performing at such levels is about 6/1000. Because their procedure required that exposure durations be lowered whenever the criterion of 30% was exceeded in a given block, it is quite likely that some subjects' thresholds were ultimately set at levels well below their true objective threshold.

performance aberrant enough with respect to the sample as a whole to conclude that the subject is from *another population*, namely the population of conscious perceivers given the experimental conditions? The proper approach to this question is the outlier method (see Barnett & Lewis, 1978), not ad hoc performance criteria or confidence interval methods. Confidence intervals apply only to the relationship between a sample mean and a population mean, not to the relationship between an individual score and a sample mean. Because the latter relationship possesses considerably more variability than the former, the use of confidence intervals in this context will mistakenly label substantially more subjects as outliers (i.e., conscious perceivers) than is appropriate (see Remington & Schork, 1985, p. 160).

Thus, the first step in our approach is simply to examine the distribution of percentage correct scores in the sample. If the mean is at chance (25%) and no outliers are present, we would then conclude that there is no evidence that conscious perception has occurred.

The second step is to look for evidence that perception of any sort has occurred. We do this by performing correlations and analyses of variance on the percentage correct scores with our independent variables, namely task strategy, strategy preference, word meaning, word structure, and subject-rated motivation. If significant results are obtained and the first step has revealed no evidence for conscious perception, we would then conclude that the stimuli are being *unconsciously* perceived and are influencing conscious intentional judgments.

Last, following Kemp-Wheeler and Hill (1988), a conscious perception interpretation would require that our findings be carried by those subjects performing above chance levels, that is, those alleged to be consciously perceiving anything at all. Thus, this view predicts that our independent variables should only show effects for subjects performing at above chance levels and therefore should not hold for subjects performing at or below chance levels. To the extent that we can show comparably sized effects for above and below chance subject groups (that is, subliminal effects uncorrelated with d'), then there is no justification for interpreting our findings in terms of conscious perception. We further examine another prediction that the conscious perception hypothesis would make: Evidence for use of partial cues should exist for subjects performing at above chance levels but not for subjects performing at or below chance levels. If no such evidence is found, the conscious perception hypothesis is further undermined.

In sum, then, the three components of our approach are: (a) examine the sample distribution for evidence of conscious perception (i.e., look for an elevated mean and/or positive outliers), (b) see if systematic variation as a function of the independent variables is present (implicating unconscious perception if it is found), and (c) test the alternative conscious perception hypothesis in two further ways (comparing above and below chance subjects regarding the effects of the independent variables, look for evidence of partial cues). If (a) and (c) are negative while (b) holds, then we would conclude that unconscious perception is taking place.

Our approach might appear self-contradictory: How could an overall sample distribution resemble that expected by chance alone and yet systematic variation as a function of certain independent variables be present? Although this state of affairs may seem contradictory, it is quite common. Consider a series of a hundred coin tosses in which the frequency of "heads" is obtained. If we conduct a number of such series we will obtain a frequency distribution with a mean very near 50 and symmetrically declining frequencies on either side. This frequency distribution will conform very closely to the distribution expected by chance. However, the outcome of any given series (indeed, of any single toss) is not the result of intrinsically random variation but rather results from a variety of causes (e.g., wind conditions, initial position of the coin, how hard the coin is tossed, etc.). What makes the outcome of our hypothetical coin tossing experiment look random is that the causal influences themselves vary essentially randomly across the trials. A similar situation obtains in our experiments: The independent variables can have facilitating or inhibiting effects depending on their values. Further, these values tend to be distributed randomly across subjects in such a way that frequency distributions resembling chance are obtained when they are ignored. In this way we will show that the effects of unconscious perception can resemble chance variation unless detected by an approach such as ours. In contrast, conscious perception is incapable of producing results which resemble chance variation because it cannot produce true inhibition (i.e., below chance performance) in normal circumstances. In the coin toss analogy, the effects of conscious perception would be like those of a biased coin—consistent distortions of the frequency distribution in a positive direction would be found.

EXPERIMENT ONE

Our primary hypothesis in the first experiment was that there would be no evidence for conscious perception, in line with the above criteria. We made no hypotheses concerning the possible effects of task strategy, task preference, or word length because they have not been explored previously in unconscious perception. We made no predictions concerning word meaning because prior work has yielded inconsistent results (e.g., Kemp-Wheeler & Hill, 1992).

Subjects

Twenty-six males were recruited at the University of Michigan through advertisement in two University newspapers and flyers posted around the campus. All subjects had normal or corrected-to-normal vision and were native English speakers. Each subject was paid $10.00 upon completion of the experiment. Clear strategy preference ratings were available for 25 of the 26 subjects. One subject's strategy preference was equivocal.

Apparatus and Materials

Words were presented at 1 ms to subjects in a Gerbrand Model T3-8 three-field tachistoscope. The luminance level was set at 10 ft-lamberts for the stimulus and fixation fields and for ambient light. Subjects initially fixated a black dot in the blank field. Then the stimulus field was flashed for 1 ms, followed by the immediate reappearance of the fixation field. The viewing distance was 75 cm. Four words (Pleasure, Rose, Fighting, Pain) were balanced for frequency as defined by the Kucera and Francis (1967) norms. The range was 62 occurrences/million to 88 occurrences/million. The words were also rated as pleasant (Pleasure, Rose) or unpleasant (Fighting, Pain) on the Evaluative dimension of Osgood, May, and Miron's (1975) Semantic Differential Scales. The four words were also selected so that there were two pairs of semantically similar words (Pleasure/Rose; Fighting/Pain) and two pairs of structurally similar words (in length—Pleasure/Fighting; Rose/Pain). Each word was printed on a white 4 × 6 card in Helvetica Light 18-point presstype. The visual angle for Pleasure and Fighting was 2.3°; for Rose and Pain it was 1.15°. The white background on the card had about four times the reflectance of the black lettered words.

