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Overcoming Stroop Interference: The Effects of Practice on Distractor Potency

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With practice, do distracting stimuli lose their ability to distract? In a series of experiments, subjects practiced counting digits, a task subject to Stroop-type interference, and then were tested in a variety of transfer conditions. The results indicate that digits do lose their ability to distract as a result of practice but that this loss is highly specific; practice in ignoring one pair of distractors (*2* and *4*) does not improve later performance when ignoring a different pair (*1* and *3*). However, this practice effect does transfer to distractor stimuli having the same meaning as the stimuli ignored in practice (TWO and FOUR, but not TO and FOR). The results can be explained either in terms of active learning to suppress distraction or in terms of habituation of competing responses.

Do distracting stimuli lose their ability to distract as a result of practice? While it is obvious that performance in selective tasks improves with practice (for laboratory demonstrations, see Rabbitt, 1967; Stroop, 1935), it is by no means clear what the source of this improvement is. Among other possibilities (a) subjects might improve their ability to allocate resources efficiently to the relevant stimuli rather than their ability to resist responding to irrelevant stimuli; (b) subjects simply might improve their performance on the central task, apart from any attentional learning at all; (c) subjects might learn to discriminate more easily or more effectively between relevant and irrelevant stimuli (Rabbitt, 1967, convincingly argues that his data are best explained in this way); (d) subjects may learn to overcome the *masking* effects of the irrelevant input; or (e) subjects could learn to adopt peripheral strategies, such as closing their eyes or orienting their heads to "ignore" distractors.

This article is concerned with whether practice can, in fact, decrease the potency of a distractor. If in learning to perform a selective task, subjects are increasing their ability to *attend* (rather than ignore—Possibility a), merely improving on the central task itself (Possibility b), or adopting some peripheral strategy (Possibility e), then they should transfer perfectly to a new situation in which the central task remains the same and the specific irrelevant items are changed. If, on the other hand, distractors do lose their power to distract through practice, then the effect might be specific to the particular stimuli involved and would fail to transfer completely to

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a new set of distractors. (Completely general transfer, a finding that did not obtain, would, of course, be ambiguous with regard to this question.)

Incomplete transfer might also be expected if improvement in selection were produced by either of the two kinds of perceptual learning (Possibilities c and d) described above. However, these alternative explanations could be eliminated if the situation was one in which perceptual difficulties, either in detecting the relevant stimulus or in discriminating it from the irrelevant stimulus, were highly unlikely. Fortunately, there are such situations. In the experiments reported, subjects were engaged in a Stroop-type task in which they judged the numerosity of strings of digits or digit names. (See Dyer, 1973; Jensen & Rohwer, 1965; Morton, 1969; and Regan, 1978, for reviews of the extensive Stroop literature.) In Stroop (1935) interference, it is implausible that subjects have any trouble discriminating the irrelevant aspects of the stimulus (the word or symbol depicted) from the relevant aspect (the color or numerosity of the symbol), or that the distractor in any way masks the attended stimulus. Learning to overcome Stroop interference, therefore, is not readily attributable to perceptual learning.

Stroop interference also provides a second advantage for purposes of the present study. Stroop interference, one might argue, stands at the extreme of the range of selective tasks. The distractor is extremely potent, extremely intrusive. If ever it is to the subject's advantage to ignore a stimulus or to suppress a response to it, as opposed to merely not attending it, it would seem to be in cases such as Stroop interference or in the paradigms used by Eriksen and Eriksen (1974) and Schneider and Shiffrin (1977). Thus, if one is looking for evidence that distractors do lose their potency with practice, Stroop interference seems a sensible place to begin the search.

In the first experiment in this series, subjects practiced a task (numerosity judgments) in the presence of Stroop interference (when counting digits, e.g., with "3 3 3 3" receiving the response "four") or no interference (when counting question marks and ampersands). Generalizing from previous results (Stroop, 1935), we expected subjects to improve on both tasks with practice. In addition, subjects should overcome the interference. Performance levels with the Stroop items should approach those of the easier non-Stroop controls. The major question was how this practice effect would transfer.

