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Twenty years of haunting eidetic imagery: where's the ghost?

Ralph Norman Haber
Department of Psychology, University of Illinois at Chicago Circle, Chicago, Ill. 60680

Abstract: This paper is a theoretical analysis of eidetic imagery, based upon the author's ten-year study of elementary-school-aged children. The presence of eidetic imagery is inferred from reports of persisting visual images of stimuli when they are no longer in view. According to the criteria for differentiating eidetic images from afterimages, eidetic images should occur even when saccadic eye movements are made during exposure to the stimulus; it should be possible to make saccadic eye movements while one is reporting the image without the image also moving; the image should last long, and it should be positive. The criteria for differentiating eidetic images from nonvisual memorial representations include: reports of seeing an image projected onto a surface in space, the consistent use of present tense when reporting images as opposed to past tense when reporting from nonvisual memory, and the ability to superimpose two images and report the composite image.

Eidetic images are only available to a small percentage of children 6–12 years old, and are virtually nonexistent in adults. However, extensive research has failed to demonstrate consistent correlates between the presence of eidetic imagery and any cognitive, intellectual, neurological, or emotional measure. The negative correlation between eidetic imagery and age has prompted hypotheses to explain eidetic imagery as a developmentally less mature memorial representation, which is gradually replaced by more abstract representations as the child acquires abstract thought, reading, and more advanced cognitive abilities. The evidence in the present review casts doubt on this hypothesis on numerous grounds: an extensive longitudinal study over the entire span of elementary school years found that eidetic abilities remain remarkably stable; there is no correlation between eidetic imagery and abstract thinking or reading performance; there is no higher incidence in preschool ages, among retarded or brain-injured subjects, or among illiterate subjects in crosscultural studies.

It is concluded that we should not abandon work on eidetic imagery or simply force it into a preconceived mold of what memory must be, but rather, expand work on the phenomenological indicators of perception and memory.

Keywords: afterimages; eidetic imagery; imagery; memory; vision

Personal preface

Twenty years ago I tested my first eidetic subject – ten years ago my last. In 1960 and for the preceding 25 years no respectable experimental work was being done on eidetic imagery. In fact, I could locate only ten papers of any kind published between 1935 and 1960 that reported data on eidetic imagery; most of these were case studies of individual and unusual patients. Of course, 1900 to 1935 had seen a vast proliferation of research, mainly in Germany, and most of it empirical, which described the major characteristics of eidetic imagery in children. This interest died out by the mid 1930s for two reasons: the major contributor to the early literature used the research to espouse "scientific" support for Nazi propaganda in the 1930s, casting doubt on the respectability of the concept and upon its researchers; and even without such a stigma, the concept was not proving productive and self-supportive. So it died.

And stayed dead until I resurrected it, for very misguided reasons. In 1959 I heard George Sperling describe his dissertation research (Sperling 1960) in which he rediscovered (see Baxt 1871) evidence for a very brief visual persistence following the termination of visual stimulation. He showed that normal adult perceivers had access to this persistence and could extract information from it for up to several tenths of a second. I raced back to my laboratory and replicated his experiment to my satisfaction. But I also remembered reading in graduate school studies by Gordon Allport (1924, 1928) describing another kind of visual persistence. Except for the order-of-magnitude differences in duration, the eidetic imagery described by Allport looked superficially similar to Sperling's brief visual process. After reading in the voluminous literature of the 1920s and 1930s and completely ignoring its cessation by the mid 1930s, I decided to get the jump on Sperling by studying information extraction without using multichannel tachistoscopes and sophisticated experimental designs.

Of course I now know there are no shortcuts in information processing, especially around George Sperling, and that eidetic imagery bears little resemblance to any other brief visual persistence. That fact was evident to me when I found my first eidetic child in 1960. Even so, in my laboratory we continued to sample over a thousand children and adults and tested a hundred of them extensively. In addition, almost a hundred other publications have appeared in these twenty years using the methods and materials that were developed and tested in this research project.

I quit after ten years of intensive work and wrote down all that I had learned about eidetic imagery in a long monograph (Leask, Haber, and Haber 1969) and in a more general summary (Haber 1969). It took me ten years to disengage because eidetic imagery was and still is a very seductive phenomenon to me. While I do not understand what it is, I am convinced that eidetic children do possess a kind of visual imagery different from that of other children, and from virtually all adults. Our theories of perception have no way to describe, let alone account for, this imagery.

I am still as confused in 1980 as I was in 1970, and far more confident in my confusion than I was in 1960. Eidetic imagery still haunts me personally, and psychology more generally. In this paper I would like to try to point to the ghost. It is an important ghost, and it requires a powerful exorcist.

What is eidetic imagery?

Since methods of measurement play such an important role in this research, I will first describe the typical procedures used in most of

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the research on eidetic imagery. I will then provide some examples taken from verbatim transcripts of subjects who met most of the criteria for eidetic imagery. The following paragraphs are from Haber and Haber (1964), pp. 133–38.

"S was brought into a small room which contained a table with an easel on it. The easel (30 in. wide by 24 in. high, in a neutral grey finish) was tilted away from S slightly, and had a narrow ledge along the bottom on which the pictures were rested. S was seated 20 in. away from the easel, his eyes level with the middle of it. Room illumination was normal, with strong sunlight blocked by curtains when necessary. A tape recorder transcribed both S's and E's voices.

"The sequence of events was the same for each S. He first was shown a 4-in. red square, mounted on a board 10 in. by 12 in., of the same material as the easel. E placed the stimulus on the easel, left it there for 10 sec, and then removed it rapidly. S reported what he still saw on the easel. Three other colored squares (blue, black, and yellow), always in this order, were presented in a similar fashion. After the fourth square was shown, four pictures were presented for 30 sec each, in the same manner.

"The following instructions were given to S at the beginning:

" 'We are going to play a game with colors and with pictures. Here on this easel I am going to show you some colors and some pictures, and then we are going to talk about them. When I put a colored square here (pointing), I want you to stare at the center of it as hard as you can, and try not to move your eyes at all as long as I leave the square there. When I take the square away, I want you to continue to stare as hard as you can where the square was. If you stare hard enough, you will still be able to see something there. It is very much like when you stare hard at a light bulb and then look away – you can still see something out there in front of your eyes. (If any child acted as if he was unfamiliar with this demonstration, he was instructed to try it then with one of the overhead lights in the room.) The important thing is to stare hard at the colored square when I put it on the easel – so as not to take your eyes away or move them around. When I remove the square, do not look at me, or follow the color as I take it away, but keep staring at the place where it was on the easel. As soon as I take the color away, I want you to tell me what you still see there, if you see anything. You do not have to wait until I ask you – you can begin telling me right away. OK, here is the first colored square.'

"E was watching carefully during the exposure to be sure S did not move his eyes. If S reported that he saw nothing at all after the square was removed, he was encouraged by being assured that it was all right to see things after the color was removed. If he still said he saw nothing, he was reminded to stare hard, and not to move his eyes at all, and he was questioned again as to whether he knew what these instructions meant. Then E presented the next square, increasing the duration by 10 sec over the previous exposure.

"If S said he saw something, he was allowed to report spontaneously. When he stopped, he was questioned on whichever of the following items he had not reported: Was the image still visible? What was its color and shape? Did color and shape change, and if so, how? In what direction did the image move? How did it disappear? Did it move when the eyes moved (S was instructed to try to move his eyes to the top of the easel)? After these points had been covered, and the image had faded completely, E gave the initial instructions again, and showed another square. The same procedure was followed for the four squares.

"After the last square was shown and S had finished his response, the instructions for the pictures were given.

" 'Now, I am going to show you some pictures. For these, however, I do not want you to stare in one place, but to move your eyes around so that you can be sure you can see all of the details. When I take the picture away, I want you to continue to look hard at the easel where the picture was, and tell me what you can still see after I take it away. After I take it away, you also can move your eyes all over where it was on the easel. And be sure, while the picture is on the easel that you move your eyes around it to see all of the parts.'

Figure 1. Indian hunting silhouette picture used in both New Haven (Haber and Haber 1964) and Rochester studies (Leask, Haber, and Haber 1969), as well as many others.

"All four pictures were presented for 30 sec each. E watched closely to be sure the pictures were scanned and not fixated. The first picture was of a family scene, black pictures pasted on a grey board to form a silhouette. The second, constructed in the same way, was of an Indian hunting, with a deer, other animals, and some birds (reproduced in Fig. 1 above). The third, in full color, showed an Indian fishing in a canoe with many fish in the water. The fourth, also in color, from *Alice in Wonderland*, depicted Alice standing at the base of a large tree staring up at the Cheshire cat (reproduced in Fig. 2). A number of other similar pictures had been used in pretesting and in extra testing with some of the same Ss.

Figure 2. Alice picture, used in both sets of studies.

"After the first picture was removed, S was told to continue to look at the easel, and to tell E whatever he could still see. S was reminded that he could move his eyes. If S reported seeing something, E asked if he was actually seeing it then or remembering it from when the picture was still on the easel. E asked frequently if he was still seeing it, since Ss often would not report the fading of the image but would continue reporting it from memory. If S stopped his report, E asked if he could see anything else. If S said no, but said he was still seeing an image, E asked if he could describe anything else about that image. E probed for further description and attributes of all objects still visible in the images. S was also asked to move his eyes if he had not done so spontaneously. E noted the relation between direction of gaze and details of report. This process was repeated for all four pictures. The average time for testing varied from 4 to 5 min with a young S having no visual imagery to more than 30 min for an older S with extensive imagery.

"To score the tape recordings, they were encoded onto specially prepared data sheets, which indicated the content of all responses (images and memory). A different coding sheet was set up for each stimulus. The reliability of this condensation of the data was nearly perfect, since the coding sheets had categories for every object and most of their attributes for each stimulus; the coder rarely had to make any scoring decision. All further scoring was done from these data sheets except the durations of responses, which were taken directly from the tape recordings."

More details on procedures and variations are provided in Leask, et al. (1969), and these are reviewed in some detail by Gray and Gummerman (1975).

The following excerpts from transcripts provide some feel for what subjects with extensive imagery say when tested in the above fashion. These are all taken from Leask et al. (1969), pp. 29–32.

"Example 1 is from a 10-year-old boy to the Indian silhouette (Fig. 1).

E: Do you see anything there?
S: I can see the cactus – it's got three limbs and I can see the Indian, he's holding something in his hand, there's a deer beside him on his right-hand side – it looks like it's looking toward me and three birds in upper left-hand corner one in right-hand corner, it's larger and a rabbit jumping off the little hill.
E: Can you tell me about the Indian – can you tell me about his feathers, how many are there?
S: Three or two.
E: Can you tell me about the feet of the deer?
S: They're small.
E: Are they all on the ground?
S: No.
E: Can you tell which ones aren't?
S: One of the front ones isn't.
E: Tell me if it fades.
S: I can still see the birds and the Indian. I can't see the rabbit anymore. (pause) Now it's all gone.

Example 2 is by the same S to the Alice picture (Fig. 2).

E: Do you see something there?
S: I see the tree, grey tree with three limbs, I see the cat with stripes around its tail.
E: Can you count those stripes?
S: Yes, (pause) there's about sixteen.
E: You're counting what? Black, white, or both?
S: Both.
E: Tell me what else you see.
S: And I can see the flowers on the bottom, there's about three stems, but you can see two pairs of flowers – one on the right has green leaves, red flower on bottom with yellow on top . . . and I can see the girl with a green dress – she's got blond hair and a red hair band and there are some leaves in the upper left-hand corner where the tree is.
E: Can you tell me about the roots of the tree?
S: Well, there's two of them going down here (points) and there's one that cuts off on the left-hand side of the picture.
E: What is the cat doing with its paws?
S: Well – one of them he's holding out and the other one is on the tree.
E: What color is the sky?
S: Can't tell.
E: Can't tell at all?
S: No, I can see the yellowish ground, though.
E: Tell me if any of the parts go away or change at all as I'm talking to you. What color is the girl's dress?
S: Green – it has some white on it.
E: How about her legs and feet?
S: (S looks away from easel and then back again)
E: Is the image gone?
S: Yes – except for the tree.
E: Tell me when it goes away.
S: (Pause) It went away.

Example 3 is also by the same S to the Indian and Animals picture (Fig. 3).

E: Can you see it?
S: Yes, I can see the white and blue sky and the ground has two different shades of green in it with some blue on it . . . and I can see two different squirrels, one is gray and the Indian's holding him in his hand and he's eating a nut. The one on the ground – he's red with a white stripe on him. There are three birds in the air – they're green, orange – they've got some red on them.
E: Can you see the birds' mouths?
S: No – I can see the deer and the cloth on the Indian's belt, it has many colors on it, yellow is the biggest color – and I can see his bow he's holding, it's got zigzag red on it.
E: Anything else – any other animals?
S: There's three rabbits – two of them are brown and one of them is white – the one brown and white one are next to each other and there's another brown one in the right-hand corner.