Procedure

Each subject was tested individually during a single session that lasted about 1½ h. We told subjects that the purpose of the experiment was to see how well people can identify words when they are presented for very brief periods of time. We asked them to fixate on the black dot in the blank field following a cue from the experimenter that a word was about to be flashed. After each word was presented, the subjects said which one of the four words they thought was presented. We informed subjects before the trials began what the words were and that they would be presented in random order, with no word being presented twice in a row. They were also informed that the words would be presented an equal number of times. Additionally, we told subjects that they would probably find it difficult to see the words but that it is sometimes possible for individuals to identify words even when their experience is that they see nothing. They were also informed that people get better with practice at this task. We told subjects to always reply by giving one of the four words, even if they felt unsure as to which one was actually presented.

After a practice block of 24 trials was completed, subjects were asked to describe their experience. All of the subjects protested that they could see nothing at all save an occasional slight change in apparent brightness or perhaps a darkening around the fixation point. None of the subjects claimed to see any more than this and the usual comment was that they were simply guessing as to the words' identity. If necessary, the experimenter reassured the subjects that their experience was common, that the task was difficult, and to continue doing the best they could. Also, the response frequencies for the four words was noted. If any word was named more than seven times or less than five times, they were reminded that the words were presented an equal number of times.

Following the practice block, subjects were told "some people look very hard where the word is presented, around the black dot, for anything they can see. People who use this method can sometimes pick up subtle clues that can help them identify the word, like little pieces of letters or perhaps shadows of the words. Other people just look where the word is presented and say whatever word pops into their head. Rather than look really hard for whatever they can see, they just relax and say which of the four words comes to mind." Subjects were then informed that a primary focus of the experiment was to examine the effects of using these strategies on one's ability to identify these hard-to-see words. All subjects then completed five blocks each of 24 trials in the look and pop conditions. There were thus 120 trials in each condition—30 per word. The order of the two conditions was counterbalanced among the subjects and they were asked to confine themselves to one strategy or the other in the relevant condition. After each block, we told subjects their percentage correct. After the two conditions, the experimenter asked the subject which one of the two conditions he preferred. The subject was then paid and debriefed.

The experimenter was blind to the stimulus cards' identity, which were coded by letters on the back of each card. The computer program that provided the random stimulus presentation order referred to the cards by code letter. However, because the subjects were given performance feedback following each block, they were not blind to their percentage correct when they were asked to indicate which condition they preferred. We considered eliminating performance feedback altogether, but decided not to because pilot subjects rapidly lost interest without it. Although giving performance feedback created a possible confound, the pattern of results, as well as Experiment Three, provided several converging lines of evidence that this was not the case.

Results

Preliminary analyses were planned to check for response bias and to see if there were systematic differences among individual words on the percentage correct obtained. Because these analyses involved only within-subject effects, they had the full N of 26. Then followed the main analyses in which percentage correct served as the dependent variable for correlations and various repeated measures MANOVAs. Analyses involving strategy preference had an N of 25. In the analyses of variance, a multivariate approach to the data was used initially because the SPSS-X User's Guide (1989, 3rd ed.) recommends such an approach to the data with repeated measures designs in general. Further, in this package the univariate results are provided and these mixed-model results can be used provided the homogeneity of variance and sphericity assumptions are met.[3] Where an effect has only two levels, as is the case with the analyses of primary interest in the present experiment, the multivariate results are simply equal to the univariate (ANOVA) approach. All significance levels are two-tailed unless otherwise specified.

Preliminary analyses. A one-way MANOVA was performed to examine possible response bias. The within-subject factor was the response frequency for individual words. Mauchley's sphericity test ($W = .813$, $p > .10$) was not significant,

[3] Even if sphericity assumptions are not met, the univariate results can still be used if appropriate adjustments to the degrees of freedom are made (e.g., the Geisser-Greenhouse or Huynh-Feldt corrections). These adjustments always reduce the significance of the F values in question. Therefore, when sphericity is violated and the unadjusted univariate F values are nonsignificant there is no need to make the adjustments discussed above.

allowing the univariate (averaged) results to be used. There were no differences among the words, $F(3, 75) = 1.01, p > .10$. There was therefore no evidence for response bias. The response frequency means for Pleasure, Rose, Fighting, and Pain were 60.72, 61.20, 57.32, and 60.76, respectively (a frequency of 60 represents perfect response balance).

The percentage correct for individual words was also analyzed with a one-way MANOVA to see if any individual word was markedly discrepant from the others. Mauchley's sphericity test was significant ($W = .619, p < .05$), but this was irrelevant since the individual word effect was not, $F(3, 75) = 1.05, p > .10$. This finding suggested that no single word stood out, indicating that we could combine them appropriately in the word meaning and word structure effects.[4]

Main analyses. The percentage correct grand mean was 25.00 ($SD = 2.85$). Thus, as a whole, our sample had a $d' = 0$, suggesting that subjects could not discriminate consciously among the four words. Following procedures outlined in Barnett and Lewis (1978, pp. 93–94) the studentized deviation from the mean test was performed on the highest score in our sample. Results were negative, suggesting that there were no subjects in our sample who were outliers—that is, whose performance was high enough to indicate the operation of conscious perception. Our first prediction, that words presented at 1 ms under our lighting conditions meet stringent criteria for the objective threshold, was therefore confirmed. Further, overall performance in the two strategy conditions was nearly identical. The pop condition mean was 25.22 ($SD = 4.29$), while the look condition mean was 24.77 ($SD = 4.55$). Nevertheless, task strategy and strategy preference mediated performance in the absence of conscious awareness. Specifically, preference predicted pop condition performance ($r = .44$, $p < .05$, $N = 25$) but not look condition performance ($r = -.10$, $N = 25$). (A positive correlation means that preferring pop increases the percentage correct.) Finally, pop and look condition performance levels were unrelated ($r = -.17$, $N = 26$).