In the second half of the experiment, subjects performed the same counting task, but with new items (new digits and new noninterfering items). If learning is stimulus specific, that is, if learning from the first half of the procedure does not transfer completely to new stimuli in the second half, then learning could not merely be a question of peripheral adjustment or practice at the counting task itself.

Although not addressing the issue of practice per se, Neill (1977; following Greenwald, 1972, Table 3) in fact found stimulus specificity in a Stroop color-naming task. Subjects were slower to name the color of the ink of a printed word if the preceding (ignored) word had been that color name. That is, a subject would be slower to respond "blue" to the stimulus GREEN printed in blue if the preceding stimulus had been BLUE printed in red. Neill argues that a subject must in some way inhibit his own responses to the printed word in order to respond to the ink color; succeeding responses are slowed by a residual effect of this inhibition.

The inhibitory mechanisms evidenced in Neill's results might be a locus of the effects of practice on distractor strength, if such effects can be shown to exist. Our first experiment is designed to ask if extended practice does have an effect on distractor potency.

Experiment 1

Method

Subjects. Sixteen paid undergraduate volunteers from the University of Pennsylvania served as subjects. Each participated for approximately 1 hr.

Stimuli. On each stimulus page, the symbols to be counted were typed in 24 double-spaced rows.

Each row contained one, two, three, or four characters. Four types of stimulus pages were prepared: *1,3* pages contained rows of *1*s and rows of *3*s; there were also *2,4* pages,),& and ?,# pages. Rows on each page were randomly ordered with respect to character type and numerosity. Each combination of character and numerosity was repeated three times on each page.

Ten pages of each of the four types (*1,3*; *2,4*;),&; ?,#) were constructed. The four sets of pages were matched for response sequence—each page had a corresponding page of each of the other three types having the same pattern of correct responses. Pages were randomly ordered within types to obscure this correspondence.

Procedure. Subjects were run individually in a single session. They were instructed to name aloud the number of characters in each row on the page as rapidly as they could without making errors. The experimenter handed the subject each stimulus page covered by a blank cover sheet. When the experimenter pronounced the start signal, the subject turned over the cover sheet and began to count. Errors and time to complete the page (measured on a stopwatch) were recorded. Thus, a trial consisted of a run through a single page, beginning with the experimenter's signal and ending when the subject reached the bottom of the page.

The experiment consisted of two segments. In each, subjects counted symbol pages and digit pages on alternate trials. The segments differed only in the stimuli: In the first segment, subjects saw one type of symbol page, for example,),&, and one type of digit page, for example, *1,3*. In the second segment, these sets of pages were replaced by the other symbol pages and the other digit pages (e.g., ?,# and *2,4*). The order of page-type and whether the alternation began with a symbol or a digit page were counterbalanced across subjects. Each segment of the procedure consisted of 60 trials—30 of each stimulus type (i.e., three times through a stack of 20 alternating pages).

At the beginning of the session, the subject's task was explained to him or her. Subjects were told that the pages would alternate between types and were shown a page of each of the two types used in the first half of the procedure. Between halves of the experiment, subjects were told that the characters to be counted would change and were again given a chance to examine the two new kinds of pages.

Results and Discussion

For purposes of analysis, the data were divided into blocks of 10 trials (pages) each. Thus, each half of the procedure consisted of three digit blocks and three symbol blocks. (A single run through the stack of alternating digit and symbol pages produced one data block of each type.) Within each block, the mean response time was found for those trials containing no errors and then was log transformed. Means of log scores across subjects are shown in Figure 1. A linear regression line was fitted to the log data (vs. log blocks) for each subject in each condition and in each half of the experiment.[1] The regression curves shown in Figure 1 represent the mean slope and zero intercept across subjects. (Because the regression is computed on log blocks, the zero intercept is a measure of initial, first block, difficulty.)

Note that the null hypothesis predicts full transfer, with no effect of changing the distractor items. That is, there should be no discontinuity between halves of the experiment in either the digit or symbol condition. Thus, the three blocks of the second half were considered in the regression to be Blocks 4, 5, and 6. The null hypothesis predicts second-half slopes and intercepts equal to first half. The experimental hypothesis predicts second-half intercepts higher than those of the first half but makes no specific prediction with regard to slopes.

