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The Locus of Interference in the Perception of Simultaneous Stimuli

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In a wide variety of well-practiced target detection tasks, both auditory and visual, performance is little influenced by the number of simultaneous nontargets, but suffers if simultaneous targets must be detected separately. It is suggested that only targets need pass through a limited-capacity system leading to awareness (and hence availability for report); nontargets can be identified and rejected by earlier parallel, unconscious processes. Since, for example, nontarget words can be rejected on the basis of meaning, stimuli must be fully identified before the limited-capacity system. More generally, performance decrements due to divided attention are usually marked whenever simultaneous stimuli (psychophysical, verbal, etc.) must be identified separately and independently. This is to be expected, since under these circumstances all stimuli must pass through the limited-capacity system to awareness.

Theories of Attention

What sets the limit on a person's ability to identify (divide attention between) several stimuli at once? Accounts based on *early selection* distinguish two systems of perceptual analysis. First, a parallel system (*preattentive*) extracts such simple stimulus characteristics as color (vision) or voice (hearing). Second, a limited-capacity system (*attentive*) is responsible for the analysis of form and meaning. It is only in this second system that there is a limit on the ability to deal with several stimuli at once. Stimuli can be chosen for passage from the first system to the second on the basis of any characteristic already derived by the first system. For example, a person might wish to identify only words spoken in a certain voice. Voice is derived by the first system; only words in the chosen voice need to be passed on to the second, limited-capacity system, for the analysis of form. Accounts of this general sort have been offered by Broadbent (1971), Treisman (1964a, 1964b), and Neisser (1967).

Accounts based on *late selection* also distinguish a first, parallel system from a second, limited-capacity system. The difference lies in the supposition that even in the first system form and meaning are extracted, as well as simpler stimulus characteristics such as color or voice. Various interpretations of the second system have been offered, perhaps consolidation in short-term memory (Shiffrin, 1975), or formation of a coordinated percept from discrete stimulus characteristics (Allport, 1977). The important point is that even form and meaning are derived for all stimuli in parallel, so that even on the basis of these characteristics, it should be possible to select stimuli for passage from the first system to the second. Beginning with Deutsch and Deutsch (1963), many writers have adopted some position of this sort (e.g., Allport, 1977;

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Hoffman, 1978; Keele, 1973; Posner, 1978; Shiffrin & Schneider, 1977).

The aim of this article is to develop a new argument for late selection based partly on the literature and partly on new experiments.

Target Stimuli in Divided Attention

I shall begin by considering the influence of divided attention in certain sorts of target search experiments. One important point that will emerge is that it does not particularly matter whether a task requires form discrimination. Under some conditions, performance decrements due to divided attention will be small whether or not form discrimination is required, and equally under other conditions, they will be large. There is no reason here to consider that form processing is special.

Search for a Single Target

One important set of experiments concerns visual search. An array of characters (typically letters or digits) is searched for a prespecified target. For the moment I shall concentrate on situations in which targets and nontargets either fall into distinct, well-learned categories (e.g., search for digit targets among letter nontargets) or are distinguished by some fairly simple physical characteristic such as color. It is well-known that under these conditions, functions of reaction time or accuracy against the number of characters in the array can suggest some approximation to parallel search (Egeth, Jonides, & Wall, 1972; Jonides & Gleitman, 1972; Schneider & Shiffrin, 1977; Treisman, Sykes, & Gelade, 1977). Thus there is little evident limit on the ability to divide attention between characters.

Even in the simplest and best practiced tasks, there is usually some small performance decrement (decline in speed or accuracy) as the size of the array is increased (Corcoran & Jackson, 1977; Egeth et al., 1972). But this is often to be expected for reasons having nothing to do with attention. In many experiments characters are more closely packed in larger arrays (increasing the chance for peripheral masking), or spread further into the visual periphery. There is also an important statistical argument (Eriksen & Spencer, 1969). As array size increases, so also does the chance that a nontarget will be mistaken for the target. This is true not because each character is examined less accurately but on simple grounds of probability. Even if, at all array sizes, the probability of mistaking a given nontarget for a target is p, the probability of no such mistake, $(1 - p)^N$, where N is the number of nontargets, declines as array size increases. The subject can either accept the increased error rate (decline in accuracy), or try to go more slowly, and so increase the accuracy with which each character is examined (decline in speed).

What is needed is a technique manipulating division of attention without changing the spacing of characters or the number of chances for a "false alarm." Originally introduced by Eriksen and Spencer (1969), such a technique has been used in a long sequence of experiments by Shiffrin and his associates. A study by Shiffrin and Gardner (1972) provides a good example. On each trial four letters were presented, tachistoscopically, in the form of a square. Three were nontargets (e.g., the shape O); the task was to locate and identify the fourth, the target (the letter *T* or *F*). The dependent measure was accuracy. In the simultaneous (SIM) condition, all four characters were presented in a single exposure. In the successive (SUCC) condition, the trial was split into two halves: Two characters (forming one diagonal of the square) appeared in the first exposure, the others in a second. The subject knew in advance which diagonal would appear in each exposure and had 500 msec between the two. Just as in the SIM condition, the single target was identified after all four characters had appeared. Though both SIM and SUCC conditions involved four characters (and hence the same overall chance for a false alarm), SUCC imposed less of a divided attention demand, since only two characters were examined at a time. Strong evidence against any divided attention limitation was provided by equal performance in the two conditions, and in other experiments similar results have been obtained with a wide variety of different stimuli—visual (Shiffrin, Gardner, & Allmeyer, 1973), auditory (Shiffrin, Pisoni, & Castaneda-Mendez, 1974), tactile (Shiffrin, Craig, & Cohen, 1973), and mixed modality (Shiffrin & Grantham, 1974).

These results should be taken along with those suggesting parallel search with varying array size. Apparently, if the distinction between targets and nontargets is simple or well learned, then there is little or no effect of divided attention when a subject searches several stimuli for a single target.

It is important that this result can hold whether or not the task requires form discrimination. The target sought might be a digit among letters (Egeth et al., 1972) or a simple touch on the skin (Shiffrin, Craig, & Cohen, 1973).

Detection of Independent Targets

In a different type of target detection experiment, simultaneous stimuli must be identified separately and independently, with a separate response for each. Most of the experiments reported have used auditory stimuli. In a study by Ostry, Moray, and Marks (1976), two words were presented simultaneously, one to either ear. If the word on the left was an animal name (target), a key was to be depressed with the left hand. If the word on the right was an animal name, a different key was to be depressed with the right hand. Any combination of left and right targets was possible—no target, either target alone, or both targets—and correspondingly, any combination of responses was allowed. Other experiments have used much the same design, but with pure tone stimuli and the simplest possible distinctions (e.g., a slight difference in frequency) between targets and nontargets (Moray, Fitter, Ostry, Favreau, & Nagy, 1976; Pohlmann & Sorkin, 1976; Shiffrin & Grantham, 1974; Sorkin, Pohlmann, & Gilliom, 1973).

All these experiments show performance decrements due to divided attention. The exact form of the results is particularly striking. Let the simultaneous stimuli be A and B, each target or nontarget, and each with its separate response. Performance on one stimulus (A) can be assessed contingent on the simultaneous event of the other (B). There are four possible events: a hit (target correctly identified), false alarm (nontarget incorrectly called target), correct rejection (nontarget correctly identified), and miss (target incorrectly called nontarget). This analysis reveals a consistent pattern of results. Performance on one stimulus (A) is best when the simultaneous event (B) is a correct rejection, worst when it is a hit or false alarm, and intermediate when it is a miss (Eijkman & Vendrik, 1965; Moray, 1975; Moray et al., 1976; Ostry et al., 1976; Pohlmann & Sorkin, 1976; Sorkin et al., 1973). After practice, with a simultaneous correct rejection on B, performance on A may be as good as if attention were not divided at all (Ostry et al., 1976; Pohlmann & Sorkin, 1976). But this is certainly not so with a simultaneous hit, false alarm, or miss.

Again, results are the same whether or not the task requires form discrimination. The same picture is seen whether targets and nontargets are tones distinguished by a slight difference in frequency (Moray et al., 1976) or words differing only in semantic class (Ostry et al., 1976).

Independent Identification

The discussion so far suggests that although performance decrements due to divided attention may be small or absent when subjects search for a single target, some pattern of decrements can clearly be demonstrated if simultaneous stimuli must be independently identified as target or nontarget, with a separate response for each. Outside the context of target detection, the general rule seems still to hold that if simultaneous stimuli must be independently identified, with a separate response for each, then some effect of divided attention, often quite substantial, is to be expected.

A great many experiments could be cited in support of this. In some, the stimuli are simple, so that essentially the subject makes simultaneous psychophysical judgments. The subjects of Long (1975), for example, judged simultaneously the frequency of a tone and the intensity of a light, with a separate written response for each. Those of Lindsay, Taylor, and Forbes (1968) judged simultaneously the intensity of a tone and the position of a dot, again with a separate response for each. Other tasks require simultaneous form discriminations. The subjects of Sperling (1963, 1967) were to identify all the letters in a tachistoscopic array, separately reporting each one. Those of Broadbent (1958) and Treisman and Geffen (1967) made separate responses to dichotic speech messages.

Of course, these experiments have involved very different stimuli, responses, dependent measures, and so on. Still one gains the general impression that if simultaneous stimuli are to be independently identified, with a separate response for each, then some effect of divided attention is almost always to be expected (e.g., Allport, 1971; Egeth & Pachella, 1969; Forbes, Taylor, & Lindsay, 1967; Massaro & Kahn, 1973; Massaro & Warner, 1977; for an exception see Eriksen & Lappin, 1967). This perhaps matches the conclusions drawn from the target search studies, and sets them in a more general context.

Summary

To summarize, if subjects search for a single target stimulus (and the distinction between targets and nontargets is simple or well learned), there may be little or no performance decrement due to divided attention. If, on the other hand, simultaneous stimuli must be independently identified as target or nontarget, with a separate response for each, then a specific pattern of divided attention effects is seen. Accuracy on any given stimulus is worst when the other is a target (or is mistaken for one). In general, there are often large effects of divided attention if simultaneous stimuli must be independently identified, with a separate response for each. These conclusions apply whatever perceptual discrimination a task requires, from one-dimensional psychophysical judgment to the full analysis of form.

Of course, in this review I have been considering widely different types of experiments, with different stimuli, responses, and dependent measures. I turn next to an attempt to confirm the conclusions concerning target search in a single experimental context. The task is visual and requires some degree of form discrimination: search for digit targets among letter nontargets.

Empirical Demonstrations

A simple rule is suggested for those target-search tasks in which targets and nontargets either fall into distinct, well-practiced categories or are distinguished by some fairly simple physical characteristic. Effects of divided attention will be substantial especially if simultaneous targets can occur and must be independently identified. Any task allowing performance to be based on the detection of a single target will show little if any effect of divided attention.