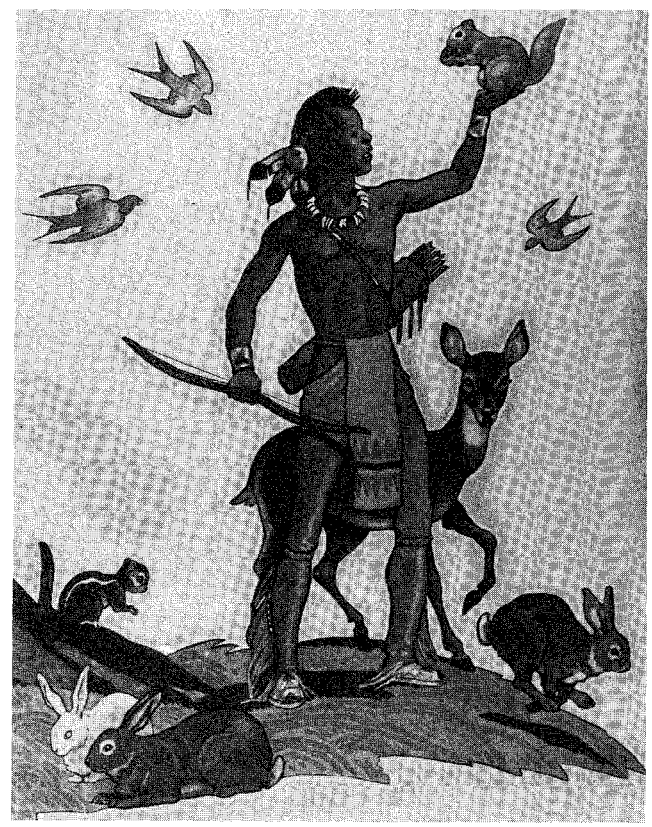

Figure 3. Indian and animals picture, used in both sets of studies.

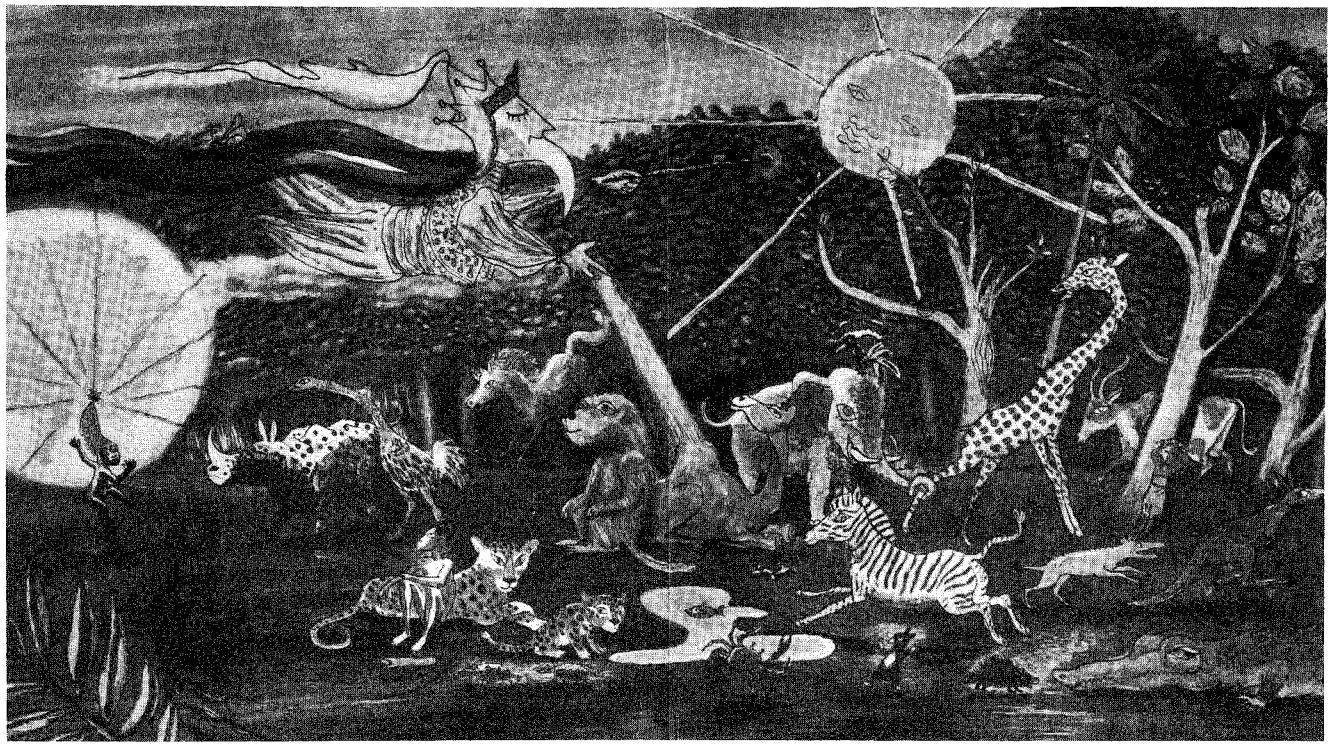

Figure 4. Kipling's animal picture, used in Rochester studies.

E: What are they doing?
S: One over in the right-hand corner is jumping and the other two are just standing around.
E: Tell me more about the Indian.
S: Well –
E: Start at the top and move down.
S: Well, he's got a headband on – he doesn't have a shirt on, he's got a belt on with a cloth hanging out which is red, yellow. He's got Indian moccasins on – I think they're brown.
E: Has he got anything else on?
S: No.
E: Anything else you can tell me – and tell me is any of the parts go away.
S: The rabbits and birds are going away (pause) and the sky (pause) that's it – it's all gone.

Example 4 is by an 11-year-old girl from the Kipling Animal picture (Fig. 4).

E: Do you see anything?
S: Yes.
E: Start at the left and tell me about it.
S: He looks sort of like an elf. He's got a yellow hat and it goes up to a yellow globe – it looks like a sun and the trees behind are sort of bubbly looking – dark green. Ground is dark greenish brown, then there's a momma and a little leopard and there's a native sitting against him. Then there's a pool with a crab on it – coming to it – with a fish in it and I think there are turtles walking in front and a porcupine down near the right-hand corner of the pool. Then back on the right, there's a tree that separates a cow in half – the cow's brown and white, and there's something up in the tree – I can't see the bottom right-hand corner – there's a sun with a lot of rays on it near the top on the right.
E: Can you count the rays?
S: About eight. . . . (pause) There's a lot in that one.
E: Can you see anything else?
S: No, (pause) there's something red in the tree around where the cow is.
E: Any other animals or people?
S: No more people – can't see the right-hand corner. The porcupine has a lot of bristles on it – oh, there's a little something down away from him to the right – it's black and white (pause). That's about all.
E: Can you still see it?
S: Most of it.
E: Tell me if it begins to go away or if you see anything else. (long pause) Still seeing something?
S: Yes, but not the sky above the trees. I can't see what's in front of the native anymore – it's sort of going, there's something in the left-hand corner like a clump of bushes – dark, it's fading.
E: Tell me what parts fade.
S: The right is disappearing – I can still see that cow that's divided by the tree. (pause) Oh! There's a crocodile or alligator in the right-hand corner. You can't see all of him.
E: Can you see the right-hand side better now?
S: No – that's all I see from it.
E: Anything else in the middle?
S: Well, there's the fish in the pool and the pool is sort of odd-shaped, there might be something in back of it.
E: Is there any left now?
S: It's very faint – only the bright yellow of the man's hat – that's about all.
E: Tell me when it goes away.
S: (pause) It's gone.

Example 5 is by the same girl from the Feast picture (Fig. 5).

E: Tell me what you see.
S: Up above it looks like stairs coming down and then there's a bench and a boy, then a girl and a couple of boys sitting on it, and then there's a very long table and on the table it looks like more plates without anything on them than food. There's people of all kinds sitting around the table and then it looks like a lady serving behind the table and then by the doorway it looks like children just gushing in and there's a clock by that – up in the left-hand corner there's a china cabinet and a big hefty woman is putting dishes in there.
E: Which hand is she using?
S: Both of them and there's a coffee pot above the doorway – and

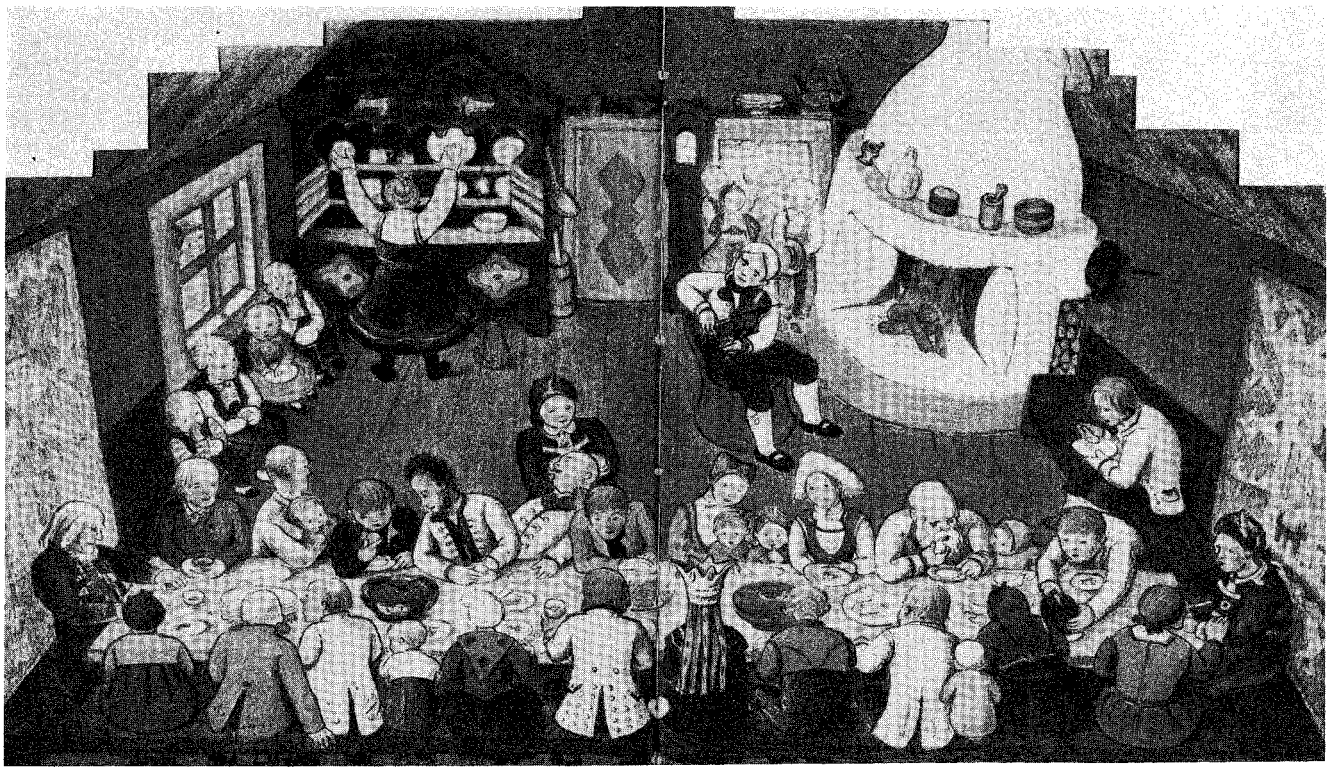

Figure 5. Feast picture, used in Rochester studies.

then there's a stove, it's round and white, there's a fire burning with about three logs and there's a mantel around the top of it with four objects on it. Then over by the corner there's a wood place, a wood pile, and a man's hat hanging on a hook and a bench with one boy eating on it, and a hook without a hat on it and then the table – looks like stairs above that corner too. There are lots of children, more grownups at the table.

E: Can you tell me what any of them have on?

S: The woman serving has on a gray dress with some red on it and then there's one man with a little child, climbing up into his lap – near the left. There are quite a few bald heads. It looks like children coming in the doorway.

E: Any other furniture?

S: No. . . . Between the clock and china cabinet there's something that looks yellow and it's made in a bunch of Vs. The china cabinet is really pretty and I think there are doors that open up, not too many plates on it. There's fish on the table and something yellow like a casserole. The clock is probably a grandfather clock.

E: Would you go around the table and tell me about the people?

S: I can't tell you about any distinctly. I think there's an old man in the left-hand corner . . . at the end of the table. I think there's a woman at the other end – can't tell you anything more about the people.

E: You can still see all these things?

S: Yes, there're a lot of people laughing at the table.

E: Can you tell me about the walls?

S: Where the yellow thing is by the clock, underneath that it's turquoise. By the china cabinet there's a wooden spoon and a broom. In the middle of the picture on the top it squares off, gets flat. It's kinda fading – mostly the table. I can't see it as one whole picture anymore. I can go around and see the different parts. (pause) It's fading – can't see too many colors although I can still see things. There's a window beside the chest on the left and I think there're lacy curtains on it, I'm not sure though. (long pause) That's it."

What criteria distinguish eidetic imagery from everything else?

The distinctive feature of subjects properly classified as eidetic is that they say they can continue to "see" a representation of a stimulus that is no longer physically present. To be interesting, the property of "seeing" must be distinguished both from "remembering" without a visualization of the previously exposed stimulus, and from less interesting (at least in this context) afterimages.

Historically, the only definition of eidetic imagery is phenomenological – a description by the subject of what he is seeing. Thus, in the older literature the defining characteristic is a report by a child that he continues to see the stimulus in front of his eyes after the stimulus is removed from sight. Usually the child reports not only that he can still see it, but that he can describe its contents in detail. It is usually the case that if a child makes one such report about one stimulus, he can do so for most of the stimuli presented to him. Therefore, this purely phenomenological report is bolstered by data on the number of such reports and on the mean duration of the reported image after stimulus removal; the strength of eidetic imagery is specified according to the magnitude of these two measures. While other criteria have occasionally been suggested, the above summarizes the definition of eidetic imagery in the empirical literature up to 1964.