Although the regression measures provide an accurate description of the present data, they are likely to be unfamiliar to many readers. It should therefore be noted that for all the results reported in this article, analyses of unregressed raw scores lead to the same conclusions as the regression analysis. The regression analysis tends to be more conservative. Raw scores were analyzed (Experiment 1) in terms of the *last* block of the *first* part of the procedure and the *first* block of the *last* part of the procedure.[2] It should also be pointed out that in the results reported, error data provide a measure of subjects' performance that is not redundant with the regression

[1] The log-log linear regression is offered only as a convenient data summary; nothing in the results depends on this particular analysis. Crossman (1959) showed practice curves to be well approximated in this way, and the present data bear this out.

[2] In Experiments 2 and 3, the analysis compared response rates in the two conditions for the *first* block of the *last* part of the procedure.

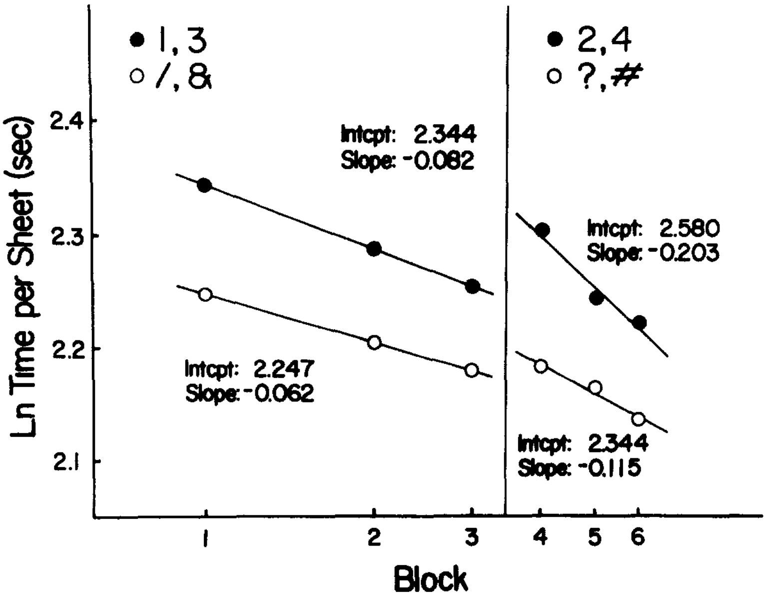

Figure 1. Mean log response times and regression lines (ln), Experiment 1. (Intcpt = intercept.)

measures, since the latter are based on errorless trials. Except in the one place noted in Experiment 1, the error results and response rate measures are in accord.

To return to the results of Experiment 1, all subjects showed the basic Stroop phenomenon. First-half intercepts for the regression lines were significantly higher in the digit than in the symbol condition, $t(15) = 10.21$, $p < .001$. Performance improved significantly and more in the digit than in the symbol condition. The slope of the digit curve was significantly lower than that of the symbol curve in the first half, $t(15) = 2.43$, $p < .025$; both were significantly less than zero, $t(15) = 4.94$, symbols, $t(15) = 7.68$, digits, $p < .001$.

Error rates were generally low (below 3% in all of the data to be reported). Error rates were higher overall in the digit condition, 2.44 errors per block versus .67, $t(15) = 5.04$, $p < .001$. However, the error rate in the first and second halves is the same for both symbol and digit conditions.

Neither condition produced complete transfer. In both conditions, second-half intercepts tended to be higher and slopes lower (i.e., steeper) than in the first half. For symbols, the intercepts changed significantly, $t(15) = 1.75$, $p < .05$, whereas slopes did not, $t(15) = 1.63$, *ns*. Both slopes and intercepts changed for digits, $t(15) = 3.18$ and 4.19, respectively, $p < .025$.

The difference between conditions in the attained significance levels of these changes reflects differences in the consistency and the size of the effect. Changing the Stroop items tended to cause a greater loss of speed than did changing the control items. A subject by subject comparison showed the change in intercept to be larger in digits than in symbols, although this difference is just short of significance, $t(15) = 1.74$, $p < .10$. A trend in the same direction, with the change in digits larger than in symbols, can be seen in the change of slopes, $t(15) = 1.46$, *ns*.