Experiment 1

Experiments 1a, b, and c were similar. Subjects searched four-character stimulus arrays for digit targets among letter nontargets. The technique described earlier in connection with the work of Shiffrin and Gardner (1972) was used to manipulate the degree of divided attention. In the SIM condition, all four characters were presented together, whereas in SUCC they appeared two at a time, imposing less of a divided attention demand. SIM and SUCC conditions were compared for two different search tasks. In one task, subjects essentially searched for a single target. Here, little if any SUCC advantage was predicted. In the other, targets were to be detected independently in the two halves of the array. A substantial SUCC advantage was predicted, especially for either half of the array if on the same trial a target was detected in the other half.

Method

Displays. The experiment was run on-line on a PDP-15 computer. Stimulus arrays consisted of four characters arranged on a cathode ray tube (CRT) in the form of a cross (plus sign), with the center of the cross corresponding to the point of fixation. Each character was constructed by illuminating the appropriate points in a 7 × 5 matrix, at the viewing distance of approximately 36 cm (from a chinrest) subtending about .8° × .6° visual angle, centered 1.75° from fixation. Center-to-center adjacent characters were separated by about 2.5°. The characters at 9 and 3 o'clock made up the *horizontal limb* of the cross; those at 12 and 6 o'clock made up the *vertical limb.* The position of each character could be indicated by a bar marker, .33° long, pointing toward the character and centered .83° from its outer edge. A central fixation mark also subtended .8° × .6°. A shaded reading lamp provided dim background illumination in an otherwise darkened room.

Arrays consisted of digit targets among letter nontargets. Digit targets appeared independently in horizontal and vertical limbs of the array. Thus a single trial might contain targets in both limbs, though there was never more than one target per limb. In each limb the probability of a target was one third. When any limb contained a target, its position within the limb was determined at random, each position being equally likely. Letters were drawn from the set A,E,G,J,M,P,T,X, digits from 2–9. They were drawn independently on each trial, with the sole constraint that all four chosen characters be different.

Presentation. Each trial of the SIM condition began with the central fixation mark, presented for 500 msec, accompanied for the first 250 msec by the four bar markers. Then, at 500 msec the four-character stimulus array was presented for an exposure duration of T msec, $T = 20$ in Experiment 1a, $T = 15$ in Experiments 1b and 1c. This array was immediately replaced by a corresponding array of four masks (character-sized rectangular light patches covering the positions that the characters had just filled). These remained for 2,000 msec, when the trial ended and the next immediately began.

Each trial of the SUCC condition was split into two halves. The first began as before with a 500-msec presentation of the central fixation mark. For the first 250 msec, this was accompanied by two bar markers indicating either the horizontal or the vertical limb of the stimulus array (designated the *first* limb). At 500 msec the two characters of this limb were presented, the two positions of the other (*second*) limb being filled by masks. As before, characters were replaced by masks after T msec, and the resulting display of four masks then remained for 500 msec. At this point the second half of the trial began. The fixation mark appeared for a second 500 msec, accompanied for the first 250 msec by bar markers indicating the second limb of the stimulus array. At 500 msec the two characters of this limb were presented, masks filling the two positions of the first limb. Again, characters were replaced by masks after T msec, and the display of four masks remained for 2,000 msec. The trial then ended and the next immediately began.

In Experiment 1a the horizontal limb was always first for all subjects. In Experiments 1b and 1c, the horizontal limb was always first for half the subjects; the vertical limb, for the other half. Thus, subjects always knew in advance which limb to examine in each half of a SUCC trial, and though just as in SIM the eventual decision was always based on a total of four characters, only two were examined at a time.

Tasks. There were two different search tasks. In the *combined task* there was a single response key to be depressed if any target was detected. Since targets occurred independently and with probability of $\frac{1}{3}$ in each limb, the overall probability of at least one target was $\frac{1}{3} + \frac{1}{3} - \frac{1}{9} = \frac{5}{9}$. A single key depression was sufficient even if targets were detected in both limbs. (This applied even in SUCC, when the two limbs were exposed separately.) Tentatively, this was considered equivalent to search for a single target. Though simultaneous targets could occur, it was only necessary to find one. Little if any SUCC advantage was expected. (Experiment 2 contains a task in which simultaneous targets actually cannot occur).

In the *separated task* the two limbs were to be considered independently. There was one key (left hand) for targets detected in the first limb, another (right hand) for the second. (For a given subject, the limb actually first in the SUCC condition was designated first also in SIM, both for key assignment and in data analyses.) Thus on any trial the subject might depress zero, one, or two keys. Here simultaneous targets were to be independently identified. A substantial difference between SIM and SUCC was expected. In SIM a particular difficulty for each limb was expected when simultaneously a target was detected in the other.

Subjects were strongly encouraged to ignore speed and concentrate on accuracy. In SUCC the response to a target detected in the first limb could be made either immediately or after presentation of the second limb. Subjects were fully aware of target probabilities and were encouraged to respond on about the correct proportion of trials.

Design. Subjects were recruited from the subject pool of the Department of Psychology, University of Oregon. Each served in several hourly sessions on different days.

In Experiment 1a there were two groups of eight subjects each, 10 women and 6 men, ages 18–32. The combined task was given to one group, the separated task to the other. Each subject served in two sessions.

In Experiment 1b there was a single group of four subjects, three women and one man, ages 21–33. Each subject served in 10 sessions. Half of the subjects had the combined task for the first 5 sessions and the separated task for the second 5 sessions, half the reverse.

In Experiment 1c there was a single group of four subjects, three women and one man, ages 19–28. Each served only in five sessions of the combined task.

Procedure. Each session was devoted entirely to either the combined or the separated task, and had two blocks of the SIM and two of the SUCC condition, in ABAB or BABA order (counterbalanced across subjects and for any one subject alternating from session to session). Each block was made up of five sub-blocks, the first (for warm-up) of 24 trials, the others each of 48 trials. On cue (SIMULT or SUCCESSIVE appearing on the CRT) the subject pressed a key to begin the sub-block. The first trial began 500 msec later, and thereafter trials followed one another without pause, as previously described. Independently, for horizontal and vertical limbs, one third of the trials in the sub-block had targets; otherwise, sequences were random. Sub-blocks were separated by a short pause.

Analysis. Data were stored on magnetic tape and analyzed by computer program. For each subject, the first session (in Experiment 1b, the first session of each task) was considered practice and discarded. For the remaining sessions, all 24-trial warm-up sub-blocks (from the start of each SIM or SUCC block) were also discarded, and analyses were then based on the remaining pooled 48-trial subblocks.

The combined task was scored in terms of the whole stimulus array, in both SIM and SUCC conditions. Thus a hit was scored if the key was depressed and (at least) one target was present in either limb. A false alarm was scored if the key was depressed and neither limb contained a target. The separated task was scored separately for the two limbs. Thus a hit was scored for the first limb if it contained a target and the left-hand key was depressed, a false alarm if this limb contained no target yet the key was depressed, and similarly for the second limb and the right-hand key.

Separately for each subject and task, hit and false alarm rates were transformed to the measures of signal detection theory, d' and β. These measures will be presented; however, every experiment in this article was scored also in terms simply of percentage correct (mean of hit and correct rejection probabilities). In no case was the pattern of results changed (though in a few cases statistical significance was lost). Some caution is still required, since neither measures based on signal detection theory nor percentage correct measures may be ideal. Still, a result demonstrated with both measures is probably real.

Values of d' and β cannot be obtained with hit or false alarm rates of 1.0 or .0, and some pooling of the data was sometimes needed to avoid this. In particular, data from first and second limbs in the separated task sometimes had to be pooled, so that values of d' and β given here reflect an approximate average of performance on the two. Cases of this sort are mentioned in the text, and the data were always checked to ensure, as far as possible, that the pooled results fairly reflected separate performance on the individual limbs.

Results

Combined task: Single targets. For the major analysis of combined task performance, trials with double targets (one in each limb) were dropped. Thus a percentage of hits was obtained from those trials with a single target and a percentage of false alarms from those trials with none. Mean derived values of d' and β are shown in Table 1, separately for Experiments 1a, b, and c. For Experiments 1b and 1c, pooled sessions 2 + 3 of the combined task are shown separately from pooled sessions 4 + 5.

Table 1
Experiment 1: Mean Values of d' and β for Detection of a Single Target in the Combined Task

			Condition	
Experiment	Session	Measure	SIM	SUCC
1a[a]	2	d'	2.02	2.08
		β	1.10	.89
1b[b]	2 + 3	d'	1.92	2.27
		β	2.55	1.74
	4 + 5	d'	2.24	2.49
		β	3.16	2.65
1c[b]	2 + 3	d'	1.59	1.87
		β	1.51	1.21
	4 + 5	d'	1.83	2.14
		β	1.92	1.44

Note. SIM = simultaneous presentation; SUCC = successive presentation.
[a] Each value is the mean over eight subjects.
[b] Each value is the mean over four subjects.

In Experiment 1a the effect of divided attention, as assessed by higher values of d' in SUCC than in SIM, was minimal. The SUCC advantage held for only four out of eight subjects. Values of d' were examined by analysis of variance, with the single (within-subjects) factor of condition (SIM vs. SUCC) being nonsignificant, $F(1, 7) = .3$. There was a tendency for β to be higher in SIM than in SUCC, but this, too, was nonsignificant, $F(1, 7) = 3.7$. Differences in β will be given little attention here, though they tend throughout to be in this same direction.

In Experiment 1b the d' advantage for SUCC was .30. Values of d' were examined by analysis of variance with the (within-subjects) factors condition (SIM vs. SUCC) and session (2 + 3 vs. 4 + 5). No effect was significant; for condition, $F(1, 3) = 3.3$. For every subject at each level of practice, however, SUCC was slightly superior to SIM. All differences in β were similarly nonsignificant.

Table 2
Experiment 1: Observed and Predicted Probabilities of a Detection Response in the Combined Task

		Condition							
		SIM				SUCC			
Experiment	Session	p	q	r	r_p	p	q	r	r_p
1a[a]	2	.180	.832	.929	.957	.215	.873	.953	.974
1b[b]	2 + 3	.140	.749	.927	.919	.108	.815	.938	.955
	4 + 5	.088	.775	.943	.941	.079	.824	.986	.963
1c[b]	2 + 3	.162	.658	.761	.792	.165	.717	.797	.827
	4 + 5	.117	.628	.728	.715	.129	.722	.781	.811

Note. SIM = simultaneous presentation; SUCC = successive presentation.
[a] Each value is the mean over eight subjects.
[b] Each value is the mean over four subjects.

In Experiment 1c the d' advantage for SUCC was again .30. In a similar analysis of variance, the effect of condition was significant, $F(1, 3) = 19.5$, $p < .025$. No other effect was significant, and neither was any effect in a corresponding analysis for β.

There does seem to be a very slight effect of divided attention for this task, an effect in d' of somewhat less than .3. Though it is small and (presumably for this reason) somewhat unreliable across subjects, it is almost certainly real. More evidence on this will be added later.