These correlations suggested the presence of a Strategy Preference × Strategy Condition interaction. Two three-way MANOVAs were performed to examine these and other effects. The two three-way MANOVAs employed preference as the between-subject effect, with strategy condition and either word meaning or word structure as the within-subject effects. The word meaning and word structure effects could not be examined simultaneously because they were not completely crossed. For these two analyses, the only finding that approached significance was a trend for the Preference × Strategy interaction, $F(1, 23) = 3.10$, $p < .10$. These means are presented in Table 1.

Discussion

Several conclusions can be drawn from Experiment One. It demonstrated that verbal stimuli presented at the exposure durations and lighting conditions used in our laboratory are subliminal—that is, there is no evidence that subjects can

[4] Later we perform analyses in which word items are included as a random factor in the MANOVA model. Results were unchanged.

TABLE 1
Mean Performance by Preference and Strategy Condition for Experiment One

	Strategy condition	
Between-subject effect	Pop	Look
Preference		
Pop	27.50	24.24
(n = 11)	(4.33)	(5.10)
Look	23.81	25.18
(n = 14)	(3.50)	(4.40)

Note. Standard deviation and n's are in parentheses. Mean performance is expressed in terms of percentage correct (chance = 25).

consciously perceive them. Nevertheless, the pattern of correlational findings and the pattern of means in Table 1 suggest that unconscious perception is taking place as a function of preference and strategy condition. This supports the conclusions drawn by Shevrin (1973, 1988) and buttresses our claim that unconscious perception can occur under conditions of energy masking. Additionally, the differences between the pop condition means for the pop and look preference groups seem to be both above and below chance. This finding tentatively suggests that unconscious perception can be inhibited as well as facilitated by these factors. Or, more accurately, that subjects' intentional judgments about the unconsciously presented words can be skewed positively or negatively. In either case, however, the information is being unconsciously perceived.

Firm conclusions along these lines are premature, however. A significant Preference × Strategy interaction was not obtained, and the placement of means above and below chance may simply reflect chance variation. For these reasons, we undertook a replication of our first experiment to evaluate the robustness of our results and to firmly establish the Preference × Strategy interaction.

Further, our conclusions could be challenged on methodological grounds. In Experiment One the experimenter and the subject were aware of the subject's percentage correct by individual block. Hence, it could be suggested that the subject's preference ratings could simply have been based on his perception of which condition he did better in. However, if this had been so one would have expected the correlations between preference and performance to hold true for *both* strategy conditions. In the present study, however, preference predicts only pop condition performance and not look condition performance. The correlation in the look condition is near zero. Thus, we suggest that this possible artifact was not responsible for our findings.

EXPERIMENT TWO

A minor addition was made in the procedure for the second experiment. We now asked subjects to rate their motivation following the completion of the two strategy conditions. We simply asked them to rate "How motivated were you to do your best in the experiment today?" on a 5-point scale (1 = low, 5 = high).

Otherwise, the procedure was exactly as in Experiment One. The experimenter was not aware of the results from Experiment One.

Based on the results from Experiment One several a priori predictions were made for Experiment Two. First, we predicted that overall performance would again be at chance levels (about 25%) with no above-chance outliers. We also predicted that there would be no main effect for strategy condition. Second, we predicted a significant Preference × Strategy interaction, along with the associated correlational pattern we obtained in Experiment One. Third, we predicted that the means would be significantly above and below chance level (25%) for the Preference × Strategy interaction. That is, we predicted subjects who preferred to pop would perform above chance in the pop condition while subjects who preferred to look would peform below chance in this condition. Under Further Analyses and General Discussion we will present evidence suggesting that findings relevant to our third prediction (e.g., above chance performance for some subject groups) are the result of *unconscious* rather than conscious perception. Fourth, we predicted that these interaction means would differ significantly from each other in the relevant conditions even if one or more of them did not differ from chance level individually. Finally, we made no predictions concerning the new motivation variable.

Subjects, Materials, Apparatus, and Procedure

As in the first experiment, 26 males were recruited for an experiment on perception by ads in the campus newspapers and posters. Again, they were paid $10.00 for their participation. With the exception of the motivation ratings, all procedures and equipment were identical to the first experiment.

Results

Preliminary analyses. As in Experiment One, one-way MANOVAs were performed on response frequency and individual word performance. For the response frequency MANOVA, Mauchley's sphericity test was significant (W = .348, $p < .001$), but this was irrelevant because the response frequency effect was not significant, $F(3, 75) < 1$. Once more there is no evidence for response bias. The response frequency means for Pleasure, Rose, Fighting, and Pain were 59.84, 60.16, 58.62, and 61.38, respectively. Similarly, the MANOVA on individual word performance was not significant, $F(3, 75) < 1$, suggesting that it was appropriate to combine the words into the word structure and word meaning effects as in the previous experiment.

Main analyses. Almost all of our hypotheses were confirmed; the results were strikingly similar to those of the first experiment. First, overall performance was 25.26 (SD = 3.08), suggesting that subjects were unable to consciously discriminate among the four words. The studentized deviation from the mean test again showed no above chance outliers. Performance levels did not differ between the two strategy conditions; the pop condition mean was 25.19 (SD = 4.55), while the look condition mean was 25.32 (SD = 4.52).

The second finding was that the correlational pattern, presented in Table 2, is

TABLE 2
Correlations among Preference, Performance by Strategy Condition, and Motivation for Experiment Two

Variable	1	2	3	4
1. Preference	—	.45[a,*]	−.05[a]	−.05[c]
2. Pop %		—	−.07[b]	−.55[c,*]
3. Look %			—	.33[c]
4. Motivation				—

Note. Pop % is the percentage correct performance in the pop condition. Look % is the percentage correct performance in the look condition. All correlations with preference are point-biserial. In correlations with preference, a positive sign indicates a relationship with preferring the pop strategy, while a negative sign indicates the reverse.