In our first work (Haber and Haber 1964) we proposed seven criteria that would specify the presence of eidetic imagery and differentiate it from memory and afterimages, between concurrent perception and subsequent memory, from confusion and from demand characteristics of the testing. These criteria are: (1) the child reports that he sees an image; (2) that image is located in front of his eyes on the plane of the stimulus he has scanned; (3) the duration of the image is substantial, especially compared to afterimages from comparable stimuli; (4) he uses the present tense to describe his image but shifts reliably to the past tense to describe from memory those parts that have faded or that he never saw in his image at all; (5) he can move his eyes over the stimulus during the inspection of it; (6) he can move his eyes over his image – for example, he looks on the left side of the surface when describing the left side of the image; and (7) accuracy of detail should he high and better than would be

expected from normal memory. Much of our subsequent work provided evaluations of these criteria to determine which ones were critical.

The first three criteria are the phenomenological ones – they depend upon our acceptance of the report from the child that he sees an image in front of his eyes. The other criteria are converging operations (Garner, Hake, and Eriksen 1956) to increase our confidence in the phenomenological descriptions. In developing these criteria, my greatest interest was to isolate eidetic imagery as a visual process. When a child says he sees something on the easel when the easel is blank, how can I be sure that he is relating a visual experience, and not some kind of nonvisual memory which he has chosen to describe with visual metaphors. The critical distinction is visual versus nonvisual, and not visual versus memory. By definition, reports of imagery are memories; they require some maintenance of a representation of information that outlasts stimulation. When an eidetic child says he see the stimulus on the easel, he is telling us that at that moment, his memory representation is visual. The purpose of the study of eidetic imagery is to understand and explain how some memory representations can be visual.

I will first discuss criteria 4 through 7, and comment on an eighth; none of these depends directly upon phenomenological description. Then I will return to the first three, which are direct reports of perceptual experience.

Consistent use of the present tense to describe images and the past tense to describe memory after an image has faded (criterion 4) provides some validation for the distinction between current perception and memory for prior perception. In 1969 we reported experiments testing whether eidetic children are consistent in their use of tenses. Our results were quite strong. Of the 12 eidetic children (as defined by all seven criteria) who were questioned without the use of a tense by the experimenter (e.g., "tell me about the color of the cat") six children were always consistent, three continued to use the present tense for more than half of the stimuli to report memory after the image faded, two used the present tense occasionally (less than 15% of the time), and one child used present and past tense interchangeably for both image and memory. With this last child as the exception, 11 of the 12 subjects always used the present tense when reporting what they said they saw before their eyes, though as noted, half of the children sometimes also used present tense to report their memories. In a control condition testing 12 noneidetic children (who never reported seeing anything after the picture was removed), no present tense descriptions were ever used to describe their memories of the pictures. Therefore, the tense criterion is a powerful one. When a child says he sees an image before his eyes, he always describes it in the present tense.

Criteria 5 and 6 concern eye movements and are designed primarily to differentiate eidetic imagery from afterimages. Once an afterimage is formed, presumably by differential adaptation of receptor or neural units, subsequent eye movements should cause the image to shift with the movement, so that the image itself cannot be scanned. All eidetic children can move their eyes while looking at the stimulus (which should prevent the formation of an adequate afterimage) and can move their eyes to different locations on the easel when reporting the content that had originally been in those locations. Further, it is unlikely that afterimages last as long as most of the images reported by these children, especially in positive as compared to negative forms.

In our experiments there are many subjects who report relatively brief negative afterimages. When such subjects are asked to move their eyes around the easel, without exception they report that the image moves as well. Therefore, the two eye-movement criteria seem to be important differentiators of eidetic images from afterimages. They also have an important bearing on the question of the demand characteristics of the task, since presumably the children would not have had expectations about what kinds of movements they ought to report in order to meet these criteria.

Based on their review of the evidence, Gray and Gummerman (1975) find eye movements less convincing as a criterion than the above comments would suggest. They reported that only Pollen and Trachtenberg (1972) actually measured the eye movements of an eidetic subject during scanning and imagery, and while the movements were sufficient to preclude afterimages, they were much smaller and more variable than would have been predicted from the verbal report of the images. All the other evidence is less precise and usually involves no more than the experimenter watching the subject's eyes during the task.

Haber and Haber (1964) proposed the criterion only to avoid confounding eidetic images with afterimages, and not as a necessary feature of eidetic imagery itself. Even the less precise visual inspection of eye movements is sufficient for that distinction. There is no reason why an eidetic child can not maintain fixation on one part of his image while reporting another part, or shift eye position around while continuing to describe a small area. Both of these take place while stimuli are actually being viewed, so they can in theory also occur while one is looking at images of such stimuli. Hence, the eye-movement scanning criterion should not be used as a defining characteristic of eidetic imagery, buy only to differentiate it from afterimages.

The seventh criterion suggested by Haber and Haber (1964) concerned the accuracy of the reported details of the images. This criterion is often cited in the older literature and reflects the frequent confusion between eidetic imagery and photographic memory (whatever that might be). In any event, Leask et al. (1969) reported a number of experiments and demonstrations to show that accuracy cannot be used as a criterion. Some eidetic children can report highly detailed images while others report quite sketchy and fragmentary ones. Several of the tests involve comparing the accuracy of eidetic children's imagery, evoked by a picture, with the accuracy of control subjects reporting a picture from memory. We never found a significant difference in accuracy in any of these tests. These results are strongly supported by Gray and Gummerman's (1972) data on a single eidetic subject who was strikingly eidetic but whose memorial accuracy was no better than that of control subjects.

The accuracy criterion for eidetic imagery must accordingly be abandoned, despite its appeal. We suggested this ten years ago (Leask et al. 1969), and Gray and Gummerman (1975) strongly concurred. However, the popularity of accuracy as a criterion, has been most resistant to contrary evidence since many generalizations about eidetic imagery are based exclusively on the most impressive subject in the sample (as, for example, in the verbatim transcripts presented earlier). Even so, fidelity of imagery to the stimulus is not a distinguishing characteristic of the reports of the subjects in these experiments, nor is it in any other research on imagery.

Leask et al. (1969) proposed an eighth criterion, also an objective one, designed to isolate the visual characteristics of eidetic imagery from possible memorial, demand, or suggestibility components. We took a meaningful picture and decomposed it into two separate pictures by assigning the different contours of the complete version to one or the other of the decomposed versions. Figure 6 shows the first example we tried. One of the two decomposed versions is presented for 30 seconds and is then removed. Then the other version is presented after a delay of 5 to 30 seconds. If the child can form and maintain a complete image of the first picture and align that image with either the stimulus picture or an (internal) image of the second one, that alignment should permit him to see and describe the original composite, even though the composite had never actually been presented.

We found that only 4 of 23 eidetic children could do the composite image test, though none of a large control group of either children or adults was able to describe the composite from seeing either version separately. It is clearly a more difficult test of eidetic imagery because it depends upon having good long persisting visual images that are complete rather than fragmentary and are sufficiently manageable so that the child can align them properly.

The composite image test has also been used by Gray and Gummerman (1975), with comparable success. Its most dramatic use was reported by Stromeyer and Psotka (1970): the two decomposed versions were the appropriate halves of a Julesz random dot stereogram pair. The one adult subject they tested was able to report the

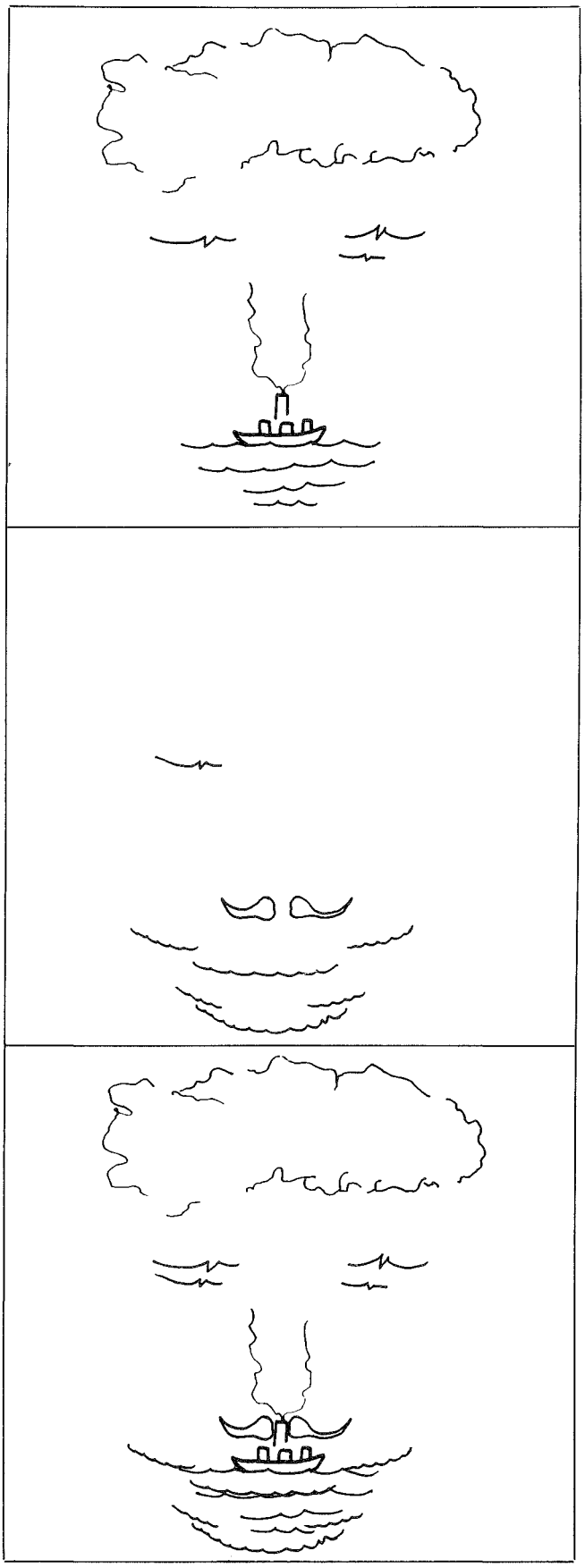

Figure 6. Composite picture test item. One of the two upper pictures is presented for 30 seconds and then removed. After a brief delay, the other upper one is presented and then removed. The lower picture is a superimposed composite, which is what a good eidetic subject describes.

correct object in depth, even when the two pairs were substantially separated in time. Since we have typically been unable to get good images from nonsense or meaningless forms or patterns in eidetic children, their report describes one of the most powerful uses of the composite criterion.

In any event, as a criterion the composite picture test is overly restrictive. It seems unlikely that we could account for the behavior of the children who achieve it, by any other interpretation than a capacity to maintain a detailed visual image. Those who fail it may still be quite eidetic by the other criteria, but they may not have sufficiently complete, moveable, or persisting images to perform this demanding task.

I have briefly described criteria 4–8 (tenses, scanning of stimuli, scanning of images, accuracy of report, and composite images), which depend upon some objective measure of perceptual behavior over and above the subjective report, "I see something." These criteria are relatively powerful and persuasive, but only in the context of the phenomenological report of imagery. Therefore in this sense the first three criteria – the report of seeing an image, the location of the image in front of the eyes, and a substantial duration of the image – are still going to be the most important, and of these the first is obviously the most crucial.

Much of the research reported by Leask et al. (1969) was designed to examine operations converging around the first criterion. The following is a brief summary taken from Leask et al. (p. 46) of the kinds of observations or experimental results that provide converging evidence to support the first of the alternatives – that the child's report is of an image he is presently looking at when he says he can see it in front of his eyes.

1. The duration of exposure determines the probability of an image appearing, even from a fully familiar picture that the child has seen many times.

2. Nearly all eidetic children use blinking as a mechanism to terminate their images. This is supportive not only because of its universality, but also because it is a visual rather than a cognitive control mechanism.

3. An eidetic image seems to be restricted to the eye exposed. While this may be due to erasure through blinking, it suggests that the child must be reporting what he sees, rather than what he remembers or knows, which need not be limited to one eye.

4. Even though the child may be familiar with the picture, he reports he can see an image of only those parts of the picture he has just looked at.

5. Conversely, he can remember parts that he cannot see in his image, suggesting that he has some basis for a distinction between seeing and remembering.

6. A few eidetic children report three-dimensional images of three-dimensional objects.

7. These same eidetic children can see reversals of orientation in their image of a Necker cube. While they are aware that reversals occur during examination of the cube itself, reporting them also in their image must be the result of seeing the cube itself in their image.

8. Nearly all children report their image 'falls off' the edge and disappears when they attempt to move it from its original surface to another one. This is a visual description without doubt.

9. When viewing single letters that slide into place in a window, all eidetic children report that they move their image of each previously seen letter along the surface until they 'fall off' at the edge.

10. All children report their images disappear in parts by fading; and that the nature of the fading process seems to be the same for all children, too.

11. When any child reports from memory, he does so with less confidence, more hesitations and 'searching memory' than an eidetic child does when he reports his image. This is true even though there are no differences in the content of the reports.

12. Images of nonsense verbal material are no different in quality or content from meaningful words. Since memory representations clearly are different, this must be prior to memory.

13. Quality of the image report is clearly related to recency of scanning, and not to variables associated with verbal learning and memory research.

14. Nonsense words, at least, can be reported as easily (if not more so) in the reverse order to viewing the letters. While this needs to be demonstrated for meaningful words as well, there is no reason to doubt it.