One aspect of the results suggests that the slowing in the symbol condition is not a result of increased difficulty due to the novelty of the symbols being counted. Instead, it might be that subjects experience great difficulties with the new digits in the second half of the experiment and cautiously slow down their response rate in both digit and symbol conditions, producing the intercept increase in the control condition. This caution hypothesis is supported by the pattern of errors. If subjects are slowing down in the symbol condition out of caution and not because of an increase in task difficulty, then one might expect a time–error trade; that is, error rates should decrease in the second half. Error rates do tend to be lower in the first block of the second half than they were in the first half, .31 per block versus .52, $t(15) = 1.54$, $p < .10$. (This is the only reported experimental result in which the error rate and response rate measures do not concur.) This stands in contrast to the digit condition, in which errors increase in the first block of the second half, 3.25 versus 2.21, $t(15) = 2.13$, $p < .05$. The difference between conditions is significant, $t(15) = 2.54$, $p < .01$. Thus, the most appropriate conclusion seems to be that the interference condition shows a greater loss of efficiency overall than does the control.

Apparently, then, the effects of practice are highly specific. Changing the distractor items increases the task difficulty, whereas changing the nondistracting control seems not to; learning to ignore one pair of distractors helps little in ignoring another pair.[3] Experiments 2 and 3 were intended to determine at what level the practice effect seen in the first experiment is produced.

Experiment 2

This experiment attempted to address two issues. First, we sought to strengthen the evidence for the highly specific practice effects indicated in the first experiment. In particular, one might argue that second-half response slowing in Experiment 1 was due to the mere physical novelty of the new stimuli, rather than to a lack of transfer of practice to the new distractor digits. This slowing was greater in the digit condition, according to this alternative argument, because novelty potentiates Stroop interference effects. Conceivably, novelty causes subjects only to note the distractor stimuli; having noted them, digit stimuli interfere more than symbol stimuli. (Many of the results in Greenwald, 1972, can also be explained in this way.)

In Experiment 2, a comparison was arranged between transfer conditions in which the stimuli were equally novel (physically) and, what is more, equally likely to produce interference in the absence of relevant practice. However, one condition, but not the other, may benefit from specific transfer. Thus, differential transfer in the two conditions would unambiguously indicate that a specific practice effect was occurring.

The second purpose of Experiment 2 was to identify the locus of the specific practice effect. When a subject overcomes the distraction of a particular pair of digits, is he or she coming to ignore the visual stimulus, or is he or she coming to ignore the phonetic (acoustic or articulatory) or semantic codes elicited by that visual stimulus?

In the first half of this experiment, subjects practiced counting the digits on the *1,3* sheets (*2,4* for half of the subjects) from the first experiment. In the second half of the procedure, subjects counted words. On half of the trials, subjects counted clusters of ONEs and THREEs, and, on the other half, TWOs and FOURs. By one hypothesis, subjects should be coming to ignore the visual stimuli and should not show transfer to either kind of trial in the second phase. By another hypothesis, subjects should be coming to ignore a phonetic or semantic code in the first phase. Consequently, they should show

[3] While the present results suggest that there is incomplete transfer (less than 100%) between counting one digit pair and counting another, a further experiment suggests that there is in fact no transfer whatsoever. In the first half of this experiment, 20 subjects counted symbols (control group) or one of the digit pairs (experimental group) for 30 trials. In the second half, all subjects counted digits and symbols on alternating trials. Second-half interference from one pair of digits was no less for subjects who had previously counted the other digit pair than for subjects who had counted symbols. (Second-half interference was measured as digit-page times minus symbol-page times. No comparisons between the groups showed significant differences, $t[18] = .34$, intercepts, $-.40$, slopes, .54, errors.)