[a] $N = 25$.

[b] $N = 26$.

[c] $N = 20$.

* Significant at $p < .05$, two-tailed.

almost identical to that of the first experiment. Preference once again predicted performance, but only for the pop condition. Additionally, motivation had a negative relationship with pop condition performance. The more motivated a subject perceived himself to be, the *worse* he did in the pop condition. Motivation had no effect in the look condition.

A three-way MANOVA was performed with the between-subject effect being preference and the within-subject effects being either strategy and word meaning or strategy and word structure. The results confirmed our predictions; the Preference × Strategy [$F(1, 21) = 5.00, p < .05$] interaction was significant. The means are presented in Table 3. For the Preference × Strategy interaction the pop condition means differed by planned comparison, $t\,(23) = 2.26, p < .05$. Further, the mean for look preference subjects in the pop condition was significantly *below* chance as well. The mean for pop preference subjects in the pop condition, however, did not differ significantly from chance. This is the only prediction for Experiment Two that was not confirmed.

TABLE 3
Mean Performance by Preference and Strategy Condition for Experiment Two

	Strategy condition	
Between-subject effect	Pop	Look
Preference		
Pop	26.11[1]	25.37
(n = 18)	(4.20)	(4.99)
Look	21.79_a[1]	25.83
(n = 7)	(3.35)	(3.26)

Note. Standard deviation and n's are in parentheses. Mean performance is expressed in terms of percentage correct (chance = 25). The 95% confidence intervals of means having a subscript do not include 25 (chance).

[1] Means having the same superscript differ significantly by a priori contrast at $p < .05$.

TABLE 4
Mean Performance by Strategy Condition and Word Meaning for Experiment Two

Grouping factor	Word meaning: Pleasant	Unpleasant
Strategy condition		
Pop	53.07	46.53
(n = 25)	(12.58)	(14.13)
Look	48.00	54.00
(n = 25)	(12.42)	(15.37)

Note. Standard deviation and n's are in parentheses. Mean performance is expressed in terms of percentage correct (chance = 50).

Further, the Strategy × Word Meaning interaction was significant, $F(1, 21) = 13.06$, $p < .01$. Although Cicchetti post-tests revealed no significant differences, the pattern of means presented in Table 4 suggests that pleasant word performance was greater than unpleasant word performance in the pop condition, while the reverse seemed to hold for the look condition.

Discussion

The virtually identical pattern of results found in Experiment Two constitutes a robust replication and supports Experiment One's findings. Taken together, these results suggest that even when overall performance is at chance levels and therefore shows no evidence of conscious perception ($d' = 0$), the Preference × Strategy interaction shows that these variables mediate performance. Further, the interaction of word meaning with strategy condition demonstrates the importance of the affective valence of the stimuli. Finally, motivation also mediated subjects' performance as a function of strategy condition. We believe that these findings demonstrate that intentional judgments can be significantly influenced by unconsciously perceived information.

This conclusion could be challenged on the grounds that our results simply demonstrate that under certain conditions conscious perception of very briefly presented stimuli can occur (e.g., when subjects prefer the task they are engaged in). How, then, can one tell if our direct measure effects are the result of conscious or unconscious perception? Upon what grounds can such a determination be made? Until now, the consensus has been that this differentiation is difficult if not outright impossible (see Reingold & Merikle, 1988). We feel, however, that our findings cannot be reasonably explained by conscious mechanisms. The conscious perception hypothesis would be forced to posit that look preference subjects in the pop condition were consciously choosing to skew their responses negatively—an implausible view at best. Indeed, here we agree with the usual view that incorrect identification responses constitute prima facie evidence that the word was *not* consciously perceived.

Finally, it could be argued that our findings involving preference and motivation are artifactual, given the presence of performance feedback. However, the pattern of results in Experiment Two provides two lines of evidence why this is unlikely to be the case. First, just as in Experiment One, preference predicts performance only in the pop condition. The same situation holds for the new motivation results. The artifact hypothesis would hold that either rating should predict performance in both conditions. Second, the artifact hypothesis has further difficulty with the motivation results. It strains credulity greatly to imagine that subjects, having received performance feedback, would tend to rate their own motivation as poor when their pop condition performance was good and vice versa. For these reasons we believe our findings involving motivation and preference to be genuine. Even if the reader is still unconvinced, however, it should be noted that the Strategy × Word Meaning interaction does not depend on subject ratings at all and hence cannot possibly be artifactual in this way. Nonetheless, it is still possible that our primary results might be artifactual because the experimenter and the subjects were aware of the performance feedback. To address this issue definitively, we conducted a third experiment.

EXPERIMENT THREE

Experiment Three was in fact a pseudoexperiment; that is, no actual stimuli were presented to the subjects. Instead, we simply used blank cards as the stimuli. Neither the subjects nor the (new) experimenter were aware that the stimulus cards were now blank. Just as in our prior experiments, the stimulus cards were identified by coded letters on the back of each card and the computer program provided the random stimulus presentation order by code letter. Identical instructions and procedures were used, such that subjects again gave one of the four words as a response after each trial. In this way, dummy performance feedback was also generated and provided after each block to the subjects as we did before. Similarly, preference and motivation ratings were obtained in the same manner.[5]

Further, we added a forced-choice detection task to the procedure to provide additional evidence that our exposure durations are subliminal. Following the completion of the two strategy conditions and after the preference and motivation ratings were obtained, we told subjects that they were now to decide on a forthcoming series of trials whether a word or blank card had been presented. For this task, we did of course use a real word. We chose the word "fighting" for this purpose and subjects were informed of the word's identity. We also informed subjects that the stimuli would be presented in random order with no stimulus being presented more than twice in a row. We also asked them to distribute their responses evenly between "word" and "blank." The experimenter was unaware of the preceding experiments' results, as well as Experiment Three's purpose as a pseudoexperiment.