15. Images are most likely to be formed when S pays no cognitive attention to the stimulus and makes no attempt to memorize it. Imagery and memory seem opposites, not confounded.

This list is a very mixed bag of evidence, some of which can be relied upon, some merely suggestive. Taken together it lends substantial credence to the conclusion that when an eidetic child says he sees something on the blank easel, his report should be taken at face value in the same way that we take a report of seeing something when the stimulus is still present on the easel.

Given all of the criteria, and the converging evidence in support of the subjective ones, it is not surprising that the few children who satisfy the latter alone are classified as eidetic. In fact, one of the "extra" criteria that we have often used is to take skeptical colleagues along to observe or participate in the testing. This never fails to convert them into accepting the report as a genuine description of visual imagery the child can see before his eyes even though the easel is blank.

Paivio and Cohen (1977) have reported a factor analysis of the correlation matrix among the criterion scores they collected on a sample of 242 second- and third-grade children in London, Ontario. The procedures were modeled closely after those of Haber and Haber (1964). They classified 21 children (8.6% of the sample) as eidetic based upon these criteria and found that two factors accounted for most of the variance among the criterion score correlations. The strongest factor, accounting for 81% of the variance, loaded highly on all of the non-memory items, such as reported imagery, duration of over 40 seconds, projection of the image in front of the eyes, being able to scan both the stimulus and the image, and the use of the present tense. These criteria relate to or describe the phenomenological characteristics of the imagery. The second factor was clearly a memory factor, with loadings on the criteria concerned with accuracy of recall of detail and color. The independence of these two factors in their study further supports our conclusion that the memory-based criteria can be abandoned.

A final note about criteria: Several test-retest reliability analyses have been reported. Each of the children in the original Haber and Haber (1964) study was retested with different experimenters several times over a five-year period. Only one of the 12 children was reclassified as noneidetic, and he was the poorest one originally. A comparable test-retest was carried out in the second sample in Rochester (also reported in Leask et al. 1969) over a six-month period with equivalent results. Cohen (1976) also did a retest after nearly a year's delay (with new experimenters) in which all 20 eidetic subjects plus a comparable number of controls were retested. He found nearly 90% agreement in classification. Both of the last two intervals are too short to be considered longitudinal data, but they do provide evidence for the reliability and short-term stability of the eidetic classification.

The phenomenology of eidetic imagery

Having accepted eidetic imagery as the report of a perceptual process, which can be reliably elicited and scored, what do we know about the content of the images? Some of this information has already been described in the discussion of criteria, but I want to discuss this question in greater detail. These data and observations come mainly from Leask et al. (1969), though some are described by other experimenters as well. Unfortunately, except for a few of the case studies, there is very little about the content of eidetic imagery in published reports. The Leask, Haber, and Haber monograph, therefore represents a rare data base. I will not provide information on the procedures that elicited this information, but will only describe the results and conclusions. Further details can be found in the monograph.

Eidetic imagery is not photographic. I have already argued that the accuracy criterion has to be dropped, because eidetic children are no more accurate in describing details from their images than from their memory (nor are their descriptions more accurate than the memory of noneidetic children). It appears as if the images are constructed or organized in the same way that verbal memory is, so that some visual details are omitted, others moved around, and some added. Thus, the content of imagery is also organized, and not simply an internal template or photograph of the stimulus.

An eidetic image may often contain less information than the child's subsequent verbal description based on memory for the picture that evoked the image. Whether or not an object appears in the image depends on whether that area of the picture is looked at for at least 3 to 5 seconds. Shorter glances usually fail to produce images of a particular part, even though the child can remember those details when asked about them. The only way to get a complete image of a picture is to look at each part of it for enough time. If the picture is quite large, this requires a substantial inspection time. Parts omitted are not imaged, even though they are usually remembered.

On the other hand, the eidetic child's report of his image is made fluently and with confidence, as if he is actually looking at what he is describing. However, when he describes his memory, or when a noneidetic child describes his memory of a previous stimulus, after the first few items are reported, the verbalizations become hesitant, as if the child is searching an imperfect memory. The fluidity and confidence of the report of images as compared to memory could be used as a distinguishing criterion for eidetic imagery, if we had an easy way to score it.

Several techniques are used by eidetic children to prevent images from forming. Some children report they have images only when they concentrate on the stimulus visually, while others say that they always get images of what they look at unless they actively prevent them. The main prevention technique described by nearly all eidetic children is active, cognitive, verbal rehearsal of the stimulus. If, while looking at a picture, the child names each of the items in it, no visual image develops. Thus, it appears as if eidetic children have two modes of processing visual stimuli: a visual mode, which leads to a visual image, and a verbal rehearsal mode, which blocks the image, though it probably aids verbal memory.

The opposition of these two modes probably also accounts for the difficulty eidetic children have in achieving images of print or highly informative or detailed stimuli that they expect to be questioned about. Presented with print, they tend to read it; this, by the very nature of the naming process, prevents an image from forming.

Nearly all eidetic children say they can terminate an image by blinking their eyes, looking away, or shifting their gaze to a new stimulus. This apparently helps them control unwanted images, as well as preventing images from overlapping when eidetic subjects scan the world or turn pages.

Nearly all eidetic children report the same pattern of fading for their images; this fading is due to a combination of purely visual factors (such as loss of clarity, contrast, color, and distinctiveness) and organizational or meaningful factors that cause whole items to fade out together. Since the descriptions of the images are verbal, the latter factors are probably exaggerated, as it is much easier to say, for example, that the tree is fading than that the left side of the bark is now harder to see.

The eidetic children say that they see their images projected onto the surfaces that contained the stimulus. A few, however, do say they can sometimes see an image inside their head. The latter is a much more typical report from some of the eidetic subjects in the African societies studied by Doob (1965, 1966, 1970). While most eidetic children can move their images around on the surface, a small number (about 25%) can shift them to another coplanar surface, and one or two can move them to any surface at will. One of these last children reported that she could also change the size of her image at

will (making it difficult to test Emmert's law with such a subject), and even turn it upside down. Some tests were made to substantiate this last claim, with equivocal results.

If a picture produces an image that is still visible when a second picture is presented, some eidetic children report a composite image of the two pictures. They can describe which parts of the image came from which picture, but they are still seeing a single image of two stimuli. This does not happen to all children, because for some, presenting the new picture terminates the image of the first (presumably because they blink their eyes or glance away).

A few examples of three-dimensional images have been reported, and some children can develop such images with appropriate stimuli easily. A two-dimensional drawing of a Necker cube produced reversals of three-dimensional orientations in images, further supporting the three-dimensionality of eidetic images, at least for some children.

Most eidetic children have no difficulty in getting monocular images to monocular presentations, but transferring an image to the other eye is difficult because of the inevitable opening and closing of eyes, which routinely terminates images. Every child who could get a monocular image in one eye could do so for the other eye as well – contrary to the report by Freides and Hayden (1966), who showed that at least some of their eidetic children were unilaterally monocular.

This is obviously a very abbreviated list, but it should give some sense of the flavor of eidetic imagery, and of some of the eliciting conditions that affect imaging. This list is particularly important because it continues to point out the visual nature of eidetic imagery and its independence from nonvisual memory for the same stimuli.

Accepting the evidence and interpretation that there are some children who can maintain detailed visual images beyond reasonable bounds of physiological time, what do we know about the prevalence and distribution in the population, and what do we know about the causes or at least the correlates of eidetic imagery? Both of these questions can only be answered in sketchy and somewhat unsatisfactory ways.

Incidence of eidetic imagery

The literature before 1935 implies that on the average 50% of all school children have some eidetic abilities, with a range from 0 to 100%. Since no subsequent sampling has approached yielding such high percentages, I assume that the earlier figures include substantial numbers of misclassifications resulting from inclusion of children with good afterimages and children reporting their memory rather than their current images.

Virtually every study carried out since 1964 (no empirical studies are reported between 1935 and 1964) uses the methods described above. With these methods, the frequency of eidetic imagery among elementary-school-aged children ranges from less than 2% to about 15% (see bibliographical section on empirical studies for references). In our own work in New Haven (Haber and Haber 1964) and in Rochester (Leask et al. 1969; Haber 1969) we never exceeded 10% of any sample. Several studies have been done with cross-cultural samples, with subjects presumably selected to have far less exposure to literacy training. These studies report frequencies in the same range or slightly above (see section on cross-cultural studies in bibliography). A few studies have used clinically impaired subjects, especially mentally retarded or brain-damaged children and young adults. The results are less consistent, though most of them find percentages in the same range as with normal subjects. Finally, several studies have sampled adult subjects. Reported frequencies are uniformly near zero except in one study by Giray, Roodin, Altkin, Yoon, and Flagg (1978), who found elevated percentages in a geriatric population, approaching 25% in ninety-year olds.

Testing procedures vary substantially, of course, even when they are closely modeled after those of Haber and Haber (1964). Without providing a detailed methodological analysis it seems most parsimonious to believe that all of these child samples, regardless of composition, are drawn from the same population, with a mean frequency of around five eidetic subjects per one hundred, and that all of the adult samples are drawn from a different population containing less than one eidetic per thousand. The observed variation is probably as much due to inconsistencies in applying all the criteria during testing and in the inability of some subjects to understand what it means to report images. In fact, no single study, including those reported by Haber and Haber (1964) and Leask et al. (1960) applied all the criteria to each child throughout the sample. The tense criterion, quite a powerful one, for example, is rarely applied. This failure produces a bias toward overestimating the frequency of occurrence of eidetic imagery. Hence, I have selected the more conservative population estimates.

Is there a continuum from eidetic to noneidetic imagery?

In both Haber and Haber (1964) and Leask et al. (1969) we provided evidence of a discontinuity between the few children classified as eidetic and most of the rest of the children sampled. When the criteria are used separately to classify children, some of them individually produce discontinuous distributions in which there is no overlap between the classifications. This is true for the eye-scanning measures and the tense switching especially, and to only a slightly lesser degree for the percentage and duration of images. When several criteria are combined together, however, there is no doubt of a discontinuity. The occasional child who has a very long afterimage, for example (and who would be classified as eidetic on the duration criterion alone), fails to meet any of the other criteria, so he is classified as noneidetic with great confidence.

The discontinuity in classification makes it tempting to conclude that eidetic imagery is a kind of memory representation qualitatively different from any other kind, and distinct from normal perception as well. I expressed such a conclusion in 1969, though without great conviction. Nothing has been published in the ensuing ten years to change either the conclusion or the confidence, though Gray and Gummerman (1975) have argued for an apparently contrary position – a difference in degree rather than kind. The continuum implied in their argument is in terms of the vividness of a visualization component in memory, with some people sometimes able to represent memory entirely in visual terms (which we call eidetic), others able to do this only occasionally and only hazily or weakly, and some never having a visual representation of nonconcurrent stimulation. In the following pages most of Gray and Gummerman's arguments are covered (though not always explicitly attributed to them). As will be apparent, I find the visualization capacity of children categorized as eidetic so impressive that I still lean toward a discontinutiy conclusion.

What might the continuum be if eidetic imagery is merely one end of it? While there is a growing literature on the functional role of visual imagery in memory (for example, Paivio 1975; Erdelyi 1976), in discrimination (for example, Shepard 1978) and in general cognitive processes (for example, Kosslyn, Pinker, Smith, and Schwartz 1979, this issue) in none of these studies are the subjects ever asked to describe the vividness or the content of their images directly. Rather, the fact that the subject used visual imagery is inferred from the nature of the stimulus material (e.g., using stimuli independently ranked as being exemplars of easily imaged objects), or the instructions to the subject (e.g., "form a visual image of the stimulus before you answer"), or a postsession questionnaire. Therefore, in spite of the success of these kinds of experiments, they tell us virtually nothing about the phenomenology of visual imagery and even less about the individual differences in such subjective states. While I consider the data of Kosslyn et al. to be overwhelming evidence for the use of visual imagery in their tasks, I cannot learn much from their results about how visual their subjects' images are, how long the images last, how complete the details of their images are, and particularly, whether the subjects "see" the objects being imaged in the same sense that eidetic subjects say they do.

Paivio and Cohen (1977) provide some data relevant to the discontinuity question. In addition to testing for the presence of eidetic imagery in their sample of 242 children, they administered four tests of spatial abilities, one self-report test of vividness of visual imagery, and two tests of verbal ability. When all the intercorrelations were factor analyzed, these investigators still found the same phenomenological eidetic factor described earlier. This factor had no loadings in common with factors reflecting spatial relations, vividness of imagery, and verbal ability. Granting that the reports of eidetic children do reflect something they are seeing, Paivio and Cohen's data imply that whatever that is, it is different from either spatial or verbal abilities, and even from self-report measures of the vividness of imagery in everyday life.

From this kind of evidence it seems difficult to know how one is simply to place eidetic imagery at the extreme end of a continuum of memory representation. However, to say that eidetic imagery is some unique perceptual ability qualitatively different from all other perceptual abilities is equally difficult in the absence of any converging evidence for the uniqueness of the children who possess eidetic imagery. As the following sections will show, such evidence is virtually completely absent.

A major focus of much of the work reported in Leask et al. (1969) was to find the ways in which eidetic children differed from other children who did not possess eidetic imagery. We looked at reasonable correlates, such as intellectual measures, achievement measures, school performance, psychological adjustment and mental health, neurological indexes, visual performance measures, and genetic factors. Even hints – no matter how unreasonable or how ungeneralizable – that might differentiate children were pursued. The results of this quest can be summarized simply: the eidetic children appeared to be a random sample drawn from the school population on all measures except their eideticism. We could find virtually no evidence of any kind that separated these children from the noneidetic ones. We did find that eidetic children were more likely to wear glasses than noneidetic children, but the difference was due primarily to corrections for refractive error. Since refractive errors are not likely to produce eidetic images, I dismissed this finding as a chance effect. A search of the published literature provides the same picture. The findings by Paivio and Cohen summarize it all. Eidetic imagery stands alone.

