Notes and Comment

Attentional control

RICHARD M. SHIFFRIN
Indiana University
Bloomington, Indiana 47401

In a recent note, Keren and Skelton (1976) set forth a number of objections they had to a study by Shiffrin, McKay, and Shaffer (1976). These various objections bear only slightly upon the conclusion reached in the study. To show this, let me review briefly the paradigm and results.

A square array of 49 dots, called the target display, was presented briefly with one of the dots possibly missing. Following the display, an array of 48 dots, called the cue display, was presented with the missing dot cueing the test position in the preceding target display. If a dot was missing from the target display, its position was the cued position. The subject gave a present or absent response after each trial.

Three types of blocks were run, varying the degree to which attention had to be divided among the 49 display positions. In C49, all display positions were relevant; in C9, only the innermost 9 display positions were relevant; in C1, only the center position was relevant. In each of these conditions, the tested positions were always in the relevant locations, the subject was told this in advance, and the subject was instructed to give all his attention to the relevant locations. In addition, in the C9 and C49 conditions, he was instructed to attempt to allocate as much attention as possible to positions exterior to the center position.

The results showed performance when the central position was tested to be virtually the same in all three conditions. Furthermore, performance was the same in the C9 and C49 conditions when any of the eight dots surrounding the central position was tested. (A later study using a similar paradigm with letters rather than dots gave similar results.)

The conclusion, stated several times in the paper, is quoted as follows: "Either the subjects could not differentially distribute attention across the three conditions, or the effects of attention distribution are essentially negligible." This conclusion was based on the assumption that for selective attention to be a meaningful concept, its allocation should be controllable by the subject; hence there should have been differential allocation of attention to the central position in the three conditions.

Let us now turn to the objections of Keren and Skelton. The most sensible point they raise is the possibility that in all three conditions the procedure

could have biased the subject to give all his attention to the central position. The bias could have arisen because, in order to collect sufficient data, we tested the central position more often than any single exterior position. Nevertheless, the exterior positions as a group were always tested more often than the (group of) interior positions; display durations were chosen so that performance was better at exterior positions (which should have biased attention toward those locations); and instructions were clearly given to allocate *all* attention to exterior positions. A failure to change attention allocation from the C1 to C49 conditions under these circumstances does not speak well for any assumed ability to do so.

It would have been desirable for us to have included a condition in which *only* the exterior 48 positions were tested. If performance again did not differ from the C49 condition, then the issue would have been settled. It is interesting to note that some preliminary data was collected indicating this to be the case. Pretesting was carried out on pairs of diagonally opposite display positions (to set the durations for different positions). Although not enough data were collected to provide a strong test, and these conditions were run prior to the main study, performance at comparable positions did not vary between the pretesting and C49 conditions. Because of the preliminary results, and because we did utilize a condition that instructed the subject to attend only to the exterior 48 positions, we did not utilize another condition in which only the exterior 48 positions were tested. I admit that the power of the conclusions would have been increased had we utilized such a condition. Nevertheless, the conclusion is still justified. An attention allocation system that does not respond to instructions is highly suspect.

I would like to point out, also, that the Shiffrin et al. (1976) study was the last in a long series, including two previous studies in the visual modality (Shiffrin & Gardner, 1972; Shiffrin, Gardner, & Allmeyer, 1973). These earlier studies all gave results equivalent to those found by Shiffrin et al. (1976), but were not subject to objections similar to the present one of Keren and Skelton (or to any others I am aware of).

The remaining objections of Keren and Skelton are argumentative rather than substantive. They mention a number of studies which they contend show an early processing locus for attentional selectivity. I have argued elsewhere that these studies are, in fact, quite consistent with my view that attentional limitations arise late in processing. Space limitations precluded discussion of these studies in

the Shiffrin et al. (1976) report and also in this comment, but a discussion is given in Shiffrin (1976).

Keren and Skelton also object to the statistical analyses, claiming that the "power" of the tests was not given, and that there was a nonsignificant trend in the direction of a selective effect. This objection is silly—the results for each subject were given and Keren and Skelton could have carried out their own tests if they wished. But this is beside the point. Even were the observed differences significant, the conclusions would be unchanged. The real point is that the results showed that any possible difference between conditions was certainly very small, a conclusion that would be demonstrated by any conceivable statistical analysis.

This point concerning the magnitude of any possible attentional effect is especially important when one considers the enormous magnitude of selective effects that can be demonstrated if one is willing to manipulate the load upon short-term search and decision processes. Such a demonstration along with a great deal of convincing data and a full discussion of these issues may be found in Schneider and Shiffrin (1977) and Shiffrin and Schneider (1977).

Keren and Skelton end their comment by quoting Egeth's (1967) view that the locus of attentional selectivity was not yet known. It is interesting that Egeth himself does not completely share this view: while admitting that much more work is needed, Egeth (Note 1) takes into account the results reviewed in Shiffrin (1975) and states: "It currently seems that while attention may be drawn by external signals, it cannot be directed voluntarily when peripheral orienting responses such as eye movements are not permitted."

What is my own view? I am much less dogmatic in my beliefs than Keren and Skelton contend. While I believe that the present evidence is consistent with a view that attentional selectivity arises during controlled processing after perceptual encoding, I would not be very surprised if it should eventually be shown that a small amount of attentional control over early processing stages can occur. It was in response to the flood of studies showing various types of "attentional" limitations that could have arisen in virtually any of the stages of processing that I and my colleagues began to carry out our series of studies. In these, we showed that careful control of "late" sources of attentional limitations could lead to results showing little or no selectivity. Our hope was that

these studies would cause future researchers to be much more careful in their experimental designs, so that the locus of attentional effects might be pinpointed more precisely, and so that uninteresting sources of selectivity, like eye movements, and non-selective causes of performance changes, like lateral masking, may be eliminated as causal factors.

I think, finally, that the present evidence is absolutely clear-cut in attesting to the following conclusion: The magnitude of any attentional deficits that might arise through attentional control over early stages of processing is far smaller than the magnitude of selective effects that arise due to limitations of controlled processing in short-term store following perceptual encoding. I refer the readers to Schneider and Shiffrin (1977) and Shiffrin and Schneider (1977) for discussion of this conclusion and for a new, dramatic demonstration of its validity.

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(Received for publication November 18, 1976.)