The predictions for Experiment Three were simple: Nothing should happen. We reasoned that if our obtained effects in the preceding experiments were genu-

[5] In Experiment Three we also had the experimenter rate the subjects' motivation. We tried this in Experiment Two and found that experimenter-rated motivation predicted performance in the look condition. Results from Experiment Three, however, suggested that experimenter-rated motivation effects were artifactual after all.

ine, they should disappear in Experiment Three because no actual stimuli were shown. On the other hand, if the artifact hypothesis was correct, the continuing presence of (dummy) performance feedback should produce similar findings with respect to results involving preference and motivation that we found in our prior experiments. We also predicted that subjects would not perform above chance in the detection task.

Subjects, Materials, Apparatus, and Procedure

Similar to the previous experiments, 20 males were recruited for an experiment on perception by ads in the campus newspaper and posters. Again, they were paid $10 for their participation. With the exceptions discussed above, all procedures and equipment were identical to the first two experiments.

Results

Preliminary analyses. As in the prior experiments, a one-way MANOVA was performed on word response frequency. Mauchley's sphericity test was nonsignificant ($W = .633, p > .10$). The response frequency effect was also nonsignificant [$F(3, 17) = 1.03, p > .10$], suggesting that response bias was not present. The frequency means for the four words (Pleasure, Rose, Fighting, Pain) were 61.85, 61.00, 57.40, and 59.90, respectively. Unlike the previous experiments, a MANOVA on individual word performance was not conducted because it made no sense to do so given that actual words were not presented.

Main analyses. As one would expect, overall performance was at chance levels ($M = 26.33, SD = 3.38$), and performance levels were similar for the two strategy conditions. The pop condition mean was 26.37 ($SD = 4.80$), while the look condition mean was 26.29 ($SD = 4.91$).

The overall picture becomes clear when the correlations in Table 5 are examined. As we predicted, preference and motivation were not related to performance in either strategy condition. To further compare the results of the pseudoexperiment with our prior experiments, a three-way MANOVA was performed with

TABLE 5
Correlations among Preference, Motivation, and Performance by Strategy Condition for Experiment Three

Variable	1	2	3	4
1. Preference	—	−.10	−.18	−.27[a]
2. Pop %		—	−.03	.16[a]
3. Look %			—	.10[a]
4. Motivation				—

Note. Pop % is the percentage correct performance in the pop condition. Look % is the percentage correct performance in the look condition. All correlations with preference are point-biserial. In correlations with preference, a positive sign indicates a relationship with preferring the pop strategy, while a negative sign indicates the reverse. All correlations were nonsignificant, p's > .10, two-tailed.
[a] $N = 19$; otherwise $N = 20$.

preference as the between-subject effect and strategy and word meaning as the within-subject effects. The MANOVA results were similar to the correlational findings. Neither the Preference main effect nor the Preference × Strategy interaction was significant, both Fs < 1, $p > .10$.

Detection threshold check. On the postexperimental forced-choice detection task, subjects were clearly unable to discriminate the word from the blank card—the mean for correct identification was 11.85 ($SD = 2.62$). Chance level performance was 12 correct of 24 trials. Further, thre was no evidence for response bias. Subjects did not differ [$F(1, 18) = 1.14$, $p > .10$] in the frequency with which they gave "word" or "blank" as responses; the means were 11.11 ($SD = 3.30$) and 12.89 ($SD = 3.30$), respectively.

Discussion

The results from Experiment Three are clear. Preference and motivation effects did not approach significance, suggesting that subjects were unaffected by performance feedback in making these ratings and that therefore the previous findings involving these variables were genuine. Further, results from the postexperimental detection threshold task showed that our remaining findings are not due to conscious perception. Subjects were unable to reliably distinguish words from blanks.

FURTHER ANALYSES

Overall Analyses

To obtain the best parameter estimates for the effects that we obtained consistently and to see if the unexpected Strategy × Word Meaning finding in Experiment Two would hold up, a three-way MANOVA with preference as the between-subject effect and strategy and word meaning as the within-subject effects was performed. This MANOVA used the pooled data from Experiments One and Two (not, of course, Experiment Three).

The means for the Preference × Strategy interaction are presented in Table 6. The Preference × Strategy interaction was significant, $F(1, 48) = 4.82$, $p < .005$. Further, for the Preference × Strategy interaction the means in the pop condition were significantly both above and below chance. Thus, for the pooled sample there was facilitation as well as inhibition with respect to chance performance for this effect.

The means for the Strategy × Word Meaning interaction are also presented in Table 6. The Strategy × Word Meaning interaction was marginally significant for the pooled sample, $F(1, 48) = 3.81$, $p < .06$. Inspection of the means suggests that performance was higher for pleasant words than unpleasant words, but only for the pop condition.

It could be held that our MANOVA model has certain limitations—namely, that by pooling performance across words we fail to establish word generalizability. Accordingly, we reanalyzed these effects using a MANOVA model including word items as an additional random within-subject factor. This revised model

TABLE 6
Mean Performance by Preference, Word Meaning, and Strategy Condition for Experiments One and Two (Pooled)

	Strategy condition	
Grouping factor	Pop	Look
Preference		
Pop	26.64_a	24.94
(n = 29)	(4.23)	(4.98)
Look	23.14_a	25.40
(n = 21)	(3.50)	(3.98)
Word meaning		
Pleasant	53.07	49.60
(n = 50)	(13.31)	(12.28)
Unpleasant	47.60	50.93
(n = 50)	(12.40)	(14.62)

Note. Standard deviation and n's are in parentheses. Mean performance for the Preference × Strategy condition interaction is percentage correct by individual word (chance = 25). Mean performance for the Word Meaning × Strategy condition interaction is percentage correct by pleasant and unpleasant words (pooled; chance = 50). The 95% confidence intervals of means having the same subscript do not include 25 (chance).

also entailed changes in the appropriate error terms. The results were supportive—the new Preference × Strategy effect was significant, $F(1, 3) = 50.53$, $p < .006$. The new Strategy × Word Meaning effect was also significant, $F(1, 1) = 123.6$, $p < .06$. These results are identical to our prior findings and suggest that they are generalizable to the population of words.