There is of course one correlate that is universally reported. Eidetic imagery is found almost exclusively in school-age children and is virtually absent in adults. Does this finding help at all to explicate the discontinuity?

Age and eidetic imagery

The literature prior to 1935 reported substantial negative correlations between the prevalence of eidetic imagery and the age of the subjects, with the incidence in high school years a fraction of that in elementary school, and virtually zero among adults. Since most of the recent empirical studies have assumed this relationship to be valid, there is little new evidence on subjects beyond puberty. Gummerman, Gray, and Wilson (1972) tested a large sample of adults and found no eidetics, and Giray et al. (1978) found a percentage near zero among ages 20–60. There have also been a number of cross-cultural studies primarily using adults in less literate societies, but I will consider these separately below, since they represent a special problem of interpretation.

I should add that there are many published reports of individual adult eidetic subjects who are studied and described in great detail. In fact, nearly a third of all publications on eidetic imagery since 1930 are such case studies. However, since the sampling distribution of these case studies cannot be determined, nor the characteristics of the populations from which they are drawn, the mere popularity of such descriptions tells us nothing about the likelihood of there being substantial numbers of adult eidetics. Further, since a careful reading of these case studies shows that many of the subjects do not even meet the basic criteria of eidetic imagery and that many of them show substantial symptoms of hallucinatory or confabulatory activity, I feel that my earlier claim of less than one eidetic per thousand adults is quite generous.

But what about the younger ages? What are we to make of the concentration of eidetic imagery in children of ages six to twelve? Perhaps the oldest and most important hypothesis concerning the origins and functional significance of eidetic imagery is tied to its age correlate: eidetic imagery is typical or common in very young children and disappears as more advanced cognitive and perceptual processes develop, especially those associated with abstract thought and reading. Nearly every investigator has pursued aspects of this hypothesis in one form or another.

For support, the hypothesis has to account for more than just the negative correlation of eidetic imagery with age, since that is what it was based on. There are, however, no confirmatory data for this hypothesis beyond the correlation with age (and even that correlation is problematic, as I shall show below). One test of the hypothesis is the prediction that children with eidetic imagery are delayed or retarded on abstract tasks, since such tasks interfere with, replace, or overlay eidetic imagery. No experiment since 1930 has reported any significant correlation between the presence or absence of eidetic imagery (or even the degree of eidetic imagery) and intellectual ability, skill at abstract reasoning, school achievement, or reading ability. Eidetic subjects are neither better nor worse on any of these skills or abilities than noneidetic controls at any age. The lack of any such differences between eidetic and noneidetic subjects casts strong doubt on the hypothesis.

In addition, there is some positive evidence against it. In the only reported longitudinal study on eidetic imagery (Leask et al. 1969), only one of the original twelve children failed to remain eidetic. Further, we could find no significant differences on any of the criteria from year to year. Since the age of five to fifteen covers the years over which the developmental hypothesis predicts that eidetic imagery should drop out, the lack of changes within the same subjects has to be considered damaging to the hypothesis.

The stability of eidetic imagery over nearly the entire elementary school period is clearly inconsistent with the developmental hypothesis. In fact, in the studies that tested enough children at each age, there is not much difference in the prevalence of eidetic imagery at age 7 as compared to age 10 or 11 (see also Giray, Altkin, Vaughton and Roodin 1976). It is only when older children or adults are tested that it is difficult to find any that are eidetic.

Another failure of the developmental hypothesis is that it should predict especially high eidetic-imagery prevalence at very young ages. Five to ten per hundred is hardly a high percentage. It is of course possible that children younger than six or seven would be more likely to be eidetic, and that most would have lost the ability by the first or second grade. Only Giray, Altkin, Vaught, and Roodin (1976) have reported data on children of five or six. They found higher percentages than at ages 7–18, though not nearly large enough to support this kind of hypothesis. In 1961 we tried to test some five and six year olds and found no eidetic children. However, we felt that the testing procedures were not adapted to such young children, so a few may have been eidetic. On the other hand, we were convinced by the little testing we did do that eidetic imagery is not at all typical in kindergarten and first grade.

Cross-cultural data

A slightly different version of the developmental hypothesis was initially explored by Doob (1964) followed by a number of others (see the bibliographic section on cross-cultural studies). Doob argued that if eidetic imagery was cognitively more concrete, then if subjects were selected who never developed complex abstract skills such as reading, or were less exposed to Western-style educational systems, they should show a higher prevalence of eidetic imagery. Following this reasoning he tested adult subjects in several African societies, contrasting groups that differed in exposure to education or Western culture. In Doob's work, and the six subsequent publications using

African, Brazilian, or Australian subjects contrasted for acculturation and education, there is no consistent support for this version of the development hypothesis. Specifically, while the incidence of eidetic imagery is more variable, ranging from 0 to 20%, there is no consistent pattern of differences between the acculturated and the less acculturated groups. Further, since each of the published articles describes the difficulties in maintaining consistent testing procedures, particularly in assuring that the instructions about what to report mean the same thing to the subjects that they do to the testers, significant results would need to be treated with great care. Since the data are so messy, the cross-cultural data cannot easily be applied to the developmental hypothesis, and certainly not as positive support.

Eidetic imagery and neurological pathology

Siipola and Hayden (1965; see also Giray, Altkin, Vaught, and Roodin 1976; Giray, Altkin, and Barclay 1976, Giray et al. 1978) pursued another version of the developmental hypothesis, arguing that subjects with either mental deficiencies or neurological deficits that interfere with abstract thought processes should still have access to their eidetic abilities because high levels of cognitive abilities did not develop to replace or interfere with the eidetic skills. In their experiment, they tested sixteen brain-injured retardates and found eight of them eidetic, using the Haber and Haber procedures and criteria. This high percentage has never been found again. Subsequent studies by Richardson and Cant (1970), Symmes (1971), Gummerman, Gray, and Wilson (1972) and Giray, Altkin, and Barclay (1976) found, respectively, 2%, 19%, 0%, and 10%, eidetic subjects among brain-injured retardates. Among non-brain-injured retardates, Siipola and Hayden, Symmes, Gummerman, Gray, and Wilson, and Giray, Altkin, and Barclay found, respectively, 6%, 13%, 11%, and 4% to be eidetic. The two exceptions to these low numbers are the original finding by Siipola and Hayden (1965) of 50% among sixteen brain-injured subjects and a more recent finding by Giray, Altkin, and Barclay (1976) of 78% eidetic among fourteen hydrocephalic subjects. It is difficult to know how to interpret a few positive findings among a larger number of negative ones, especially since the procedures seem adequate. But with this variable pattern of results, the evidence does little to sustain the neurological-deficit version of the developmental hypothesis.

To this negative conclusion should be added the highly variable data reported on individual clinical case studies of eidetic imagery. It seems much more likely that such data are contaminated with hallucinatory types of reports. As Gray and Gummerman (1975) note, on the surface, eidetic imagery may resemble some kinds of hallucinations, but it is certainly possible and easy to differentiate the two kinds of phenomena in order to avoid confounding conclusions. The clinical literature rarely does this. As a final note, even given the low incidence, it is still relatively easy to find a few eidetic children in any classroom of a North American elementary school, and these children do not have any obvious signs of neurological pathology. In neither of the samples collected by Haber and Haber (1964) or Leask et al. (1969) were there any obvious signs of pathology in their subjects. Therefore, any link between neurological deficits and eidetic abilities is yet to be demonstrated.

Giray and his colleagues have pursued the neurological version of the developmental hypothesis most vigorously in recent years. Since the correlations between neurological pathology and eidetic imagery are generally so low and inconsistent across categories of pathology, and since there is no evidence for assuming they ought to be related, I find it unlikely that this hypothesis will be supported.

The discontinuity issue revisited

The last few sections have been written as if eidetic imagery were not continuous with other kinds of perceptual behavior or memory. There exists little more evidence than what I have discussed. Gray and Gummerman (1975) covered the same territory but concluded that eidetic imagery was just a more vivid visual variety of memory imagery, and that there was no dichotomy or discontinuity. They also considered the possibility that eidetic imagery is a perceptual anomaly, a deviation from normal perceptual functioning. They rejected this argument for lack of any reliable data, or even a viable theoretical framework with which to describe the nature of the anomaly.

I do not consider eidetic abilities to be anomalous, nor do I consider them to be just a vivid form of memory. The entire thrust of this paper, and of my ten years of research, is to distinguish poststimulus reports that are visual from those that are nonvisual. This is not a memory issue, since any residual information remaining after stimulation terminates must be called memory. In this sense, eidetic imagery is a memorial phenomena, just as is any verbal description of the content of a previously seen stimulus. What distinguishes those memories called eidetic is the report by the perceiver that he can "see" the absent stimulus as if it were still present. Eidetic imagery is not just more vivid, as Gray and Gummerman (1975) suggest: it is visual, something the perceiver is capable of looking at.

There certainly is a continuum of visual imagery, from the very vivid, which characterizes most eidetic responses, to hazy, ill-formed, and unreliable visualization that many people report when asked to form a visual image of something familiar or seen before. Some people are never able to create or maintain a viewable visual image. It is this continuum of visualization that we need to study and understand, instead of arguing about whether eidetic imagery is really just an extreme form of memory.

So, where's the ghost?

I can summarize my main points easily. (1) The production of eidetic images is a relatively easy perceptual behavior to elicit from a small but stable percentage of elementary-school-aged children. (2) The criteria that differentiate eidetic imagery from afterimages and from memory reliably discriminate between children with and without eidetic capacity; further, the criteria are internally consistent, and the categories so specified have very high test-retest reliabilities. (3) The evidence that the criteria specify visual memory as distinct from some nonvisual memorial process is overwhelming. (4) There is no evidence to support any version of a developmental hypothesis, even though the incidence of eidetic subjects is clearly less among adults than among children. Its incidence is not negatively related to age per se, abstract cognitive abilities, education, or acculturation, and it is not positively related to neurological deficits that may reduce abstract cognitive abilities.

In light of conclusions like this, it is not surprising that some researchers (e.g., Morsh and Abbott 1945) have suggested abandoning the concept of eidetic imagery altogether. I feel that this is a silly suggestion, equivalent to arguing that the best way to handle any problem of unknown dimensions is to turn your back on it and maybe it will go away. There are a few children who possess an eidetic ability rather different from any other kind of perceptual behavior. Such children seem to be just like all other children not so endowed, in all respects except this one. For a scientist, this is an unreasonable state – something must make these children different.

Two directions are possible for future work. One is to continue to search for correlates. For example, try and find a new test of neural function or spatial abilities or a new index of cognitive development. If we work hard enough, we should be able to find something that works. The other alternative is to study eidetic images and imaging more carefully, so that we can learn more about the components and content of the images and the conditions and stimuli that produce and terminate them. I consider the latter a far more fruitful approach. It is unfortunate that most of the empirical studies that locate eidetic children by sampling classrooms spend so little time and effort in examining the content of eidetic imagery beyond what is needed simply to satisfy the classification criteria. Some of the clinical case studies do provide such information, but often on

subjects that are otherwise so atypical that the generalizations are very dubious. Further, such case reports rarely include control observations elicited by different conditions or stimuli of the kind reported in detail by Leask et al. (1969).

A few people have absolute pitch for auditory tones. It is a remarkable ability that mystifies the vast majority of us, who do well just to make accurate relative judgments of pitch. But we do not decide, just because the ability is rare or remarkable or uncorrelated with other perceptual skills, that it does not actually occur, or that it is really no different from what everybody else can do except a bit more so. I grant that absolute pitch can be examined by discrimination techniques that do not require any phenomenological description. But I have spent a decade of my professional life trying to show that phenomenological indicators of perception can have the same value in a science of perception as do psychophysical discrimination indicators. What is needed to understand eidetic imagery is more attention to the experimental psychology of phenomenology. Then we can have a chance to discover not only the nature of eidetic imagery, but how it comes about and develops and what other perceptual processes, if any, occur with it.

The ghost of eidetic imagery is the fear of phenomenology. Psychologists in general, and perception researchers in particular, have invested enormous energy in developing methodologies in which we never have to trust what the subject says. Often we have found techniques in which we never ask him questions requiring a more complex or ambiguous answer than yes or no. If more words are required, we arrange converging operations which allow us to replace the content analysis of the words with other indicators. Subjects in such experiments are never asked, or even allowed, to tell us what the stimulus display looked like. Sometimes we can even get all the psychophysical information we want without letting the subject open his mouth. But when we believe that we can discover and understand all the rules of perception by treating the subject as a null indicator then we must fail. The study of visual imagery is perhaps the clearest case of this failure.

To avoid the failure in this context we have to know more about the imagery itself, and that means we have to depend upon what subjects tell us. We should not abandon experimental control – such control becomes even more important for the analysis of reports of perceptual experience than for null indicators (see especially Natsoulas 1965). But we should not try to circumvent these reports or redefine them as simply the same as other more conventional or acceptable indicators until we understand them. And to do that we have to ask more questions about the phenomenological indicators of perception, not less. Until we do so, eidetic imagery will haunt us all.