Combined task: Double targets. Double targets offer two potentially independent chances for correct detection. With certain assumptions the expected increment in detection probability is easy to calculate. Let p, q, and r be the observed probabilities of a target response given 0, 1, and 2 targets in the whole display. Now consider a single limb, horizontal or vertical. It may contain one target and one nontarget. In this case let the theoretical probability of the subject's appearing to see at least one target in this limb be x. Alternatively, the limb contains two nontargets. In this case let the theoretical probability of appearing to see at least one target in this limb be y. Assume that the two limbs are examined separately, independently, and with equal accuracy, the subject simply responding "target" if a target is perceived in either one. This gives

$$p = 2y - y^2, \tag{1}$$

$$q = x + y - xy, \tag{2}$$

$$r = 2x - x^2. \tag{3}$$

Separately for each subject, the observed values p and q were used to derive y, x, and hence a prediction for r, r_p. Mean values of p, q, r, and r_p are shown in Table 2. There was no large or consistent difference between r and r_p. One must be careful here because the estimate r_p rests on assumptions (e.g., equal accuracy on the two limbs) that will not be precisely correct. Still, there is no obvious reason to doubt that, when the task is simply to detect at least one target, double targets act independently.

Separated task: Overall performance. The upper half of Table 3 shows mean values of d' and β from the separated task of Experiment 1a, based on pooled data from first and second limbs.

Table 3
Experiment 1a: Single-Limb Performance

		Condition	
Task	Measure	SIM	SUCC
Separated	d'	1.62	2.32
	β	1.22	.99
Combined	d'	2.34	2.40
(estimates)	β	1.86	1.51

Note. Each value is the mean over eight subjects. SIM = simultaneous presentation; SUCC = successive presentation.

Table 4
Experiment 1b: Single-Limb Performance

Task	Measure	Condition: Sessions 2 + 3 SIM	SUCC	Sessions 4 + 5 SIM	SUCC
Separated					
First limb	d'	2.00	2.67	2.26	2.79
	β	2.42	2.90	1.60	1.70
Second limb	d'	2.29	2.97	2.61	3.47
	β	1.81	.87	1.04	1.14
Combined	d'	2.22	2.58	2.53	2.79
(estimates)	β	4.69	3.08	5.67	4.78

Note. Each value is the mean over four subjects. SIM = simultaneous presentation; SUCC = successive presentation.

This task showed a relatively large effect of divided attention. The reduction in d' from SUCC to SIM was .70. Values of d' were examined by analysis of variance, with the single (within-subjects) factor condition (SIM vs. SUCC). This was highly significant $F(1, 7) = 16.4$, $p < .005$. The usual trend for β to be higher in SIM than SUCC was also significant, $F(1, 7) = 6.4$, $p < .05$.

The upper half of Table 4 shows corresponding data from Experiment 1b. Results are presented separately for first and second limbs. The mean decrement in d' from SUCC to SIM was .69, almost exactly the same at each level of practice. Values of d' were examined by analysis of variance, with (within-subjects) the factors condition (SIM vs. SUCC) and session (2 + 3 vs. 4 + 5). The only significant effects were condition, $F(1, 3) = 35.2$, $p < .01$, and session, $F(1, 3) = 22.5$, $p < .025$. Both conditions showed modest improvement with practice. A corresponding analysis of variance for β showed no significant effects.

Thus, the overall effect of divided attention in this task is relatively striking, an effect in d' of approximately .7.

Separated task: Contingent event analysis. Accuracy on each limb was assessed contingent on the concurrent (same trial) event of the other (cf. Moray et al., 1976; Ostry et al., 1976). Thus, accuracy on the first limb was measured separately for trials with (respectively) hits and correct rejections on the second limb, and vice versa. Scores were not obtained for concurrent misses or false alarms, as these events were too rare.

Table 5 shows mean values of d' and β for Experiment 1a, again based on pooled data from first and second limbs. (For this analysis one subject had to be dropped, since even after pooling his hit rate was 1.0 in one cell.) The results were very similar to those of earlier authors (e.g., Moray et al., 1976; Ostry et al., 1976; Sorkin et al., 1973). In SIM, performance was much worse with a concurrent hit than with a correct rejection. Obviously there was no such interaction between limbs in SUCC. The d' data were examined by analysis of variance, with the (within-subjects) factors condition (SIM vs. SUCC) and concurrent event (hit vs. cor-

Table 5
Experiment 1a: Contingent Event Analysis for the Separated Task

Concurrent event	Measure	Condition: SIM	SUCC
Hit	d'	1.23	2.14
	β	2.58	1.74
Correct			
rejection	d'	1.82	2.08
	β	1.08	.88

Note. Each value is the mean over seven subjects. SIM = simultaneous presentation; SUCC = successive presentation.

Table 6
Experiment 1b: Contingent Event Analysis for the Separated Task

Concurrent event	Measure	Sessions 2 + 3 SIM	Sessions 2 + 3 SUCC	Sessions 4 + 5 SIM	Sessions 4 + 5 SUCC
Hit	d'	1.62	2.68	1.94	3.00
	β	1.60	1.86	2.20	1.07
Correct	d'	2.42	2.85	2.73	2.98
rejection	β	2.51	2.07	1.91	1.59

Note. Each value is the mean over four subjects. SIM = simultaneous presentation; SUCC = successive presentation.

rect rejection). The only significant effects were condition, $F(1, 6) = 14.6$, $p < .01$, and the Condition × Concurrent Event interaction, $F(1, 6) = 14.9$, $p < .01$. By planned comparison, performance in SIM was significantly worse with a concurrent hit than with a correct rejection, $F(1, 6) = 20.0$, $p < .005$; and with a concurrent hit, performance was significantly worse in SIM than in SUCC, $F(1, 6) = 28.4$, $p < .005$. With a concurrent correct rejection, performance was not significantly different in SIM and SUCC, $F(1, 6) = 2.2$, but SUCC did show a small advantage (a difference in d' of .26). A similar analysis for β showed no significant effects.

Table 6 shows corresponding data from Experiment 1b. Again, data have been pooled from first and second limbs. The pattern of results in d' was repeated and remained the same at each level of practice. Values of d' were examined by analysis of variance, with the (within-subjects) factors condition (SIM vs. SUCC), session (2 + 3 vs. 4 + 5), and concurrent event (hit vs. correct rejection). The only significant effects were condition, $F(1, 3) = 17.6$, $p < .025$; concurrent event, $F(1, 3) = 50.5$, $p < .01$; and their interaction, $F(1, 3) = 15.3$, $p < .05$. By planned comparison, performance in SIM was significantly worse with a concurrent hit than with a correct rejection, $F(1, 3) = 28.6$, $p < .025$. SUCC was significantly better than SIM with a concurrent hit, $F(1, 3) = 18.7$, $p < .025$; and almost with a concurrent correct rejection, $F(1, 3) = 9.3$, $p < .1$. This last difference, though small, held for all four subjects in Sessions 2 + 3 and held for three out of four in Sessions 4 + 5. Its slight decline with practice did not approach significance, $F(1, 3) = 3.5$. A corresponding analysis for β showed no significant effects.

Taking the two experiments together, the decline in d' from SUCC to SIM is around 1.0 with concurrent hit and around .3 with a concurrent correct rejection. This latter effect, rather similar to that seen in the combined task, though small is almost certainly real. Evidence on this will be added later.

Comparison of the two tasks. As they stand, performance measures from combined and separated tasks are not directly comparable. One reflects a decision about the entire array, the other reflects separate decisions about each limb. With the assumptions outlined before, however, combined task performance can be used to derive a measure of accuracy on each single limb. By Equations 1 and 2, the observed values p and q were used, separately for each subject, to estimate single limb hit and false-alarm rates x and y, and hence single limb values of d' and β. (The estimate of x was derived from Equation 2 rather than 3 because the observed value q was based on many more observations than r. Obviously the choice made very little difference anyway.)

Mean estimated values from the combined task of Experiment 1a are shown in the lower half of Table 3. Again, any advantage for SUCC was minimal. To allow a comparison with overall performance in the separated task, the d' data of Table 3 were examined by analysis of variance, with the (between-subjects) factor task (combined vs. separated) and the (within-subjects) factor condition (SIM vs. SUCC). The only significant effects were condition, $F(1, 14) = 14.1$, $p < .005$, and the Task × Condition interaction, $F(1, 14) = 9.7$, $p < .01$.

Comparable data from Experiment 1b appear in the lower half of Table 4. A similar analysis of variance on d' had the (within-subjects) factors task (combined vs. separated), condition (SIM vs. SUCC), and session (2 + 3 v 4 + 5), means over first and second limbs being used for the separated task. The only significant effects were condition, $F(1, 3) = 14.1$, $p < .05$; session, $F(1, 3) = 38.7$, $p < .01$; and the Task × Condition interaction, $F(1, 3) = 14.3$, $p < .05$.

Again, one must be careful with these estimates of single-limb performance in the combined task, based as they are on assumptions that undoubtedly are not precisely correct. Still, it tentatively seems that even with approximately comparable performance measures, effects of divided attention are substantially larger in the separated than in the combined task.

Discussion

Summary. The results were more or less as expected. In the combined task there was a slight but fairly reliable effect of divided attention, a decline in d' from SUCC to SIM of approximately .3. A further point was that in this task, double targets (one in each limb) acted approximately independently. In the separated task the effect of divided attention was much larger, a decline in d' from SUCC to SIM of approximately .7. In SIM, performance on the two limbs was far from independent, each limb being much worse with (on the other) a concurrent hit than with a correct rejection. The decline in d' from SUCC to SIM was approximately 1.0 with a concurrent hit, .3 with a concurrent correct rejection. With roughly comparable performance measures, effects of divided attention were significantly greater in the separated than in the combined task.

Replications. I have replicated this whole pattern of results several times, with various search tasks and an experimental design somewhat like that of Experiment 1b. Among the tasks studied have been the following:

1. Search for the target digit 5, with nontarget letters S and E. This task was special in that with the character set used, a 5 could be exactly made by combining elements of S and E. Though important in other contexts (Treisman, 1979), this seemed to have no special effect here.

2. Search for the target digit 5, with nontarget letters S and G. Here components of the nontargets could not be combined to form the target so that targets could be found by searching for a single stimulus component (straight lines and angle at the top). This, too, left the results unchanged.

3. Search for the target digit 5, with all nontarget letters S. As in experiments by Shiffrin (1975), all nontargets were identical. Again, the results were as before.

4. Same as for 2, but with the details of SUCC presentation changed. In each half of the trial, while the characters of the attended limb were presented, the unattended limb contained not masks but irrelevant (nontarget) characters, which the subject knew to ignore. This control for possible peripheral masking effects (Estes, 1972) left the results much the same.

In every one of these cases, and in various other small pilot studies, results closely matched those of Experiments 1a, b, and c.