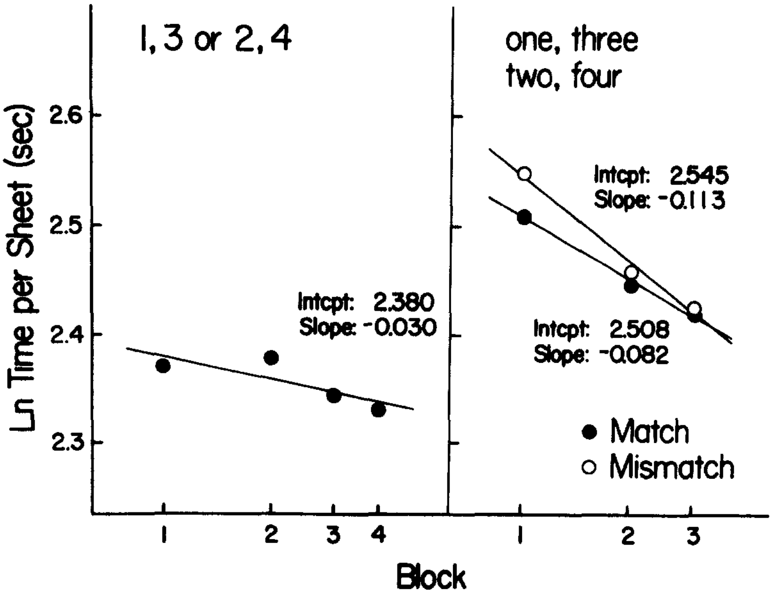

Figure 2. Mean log response times and regression lines (ln), Experiment 2. (Intcpt = intercept.)

more transfer in the second phase to counting the digit words corresponding to the digit symbols used in practice.

Method

Subjects. Eight paid undergraduate volunteers served as subjects. (No subject was used in more than one experiment in this series of experiments.)

Stimuli. The *1,3* and *2,4* pages from the first experiment were used in the first half of this procedure. For the second half, TWO, FOUR and ONE, THREE pages were constructed. The words were in vertical clusters of one, two, three, or four, instead of rows. Clusters were spaced down the page, and there were four columns of clusters per page. Thus, the TWO, FOUR page might contain the cluster

TWO
TWO
TWO

with the correct answer "three." Each combination of character and numerosity was repeated three times on each page. Ten pages of each kind were constructed and assembled into one stack of alternating ONE, THREE and TWO, FOUR pages. As in the first experiment, the sets of digit word pages were matched for response sequence and then randomly ordered to obscure this correspondence.

Procedure. The procedure was the same as in Experiment 1 except for the following changes. The first half of the experiment contained only one type of trials (*1,3* or *2,4*). There were four blocks of 10 trials each. In the second half of the experiment, trials alternated between page types. (The kind of page used on the first trial was counterbalanced across subjects.) One run through the stack of pages consisted of 10 trials of each type and thus produced two data blocks. There were three runs in all.

Results and Discussion

The data, shown in Figure 2, were partitioned into blocks and a regression line was fitted to them, as in Experiment 1. As the null hypothesis is that there is no difference in performance between conditions in the second half, the three blocks of the second half were treated as Blocks 1, 2, and 3 in the regression. Thus the visual-code hypothesis predicts that slopes and zero intercepts will be the same whether the new distractors match the old ones in name or not. Yet, as can be seen, the intercept for the digit words that match the practice digits is lower than the intercept for those that do not match, $t(7) = 2.01$, $p < .05$. Slopes for match tend to be slightly higher than for mismatch, $t(7) = 1.35$. The striking result, however, is in the error data: Subjects made considerably more errors (errors per block) in counting digit words if these words did not match the practice digits, 6.00 per mismatch block versus 3.58, match, $t(7) = 5.33$, $p < .001$.

The specificity of the learning seen in this experiment both replicates and extends the results of the first experiment.[4] These results strongly indicate that the practice effect that is occurring, although specific to the distractors used in practice, is not specific to the visual stimuli themselves. Rather, subjects seem to be learning to ignore the phonetic or semantic representations of the distracting stimuli. The third experiment attempted to differentiate among these possibilities.

Experiment 3

What is happening when a 2 or a 4 loses its potency as a distractor? Experiment 2 ruled out the possibility that subjects were simply becoming less likely to respond to a visual code. Perhaps a subject becomes less likely to respond to the phonetic code elicited by the visual stimulus. If this is the case, then subjects should show considerable transfer to TOs and FORs after counting 2s and 4s. On the other hand, if subjects are becoming less likely to respond to the semantic code elicited by the visual stimulus, then they will show relatively little transfer to the homophones.