NOTE

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by **Akhter Ahsen**

Eidetic Analysis Institute, Yonkers, N.Y. 10705

Eidetics: redefinition of the ghost and its clinical application

In an earlier overview article on the eidetic (Ahsen 1977), I had clarified the issues concerning the definition of the eidetic in the classic setting of the criteria for distinctions (Russell 1921, p. 145). The consensus of the classical experimental research on eidetics is that it is an intermediate phenomenon between afterimage and memory in particular, and other imagery levels in general. G. Allport in his definition of the eidetic (1924) had emphasized four important aspects: (1) being of intermediate position; (2) being not necessarily dependent on an external stimulus; (3) "seen" as projected outward and the seeing being accompanied by clear ocular tension, other somatic events, feeling, and meanings; and finally (4) the "healthful" structure of the eidetic, which excludes the possibility that it can be considered pathological in any form of inference whatever. Allport, whose definition of the eidetic differs from Haber's, stated that this "definition should be understood to exclude both pathological hallucinations, and dream images, and to admit those spontaneous images of phantasy which, though possessed of perceptual character, cannot be said to be literally revivals or restorations of any specific previous perception" (Allport 1924, p. 100).

In most current publications, the eidetic, however, continues to be erroneously characterized as "an especially vivid memory image" (Horowitz 1970, p. 22), "an exceptionally vivid memory image that occurs immediately after the perception" (Hebb 1972, p. 242), "the ability possessed by a minority of people to 'see' an image that is an exact copy of the original sensory experience" (Kagan and Havemann 1972, p. 588), the "half-way house to hallucination" (Drever 1964, p. 80). Even Leask, Haber, and Haber (1969) provide too narrow a definition when they describe the eidetic as "a visual image, representing a previously scanned stimulus, persisting for up to several minutes, and phenomenally located in front of the eyes" (p. 25).

R. N. Haber's target article, unfortunately, continues to perpetuate the fallacy arising from an overnarrow definition. In my attempt to diagnose the cause of this trend and bridge the gap, I had tried in the overview article to deal with the fallacy in a practical way, suggesting that in the future we should adopt the use of two separate terms: (1) "typographic eidetic," to represent the eidetic induced through an external stimulus, as in Haber's own and similar studies and (2) "structural eidetic," to represent the study of those eidetic images that are in no way dependent on an external stimulus but are in essence internal evocations, as in Allport's treatment. I had hoped that once we achieved a basic clarity through the use of these two separate terms, the seeming contradictions in the research data would be cleared up.

The unqualified term "eidetic" historically covered only those internal eidetic phenomena that had been noted from the time of Müller under "subjective vision" (1826). Later, as this eidetic became a subject of interest to the experimentalists in the laboratory, two separate methodologies continued to be employed by the researchers, one based on presentation of a current external picture to a subject (typographic method) and the other introspective study of spontaneous eidetic pictures from both the recent and the remote past (structural method). Both bodies of research were ultimately unified in theory and concerned with the study of the dynamics of the eidetic as a subjective phenomenon. The older researchers had hoped that the study of eidetics through these two methods would be helpful in the study of mental dynamics and would help develop an objective system of psychotherapy (Kroh 1922; Jaensch 1930). The psychotherapy field, in general, including behaviorism (Pavlov 1941) and psychoanalysis (Schilder 1926), was aware of the limitation of the prevailing methods, which were restricted to mere conditioning or verbal reenactment of experience rather than making full use of the potential of the experience itself. Eidetics were expected to make available the very fabric of experience and finally generate a revolutionary method of handling mental issues in a much more direct and fruitful manner. Haber's bibliography contains numerous references to early publications on the clinical use of eidetics, involving such topics as synthesis (Allport 1924; Jaensch 1930), fragmentation (Klüver 1930), alpha wave characteristics (Furst, Fuld, and Pancoe 1974; Pollen and Trachtenberg 1972), hallucination (Barber 1959a; Saltzman and Machover 1952), psychosis (Floyd 1956; Kao and Lyman 1944; Miller 1931a), obsessive neurosis (Essler and Rowe 1926), hypnosis (Walker, Garrett, and Wallace 1976), insomnia (Sheikh 1976), hydrocephalic children (Giray, Altkin, and Barclay 1976), brain-injured children (Giray and Barclay 1977; Symmes 1971), retardates (Siipola and Hayden 1965), learning disabilities (Pierro 1966), phobia, (Dolan and Sheikh 1977), identification processes (Dolan 1977), and eidetic psychotherapy (Ahsen 1968, 1977; Panagiotou and Sheikh 1974). Some further references deal with hemispheric experiments on eidetic images of

parents (Ahsen 1959), eidetics and evocation of mental structures (Ahsen 1962), eidetic images, symptoms, and character formation (Ahsen 1964), a short introduction to eidetic psychotherapy (Ahsen 1965), eidetics and self-analytic consciousness (Ahsen 1977a), eidetics in the treatment of accident trauma, stress conditions, and chronic emotional blocking (Ahsen 1978), eidetic psychotherapy (Sheikh 1978), and eidetics in the hospital setting (Twente, Turner, and Haney 1978). The renaissance of clinical eidetic research which began vigorously in the mid-sixties started to feel comfortable and bear fruit in the beginning of the seventies and is now an independent school in psychotherapy (Singer and Pope 1978; Lazarus 1972; Wolpe 1969).

There is a functional difference between typographic eidetics and structural eidetics caused mainly by methodological differences. The clinical structural study of the eidetic is concerned with the nature of growth experience, and the developmental potential of the eidetic image and its possible long-term impact on the personality. This the typographic method cannot study because it uses clinically indifferent material and a brief time format. The emphasis and direction of research, by necessity, differ drastically if one looks at three examples from Haber's own article.

1. The experimental material in the typographic method is the easel and a few photographs or paintings, which do not allow ample flow of information from diversified fields of developmental areas.

The materials and questions in the structural method are selected from the vast field of clinically relevant imagery events. Eidetic pictures are presented either from a problematic memory field where neurotic and healthy components interpolate, or from a positive fundamental developmental situation which will evoke a rich eidetic response and throw light on conflicting memory material. The following two instances will make the point in question clear.

a. The subject is instructed to picture mentally the following projection (for fuller details, see Ahsen 1977a, pp. 23–32).

Eidetic Parents Test instruction no. 1: "Picture your parents in the house where you lived most of the time with them, the house which gives you the feeling of a home. Where do you see them? What are they doing? How do you feel when you see the images?"

b. Or, following the above-mentioned rationale, an important element from a subject's reported symptoms is marked out for exposure to a positive eidetic from a developmentally important situation. The eidetic is made to focus on the symptom through repeated mental projection. This results in resolution of the symptom.

Case history: The patient had a much larger pupil dilation in the left eye than in the right one. No other problem or deformity accompanied the condition. The patient was treated through a special set of left-right eidetic images of parents. In the course of a few months of this treatment, the overly dilated pupil became normal.

2. The character of presentation of material and the dialogue with the subject differ in an important way also, in the clinical approach. For instance, in the transcripts Haber describes E asking this question of S:

E. Tell me if it fades.
S. I can still see the birds and the Indian.

In the clinical setting, the emphasis is naturally on the opposite direction, that is, on the search for more lucidity. One asks which features tend to persist in the eidetic so that one can study the permanent features that tend to indicate what controls the structure of an experience. The clinician would ask the question instead:

E. Tell me if someone becomes clearer as you see this picture.
S. I can now see my mother more clearly after concentration.

3. In an opposite way the emphasis in Haber's transcript is on the visual fixity of the image which in the clinical literature is rather a sign of fixed neurotic memory than a flowing emotional response that shows itself in a moving picture. Haber's leading question on this aspect looks for visual fixity along these lines:

E. Can you count those stripes?
S. Yes, (pause) there's about sixteen.

In the clinical setting, the eidetic researcher would look for details carrying emotions in the image. It is a sign of fuller response to the picture with release of therapeutic values, such as:

E. How do you feel when you see the mother?
S. I feel pleased (or indifferent, or hostile, as the case may be).

The fact of visual availability of emotion is next treated with a view to see if experiential transformation can be effected in the disturbed area. In Haber's transcript, all the leading questions are asked in black-white fashion to confirm or to deny visual fixity alone, which is not the central quality of the eidetic image (Allport 1924).

One can see why the eidetic had a greater chance of surviving through the clinical literature where observations were directed at developing it rather than challenging it, or putting it into a straitjacket. Through the favorable structural approach, eidetics show up where none were reported before and evolve in a fascinating way from mere smears of light, blank spaces, and dark holes to bright and sensuously dazzling experiences in the mind.

The clinical theory of eidetics in the form of the current school of eidetic psychotherapy developed out of the full-blooded use of the classical orientation toward structural study of the eidetic. The work reported by this school has successfully destroyed the false myth that the eidetic appears only in a few gifted children and very rarely in adults. Allport, who considered memory a structured imagery process entirely different from the eidetic, recognized the fact that there are memory levels at which issues regarding needs of the organism had been settled, as there are also levels at which the significance of the material was still being comprehended. He suggested that in the latter areas the importance of the eidetic was fundamental: "The EI seems to serve essentially the same purpose in the mental development of the child as does the repetition of a stimulus situation. . . . Such pseudo-sensory experience enables him to 'study out' in his own way and in his own time the *various possibilities* [italics mine] for response contained within the stimulus situation" (Allport 1924, p. 117). The individual, as he grows from childhood to adulthood, retreats into fixed memory, using less and less of the vast creative potential in the eidetic response (Pavlov 1941). Neurotic as well as uncreative behavioral tendencies of the adult emerge essentially out of this retreat into the memory mechanism. Clinical study of eidetics discloses to us that this process can be easily reversed through release of the eidetic process again, in the locked-up areas. The creative function of the eidetic does not stop near adolescence; instead it continues on in the adult right into middle and old age. This special orientation gives eidetic psychotherapy its special status among other psychotherapies.

In brief, the main principle in structural eidetics and eidetic psychotherapy involves the enactment of problematic experience with a view to changing it through eidetic interlocking. Various possibilities inherent in the original experience are eidetically reenacted through brief eidetic experiments. As a result the best developmental structure when it emerges through these various progressions is spontaneously confirmed by the organism for future use. Through this simple principle, techniques are being developed by various researchers to treat pathological orientations, neuroses, psychoses, and the like.

John Dewey (1891), in discussing how concepts arise from percepts, said that the concepts are abstract, not sensuous, general, not particular, and construct through omission of details rather than through inclusion. The concepts that build with reference to this principle of construction omit the "knowledge of the concrete object in its qualitative, spatial and temporal limitations." Psychotherapy through eidetics makes the patients reevaluate their abstract "concepts," that is, their developmentally inaccurate and unreal views through enactment of the original experience. The wholesome pieces of information and percepts are retrieved and confirmed for future use. The eidetic release in the enactment of these original percepts procures a spontaneous reevaluation of neurotic mechanisms and transforms biased memory structures into better experiential structures. The reappraisal and reconstruction of the personality mechanisms at this root point of percepts is the essence of structural eidetics, an approach with a distinctive flavor and ethos.

In broad conclusion, Haber's role in reviving interest in eidetics must

be recognized. The target article is a good summary of his efforts and his views, not excluding, of course, the admission of wonder and confusion regarding certain behaviors of this important ghost. In the opinion of this commentator, the ghost would be less enigmatic if some of the terms were redefined as suggested and if the vast clinical literature were involved in this redefinition and search for the identification of the ghost.

by **R. Ashton**

Department of Psychology, University of Queensland, St. Luica, 4067, Australia

Eidetic imagery and stimulus control

The thrust of Haber's paper is that there is a qualitative difference between memory and eidetic images. He argues against any notion that they are merely quantitatively different. An important semantic problem arises in his discussion of memory. One presumes that when he uses the word "memory," he is talking about visual images, as assessed by such questionnaires as Betts's Questionnaire upon Mental Imagery (1909). But this usage carries with it the implication that all such invoked images must be memorial – a similar percept must have been experienced at some time in the past. What, however, about the formation of images of things never experienced, clear images evoked by vivid prose describing life on other planets, for example? Haber must be arguing that such evoked visual images are not the same as eidetic ones. His data are not, I submit, compelling enough for such a distinction to be needed.

Haber himself dismisses most of the evidence for qualitative differences, such as the presence of eye movements (present during the production of "ordinary" images; not very large; and, anyway, what is the pattern during real perception?), and the finding that eidetic children can read off the letters of nonsense words in their reverse order (who knows what would happen with real words, or when instructed to form an "ordinary" image?). The pieces of evidence to which he attaches most weight are the use of the present tense when describing the image, and the fact that the image seems to be projected onto a screen by the eideticker. The former evidence seems too thin. Ask most people to form an image of the face of a colleague and then describe the *image* formed (not the colleague's face), and the present tense is invariably used. The projection evidence alone seems to remain. This result, however, is not unique to eidetic images. Hypnogogic hallucinations, which sometimes occur when we are falling asleep, are often projected as images out into the real world. They are not uncommon. Furthermore, even if such projection were unique to the reports of eidetickers, this fact would not necessitate the formulation of some unique kind of cognitive processing ability. The evidence could be reinterpreted in terms of the idea of stimulus control. That is, the frame of the screen helps some subjects (the eidetickers) to visualize the material previously viewed, probably by facilitating the retrieval process. Again, this is not uncommon. Some people, when trying to retrieve written information, evoke an image of the page on which it was printed. And going back to places not visited for some time invariably evokes memories and images of things experienced there. This stimulus control theory also explains the finding reported by Haber of nontransference from an exposed to a nonexposed eye. This sounds just like the findings reported when spreading depression of hemispheric activity is used to study learning. Schneider (1967) explained the nontransference of the information between hemispheres in this case in terms of stimulus control. Thus eidetickers are simply more dependent upon the presence of the seemingly irrelevant aspects of the testing environment for the retrieval of information.