Finally, it is possible that above chance performance for pop preference subjects in the pop condition could be due to practice effects across blocks, possibly also reflecting some facilitating effect of the performance feedback itself (cf. Doyle & Leach, 1988).[6] Accordingly, we reanalyzed pop condition performance to check for block and preference by block effects. Both Block [$F(4, 192) = 1.06$, $p > .10$] and Preference × Block [$F(4, 192) = 1.42$, $p > .10$] effects were nonsignificant, suggesting no support for effects of practice and/or feedback proper.

Conscious versus Unconscious Perception

We have already suggested that the conscious perception hypothesis cannot account for our findings regarding the below chance performance of look preference subjects in the pop condition. There are at least two other ways to test the conscious perception hypothesis. First, it would predict that our results are being carried by those subjects performing above chance, that is, those whose high performance was presumably due to conscious perception. The conscious perception hypothesis would then predict differences in the effects of preference and motivation when above and below chance subject groups are compared. Simi-

[6] We are indebted to several anonymous reviewers for suggesting these possibilities to us.

larly, the conscious perception hypothesis would predict that above chance subjects should show evidence of picking up partial cues, whereas those below chance should show no such evidence.

To test the first hypothesis, two three-way MANOVAs were performed. The first analysis had preference and performance level (above vs at or below chance) as the between-subject effects, and strategy condition as the within-subject effect. The conscious perception hypothesis would hold that the Preference × Strategy interaction should not exist for subjects performing at or below chance, but only for subjects performing above chance. By contrast, we predicted that no such interaction with performance level would be found. The Performance Level × Preference × Strategy interaction did not approach significance, $F(1, 39) < 1$, confirming our prediction.

The second analysis had preference and performance level as the between-subject effects, and strategy condition and word meaning as the within-subjects effects. The conscious perception hypothesis would predict that the Strategy × Word Meaning interaction should interact with performance level. As before, we predicted that no such interaction would be found. The Performance Level × Strategy × Word Meaning interaction was nonsignificant, $F(1, 40) < 1$, confirming, our prediction. Thus, the above effects appear to hold just as strongly for those subjects below chance as those above chance, just as Kemp-Wheeler and Hill (1988) found.

We also performed a three-way MANOVA on the pooled data to test for the presence of partial cues. We reasoned that if the conscious perception of partial cues were responsible for our results, there should be differences in the distributions of incorrect answers, such that above chance subjects should give relatively more structurally related wrong answers than below chance subjects. Accordingly, the within-subject effect in this analysis was error type, with structurally related, semantically related, and unrelated errors being possible. For the stimulus word Pain, for example, the response Rose would be a structurally related error, whereas Fighting would be a semantically related error. In this case, Pleasure would be an unrelated error. The Performance Level × Error Type interaction was not significant, $F(2, 49) < 1$. Again, the conscious perception hypothesis does not seem capable of tenably explaining our results. We propose instead that subjects who perform above chance are doing so by dint of unconscious rather than conscious perception.

GENERAL DISCUSSION

Taken together, our results show that even though there is no evidence of conscious perception ($d' = 0$, no positive outliers), the Preference × Strategy interaction and the correlation of motivation with performance show that these variables mediate performance both above and below chance. Further, the affective meaning of the stimuli mediates performance as well, as shown by the Strategy × Word Meaning interaction. We believe that these results demonstrate that intentional (i.e., conscious) judgments can be determined by unconsciously perceived information in the absence of conscious perception. This phenomenon has significance not only for subliminal perception and cognitive models of the mind in general, but for fundamental psychoanalytic postulates as well—namely, that meaningful unconscious cognitions can and do importantly influence ongoing conscious judgments and acts (cf. Shevrin, 1992).

How, then, can these findings be interpreted in terms of the nature and function of unconscious perception? We believe that our findings converge in an interlocking pattern. Only in the pop condition—*not* in the look condition—do preference, word meaning, and motivation mediate the influence of unconscious perception upon intentional judgments. Thus, subjects' consciously held task strategies have a major influence on the effects of unconscious perceptions upon intentional judgments. It may be that task strategy is a crucial initial gateway that determines whether subliminal effects manifest at all.

These strategic effects are broadly consistent with the recent findings of Dagenbach et al. (1989). It seems that consciously adopted strategies can affect even the most "automatic" unconscious perceptual processes. Dagenbach et al. explained their results by suggesting that subjects whose thresholds were obtained through a semantic similarity task tried and failed to consciously retrieve this information and that this failure was responsible for the resultant inhibition on the lexical decision task. They did not find such inhibition, however, for subjects whose thresholds were set using a forced-choice discrimination procedure. Because this is the type of task we had subjects attempt in our experiments, and because our precedure differed in a variety of other ways from that of Dagenbach et al., the specific explanation that they offered for their findings does not directly apply to our situation. In any event, our findings support Dagenbach et al's. (1989) conclusion that the "disconnection" hypothesis (e.g., Carr et al., 1982; Marcel, 1983) is wrong. That is, that conscious and unconscious perceptual mechanisms are deeply interconnected rather than neatly separable, as the disconnection hypothesis holds.

More specifically, why are the effects of unconscious perception apparent in the pop condition but not the look condition? We propose that the task demands of the pop condition explicitly instruct subjects to open themselves to unconsciously perceived stimuli—that is, to say what "pops into their head"—while in the look condition subjects are instructed to focus effortfully and purposefully upon whatever consciously available information they can detect. These kinds of focused volitional efforts may overwhelm or inhibit the presumably subtle, transient effects of unconscious perceptions. This rationale is also consistent with an essential tenet of psychoanalytic theory—that is, Freud's suggestion that the effects of unconscious processes are more likely to manifest when individuals free associate rather than when they attempt to deliberately control their thought processes.