In the first half of this procedure, subjects practiced counting on the *2,4* pages. In the second half, performance on TWO, FOUR pages was compared to performance on TO, FOR pages. To determine the difference in baseline difficulty in the digit word and homophone tasks, a different group of subjects practiced counting 1s and 3s and then counted clusters on the digit word and homophone pages. On the basis of the specificity of transfer in Experiments 1 and 2, counting 1s and 3s should have little effect on the potency of the distraction from either the digit words or the homophones. Data from this control, therefore, would allow an uncontaminated comparison of the difficulty of counting digit words and counting the homophones.

A comparison of the controls and the experimental subjects would indicate whether the practice in the first phase was having its effect on phonetic or semantic codes. If, through practice, subjects become less likely to respond to a potentially distracting semantic code, then performance on counting digit words after counting digit symbols should improve relative to counting the homophones. If subjects are learning about phonetic codes, both digit word and homophone performance should improve in the experimental condition.

Method

Subjects. Sixteen paid volunteers served as subjects and were randomly assigned to the control and experimental groups.

Stimuli. The *1,3 2,4* and TWO, FOUR pages described previously were used. In addition, 10 TO, FOR pages were constructed in the same format as the digit word pages. Again, response sequences were matched in the homophone and digit word sheets. The digit word and homophone pages were assembled into a single stack of 20 pages, alternating between types.

Procedure. Subjects had four blocks of practice with the digit pages. Control subjects practiced with *1,3* pages, and experimental subjects, with *2,4* pages. In the second half of the procedure, both groups of subjects had alternating trials of digit words (TWO, FOUR) and homophones (TO, FOR), counterbalancing the type used as the first page of the alternation. There were three runs through the 10 digit word and 10 homophone pages.

Results and Discussion

The results are shown in Figure 3. The two groups did not differ significantly during practice in intercept, $t(14) = .78$, $p > .80$, or error rates. (Experimental subjects tended to commit more errors—6.28 per block vs. 3.97, $t[14] = 1.54$, $p > .10$.) The groups did tend to differ in slope

[4] Two additional subjects were run in a procedure identical to the present one, except that the first phase of the experiment was lengthened from 40 to 170 trials spread out over 5 days of practice. On the 5th day, subjects were tested with "match" and "mismatch" stimuli, as in the experiment reported. Two aspects of the data are notable. First, the specificity still obtained after the greatly extended practice. Second, the learning curve was surprisingly smooth, despite the day-long rest intervals between successive days of running.

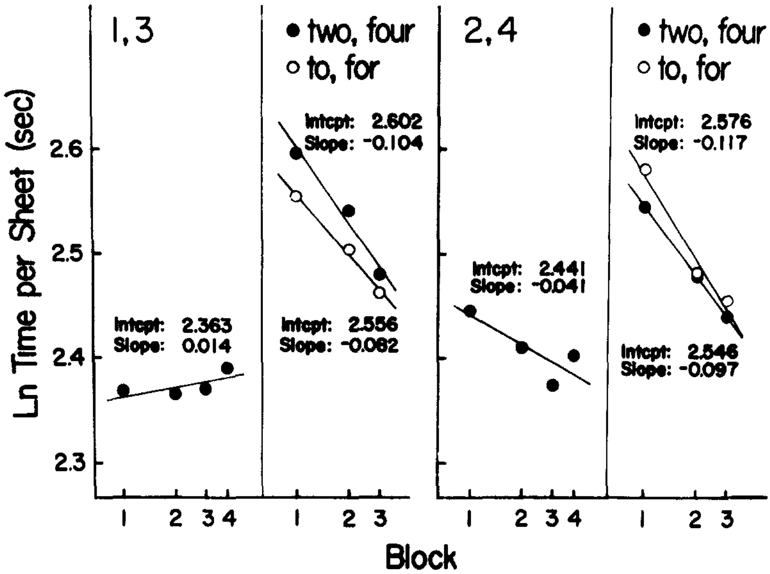

Figure 3. Mean log response times and regression lines (ln), Experiment 3. (Control subjects are shown in the left panel, and experimental subjects, in the right panel. Intcpt = intercept.)

during practice, with the slope higher in the control group, $t(14) = 1.85$, $p < .10$, two-tailed. (The positive slope in the control subjects during practice is probably due to the effects of fatigue, presumably masking the benefits of practice. In general, the problem of fatigue makes it difficult to evaluate these practice effects in terms of absolute response rates. Thus we have preferred to infer the effects of practice from differential transfer rather than to examine the practice data per se.)