Fit with the literature. Obviously, these results fit well with those reviewed at the start of the article. Only one point deserves further mention. In my experiments I have consistently observed small SUCC advantages, both in the combined task and in the separated task with a concurrent correct rejection. These are always small and often nonsignificant in a particular experiment, but no experiment has ever showed a reversal. Undoubtedly, both effects are real.

Other authors have sometimes reported absolutely no effect of divided attention when subjects search for a single target (e.g., Shiffrin, 1975). Similarly, when stimuli are to be examined independently, one may show absolutely no effect of divided attention with (on the other) a concurrent correct rejection (Ostry et al., 1976; Pohlmann & Sorkin, 1976). Although these results contrast with mine, the contrast need not be particularly surprising. That such small effects of divided attention sometimes do not show up in particular experiments is to be expected. They may be obscured by noise, or possibly offset by other small effects specific to particular experimental contexts. For example, much of Shiffrin's work on target search (e.g., Shiffrin & Gardner, 1972; Shiffrin, Gardner, & Allmeyer ,1973) has used rather a special task. On every trial there is a target: The task is to report its position and/or identity (there typically being two alternative targets, e.g., T and F). Consider the task faced by the subject here. Suppose, for example, that no stimulus looks particularly like a target. Still the most targetlike should be chosen. Suppose alternatively that two stimuli seem to be targets. Again, only one can be reported. The requirement here is not to examine the various stimuli independently. It is more properly to compare them and choose the most targetlike. Some such possibility holds for most if not all of the Shiffrin work. If comparison is important, an obvious disadvantage for SUCC could arise. There must be comparison across observation intervals. Perhaps this SUCC disadvantage can offset a slight SUCC advantage of the sort seen here, sometimes even producing performance slightly inferior to SIM (Shiffrin & Gardner, 1972; Shiffrin, Gardner, & Allmeyer, 1973).

In any case, the important point is clear. The SUCC advantages typically observed here, both in the combined task and in the separated task with a concurrent correct rejection, though real, are substantially smaller than those observed overall in the separated task, and in particular with a concurrent hit. This result is in complete accord with the rest of the experimental literature.

Practice. The possibility must always remain that the results described here are specific to a particular level of practice, and that with further practice they would change. In Experiments 1b and 1c, there were clear effects of practice in the sense that even by the end of these experiments performance was still improving. On the other hand, there was never a strong suggestion of an interaction between practice and other experimental variables. If the *pattern* of results (as opposed to the absolute level of performance) was changing, then the change was so slow as to be undetectable.

In Experiment 1b, each subject served for 10 sessions, for a total of 8,640 trials. At the end of this time, the pattern of results was still similar to that seen at the beginning. The two subjects serving in the combined task still showed a small advantage for SUCC over SIM (Sessions 9 + 10; mean difference in $d' = .19$). The two serving in the separated task still showed a much larger SUCC advantage (mean difference = .76), particularly with a concurrent hit (mean difference = 1.30), less so with a correct rejection (mean difference = .33). Typically, changes with practice, if they occur at all, are fastest at the beginning. Since in the present case they could not be detected over the first 10 sessions, they must, if present, have been very slow indeed.

As some sort of comparison, in the experiments of Schneider and Shiffrin (1977), subjects searched for digit targets among letters (or letters targets among digits) for approximately 7 sessions in Experiment 1, 2 sessions in Experiment 2, and 10 sessions in Experiment 3, with the same subjects serving throughout. The total amount of practice was only perhaps twice that given to subjects in the present Experiment 1b, and the authors clearly regarded the task as well practiced, even during their first experiment.

Experiment 2

Experiment 1 supports the suggested rule for these target-search tasks. Effects of divided attention are substantial only if simultaneous targets must be independently detected. Such effects are comparatively small if performance can always be based on detection of a single target.

There does remain a doubt. Certainly combined and separated tasks are differentially sensitive to divided attention. But is this because the former allows performance to be based on detection of a single target? There are several other differences between these tasks. The separated task simply requires that more information be transmitted about the display. Spatial information is especially important. The subject must know, not simply whether a target occurred, but if so then where it occurred. Along these lines, several different arguments could be developed to account for the results.

A different search task would be useful here.[1] The subject decides whether an array of four characers contains one target or two. As in the combined task, only one bit of information need be transmitted, and no spatial information. Still, since performance cannot be based on detection of a single target, effects of divided attention should be substantial.

Method

Subjects. The final version of the experiment had six women and two men, recruited as before, ages 21–30. Three original subjects had to be replaced. Two gave hit or false alarm rates of 1.0 or .0 in one of the experimental conditions. The third did not understand the instructions.

Tasks. On each trial, four characters were presented either with SIM or SUCC exposure, just as before. Again, targets were digits and nontargets letters from the same sets as before. The subject was simply to determine the number of targets in the trial. There were two tasks. In 0-versus-1 half of the trials had no target, the others had one. To indicate the number of targets, the subject pressed one of two keys. Performance could not be based on detection of a single target. Task 1-versus-2 was the same, except that half of the trials had one target, the others had two. Performance could not be based on detection of a single target. For half the subjects, the left key was for the smaller number of targets in each task, and for the other half, the reverse was true.

A target was equally likely to appear in any position, determined independently and at random for each trial. If there were two targets, they were equally likely to appear in any two positions. Note in particular that in both SIM and SUCC conditions, double targets could both be in the same limb, or one in each. Thus there was never any obvious reason for the subject to be concerned with the position of a target detected. This gave no clue as to where another potential target might be.

In all other respects, displays were just as before. Again all characters in a single display were different.

Procedure. Each subject served in three hourly sessions. Each session had four blocks of trials, one with each type of exposure (SIM or SUCC) for each task. Half of the subjects had the two blocks of 0-versus-1 first and the two blocks of 1-versus-2 second, half had the reverse. The order of SIM and SUCC exposure types was either ABBA or BAAB, counterbalanced across subjects. The order of blocks for any one subject was the same in all three sessions.

Each block had five sub-blocks, just as before. The subject was cued to begin each sub-block by a set of instructions that appeared on the CRT describing the particular condition. Trial sequences continued just as before, except that on each trial the final display of four masks remained until response. A 1-sec blank interval then preceded the onset of the next trial. Within each sub-block, exactly half of the trials contained each possible number of targets; otherwise, trial sequences were random. Exposure duration (T) was 15 msec.

Analysis. The first session was considered practice and discarded. Data from the 48-trial sub-blocks of the second and third sessions were pooled. In the 0-versus-1 task, a hit was a "1" response made with one target in the display; a false alarm was a "1" response made with no target. In the 1-versus-2 task, a hit was a "2" response made with two targets in the display; a false alarm was a "2" response made with only one target. Hit and false alarm rates were transformed to values of d' and β.

Predictions. On the whole, the expectation was that 0-versus-1 would be comparatively easy, and would show little difference between SIM and SUCC. In this task it was never necessary to detect simultaneous targets independently. Indeed simultaneous targets could not occur.

1-versus-2 was expected generally to be more difficult. Even in SUCC, one third (the chance proportion) of the double targets occurred simultaneously (in the same limb) and should then have been hard to detect. But much worse performance was expected in SIM, where double targets were always simultaneous.

In the SUCC condition of 1-versus-2, it would have been interesting to compare detection of double targets in the same limb (simultaneous) and in different limbs. Unfortunately this was impossible, since separate false alarm rates were not available for the two cases. (Subjects purposely were not asked to indicate *where* they thought double targets had appeared.)

To put these expectations in context, the appendix considers the predictions of a model in which no special difficulty attaches to the detection of simultaneous targets. Its assumptions are that the stimulus array is examined with equal accuracy in 0-versus-1 and 1-versus-2 tasks, but slightly more accurately in SUCC than in SIM. The results of Experiment 2 are not predicted.

Results

Mean values of d' and β are shown in Table 7. Results were just as predicted. Task

[1] I am extremely grateful for a discussion with Anne Treisman, which directly suggested this experiment.

Table 7
Performance in Experiment 2

Task	Measure	Condition SIM	SUCC
0-versus-1	d'	1.88	2.18
	β	2.00	2.09
1-versus-2	d'	1.07	1.74
	β	1.19	1.44

Note. Each value is the mean over eight subjects. SIM = simultaneous presentation; SUCC = successive presentation.

0-versus-1 was easier than 1-versus-2 even in the SUCC condition, and was substantially less sensitive to divided attention. Differences in d' between SUCC and SIM were .30 in 0-versus-1 (comparable to the previous combined tasks), and .67 in 1-versus-2.

Values of d' were examined by analysis of variance, with the (within-subjects) factors task (0-vs.-1 vs. 1-vs.-2) and condition (SIM vs. SUCC). There were significant effects of task, $F(1, 7) = 25.7$, $p < .005$; condition, $F(1, 7) = 39.7$, $p < .001$; and their interaction, $F(1, 7) = 10.0$, $p < .025$. By planned comparison, 0-versus-1 was significantly better that 1-versus-2 both in SUCC, $F(1, 7) = 7.2$, $p < .05$, and in SIM, $F(1, 7) = 64.0$, $p < .001$. SIM was significantly worse than SUCC both in 0-versus-1, $F(1, 7) = 10.5$, $p < .025$, and in 1-versus-2, $F(1, 7) = 43.7$, $p < .001$.

A corresponding analysis for β showed no significant effects.

Discussion

Experiments 1 and 2 both support the suggested rule for performance in these search tasks. Special difficulties arise when simultaneous targets must be separately detected. Otherwise, effects of divided attention are real but rather small. The next issue to consider is the relevance of these results to the distinction between early and late selection.

A Late Selection Theory

A simple suggestion has been made by several recent authors who in common have been struck by the ability to find a digit target among letter nontargets (or vice versa) rather uninfluenced by the size of the array. Perhaps a target "draws" attention directly to itself (e.g., Hoffman, 1978; Schneider & Shiffrin, 1977; Taylor, 1978). Attention is never assigned to nontargets.

It is hard to detect simultaneous targets; yet the number of simultaneous nontargets is rather unimportant. The obvious inference indeed is that on the whole, ***nontargets do not compete for limited capacity processes.***

The Theory

Outline

The theory distinguishes two levels of perceptual representation. The work of stimulus identification and classification is performed at the first level, but outputs must pass through a limited-capacity system to a second level before forming a reportable perception, or in other words before reaching awareness.

First Level

At the first level, simultaneous stimuli are examined in parallel without effects of divided attention.

At this level the perceptual scene is segregated into figure and ground, and "parsed" into discrete stimuli or objects. Each stimulus is fully identified in form, color, size, position, and so on. Already much well-learned information is derived from memory. A visual stimulus is named (if the name is well-known). Aspects of meaning are also derived, classification as letter or digit, meaning of a word (to some level of detail), and so on.