Turning now to a closer examination of how the aforementioned factors mediate pop condition performance, we hypothesize that our results reflect modifications mainly on the output side of unconscious processing systems. Intuitively, it seems that individuals in the pop condition are instructed not to apply the usual conscious volitional control to their perceptions, but rather to allow their unconscious perceptual processes to operate unimpeded.

First, strategy preference reflects a disposition to allow this process to occur or not; that is, how ready is someone to allow subliminally perceived information to affect intentional judgments without having a conscious experience of actually seeing something. With this interpretation in mind, the fact that strategy preference can enhance or inhibit pop condition performance becomes understandable. Look preference subjects, whose performance is below chance, are selectively *suppressing* information from their unconscious perceptions. It should be kept in mind, however, that to perform below chance subjects *must* be unconsciously perceiving the information—otherwise they would perform *at* chance levels. On the other hand, pop preference subjects can simply allow their unconscious perceptions to facilitate their intentional judgments and their performance is above chance.

Second, the word meaning effect in the pop condition suggests a more specific form of perceptual defense. Subjects' performance with pleasant words is facilitated relative to unpleasant words. This finding suggests a simple psychoanalytic explanation—subjects inhibit unpleasant information (relative to pleasant information) from unconscious channels when instructed to passively allow unconscious influences to occur.

Third, motivation's negative mediation of pop condition performance is broadly consistent with the differential effects of task strategy. Namely, to the extent that a subject employs active volitional effort, his performance is substantially inhibited. Conversely, the more they relax their efforts at volitional control, the better their performance. Notice, however, that this effect also manifests exclusively in the pop condition. For motivation to make a difference, the subject must be open to his unconscious perceptions in the first place.

These findings are consistent with a body of work summarized by Dixon (1981, p. 52), which suggests that subjects who adopt a passive, receptive strategy show considerably greater effects of subliminal stimuli than those who take more active approaches. They are also consistent with Marcel's (1983) observation that subliminal effects were only observed with subjects who adopted a passive approach to his experimental task. To underscore this point, it is useful to consider that our results demonstrate truly substantive effects. When a stepwise multiple regression with preference and motivation as the predictors and pop condition performance as the criterion is performed, the multiple $R = .70$, $R^2 = .48$, and adjusted $R^2 = .42$. Thus, conservatively speaking, preference and motivation account for over 40% of the variance in pop condition performance.

Implications for Other Issues in Unconscious Perception

Our findings bear on four major issues currently being debated in unconscious perception. These are: (a) How to differentiate between the contributions of conscious and unconscious perception, (b) the importance of motivational, dispositional, and affective variables in cognitive processes, (c) the apparent nonmonotonicity of unconscious perception as thresholds are varied, and (d) the viability of energy masking.

Whereas some investigators now agree that conscious intentional judgments may reflect input from both conscious and unconscious perceptual processes (cf. Reingold & Merikle, 1988), to date no attempts have been made to distinguish their relative contributions to such judgments. Because above chance discrimination has often been held to be prima facie evidence for conscious perception (see Holender, 1986), many investigators have apparently regarded the interpretive problems of detecting unconscious determinants on direct measures (i.e., intentional judgments) as insoluble. Instead, most have focused on the dominant paradigm that looks for evidence of unconscious processing in indirect measures while direct measures remain at chance. We believe that our approach makes a contribution in this regard in that it proffers a threefold criterion for making this distinction with intentional judgments: (a) Are the intentional judgments as a whole at chance (i.e., $d' = 0$ and there are no positive outliers)?, (b) is there present nevertheless systematic variation in performance as a function of independent variables?, and (c) given the first two criteria, are there differences in the effects of the independent variables when the above and below chance groups are compared? If not, as in the present experiments, then there is no evidence for the action of conscious perceptual processes.

Our findings also suggest a clarification in the meaning of the objective threshold concept. We suggest that the objective threshold should be understood to refer to exposure durations which eliminate the effects of *conscious* perception on intentional judgments. Normally, the effects of both conscious and unconscious perception are confounded on such tasks. In this situation, a methodological bind ensues—it can always be argued that either (a) allegedly subliminal effects are really due to conscious perception, or (b) that if $d' = 0$ unconscious as well as conscious influences will simply be eliminated (cf. Doyle, 1990).

Some investigators have developed ingenious and important methods to cope with these problems. Reingold and Merikle (1988; Merikle & Reingold, 1991) have demonstrated unconscious influences when indirect measures show larger effects than direct measures. Jacoby (1991) has shown that conscious and unconscious influences on memory tasks can be separated by placing them either in conjunction with or in opposition to each other. These investigators, however, have chosen to opt out of the absolute subliminality issue altogether. On the other hand, our approach allows evidence for unconscious perception to be obtained when the objective threshold for conscious perception has been unambiguously reached.

Our findings illustrate the usefulness of studying the role of "hot" cognitive processes. Indeed, if preference, strategy, word meaning, and motivation had not been included in the present experiments, no evidence of conscious or unconscious processing whatsoever would have been found. These factors seem important even when the task is fairly bland and neutral.

Many investigators such as Dagenbach et al. (1989) and Dixon (1981) have suggested that unconscious perception may operate most effectively when stimuli are presented somewhat below rather than at thresholds for conscious identification. We are unable to address this issue definitively since we did not set thresholds or vary exposure durations in our experiments. However, we did find that our exposure duration (1 ms) was at least at the objective detection threshold and in this regard our findings are consistent with the suggestions of these investigators. Further, the fact that we found differences only when our independent variables were considered may help explain the failure of other investigators to find evidence for unconscious perception at or below objective thresholds in that these moderating factors are not usually examined.