The control subjects (left panel of Figure 3) did significantly worse with digit word than with homophone pages. Their digit word intercepts are significantly higher, $t(7) = 2.08$, $p < .05$; their slopes tend to be lower, $t(7) = 1.36$, *ns*. No such effect exists for the experimental group. Intercepts here tend to be slightly *lower* in the digit word condition, $t(7) = 1.06$, *ns*; slopes are slightly higher, $t(7) = .59$, *ns*. This Stimulus Type × Group interaction is significant for intercepts, $t(14) = 2.25$, $p < .025$, but not for slopes, $t(14) = 1.18$, *ns*. The error results show the same pattern. Subjects in the control group made significantly more errors on digit word trials, 6.00 errors per block versus 3.50, $t(7) = 2.89$, $p < .025$; subjects in the experimental group did not, 6.92 versus 6.54, $t(7) = .29$. This interaction did not reach significance, $t(14) = 1.46$.

It is clear that much of what subjects are learning is about the meaning of the stimuli. Practice with a distractor stimulus that elicits a certain semantic code may make that code less susceptible to elicitation by any stimulus. Alternatively, practice may serve to decrease the readiness to respond to the distractor meaning code.

Are subjects learning to ignore phonetic codes as well as meaning? We presume that phonological encoding occurs because subjects do show interference in the homophone condition (pilot data). Is this interference affected by practice? If so, the experimental group's performance in the homophone condition should have been superior to that of the control group. A comparison of the first half (practice) performance with performance in the homophone condition in both groups shows no evidence of such a difference. Both groups were slower in the homophone condition than in practice, but with no difference between groups in the intercept change (second half intercept minus first half, control vs. experimental), $t(14) = .39$, *ns*, or slope change, $t(14) = .57$, *ns*. Moreover, the difference in error rates, although nonsignificant, is in the direction opposite to that predicted if subjects are learning to ignore sound codes. Compared to practice performance, experimental subjects tended to make more errors in the homophone condition than did controls, $t(14) = .31$, *ns*.

We note in this context that Martin (1978) has found that concurrent articulation of irrelevant syllables can reduce Stroop interference with a manual response. This result suggests that Stroop interference is partly phonetic. Our results do not contradict this conclusion. They suggest only that any phonetic interference in our task is not reduced by practice, but semantic interference (Regan, 1978) is reduced.

General Discussion

Practice in an attention task does appear to reduce the potency of the distractors involved in that task. In the present paradigm, this reduction in distractor strength is highly and somewhat surprisingly specific. Further, it occurs at the level of the semantic code for the distractors. Subjects become less likely to respond to the semantic code (or to activate that code) elicited by the particular distractors used in practice (independent of the visual form in which the distractor is presented).

Two large questions remain about these findings. First, to what extent are the present findings general across distractors and distractor types? The Stroop task, chosen in part because it allows one to sidestep the issue of perceptual learning, is probably not representative of distractors in the world and surely not representative of the population of distractors conventionally studied in the laboratory. Lorch, Anderson, and Well (Note 1) have found comparable specificity in a speeded classification task in which subjects judged values on one dimension (circle size) while ignoring variation on a second orthogonal dimension (e.g., background pattern). Clearly more research is required before the issue of generality can be addressed.

Second, what is the nature of the decrease in distractor strength? Are subjects becoming better able to ignore the distractor, or are they simply becoming accustomed to it?[5] These two alternatives, learning to ignore and habituation, differ in their specifications of the necessary and sufficient conditions for this distractor-weakening effect. If subjects are becoming habituated to the distractor stimuli, then mere exposure to the stimuli in a nondistracting context should cause these stimuli to be less disruptive when they later appear in a distracting context. If subjects must learn to ignore, on the other hand, exposure should not be sufficient. On this latter view, distractor stimuli become weaker because subjects have practiced, and therefore become better at, suppressing a tendency to respond or attend to them.