The single most important thing to bear in mind about this level is that though all this information is derived (so that the further action of the system can depend on it), ***none can yet serve as the basis for response.*** This applies for all information: color, position, form, meaning, and so on. No reportable perception of any sort has yet been formed. No information has reached the subject's awareness.

Information at the first level is susceptible to visual masking and decay. The information must be passed on to be preserved.

Selection Schedule

Since a limited-capacity system passes stimuli on from the first level to the second, it is not possible to pass all the stimuli from a single brief exposure. Thus some selection schedule is needed to define which stimuli should be taken. Potentially any information derived at the first level could serve as the basis for selection. In target search the usual attempt would be to take only targets. This should be possible if targets are digits and nontargets are letters. This classification is achieved at the first level. For this reason, only targets will usually pass into and compete for the limited-capacity system.

Limited-Capacity System

For present purposes, the detailed function of the limited-capacity system may be left open. It may be seen simply as a process allowing a percept formed at the first perceptual level to emerge at the second. A stimulus identified at the first level and chosen on the selection schedule currently in force is passed into this limited-capacity system for transfer to the second level.

The limited-capacity system cannot deal efficiently with simultaneous stimuli and is the important source of all divided attention effects considered here. Two simultaneous targets, if they are to be separately detected, must compete for this system. Either they are passed in turn or together but with reduced accuracy. In either case, if viewing time is limited (and especially with masking), performance must suffer.

Second Level

Emergence of a stimulus at the second level creates a reportable perception. Thus, arrival of a target at this point corresponds to its "leaping" from the array to awareness. At the second level, a stimulus can be stored (effectively indefinitely in these experiments), rehearsed, acted on by "conscious" operations, and so on. For present purposes it can be reported and is protected from masking.

A stimulus passed to the second level is, of course, a percept based on first-level processing. There is no way, for example, for the second level to know whether information received from the first level is accurate or inaccurate. The only contact between the second level and the outside world is via the first level.

Presumably, the second level has its capacity limitations, in that it cannot store for report an indefinite number of stimuli. For example, it is well-known that with increasing exposure duration, the number of letters reported from a tachistoscopic array increases steeply to about three or four, but thereafter only more slowly. Along with Sperling (1963, 1967) we might suppose that the initial steep increase reflects passage of items through our limited-capacity system, whereas the leveling off shows we have reached the maximum capacity of a later process, receiving those items that are passed. The important assumption in this article is that all such later processes, grouped together under the heading "Second Level," can for present purposes be ignored. Too few stimuli are to be separately reported for the maximum capacity of second-level processes to be reached. Thus, anything reaching this level can be reported, and all effects of divided attention arise through competition for the limited-capacity system giving access to this level.

The difficulty lies in acquiring information about two targets simultaneously, rather than in later storing this information for the brief period of choosing and executing responses. This can be confirmed by a simple experiment. In the separated task of Experiment 1, SUCC is changed so that no response is allowed until after both limbs have been presented. I have run this experiment: The requirement to store the answer concerning the first limb until after the second has been presented, and then to store both for the brief period of choosing responses, leaves performance essentially unaffected.

Account of the Data

It is important to distinguish two sources of error in the present experiments. First, since viewing conditions are poor, perceptual confusions will occur at the first level. Letters will be misidentified as digits and vice versa.

Since the first level is parallel, however, the probability of such confusions will be equal in SIM and SUCC. For the purpose of predicting differences between these conditions, first-level perceptual confusions can be ignored.

The only reason for SIM to be worse than SUCC is that in SIM, stimuli from the two limbs might compete for the limited-capacity system. All that we need to consider for present purposes is the way in which, in SIM, this competition might distort the information extracted at the first level. Recall that on the whole, *only targets will usually pass into and compete for the limited-capacity system.*

Consider first the combined task of Experiment 1, condition SIM. It might be that no target is detected in either limb at the first level. In this case nothing will pass to the second level, and no detection response will be made. Alternatively, a single target might be detected at the first level, in either limb. In this case, this single target will be selected and passed on, resulting in a detection response. We may assume that if more than one target is detected at the first level (e.g., one in each limb), at least one will be passed on. Now consider SUCC. If at the first level no target is detected in either limb, then there will be no detection response. If at the first level a single target is detected in either limb, then there will be a detection response. The same remains true if more than one target is detected at the first level (e.g., one in each limb). Given this reasoning, performance will be equal in SIM and SUCC.

In general, it can be seen that in either SIM or SUCC, the probability of a detection response is simply the probability that, at the first level, at least one of the total of four stimuli is identified as a target. Note that if the display actually contains two targets, one in each limb, these will contribute independently to the probability that, at the first level, at least one target is identified, and hence contribute independently to the probability of a detection response. This was approximately so in Experiment 1.

Why might performance actually be slightly worse in SIM than in SUCC? The most obvious possibility concerns errors in the selection schedule defining which stimuli from the first level to pass into the limited-capacity system. We have said that usually only stimuli identified at the first level as targets will pass into and compete for the limited-capacity system. But what if occasionally a stimulus, even though identified at the first level as a nontarget, still is mistakenly passed in? Note that we are not talking of a first-level perceptual confusion. The nontarget has been accurately identified as such, and will appear perceived as a nontarget at the second level. The error lay in selecting this correctly identified stimulus for passage into the limited-capacity system. If this occasionally happens, then targets in SIM will sometimes be missed because perceived nontargets from the other limb simultaneously occupy the limited-capacity system.[2] But this loss of relevant information through competition for the limited-capacity system will be slight, to the extent that perceived nontargets can be kept out of this system, so the difference between SIM and SUCC should be small, as observed.

Now consider one limb, A, of the separated task. In SIM, a concurrent hit suggests that a target from the other limb, B, occupied the limited-capacity system. It follows that performance on A will be poor, since at best fragmentary evidence concerning A can reach the second level.[3] Certainly performance will be far worse than in SUCC.

A concurrent correct rejection, on the other hand, suggests that no stimulus from B occupied the limited-capacity system. Again this is not definite, particularly since nontargets from B, even though accurately identified at the first level, still may sometimes be mistakenly passed into the limited-capacity

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[2] The result may be an increase in false alarms as well as a decrease in hits. When a nontarget from one limb emerges at the second level, the subject may realize that a target from the other limb could have been missed, and may reexamine the information (now noisy) concerning that limb. False alarms could well result.

[3] Again the result may be both a decrease in hits and an increase in false alarms.

system. But on the whole, performance on A should be relatively accurate, almost as accurate as in SUCC, to the extent that accurately identified nontargets are kept out of the limited-capacity system.

The analysis of the 0-versus-1 task of Experiment 2 is just as for the combined task of Experiment 1. The difference between SIM and SUCC should be slight. In the 1-versus-2 task, there will be serious competition for the limited-capacity system when in the same exposure two stimuli are identified as targets at the first level. Then the chance of both passing to the second level will be rather low. In SUCC, one third of the double targets occur in the same exposure (same limb), whereas this is true for all double targets in SIM. A substantial difference between SIM and SUCC is expected.

In general, as observed, there should only be strong effects of divided attention when simultaneous targets must be separately detected. Only then should there be serious competition for the limited-capacity system.

A broader point made earlier also follows. Not only in target search but under a wide variety of circumstances, divided attention effects are substantial if simultaneous stimuli must be independently identified, with a separate response for each. Given the present theory such conditions indeed imply that both stimuli must pass through the limited-capacity system. It does not matter what perceptual discrimination the task requires, from one-dimensional psychophysical discrimination to the full analysis of form. If stimuli are to be separately identified, both must reach the second level. Thus, the theory accurately predicts both the results of the present experiments and all the major conclusions drawn earlier from the literature.

Strategies of Selection

Ideally, only targets need enter and compete for the limited-capacity system. Nontargets can potentially be rejected after parallel first-level analysis. What limits must be set on this conclusion? What are the conditions under which nontargets cannot be rejected at the first level?

Errors of Selection

In the separated task of Experiment 1, performance even with a concurrent correct rejection was slightly worse in SIM than in SUCC. This suggested that even nontargets, though correctly identified at the first level, occasionally enter the limited-capacity system, so that information about one limb can occasionally be lost through competition for this system even though the other limb has a correct rejection. In other words, the selection schedule defining that only targets be passed from the first level into the limited-capacity system is not always perfectly implemented.

Under some conditions this effect seems quite substantial. For example, the subjects of Ostry et al. (1976) listened to dichotic word pairs, trying independently to identify targets (animal names) in the two ears. After a good deal of practice, performance with a concurrent correct rejection showed little or no effect of divided attention. Performance was no more accurate if only one ear was to be attended. Early in practice, on the other hand, the effect of divided attention was substantial even with a concurrent correct rejection. Early in practice on rather similar tasks, similar substantial effects have been observed by Johnston and Heinz (1978) and Treisman and Davies (1973). It seems, then, that in these particular tasks, a good deal of practice is needed before nontargets can be kept out of the limited-capacity system with any great reliability. Early in practice there is some success—performance is better with a concurrent correct rejection than with a hit (Ostry et al., 1976)—but far from complete—performance even with a concurrent correct rejection is substantially worse than that possible if only one ear need be attended.

In general, given the present theory, performance with a concurrent correct rejection will show effects of divided attention to the extent that accurately perceived nontargets cannot be successfully kept out of the limited-capacity system. Presumably this will vary between tasks and stages of practice.

Alternative Strategies

Under some circumstances it might be necessary to take this sort of argument somewhat further. All attempts to select only targets for passage into the limited-capacity system might be abandoned.

Stimuli can be selected from visual arrays on the basis of various different characteristics: spatial position (Posner, Nissen, & Ogden, 1978; Sperling, 1960), adjacency to a bar marker (Averbach & Coriell, 1961; Eriksen & Hoffman, 1973), color, size, and so on (von Wright, 1970). It seems likely that after stimuli have been processed at the first level, many different selection schedules can be implemented to define which stimuli should pass on into the limited-capacity system. Passing digits and rejecting letters is only one strategy appropriate to a particular task.

Choice of selection schedules may not be entirely free. Shiffrin and Schneider (1977) gave one excellent example. Subjects searched either for digit targets among letter nontargets or for letter targets among digit nontargets. After extensive practice with one of these two arrangements, each subject was switched to the other. Performance declined catastrophically, with reports that the nontargets (previously targets) could not be ignored. Apparently, here stimulus selection was to some extent "automatic;" that is, items long practiced as targets could not be rejected despite the changed instructions.

Leaving aside such "automatic" effects, however, it still seems that in target search the attempt to select only targets for passage into the limited-capacity system will be only one of several strategies available. As an alternative, stimuli from the array might be fed into the limited-capacity system in a fixed spatial order, taking unselectively both targets and nontargets. Might such an alternative strategy ever have its benefits? Plainly, it might. For example, the attempt to select only targets for passage into the limited-capacity system must take time. This may mean that information enters the limited-capacity system later than it otherwise would, and less may pass before first-level representations are lost through masking. If the criterion for selecting targets is made rather complex, then such selection might be abandoned as too costly. In the extreme it might be impossible. Instead, both targets and nontargets would be taken into the limited-capacity system, selected on some other basis (e.g., in a fixed spatial order).