One issue not really explored adequately by Haber is the developmental one. He claims that the ability to form such images is very stable in some children. Where do they go to, given that adult eidetickers are, on his reckoning, so rare? A developmental change (namely a decrease in incidence) must occur, and this fits into the stimulus control theory. Thus young children find it difficult to pay attention; their gaze wanders around and off what the experimenter considers to be the stimulus. Under these conditions other features (such as the easel frame) become very salient and are incorporated as part of the memory structure. Haber's own finding, that the less attention the subjects pay to the stimulus the better the image, is clear support for this argument. Here the "irrelevant" material scanned by the subject forms part of the memory, and reexposure to this material, minus the target picture, facilitates recall of the target material.

by **Theodore X. Barber**

Proseminar Institute, Cushing Hospital, Framingham, Mass. 01701

Eidetic imagery and the ability to hallucinate at will

Dr. Haber's research is very important and praiseworthy; it provides carefully gathered data and analyses that clearly enhance our understanding of eidetic imagery. However, I believe that further research in this area will proceed more effectively if investigators keep in mind that the picture-description technique used by Haber is only one possible method for studying eidetic imagery. Another method for studying what I believe is the same phenomenon is simply to ask subjects if they can see in the room an object that is not actually present. We have used this kind of procedure with individuals who were serving as control subjects for hypnosis experiments; and we found that about 1–2% of the control subjects testified that they saw, and also behaved as if they saw, the object that was not present (Spanos, Ham, and Barber 1973). Of course, in this type of experimental situation, there are obvious demand characteristics, and subjects' reports are effected by many extraneous variables; for example, whether the rating scale allows subjects to say that they *imagined* rather than *saw* the object in the room (Barber 1964b, 1970a; Barber and Calverley 1964; Barber, Spanos, and Chaves 1974, pp. 74–76; Ham and Spanos 1974; McPeake and Spanos 1973; Orne 1962; Spanos and Barber 1968; Spanos, McPeake, and Carter 1973). Nevertheless, when I say that about 1–2% of our adult control subjects apparently "see" the object they are asked to see, I am referring to the residue of subjects who testify postexperimentally that they actually saw the object, even when (a) the experimental demands make it easy for them to say they *imagined* it rather than *saw* it, and (b) we had attempted to exclude other demand characteristics by requesting several times during the experiment that the subjects give strictly truthful reports (Spanos et al., 1973). I believe that both Haber's data and our data derive from the simple fact that a small percentage of children and a very small percentage of adults have an ability or skill that others either lack or possess to a small degree. As I pointed out twenty years ago (Barber 1959b), this skill or ability has at times been labeled as "eidetic imagery" and at times as the "ability to hallucinate at will." Galton made this clear back in 1883, when he found in "sane and healthy" persons that there is "continuity between all the forms of visualization, beginning with an almost total absence of it, and ending with a complete hallucination."

I would like to emphasize that when investigators probe deeply into the phenomenon of "eidetic imagery," they find that they are studying the same phenomenon that, during the last century, was labeled as "the waking hallucinations of healthy persons" (Parish 1897). For instance, our subjects who insist that they see the object that is not present also typically insist that they can touch it, smell it, move it, manipulate it, and so on. During the past year, Sheryl C. Wilson and I have conducted intensive interviews with four individuals who have this ability, and we were surprised to find that they all have another related ability in common: they have an amazingly vivid memory of objects and events that they have encountered during their life. They can, for example, vividly recall the way a childhood doll looked, felt, and smelled. When they are asked now to see a doll sitting in a chair in front of them, their memory of the doll is so total that they literally feel that they see it, touch it, and smell it again.

From my experience in this area, which includes the studies mentioned above plus additional studies (Barber 1959a, 1964a, 1970b, 1971), I have arrived at the following conclusions, which I would like to see tested in further research:

1. The 1–2% of control subjects who insist that they can see in the room the object or person they are asked to see are the same individuals who perform well on the picture-description test used by

Haber and other investigators.

2. These individuals are the same ones who are labeled as "excellent subjects" or "somnambulistic" subjects in hypnotic experiments. More concretely, since they can easily "see" objects that they are asked to see, they easily respond to suggestions for visual hallucinations given in hypnotic experiments; since they can "block out" an object from the visual field by "seeing" another object in front of it, they can easily respond to suggestions not to see something present in the room; since they have a vivid memory for childhood events, they can easily "age regress" to childhood; and so on.

3. These subjects are also the same individuals who are prone to see ghosts or apparitions. Surprisingly, this contention was documented many years ago (Martin 1915) and then apparently forgotten.

4. Some of these subjects are also among our great novelists and poets. For instance, Marcel Proust, who was able to recall vividly and to "see" his entire life experiences when he was recording them in his novels, was almost certainly an eidetiker – he would have scored at the very top in our experimental situation or in Haber's experiments. Also, the American novelist Thomas Wolfe would almost certainly have been rated as an eidetiker, especially since he stated many times that he could vividly recall and see again the events in his life; for example,

> suddenly I would remember the iron railing that goes along the boardwalk of Atlantic City. I could see it instantly just the way it was, the heavy iron pipe; the raw, galvanized look; the way the joints were fitted together. It was all so vivid and concrete that I could feel my hand upon it and know the exact dimensions, its size and weight and shape (Wolfe 1936).

5. These same individuals who obtain high scores in our experiments (or in Haber's experiments) would be much more likely than other subjects to report visual hallucinations if they were in a sensory isolation situation or if they had ingested a small dose of LSD or mescaline (Barber 1970b).

In conclusion, I would like to urge Haber and others working in this area *not* to prematurely define eidetic imagery operationally by one type of experimental procedure – the picture-description technique – and thus to exclude by definition from the rubric of "eidetic imagery" the results obtained by other experimental procedures. The data obtained by Haber may differ in some ways from our data, not because we are studying different phenomena, but because we are studying essentially the same phenomenon with different methods and procedures. Both experimental methods have their own sets of demand characteristics and both are intertwined with suggestions (Barber 1970a; Gray and Gummerman 1975, pp. 403–4); nevertheless, both methods should be used in a careful, critical manner, first to find and then to study intensively the life history of additional individuals who have this skill or ability.

by **Jeanine Blanc-Garin**

Laboratoire de Psychophysiologie, ERA au CNRS, University of Provence, 13397 Marseille Cedex 4, France

Is eidetic imagery still eidos?

Why was the adjective "eidetic" chosen by the students of this fascinating aspect of imagery in Germany at the beginning of the century? The Greek term "*eidos*" refers to "the form of a thing in the mind," and the same root, "*idein,*" gave "idea" and "idol." At the same time, in Germany, "eidetic" was used by Husserl, in phenomenological philosophy, to indicate "essence," as distinct from "appearance."

For the early students, the eidetic image (EI) was considered the best image, the ideal way to study and analyze cognitive imagery. However, some recent experiments and contemporary theories indicate that imagery can no longer be regarded as a unitary phenomenon. Thus, it is in terms of discontinuity that Haber discusses the place of EI in imagery processes.

For a while, EI was envisaged as a genuine reproduction of the stimulus display, and, consequently, an excellent example of memory image representations (MIR). Three characteristics of EI (vividness, temporal stability, and good precision) were thought important in the preservation of sensory data. These characteristics can no longer be viewed as good criteria for cognitive MIR, however. It is now known that vividness does not fully reflect the strength and efficiency of imagery; it is not correlated with cognitive capacities, such as spatial abilities (Richardson 1977), or creativity (Florisha 1978).

The stability of EI over a long period can be viewed as the reverse of cognitive flexibility: a subject's capacity to replace an image by another – his active control over his imagery – is a manifestation of "adaptive flexibility" (Gordon 1949, Richardson 1977). Anticipatory images (Piaget and Inhelder 1966) are other examples of MIR: incoming data can be managed and transformed, and a new reality, not yet perceived, can be constructed.

Great precision is hardly an advantage in perceptual processing and memory. All the recent models emphasize the organism's ability to filter, select, reduce the flow of inputs, and choose among possible patterns; that is, to construct a personal representation. Thus, "subjects do not remember what was displayed, but the idea generated by it" (Piaget and Inhelder 1968, p. 457). The visual display is not kept in mind, but a new pattern is built up, with traces of sensory data in a mold of schemes (Piaget's sense) or schemata (Piaget's and Bartlett's sense).

Thus, the three characteristics that make EI a good reproduction of stimulus display are not good criteria for an efficient MIR, probably because a memory image is not a stimulus trace, it is a representation; unlike EI, it does not reproduce, it signifies.

These observations support the distinction between sensory materials and organization in internal representation. Sensory materials (visualized content, more or less vividly experienced) have to be distinguished from underlying mechanisms (see, for example, Blanc-Garin 1974; Kosslyn 1975). "Resembling" and "describing" (Pylyshyn 1975) coexist, but have different functions. It is probable that the surface features (the sensory aspects of which the subject is aware, or can evoke), are important for the affective record, for psychological comfort, for a feeling of permanence about the world and persons; but, certainly, the efficiency of MIR in cognitive processing is related to deep structure, to organizing and abstracting activity, which manages these sensory materials, and, probably is often without access to consciousness.

Results on EI show, dramatically, that analogs, traces isomorphic with external input, passively reproduced, have no relation to MIR; not even to the sensory dimension of MIR. This was stressed by Haber, who cites Paivio's results (Paivio and Cohen 1977), showing a lack of correlation between the eidetic factor and vividness of imagery.

The developmental hypothesis about EI was tempting because it implied that during maturation "cognitive transformation" effects were added to or replaced "reproduction" processes. But, clearly, as Haber argues, we must abandon this hypothesis. It seems to me that we have to assume that the nature of EI is differnt from that of MIR.

It appears impossible to escape the discontinuity issue; the synthesis presented by Haber has completely convinced me of the following points: (1) Although different from afterimages, EI is an aftereffect, not a memory: it is stimulus bound; it cannot be evoked by anything other than the picture. (2) EI is an image only if we agree to use this term in a very wide sense; etymologically, an image would be a replica (from Latin *imitari* = to imitate). (3) EI is a passive reproduction, not a true representation; if cognitive factors are efficient, its role is more inhibitory than constructive. Thus, EI would not be what is suggested by the Greek *eidos* (near idea), which implies seeing and knowing. (4) Only the visual aspect relates EI to the mental image, in which the visual component is often experienced. Is there any continuity, as suggested by Haber? Perhaps rather than being on a continuum, the two processes can be regarded as two branches of a common root (visual function: (a) the EI process, which preserves visual material, and is not well developed in man; and (b) the MIR process, which selects, translates, codes, and retrieves. This latter is developed in man from the visual function, and is one aspect of "semiotic processes" (Piaget).

Whatever the mechanism, it may be that in phylogenetic development, EI was once an aspect of visual function. This opportunity, not selected for by the evolutionary process, is now manifest as a blind

alley, while the other branch was expanded as the cerebral cortex enlarged.

by **Bruce Bridgeman**

Psychology Board of Studies, University of California, Santa Cruz, Calif. 95064

Toward a neurological theory of eidetic imagery

There is no question that EI (eidetic imagery) exists, even though it is rare, and Haber's well-documented article shows that the earlier methodological doubts of psychologists have been resolved. Though Haber draws a distinction between psychophysical discrimination techniques and phenomenological methods, he has invested great effort in developing phenomenological methods that have in common with discrimination techniques the crucial property that they prevent the subject from "cheating," or telling the experimenter what the subject thinks he wants to hear. He has shown that it is possible to make phenomenology as reliable as discrimination. The most extreme example of the combination of rigorous methods with eidetic imagery is Stromeyer and Psotka's (1970) use of Julesz stereograms, though the ability to combine stored and seen dot patterns to make three-dimensional images unfortunately appears to be rare even among eidetikers.

After presenting an overwhelming case for eidetic imagery and listing its characteristics, Haber expresses dissatisfaction that the ability does not seem to be linked with other unusual abilities or traits. EI is not the only trait to show a seemingly random appearance in otherwise normal subjects, however, and even neurological conditions are sometimes present in otherwise normal children. An example is febrile epileptic convulsions, which occur in up to 25% of otherwise normal children during bouts of high fever. They are not followed by subsequent epilepsy and usually do not recur. Like EI, febrile convulsion is rare in adults. This susceptibility to convulsion suggests that there is something different about children's brains, that they are more sensitive in some way than adult brains. Perhaps EI is another manifestation of this sensitivity.

The neurological basis of EI is still a complete mystery, and its implications for normal visual function are equally unknown. Some of its characteristics, however, enable us to make a few guesses about its representation in the brain. The photographic nature of EI indicates that the eidetic information should be stored in a topographic projection of the visual system, and the close relationship between attention and EI points to a location somewhere in the cerebral cortex, where interaction with higher-level processing is most likely. The only property of EI inconsistent with this interpretation is the apparent difficulty in interocular transfer, though Haber points out that that property may be an artifact of opening and closing the eyes.