A theory of attention such as that of Neisser (1976), which argues against the need for active suppression, would seem to imply the former alternative, even though Stroop interference is likely to put such a theory to its most severe test. By contrast, an attention theory that places the selective burden on suppression of responses to distractions would favor the latter possibility, learning to ignore, assuming that such suppression improves with practice. (The various versions of "filter theory," Broadbent, 1958; Moray, 1969; Treisman, 1964, are often construed in this way.) We argued earlier that if such suppression exists anywhere, it should exist in cases such as Stroop interference. (But see Treisman, 1969.) Thus, failure to find learning to ignore in the Stroop paradigm could be considered problematic to any attention model involving the active suppression of irrelevant stimuli.

[5] Habituation in the present experiments, apparently at the level of semantic codes, would parallel what is discussed in the literature as semantic satiation (Fillenbaum, 1963; but see Esposito & Pelton, 1971).

This issue, learning to ignore versus habituation, has been missed or ignored in other studies of attention. For example, Neill (1977) offers data that purport to show inhibitory processes (i.e., suppression) in a Stroop color-naming task. These results are as easily explained by habituation accounts as they are by active suppression, just like the present data.

We have attempted in several experiments to resolve this question in the present context. An empirical resolution requires only that the subject be exposed to the distractor-to-be, not practice ignoring it. If that stimulus, when later used as a distractor, is less disruptive than a novel stimulus, then habituation can be inferred as the source of the effect. If practice at ignoring a stimulus brings no additional advantage, then the entire decrease in distractor potency is attributable to habituation.

Thus we need to find a task in which the distractor-to-be is presented to the subject but is neither ignored nor attended. (Attending to the stimulus would probably strengthen its distractor strength; Stroop, 1935.) Further, this task should not involve responses that are incompatible with responses to be made in the later distraction task. Subjects might come to associate the first-task response with the particular stimuli used, causing some difficulty when different responses are required in the second (distractor) task. This response learning might offset or diminish whatever advantages are provided by the habituation occurring in the first task.

We have been unable to design or find a task having the necessary characteristics. A decrease in distractor strength can be demonstrated following tasks in which there is no measurable interference, a result confirming the habituation hypothesis, but no direct comparisons of the effects of exposure alone with exposure plus ignoring are yet available.[6] Thus, it is unclear whether habituation is the sole source of the loss of distractor potency.

The present results, therefore, leave unanswered the question of whether we can learn to suppress interference. Practice does reduce interference, and this reduction is specific to the semantic codes activated by the distractors. However, the nature of this effect cannot yet be specified.

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[6] Subjects showed no measurable interference when asked to count strings of letters (As and Cs, or Bs and Ds) rather than numbers. That is, counting letters was no slower than counting punctuation marks. However, the letter stimuli did produce a large interference effect when subjects performed a modified counting task (lettering), saying "A" if there was one item in a row, "B" if two, and so on. If letters did not interfere with the counting task, there was presumably no motivation to suppress. Thus, counting letters should produce habituation but should not produce practice at suppression. We can thus ask whether there is habituation by asking whether counting letters in the first half of the session reduces the interference from those letters in a second-half lettering task.

Further, if this reduction is as great as the reduction resulting from lettering letters in the first half, it would appear that reduction in interference results only from habituation, with no active learning to ignore (which, by definition, occurs only when a distractor actually interferes). An experiment we have run suggests that there is a habituation effect; that is, counting letters in the first half of the procedure does reduce interference from those letters in a second-half lettering task. However, to interpret this result we must make the assumption that there is literally *no* interference from letters in the counting task. As this assumption may be wrong, the result is not conclusive.

Another experiment suggests that there is a learning-to-ignore effect in addition to a habituation effect. Lettering letters in the first half does more to reduce the second-half interference from those letters than does counting letters in the first half. However, this result may be artifactual. Subjects, when counting, may generally be learning to respond with numbers. This response set may itself impair lettering performance for old (previously counted) letters relative to new, potentially obscuring the benefits of habituation. (In fact, in the second half of the procedure, some counting subjects performed *less* well with old letters than with new, an unexpected pattern that suggested the hypothesized artifact.) Thus, we cannot conclude that there is any real learning to ignore. These problems seem difficult to resolve.

Reference Note

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