Under these circumstances, the characterization of target search performance developed in this article would break down. In particular, there would be no reason for performance to be better with a concurrent correct rejection than with a hit, since targets and nontargets would be equally likely to occupy the limited-capacity system. Experiment 3 was planned more or less as a demonstration experiment to illustrate this result under conditions complicating any attempt to select only targets for entry into the limited-capacity system.

Experiment 3

In Experiment 3 subjects searched for targets occurring independently in two halves of a stimulus array, left and right. Simultaneous targets were to be separately detected. In one condition the same target was sought throughout the array. Here the usual result was expected: Performance would be better on either side with a concurrent correct rejection than with a hit. In the other condition, different targets were sought on the two sides of the array. It seemed that in this more complex situation the attempt to pass only targets into the limited-capacity system might run into difficulty. Instead, both targets and nontargets might be passed. There might be little or no difference between performance with a concurrent correct rejection and with a hit.

Method

Subjects. Two groups of 8 subjects each were recruited as before. There were 10 women and 6 men, ages 18–31.

Task. Subjects searched for targets in four-character stimulus arrays. The characters were similar to those used before, but now arranged in a rectangular display exactly centered on fixation. The two characters to one side of fixation were separated edge to edge by a vertical distance of approximately .6° or one character width, with the pair centered 1.75° horizontally from fixation (as before). The central fixation mark and general conditions of viewing are also as before.

Targets were to be detected independently on the two sides. There was one key (left hand) for targets on the left, another (right hand) for those on the right.

There were two types of targets. In one case the target was the digit 8, with nontargets the digits 3, 5, 6, and 9. Alternatively, the target was the letter G, with nontargets the letters C, O, S, and Q. Each subject had two tasks. Subjects in the *pure* group searched for 8s on both sides for one half of the experimental session and Gs on both sides for the other half. Subjects in the *mixed* group searched half the session for 8s on the left and Gs on the right, half the reverse. Nontargets on any given side were always appropriate to the target currently sought on that side. In all cases the targets occurred independently and with probability of .5 on each side. There was never more than one target per side. If a side contained a target, at random it was either the upper or the lower character, independently for the two sides if both had targets. For each position not containing a target, nontargets were chosen at random from the permissible sets, separately on each trial, with the constraint (as before) that no two in a display be the same.

There was no manipulation of divided attention. The four characters were always presented simultaneously. Trials began as before with a central fixation mark, remaining for 750 msec. It was immediately replaced by the stimulus array, masked as before after 25 msec. Masks remained on the screen for 2 sec, during which any response was to be made as accurately as possible, with no attempt at speed. Any targets that the array had in fact contained then reappeared for a further 500 msec, allowing the subject to check the accuracy of response. If the trial had contained no target, this feedback interval was blank. At its end, the next trial immediately began.

Procedure. Each subject served in two hourly sessions, each of two blocks, one block for each of the subject's tasks. Within the pure and mixed groups, the order of tasks was counterbalanced across subjects, but for any given subject the order was the same in each session.

Each block consisted of nine sub-blocks; the first (for warm-up) had 24 trials, the others each had 48 trials. On the cue "go," which appeared on the CRT, the subject pressed a key to begin the sub-block, which then continued at its own speed as previously described. Within a sub-block the four possible events—left nontarget and right nontarget, left nontarget and right target, left target and right nontarget, left target and right target—occurred equally often, but otherwise sequences were random.

Analysis. Data from the 48-trial sub-blocks of both sessions were analyzed. For each subject, separate values of d' and β were obtained for the two targets 8 and G. Though the two sides were independently scored, again data were pooled for the calculation of d' and β. (Note that for the mixed group, this meant pooling from different blocks of each session.)

Results

Results of a contingent event analysis appear in Table 8. They were as expected. The pure group showed better performance with a concurrent correct rejection than with a hit, whereas the mixed group did not. Values of d' were examined by analysis of variance, with the (between-subjects) factor group (pure vs. mixed) and the (within-subjects) factors concurrent event (hit vs. correct rejection), target (8 vs. G), and session (1 vs. 2). Most important, there were significant effects of concurrent event, $F(1, 14) = 11.0$, $p < .01$, and the Group × Con-

Table 8
Performance in Experiment 3

Concurrent event	Measure	Target: Session 1: 8	Session 1: G	Session 2: 8	Session 2: G
Pure group					
Hit	d'	1.38	1.14	1.81	1.48
	β	1.54	1.68	1.50	1.96
Correct rejection	d'	1.82	1.40	2.09	1.55
	β	1.02	1.50	1.31	1.48
Mixed group					
Hit	d'	1.16	.80	1.46	.99
	β	1.32	1.43	1.23	1.37
Correct rejection	d'	1.32	.70	1.56	.95
	β	1.13	1.14	1.43	1.24

Note. Each value is the mean over eight subjects.

current Event interaction, $F(1, 14) = 7.3$, $p < .025$. There were also significant main effects of target, $F(1, 14) = 38.3$, $p < .001$, and sessions, $F(1, 14) = 13.1$, $p < .005$. Performance was better with 8 than with G, and improved with practice. The last significant effect was the Concurrent Event × Target interaction $F(1, 14) = 7.4$, $p < .025$. Though the reason is not clear, the difference between concurrent rejections and hits was larger for the target 8.

Subjects of the pure group seemed to adopt fairly rapidly the strategy of passing targets preferentially into the limited-capacity system. The advantage for a concurrent correct rejection over a hit was well marked even in the first session. Indeed it had decreased by the second session, though the interactions Concurrent Event × Session, $F(1, 14) = 2.2$, and Group × Concurrent Event × Session, $F(1, 14) = 2.2$, were not significant. If anything, the data suggest a decline in the preferential passage of targets with practice. I will return to this later.

A corresponding analysis for β showed no significant effects.

Discussion

These results do suggest two different strategies of selecting stimuli for passage into the limited-capacity system. In the pure group, targets could be chosen from nontargets on a relatively straightforward criterion, with the same target sought throughout the array. In this case, performance was better with a concurrent correct rejection than with a hit, suggesting some attempt to pass only targets into the limited-capacity system. In the mixed group the criterion for selecting targets was more complex, with different targets in the two halves of the array. Here performance was equal with a concurrent correct rejection and with a hit, suggesting that both targets and nontargets equally gained access to the limited-capacity system.

The Visual Search Literature

The results suggest an obvious parallel with the visual search literature. There, too, it seems to be that if the criterion distinguishing targets from nontargets is complex or ill-learned, then it may not be possible to select only targets for entry into the limited-capacity system. Three types of visual search experiments should be distinguished.

Practiced Categorization

The earlier literature review rested mainly on cases in which targets and nontargets fall into distinct, well-learned categories, for example, search for digit targets among letter nontargets. Here, functions of reaction time (RT) against array size typically suggest some approximation to parallel, independent search. Since the distinction between targets and nontargets is simple and well learned, the attempt to select only targets for passage into the limited-capacity system can be made quite successfully. This corresponds to the situation in the present Experiments 1 and 2.

Unpracticed Categorization

Suppose instead that the distinction between targets and nontargets is not well learned. For example, one random set of letters might arbitrarily be defined as targets, another set as nontargets. In experiments like this, functions of RT against array size do not suggest parallel, independent search. On the contrary, RTs increase steeply with array size (Estes, 1972; Schneider & Shiffrin, 1977).

It seems reasonable to infer that in a task such as this, it is impossible to select only targets for passage into the limited-capacity system. Perhaps the sort of unpracticed categorization required here can be achieved only by the "conscious" operations that become available once a stimulus has reached the second level, so that both targets and nontargets must be passed to this level. Thus parallel, independent search is impossible. This perhaps corresponds to the mixed tasks of Experiment 3.

An Intermediate Case

There is a somewhat intermediate case. The subject knows in advance exactly which target to search for, but nontargets are from the same alphanumeric class. For example, the digit 8 might be sought among other digits, or the letter G among other letters. This exactly corresponds to the pure tasks of Experiment 3.

The results of the pure group were interesting. On the first day there was a firm advantage for trials with a concurrent correct rejection rather than hit, but if anything, this seemed (nonsignificantly) to have declined by the second day. It is as if an initial strategy of selecting only targets for passage into the limited-capacity system was later partially abandoned as too costly or too difficult. Thus, selecting 8s among other digits or Gs among other letters was possible but perhaps more difficult than selecting digits among letters (a strategy maintained through 10 sessions in Experiment 1b).

The visual search literature suggests the same sort of intermediate results. When a single possible target is sought among nontargets from the same alphanumeric class, some authors (Corcoran & Jackson, 1977; Egeth, Atkinson, Gilmore, & Marcus, 1973) have found performance comparable to search for a letter among digits, but others certainly have not (Ingling, 1972; Jonides & Gleitman, 1972). These rather mixed results do fit generally with the idea that tasks such as those given to the pure group in Experiment 3 represent a somewhat intermediate case.

Discussion

The important point emerging from this section is that preferential selection of targets is only one strategy for passing stimuli into the limited-capacity system. Presumably, various other possibilities exist; most obviously selection based in some way on spatial position. Such other strategies will be chosen if the criterion distinguishing targets and nontargets is complex or ill-learned, or if for some other reason the strategy of preferential target selection is relatively inefficient.

There might be various such reasons. For example, in an experiment by Moray et al. (1976), the stimuli were two simultaneous tones, one at 467 Hz and the other at 1510 Hz. Targets were tones slightly louder than the nontargets. With an overall target probability of .1, performance on either tone was markedly better with a concurrent correct rejection than with a hit, but this result all but disappeared with a target probability of .5. I have observed a similar interaction with target probability in a preliminary study on search for digit targets among letter nontargets. Apparently, as target probability is increased, the attempt to pass only targets into the limited-capacity system sometimes loses force.

There is good reason for this. The benefit of preferential target selection—that it reduces competition for the limited-capacity system—directly depends on target probability. Given this strategy, performance on one stimulus suffers through competition for the limited-capacity system only if the other stimulus is a target or is mistaken for one. The more likely this is, the worse overall performance will be. This strategy might well become less attractive with increasing target probability.

Unless there is an attempt to pass only targets into the limited-capacity system, the characterization of target search developed in this article will not apply. Performance will not be better with a concurrent correct rejection than with a hit, and, as in unpracticed visual search, performance will show large effects of divided attention, even though such performance can be based on detection of a single target.

Early and Late Selection

The theory developed here is clearly based on the idea of late perceptual selection. At the first, parallel level of the perceptual system, before limited-capacity processes are reached, there is already a full analysis of stimulus from and much extraction of stimulus meaning. All this information can serve as the basis for selective access to the limited-capacity system. Digit targets can be selected from among letter nontargets (as in the present experiments), animal names from among other words (Ostry et al., 1976), and so on.