Finally, our findings argue in favor of the viability of energy masking as an experimental paradigm for studying unconscious perception. That is, our results (and those of Shevrin, 1973, 1988) suggest that when important moderating variables are included evidence of unconscious perception can be found using this method. Thus, despite the claims of Marcel (1983) and others, unconscious perception does occur with energy masking under appropriate conditions and using sensitive measures.[7]

ACKNOWLEDGMENTS

This research was made possible in part by a grant from the Ford Motor Company. We thank Anthony Marcel and Ian Nimmo-Smith of the MRC Applied Psychology Unit (Cambridge, England) and William J. Williams of the University of Michigan for their helpful comments during the preparation of the manuscript.

REFERENCES

Balota, D. (1983). Automatic semantic activation and episodic memory. *Journal of Verbal Learning and Verbal Behavior*, **22**, 83–104.

Barnett, V., & Lewis, T. (1978). *Outliers in statistical data*. New York: Wiley.

Bowers, K. (1984). On being unconsciously influenced and informed. In K. Bowers & D. Meichenbaum (Eds.), *The unconscious reconsidered* (pp. 227–272). New York: Wiley.

Carr, T., McCauley, C., Sperber, R., & Parmelee, C. (1982). Words, pictures and priming: On semantic activation and the automaticity of information processing. *Journal of Experimental Psychology: Human Perception and Performance*, **8**, 757–777.

Cheesman, J., & Merikle, P. (1984). Priming with and without awareness. *Perception and Psychophysics*, **36**, 387–395.

Dagenbach, D., Carr, T., & Wilhelmsen, A. (1989). Task-induced strategies and near-threshold priming: Conscious influences on unconscious perception. *Journal of Memory and Language*, **28**, 412–443.

Dixon, N. (1981). *Preconscious processing*. London: Wiley.

Doyle, J. (1990). Detectionless processing with semantic activation? A footnote to Greenwald, Klinger, and Liu (1989). *Memory and Cognition*, **18**, 428–429.

Doyle, J., & Leach, C. (1988). Word superiority in signal detection: Barely a glimpse, yet reading nonetheless. *Cognitive Psychology*, **20**, 283–318.

Erdelyi, M. (1974). A new look at the new look: Perceptual defense and vigilance. *Psychological Review*, **81**, 1–25.

Eriksen, C. (1960). Discrimination and learning without awareness: A methodological survey and evaluation. *Psychological Review*, **67**, 279–300.

[7] However, more recently Marcel has obtained priming effects using energy masking but only with binocular rather than dichoptic or monoptic stimulus presentation conditions (personal communication, Feb. 16, 1990).

Fowler, C., Wolford, G., Slade, R., & Tassinary, L. (1981). Lexical access with and without awareness. *Journal of Experimental Psychology: General,* **110,** 341–362.

Holender, D. (1986). Semantic activation without conscious identification in dichotic listening, parafoveal vision, and visual masking: A survey and appraisal. *Behavioral and Brain Sciences,* **9,** 1–23.

Horowitz, M. (Ed.). (1988). *Psychodynamics and cognition.* Chicago: Univ. of Chicago Press.

Jacoby, L. (1991). A process dissociation framework: Separating automatic from intentional uses of memory. *Journal of Memory and Language,* **30,** 513–541.

Kemp-Wheeler, S., & Hill, A. (1988). Semantic priming without awareness: Some methodological considerations and replications. *The Quarterly Journal of Experimental Psychology,* **40A,** 671–692.

Kemp-Wheeler, S., & Hill, A. (1992). Semantic and emotional priming below objective detection threshold. *Cognition and Emotion,* **6,** 113–128.

Kucera, H., & Francis, W. (1967). *Computational analysis of present day American English.* Providence: Brown Univ. Press.

Marcel, A. (1983). Conscious and unconscious perception: Experiments on visual masking and word recognition. *Cognitive Psychology,* **15,** 197–237.

Merikle, P. (1982). Unconscious perception revisited. *Perception and Psychophysics,* **31,** 298–301.

Merikle, P., & Reingold, E. (1991). Comparing direct (explicit) and indirect (implicit) measures to study unconscious memory. *Journal of Experimental Psychology: Learning, Memory and Cognition,* **17,** 224–233.

Osgood, C., May, W., & Miron, M. (1975). *Cross-cultural universals of affective meaning.* Chicago: Univ. of Illinois Press.

Reingold, E., & Merikle, P. (1988). Using direct and indirect measures to study perception without awareness. *Perception and Psychophysics,* **44,** 563–575.

Remington, R., & Schork, M. (1985). *Statistics with applications to the biological and health sciences* (2nd ed.). New Jersey: Prentice-Hall.

Shevrin, H. (1973). Brain wave correlates of subliminal stimulation, unconscious attention, primary and secondary process thinking, and repressiveness. *Psychological Issues,* **8**(2), 56–87.

Shevrin, H. (1988). Unconscious conflict: A convergent psychodynamic and electrophysiological approach. In M. Horowitz (Ed.), *Psychodynamics and Cognition* (pp. 117–168). Chicago: Univ. of Chicago Press.

Shevrin, H. (1992). Subliminal perception, memory, and consciousness: Cognitive and dynamic perspectives. In R. Bornstein & T. Pittman (Eds.), *Perception without awareness: Cognitive, clinical, and social perspectives* (pp. 123–142). New York: Guilford Press.

SPSS, Inc. (1988). *SPSS-X User's Guide* (3rd ed). Chicago: Author.

Turvey, M. (1973). On peripheral and central processes in vision: Inferences from an information-processing analysis of masking with patterned stimuli. *Psychological Review,* **80,** 1–52.

Wong, P., Shevrin, H., & Williams, W. (in press). Conscious and nonconscious processes: An ERP index of an anticipatory response in a conditioning paradigm using visually masked stimuli. *Psychophysiology.*

Received August 19, 1992