How does the theory fit in with others in the literature? On the whole, it is consistent with most other late selection theories (e.g., Allport, 1977; Deutsch & Deutsch, 1963; Hoffman, 1978; Posner, 1978), emphasizing in places rather different issues but incorporating the same basic ideas. It is particularly similar to those theories of target search (e.g., Hoffman, 1978; Shiffrin & Schneider, 1977) that propose that targets "draw" attention or limited-capacity processing to themselves, nontargets being rejected by prior, parallel processes. The real disagreement is with early selection theories, which suppose that only simple physical stimulus characteristics are extracted at the first, parallel level of the perceptual system, so that only these can serve as the basis for selective access to limited-capacity mechanisms.

Various different sorts of evidence bear on the distinction between early and late selection. I shall consider these in turn.

Overt Selection

There is one very direct way to study selective access to the limited-capacity system. The subject is presented with several simultaneous stimuli and asked to select only a subset for report. The crucial thing is the criterion on which selection is to be based. Presumably performance will be much better if this criterion allows selection before the limited-capacity system rather than after it.

Many early auditory experiments were based on this idea. Thus Treisman (1964a, 1964b) played to subjects simultaneous prose messages, one of which was to be selected and (continuously) reported back or "shadowed." The task was relatively easy if selection could be based on a simple physical characteristic of the desired message (e.g., if messages differed in voice or arrived in different ears). It was, on the other hand, extremely difficult in the absence of such a physical distinction, in which case one message was to be followed simply on the basis of continuity of content.

Obviously, an elegant account based on early selection can be developed. Before the limited-capacity system, only simple physical characteristics (voice, spatial location) of the messages are available. If these allow selection of the desired message, then only this need pass into the limited-capacity system; otherwise (if selection depends on full word analysis and derivation of meaning), both messages must pass, with selection awaiting some later point.

Possibly, though, an account based on late selection would be equally plausible. I have considered earlier the possibility that selection criteria can differ in complexity. In Experiment 3, for example, preferential target selection was used by the pure group but not by the mixed. Suppose that in a shadowing experiment words from both messages are fully identified (in voice, location, meaning, etc.) at the first perceptual level. The one to be shadowed should be selected for passage into the limited-capacity system. Presumably, selection will be fairly simple if always based on a fixed stimulus characteristic such as voice or location. If, on the other hand, there is no such physical distinction between messages, then a new judgment has to be made for every word of the passage, whether it (rather than the simultaneous word) best fits the context of the material so far shadowed. At least for much of the message, selection based on this judgment might well be difficult or impossible, just as selection based on a complex categorization seemed difficult or impossible in Experiment 3. For that matter, there will be some parts of the message for which perfect selection could not possibly be achieved, since either of the two words presented could fit the context of the shadowed message. Even on a late selection account, one could not expect a complex selection problem of this sort to be equivalent to selection based on some fixed stimulus property such as voice or location.

Similar experiments have been done in vision, where the task is often termed *partial report*. Items for report are to be selected from a visual array on the basis of spatial position (Averbach & Coriell, 1961; Eriksen & Hoffman, 1973; Sperling, 1960), or color (von Wright, 1970, 1972), or alphanumeric class (Sperling, 1960), and so on. In some of these experiments, the cue indicating which items to report is given after stimulus offset but presumably before the decay of iconic memory; for present purposes this is immaterial.

Suppose that an array has eight characters, four to be reported (targets) and four to be ignored (nontargets), and that on average the subject reports three of the four targets, probability correct = .75. This partial report performance is compared with a whole report condition in which the subject tries to report all eight characters. Suppose that on average four characters can be given in whole report, probability correct = .5. The difference between these two probabilities correct is termed *partial report superiority.*

Just as in shadowing experiments, it has been easy here to find results consistent with early selection. Demonstrated partial report superiorities have typically been large when selection for partial report is based on some simple physical stimulus characteristic such as spatial position or color, but smaller if based on alphanumeric class (selecting digits and rejecting letters, or vice versa). Again, selection based on full analysis of stimulus form seems relatively inefficient.

On the other hand, we should scarcely expect performance to be exactly equal with all selection cues (and indeed spatial position is often more effective than color). Though it has been suggested that selection based on alphanumeric class gives no partial report superiority whatsoever, this certainly is not so (Dick, 1971; Sperling, 1960; von Wright, 1970, 1972). The difference from selection based on spatial position or color seems quantitative rather than qualitative.

There is, furthermore, a general difficulty with this sort of comparison. Suppose the selection cue is color. Selection will become less efficient as the difference in color between selected and rejected items is made finer. von Wright (1970) has demonstrated exactly this sort of continuous change in selection efficiency with changing cue discriminability. Thus any choice of a particular color difference (or cue based on spatial position) for comparison with selection based on alphanumeric class is rather arbitrary. Attempts to compare the efficiency of selection with completely different types of selection cue must always face this logical difficulty (Johnston & Heinz, 1978; Keren, 1976).

On the whole, then, it is not inconsistent to suggest that in all of these partial report experiments, selection precedes the limited-capacity system, but it happens to be more efficient when based on the particular spatial position or color cues commonly used than when based on alphanumeric class.

Some partial report experiments in fact support directly a late selection view. In an experiment by Duncan (Note 1), subjects were shown circular arrays of eight characters, four digits (to be reported) and four letters (to be ignored), in random order. At the shortest exposure durations, one or two items at most could typically be reported, yet there was still a marked partial report superiority. The probability of correctly reporting a given digit was higher in partial than in whole report. Under these exposure conditions, it certainly seems that the main limit to performance lies in feeding items through the limited-capacity system before masking (rather than in later memory or output processes), so a partial report superiority seems direct evidence of effective selection at this level (see also Allport, 1977).

Early selection theories (e.g., Broadbent, 1971; Treisman, 1964b) do have a traditional answer to findings like this. The partial report superiority may be based on criterion effects. When the task is to report only digits, even digits for which the perceptual evidence is not strong may be given (leading in the extreme to guesses). Obviously, such a criterion effect could increase the probability of correct digit reports. It would, however, also increase false alarms, and in the experiment of Duncan (Note 1), there was no evidence for this. Thus an explanation based on criterion effects seemed unlikely.

In general, then, neither shadowing nor partial report experiments provide strong evidence against late selection; if anything, the reverse.

Target Detection While Shadowing

Two important experiments were described by Treisman and Geffen (1967) and Treisman and Riley (1969). In the experiment by Treisman and Geffen, subjects received simultaneous, dichotic prose messages. The message in one ear—the primary message—was to be shadowed. In addition, certain target words (e.g., the word *tap*) occurring in either ear were to be recognized by tapping the table. Since targets were always spoken in the same voice as the rest of the message, their detection could not presumably be based on any simple physical characteristic, but required an analysis of word identity. Notably, the subject was told that shadowing was the more important of the two tasks. The results showed that targets were detected far less frequently in the secondary message than in the primary.

The theory developed in this article predicts this result directly, if it is assumed that the primary message loaded the limited-capacity system too heavily to allow much passage of the secondary. Since the most important task was to shadow the primary message, it may approximately be assumed that all words from this message passed through the limited-capacity system, and so reached awareness. Thus any target in this message should have been detected. Consider, on the other hand, a target in the secondary message. Even if it was identified as a target at the first level, it could not be passed into the limited-capacity system, since the most important thing was to pass the simultaneous word from the primary message, allowing shadowing to continue. As observed, target detection in the secondary message should have been infrequent.

The experiment of Treisman and Riley (1969) was similar, except that when targets were detected, the subject was to stop shadowing. Some targets as before were spoken in the same voice as the rest of the message, and again these were detected much less often in the secondary message than in the primary. Other targets were spoken in a different voice, and for these, performance was at ceiling in both messages.

Consider first the case with targets in the same voice. The finding of worse detection in the secondary message than in the primary is somewhat embarrassing for the present theory. As long as no target occurred in the secondary message, words from the primary message should have been passed into the limited-capacity system, allowing shadowing to continue (and any primary target to be detected). But when a target occurred in the secondary message, it should have been identified as a target at the first level, and since shadowing was to stop (the critical difference from the experiment of Treisman & Geffen, 1967), it should no longer have been necessary to pass any further words from the primary message into the limited-capacity system. Instead, the secondary target should have been passed, allowing a detection response. Ideally, target detection should have been good in both messages.

We can only fall back on the following idea, discussed several times earlier: Sometimes it may be that even though at the first level, the stimuli are fully and accurately identified, still the selection schedule defining which stimuli should pass into the limited-capacity system is imperfectly implemented. In this experiment the selection was made under time pressure, since messages were continuous. For quite a period of time, words were to be taken from the primary message, and then suddenly and unexpectedly a target was to be taken from the secondary. Apparently this often failed, because even though presumably identified at the first level, the secondary target did not pass into the limited-capacity system or reach the subject's awareness. Instead, the simultaneous primary nontarget was passed so that shadowing continued as normal. In the present theory, this is the only way to account for poor detection of secondary targets.

So why did the same not occur when targets differed in voice from the rest of the message? We can only assume that the voice difference used by Treisman and Riley (1969) was so striking that here the process of choosing items for entry into the limited-capacity system was close to perfect. It was indeed possible to pass words from the primary message for awhile, and then reliably switch when a voice change occurred in the secondary. I pointed out in connection with the partial report work that the process of selecting items for passage into the limited-capacity system will not be equally efficient whatever the selection cue. In partial report, commonly used cues based on spatial position or color seem more effective than alphanumeric class, even though alphanumeric class is (I assume) actually known after first-level processing.

Similarly here, the voice difference used by Treisman and Riley seems to have been a more effective cue than word identity, even though we must assume that both were known at the first level.

The embarrassingly ad hoc character of this account scarcely needs emphasis. But one further point should be added. Underwood and Moray (1971) repeated the experiment of Treisman and Riley (1969), but used a voice difference presumably less noticeable (though still corresponding to a difference in sex), since the detection even of targets differing in voice from the rest of the message was not at ceiling. Now even these targets were detected less often in the secondary message than in the primary. Here, then, even targets defined by voice seemed to be detected in principle no differently from targets defined by word identity, in line with the present theory.

Dissociations

Perhaps the most striking evidence for detailed identification even of stimuli never passed through the limited-capacity system is provided by certain types of dissociation between overt report and other identification criteria. In the typical experiment, a subject shadows one auditory message, presented sufficiently rapidly to ensure (it is hoped) that no other information can pass through the limited-capacity system. In confirmation, it is indeed shown that words in a simultaneous, unattended message cannot later be reported. Still other criteria suggest that they have been identified.

The best known are experiments on conditioned galvanic skin responses (GSRs). Subjects are first conditioned (by electric shock) to give a GSR to some target word or word class. Subsequently, such GSRs can be obtained even when targets occur in the unattended message during shadowing (Corteen & Dunn, 1974; Corteen & Wood, 1972; Moray, 1969; von Wright, Anderson, & Stenman, 1975; a failure reported by Wardlaw & Kroll, 1976, has been criticized on methodological grounds by Forster & Govier, 1978). Strong evidence that this depends on a full analysis of word meanings in the unattended message is provided by generalization of the GSR to synonyms of the target (von Wright et al., 1975), or more frequent GSRs arising when the critical word is in rather than out of the (syntactic and semantic) context of the unattended message (Forster & Govier, 1978).

Along the same lines, unattended words can be shown to influence the identification of the attended message. Shadowing is slowed when the simultaneous, unattended word is a synonym of the word to be shadowed (Lewis, 1970; Treisman, Squire, & Green, 1974). An unattended word can influence the interpretation of an ambiguous attended sentence (MacKay, 1973).

These results are strongly reminiscent of those sometimes obtained with very brief tachistoscopic exposures. In an experiment by Allport (1977), for example, two words were presented tachistoscopically. The one at fixation was to be reported; typically subjects were not even aware of the other. Still, performance was facilitated when the two words were associatively related. As also suggested by the perceptual defense literature (e.g., Erdelyi, 1974), the meaning of a word seems sometimes to be available to some part of the system, though neither its identity nor even existence can be reported.

Results of this sort strongly suggest that words never reaching awareness can still be fully identified, a conclusion obviously consistent with late selection theories. Apparently, a word fully identified at the first level of the perceptual system, but never passed through the limited-capacity system,[4] still can be responsible for a GSR, or can in various ways influence the identification of other words. Presumably these effects take place actually at the first level, and thus outside awareness.

There is one potential problem with the shadowing work. Some criterion must estab-

[4] It is interesting that with very brief and perhaps masked tachistoscopic exposures, as in the perceptual defense work, stimulus representations seem to be formed at the first perceptual level, but never passed on to the second (thus never reaching awareness). Perhaps quite a strong first level representation is required before anything is passed on (and preserved from masking).

lish that the supposedly "unattended" word indeed does not pass through the limited-capacity system. Though typically the words cannot be reported at the end of shadowing, the possibility must remain that they are quickly forgotten. Perhaps the best answer to this criticism was provided by Corteen and Dunn (1974). In an experiment like that of Treisman and Riley (1969), the subject was immediately to stop shadowing if a target word (the word to which a GSR had been conditioned) was detected in either message. Though by this criterion unattended targets typically were not detected, still they produced GSRs. This type of dissociation between overt response and other criteria for identification provides strong support for the idea of complete word identification without awareness, exactly as expected on a late selection view.

Summary

On the whole, evidence from the literature is consistent with late selection. It is always important to realize that the process that selects stimuli for passage into the limited-capacity system will not necessarily operate with perfect accuracy, or with equal efficiency whatever the selection cue. Often, on the contrary, it will be relatively slow and inaccurate. With this in mind there is no real reason to suppose selection based on form to be different in kind from selection based on simple stimulus characteristics such as spatial position or color.

An Alternative Theory

A substantial part of this article's argument for late selection has rested on the fact that in target search, performance can be better with a concurrent correct rejection than with a hit. Moray has offered rather a different sort of explanation for this result (e.g., Ostry et al., 1976).

In Moray's theory, information about simultaneous stimuli is available for some fixed interval. During this interval, stimuli must be examined one at a time, but the length of time for each is variable. Specifically, information about one stimulus is accumulated until this stimulus has been identified with a (prespecified) degree of certainty (cf. the familiar statistical decision models of Laming, 1968; Stone, 1960; etc.) when examination moves at once on to the next. A crucial assumption is that the system is biased against targets. The required degree of certainty is much greater for a target than for a nontarget response. Accordingly, rather a large amount of information must be accumulated from one stimulus, A, before the system will accept a decision of target, taking a good proportion of the total observation interval and leaving little time for another stimulus, B. Performance on B will be more accurate with a concurrent correct rejection than with a hit.

One difficulty with this account is that it offers no explanation for the results of Experiment 3. It is not at all clear why the pattern of results should change when different targets are sought in different parts of the array. Though an ad hoc explanation could doubtless be developed, the prediction certainly is not naturally made.

The second difficulty is more serious. The crucial assumption, that the system is biased against targets, implies that observed values of β in these experiments *must* be greater than one. Though in Moray's work this typically is so, since targets are rather improbable, elsewhere the results have been different. Sorkin and his associates have consistently obtained better performance with a concurrent correct rejection than with a hit even when target probability is .5 (e.g., Pohlmann & Sorkin, 1976; Sorkin et al., 1973), under which conditions we should not expect β to be greater than one, as indeed the published data of Sorkin (e.g., Pohlmann & Sorkin, 1976; Sorkin, Pohlmann, & Woods, 1976) confirm, certainly at least for some subjects. In the present work, values of β typically were greater than one (since targets were usually rather improbable), but this was not always true. One subject in Experiment 1b was especially striking: across five experimental sessions she was consistently biased *toward* targets, yet still with equal consistency her performance in SIM was better with a concurrent correct rejection than with a hit (a mean difference in d' of .68, as compared to a mean value of .69 for all four subjects). Data of this sort are inconsistent with the account of Ostry et al. (1976). The finding of better performance with a concurrent correct rejection than with a hit does not depend on bias against targets.

Speculations

As the present theory stands, the operations of the first level are entirely parallel. There is absolutely no effect of divided attention up to the point of full stimulus identification. The justification for this is that only targets seem to make strong demands on any limited-capacity system. Thus, targets must be distinguished from nontargets in parallel, at an earlier point.

Still, there could be some slight limit to capacity even at the first level. Even in the combined task of Experiment 1, and in the separated task with a concurrent correct rejection, there were slight advantages for SUCC over SIM. These I ascribed to the possibility that even accurately identified nontargets may occasionally gain access to the limited-capacity system following the first level, but it is equally possible that even the first level is not entirely parallel. Even here there could be a slight limit to capacity—producing a slight SUCC advantage even in the combined task, and in the separated task with a concurrent correct rejection—in addition to the more substantial limit later on, after the rejection of nontargets. Johnston and Heinz (1978) discuss a theory similar to this. All that the present data really suggest is that the *major* limit on the perception of simultaneous stimuli occurs after full stimulus identification.

A separate question is the extent to which first-level operations are "automatic." Do they always proceed in a fixed, complete fashion, irrespective of the context of the task or the subject's goals or intentions? Posner (1978), in particular, has developed an account in this spirit. The implication is, for example, that whenever a particular stimulus is presented, its identity, name, and perhaps various aspects of meaning are always fully derived, irrespective of their current place in the subject's interest. The first possibility for "strategic control," that is, for current goals to influence the course of stimulus processing, would then arise in the choice of which items to feed into the limited-capacity system. This idea has a satisfying neatness, and it will be interesting to see how far it can be taken.

What is the nature of the limited-capacity system? Is it in some way important for the strategic control previously mentioned to have a system ensuring that only selected stimuli, those of particular current importance, are admitted to the field of later, conscious processing to be fitted into the plan and context of the present task (Posner, 1978)? Or is there some specific perceptual function, such as the formation of a unified percept from the various stimulus properties—visual form, name, meaning—extracted at the first level (Allport, 1977)? Is the limited-capacity system modality specific or common to all modalities? Certainly, divided attention effects can arise with stimuli in different modalities (e.g., Lindsay et al., 1968; Long, 1975), and in target search it may be that performance is worse in Modality A when simultaneously in Modality B there is a hit rather than a correct rejection (Eijkman & Vendrik, 1965; Shiffrin & Grantham, 1974). On the other hand, some experiments do suggest that effects of divided attention are generally less severe between than within modalities (Treisman & Davies, 1973).

What is passed through the limited-capacity system? Is it a complete stimulus representation including, for example, information concerning color, size, and other attributes that might be irrelevant to the particular task? What are the "units" for passage? For example, if a person identifies an object's form, is the color, too, passed through the limited-capacity system and thus available for report at no further cost (Allport, 1971; Lappin, 1967)? Very similar is the active debate in the word perception literature: In what units are words and their component letters perceived and protected from masking (McClelland & Johnston, 1977; Reicher, 1969)?

Reference Note

1. Duncan, J. *Partial reports based on colour and on alphanumeric class: Evidence for late selection.* Manuscript submitted for publication.

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(*Appendix follows on next page*)

Appendix

Suppose that the display is examined with equal accuracy in the two tasks of Experiment 2, slightly more accurately in SUCC than in SIM. What predictions follow from a simple consideration of the different decision processes required in the two tasks?

Consider a single character of the display. Let h be the probability of a single-character hit (this character seen as a target when in fact it is) and f be the probability of a single-character false alarm (this character seen as a target when it is actually a nontarget). Let the four characters of the display be examined separately, independently, and with equal accuracy. (No special difficulty attaches to the perception of simultaneous targets.)

For the 0-versus-1 task, assume that the subject will respond "0" if no targets are seen, "1" otherwise. Let $P(a, b)$ be the probability of response a given b targets in the display. Then,

$$P(0, 0) = (1 - f)^4 \quad \text{(A1)}$$

$$P(0, 1) = (1 - f)^3(1 - h). \quad \text{(A2)}$$

For the 1-versus-2 task, assume that the subject will respond "1" if zero or one targets are perceived, "2" otherwise. Given one target, there are three chances in the display for a single-character false alarm, one for single-character hit. Response "1" will be made if there are zero single-character false alarms, or one single-character false alarm and one single-character miss:

$$P(1, 1) = (1 - f)^3 + 3f(1 - f)^2(1 - h). \quad \text{(A3)}$$

Given two targets, there are two chances for a single-character false alarm and two for a single character hit. Response "1" will be made if there are zero single-character false alarms and less than two single-character hits, or one single-character false alarm and zero single character hits:

$$P(1, 2) = (1 - f)^2(1 - h^2) + 2f(1 - f)(1 - h)^2. \quad \text{(A4)}$$

From these equations an elementary computer simulation was set up. Values of d' and β in the two tasks were obtained from assigned values of h and f. For all reasonable h and f values the picture was the same. Though the 1-versus-2 task was slightly less accurate than 0-versus-1, the difference in d' never exceeded .3 and was typically rather less (compare observed differences of .44 and .81 in SUCC and SIM conditions, respectively). To simulate the change from SUCC to SIM, starting values of h and f were assigned, and decrements in h and/or increments in f then introduced. Depending on the starting values, either task could be slightly more sensitive to such changes, but the difference was always minute. Given that the overall d' change in 0-versus-1 had to be small, the change in 1-versus-2 was typically the same to within .1. Apparently, the results of Experiment 2 cannot be explained in these terms.

This analysis is limited by the assumption that characters are examined separately, independently, and with equal accuracy. On the other hand, the bulk of the present work suggests that these assumptions are fairly accurate for the 0-versus-1 task. In particular, similar assumptions predicted quite well the probability of a hit with two targets in the array, in the combined task of Experiment 1.

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