Because eidetic children scan their images with eye movements, the image is probably held at a stage at which image information is no longer tied to retinal position, though such a "space-constant" projection area has not yet been found. The only data available on this question (Bridgeman 1973) show that the topographic map in area 17 is tied to specific locations on the retina. Recent data (Schlag and Schlag-Rey 1977; Peck and Schlag-Rey 1979) suggest that the visual system does not achieve space constancy across eye movements by use of a topographic surface with space-constant rather than retina-constant receptive fields, but rather by combining retinotopic information with eye position information in a way not yet fully understood. Thus, one or more of the known retinotopic maps in visual areas of the brain might serve as substrates for EI. The other requirements for a cortical area supporting EI correspond to Haber's list of EI characteristics, including its perceptual nature, erasure with blinks, and interference by cognitive naming operations.

I propose that EI might plausibly be modeled as a positive feedback that maintains images in the absence of other disrupting inputs to a cortical area. The pyramidal cells that constitute the cortical output also typically have recurrent collaterals which reenter the neuropil near the dendritic tree of the same cell, and might provide an anatomical substrate for the reentry of information into the cortical area that originated it. Because it is stored as an active trace, the information stored in this feedback network would be disrupted by outside input related to the image, input such as reciprocal interactions with other cortical areas involved in naming parts of the image. Blinks, with their abrupt changes in stimulus brightness, might, for similar reasons, erase the EI from the upstream rather than the downstream direction.

Normally the information coded on this substrate would be quickly recoded into nontopographic memory and lost to the topographic field that supports visible images, but in children the recoding process might be less automatic and the positive feedback stronger. A speculation that children possess less neural inhibition of positive feedback loops would be relevant to both the EI and the febrile convulsion data; both phenomena imply an overreactivity of immature cortex. As dendritic trees grow with age, and more inhibition is available, both EI and febrile convulsions would become less likely. Embryologically, excitatory connections generally develop before inhibitory ones.

If positive feedback or some similar process preserves image information (when it would normally dissipate) in the same cortical structures that normally mediate the image, then the children's matter-of-fact confidence in their image information and most of Haber's remaining properties of EI would follow immediately. Haber's final property of EI, that it is most likely to occur when the subject makes no effort to memorize an image and pays no cognitive attention to it, is a passive state of mind that might be more likely in children before their cognitive search and memory strategies are well developed. The very small number of adults who possess eidetic abilities may have learned to preserve this attitude, maintaining the integrity of their images by learning not to concentrate on them.

This modest proposal is far too simple to account for the richness and subtlety of EI without extensive further work, and it may prove to be unprovable. It shows, however, that EI can be conceived in concrete physiological terms, and that our understanding of both EI and the physiology of visual perception would be enhanced by further study of their combination.

by **B. R. Bugelski**

Department of Psychology, State University of New York at Buffalo, Buffalo, N.Y. 14226

Eidetic possession: is exorcism necessary?

Ralph Norman Haber has been haunted for twenty years by his findings of eidetic imagery in a small percentage of children (5 to 10% of 6 to 12 year olds). He calls for an exorcist, but surely neither Haber nor the children are possessed of evil spirits, and the call should properly be for a ghost chaser. Ghost chasers, however, are notoriously unsuccessful even when the ghosts are operationally defined, and eidetic children will remain with us as long as we keep writing about them. Haber has done a good job of extermination and demonstrates that his eidetic children were not different from random samples of children in any way other than their being labeled eidetic, a condition that doesn't appear to be harmful in any way. Haber remains convinced that the label has a significant and meaningful referent, and he may well be right, but it should be recognized that the label really refers to only one feature of the behavior of a small percentage of children (use of the present tense) in a rather rigidly circumscribed situation. It might be worthwhile to examine this situation.

In designing his research Haber was properly concerned about controlling the observations to be made. He created an experimental setting in which children could be observed systematically. Instructions were uniform, and all the children went through the same procedure. The procedure consisted of pretraining with colors placed on an easel to, in effect, demonstrate to the children that they could experience negative afterimages; this pretraining was followed by the exposure of several pictures in the same place and for the same time as the last of the color samples. In both cases (colors and pictures) the children were asked the same thing: do you see anything on the easel? (after the colors or pictures were removed). The pretraining with afterimages was considered desirable by Haber but by creating an experimental bias in some children, may have been responsible for the findings. Although Haber is obviously aware of the nature of experimental

demand characteristics (he frequently mentions them), he apparently feels that the demand characteristics of the task were somehow eliminated by convergence of lines of evidence. Could this really have been the case?

If we examine the procedure and data reported by Haber, we find that he lists seven criteria (augmented by a rather unsupported eighth) for deciding that a child is worthy of the designation of "eidetic." The criteria, however, boil down to whether or not a child uses the present tense and says "I see" instead of "I saw." None of the other criteria, especially that of accuracy, appear to bear any real weight. The first criterion of simply reporting an image certainly is redundant with the fourth (the use of the present tense). Moving the eyes in examination of real and imaged pictures (criteria 5 and 6) does not appear meaningful as an independent feature – all children move their eyes when told to do so. Having the image appear on the easel is an unusual demand. Memory images do not require a projection surface – they are known to be "inside" the imager, even though many adults have been heard to say, "I can see him just as plainly as if he were standing right there."

By pretraining with negative afterimages which *are* projected onto a surface, Haber might very well have created certain expectations in his child subjects. Because the children were directed to look at the easel, the images, if any, would necessarily be related to parts of the easel. If the child subject in the scientific, university laboratory were told in effect that he might very well continue to see something on the easel after it was removed and this had proved demonstrably so with the colors, would he not be inclined to believe this? He could also presumably refer to parts of the image as located at various portions of the easel. This would be an obvious requirement if he were *seeing* something. In a study with sixteen picture objects in four rows of four items, I have found people reporting the location of the previously exposed objects by pointing to spaces in a blank matrix of sixteen squares. We can all locate objects after a fashion.

When one considers the instructions and setting, plus the prior experience of real afterimages, it might be somewhat amazing that more children do not use the present tense in reporting on their just preceding visual experience. Is it so amazing that 5–10% of the children happen to use the present tense in a somewhat glib description of a just seen picture? That so few were found to be suggestible might be the concern of interest. The obvious control should include taking a large number of children and showing them only one picture to screen out those who might show some uunusual talent or claim. The lack of accuracy (i.e. no superiority over noneidetics) raises the old question of whether a difference that makes no difference really is a difference. If the children were really seeing something, they should have been able to report differently in some regard besides glibness. Haber must have thought so, too, as he listed accuracy as a criterion.

It is also possible that the children labeled eidetic used different observational techniques – they could have, for example, engaged in some story creation or looked for specific details of color, size, relationships, meanings, and so forth. The "processing" of the original stimulation might have enabled some of the children to adopt the role of visualizer when the pictures were removed with a ready-to-hand kind of account of "what I am seeing." The emphasis on the present tense seems somewhat misplaced. People who report the use of energy in mnemonic research frequently describe their images "as if" they were viewing objects at the moment. They can report "visualizing" objects never actually seen before, for example, a purple cow as if they were then and there viewing something. The only distinction from Haber's child subjects is the lack of an easel and immediately prior visual inspection.

Whether the demand characteristics account attempted here is relevant or not, Haber has made a strong contribution in removing the eidetic image from the current field of investigation of imagery by divorcing it from the memory image or other cognitive interpretations of representational operations. He has made it a strictly visual phenomenon, and it may well be that such visual phenomena do characterize some of our young citizens. The lack of support for any developmental hypothesis is also very helpful in clarifying the current theoretical field. The fewer encumbrances image researchers must bear the more likely it is that they can make progress in the field. The additional failure to find much in the abstract-concrete speculations of cultural researchers is also welcome, as is the failure of the retardate/brain-damage speculation. Haber may be left with a ghost, but he has reduced it to a rather insignificant one that requires easels, 30-second exposures, and prior afterimage training, and is neither help nor hindrance. Such a ghost we can live with and examine in a more leisurely way.

by **Daniel C. Dennett**

Center for Advanced Study in the Behavioral Sciences, Stanford, Calif. 94305, and Department of Philosophy, Tufts University, Medford, Mass. 02155

Breeding cognitive strategies

I will assume, on Haber's showing, that the phenomenon of eidetic imagery is discontinuous from normal visual memory and afterimages, and a perfectly robust phenomenon in its own right. In any event, it will be empirical studies and not worries about the "metaphysical" status of eidetic images that will settle that issue. Philosophers can be counted on to find a suitable ontological home, and a licensed description, for any phenomenon that survives the surveyed and projected empirical scrutiny. Curious it may be, and some proffered *descriptions* of it may warrant the retort, "Impossible!," but that should not forestall investigation.

Haber presents us, then, with a phenomenon in need of a theory, a circumstance that invites speculation, not premature argument. Haber's description of the phenomenon encourages the following speculations from me about how to think about the options for theory. Suppose that the developing cognitive systems of young children are opportunistic generators and adopters of cognitive strategies, relying in some measure on ransom or merely fortuitous generation of candidate strategies, but also, of course, subject to a variety of constraints: initial system architecture, history of early experience (or "stimulation"), history of cognitive "demands." Consider these factors to determine a sort of environmental niche, in which various strategies, should they happen to occur, will be candidates for short- or long-term survival. Then suppose that the strategy of developing eidetic imagery is in most individual cases a fence sitter – likely to occur in a sizable percentage of cases, and not so clearly benign (useful, efficient) so clearly detrimental, so unstable or ineradicable (whatever its value) as to permit any high-probability predictions of its fate when and if it appears. That is, suppose that for *this* strategy, the constraints underdetermine its fate, so that the random or fortuitous factor plays a magnified role in the statistics, relative to other, more predictable features of the developing cognitive system.

If this were the case, then we would expect the search for correlates to be in vain, unless we were lucky enough to discover, "from the outside" as it were, correlates very directly tied to the actual mechanism of strategy implementation (the actual footprints or skeletons of the embodied strategy). We might find that there were no salient *preconditions* for eidetic imagery, since almost everyone met them; in many subjects who meet all the identifiable preconditions the strategy simply never happens to occur as a candidate for adoption. In many others it might occur briefly and inconsequentially. And we might find no subsequent *symptoms* of adoption of the strategy (beyond the evidence that the strategy has in fact been adopted – that the child is an eidetiker) because it does not interfere (drastically or even noticeably) with the development of other needed cognitive apparatus; it can share the niche with them for some time before eventual abandonment. Apparently something does favor its extinction before adulthood; something else needs and gets the resources required for eidetic imagery, or the cognitive demands on the system change in such a way as to lower the relative utility of eidetic imagery, . . . or something. The regularity of the *loss* of eidetic capacity is apparently one of the few fixed points, and asking why there should be this upper limit on the strategy's lifespan might be a fruitful avenue for future investigation.

Certainly what is known of the phenomenon does not require that it be viewed in this light, but so far as I can see, what is known is temptingly consonant with this view. So far, eidetic imagery has the earmarks of such a fence sitter. There is no strong reason (of utility,

say) why we should all be eidetikers – so we needn't expect an answer to the question, "What's wrong with the rest of us?", and, no strong reason (of disutility, say) why no one should be an eidetiker, so we needn't expect an answer to the question, "So what's wrong with them?" Although no concomitant cognitive deficits have been found, some deficits (due to old age or brain damage) may provide a slightly better niche for the strategy to develop in, and hence favor its adoption and maintenance in those to whom it happens to occur. There are independent reasons for supposing that while it may not be a detrimental strategy, it is clearly suboptimal. It is no more accurate or voluminous an information store than ordinary visual memory because whatever form the storage takes, what is stored is clearly *postinterpretation* and hence subject to the information losses (including discards) and distortions of the interpretation process. As Haber importantly notes, it is *not* "photographic memory" except perhaps in the remarkable case reported by Stromeyer and Psotka (1970). It probably makes relatively inefficient use of the storage machinery by requiring the reintroduction of inaccurate or at least unreliable specific-image-maintaining information after this information has been discarded by the interpretation process of perception. (Consider the extra information-transmission costs of generating a police artist's sketch from nothing but a witness's verbal description and sending both the picture and the words to all patrol cars). It is certainly somewhat inefficient in requiring control loops that go through so much of the subject's visual system (requiring an eyeblink for "erasure," for instance), and hence being only indirectly controllable by the subject's will (as one might just as well say). Given this mediocre scoring on a speculative cost-benefit analysis it is somewhat surprising that the strategy ever survives, as apparently it does, but perhaps it has as yet unnoticed advantages when exploited in the solution of cognitive problems peculiar to childhood, or perhaps it survives through a loophole in the brain's no doubt imperfect implementation of cost-benefit analysis – the principles, whatever they are, of strategy selection. In any event, it is entirely possible, and not at all abhorrent to science, as Haber seems to think, that some children are eidetikers simply because it occurred to them to be (not consciously, of course), and that others aren't simply because it never occurred to them.

Haber suggests that a more aggressive and interactive exploration of the phenomenology of eidetic imagery could be fruitful, and I certainly agree. An elaboration of that approach would be the attempt to train children or adults to have, or improve, their eidetic capacities, for this might clarify the conditions under which the capacity appears and survives. But here a caveat is in order. We might well find that nothing we could devise would induce the eidetic strategy in noneidetikers or prolong it in maturing eidetikers, but in that case our disappointment should be mixed with relief, for we might succeed, and we don't yet *know* that there are no steep prices being paid by those who are lured into this curious cognitive mode, and even if we became *very* confident that the strategy *as it occurs in nature* is benign, we would have no guarantee that artificially induced or enhanced *mutations* of the natural strategy might not prove more persistent or more imperialistic in their demands on the available cognitive machinery. So on prelude to any further experimentation that might uncover techniques for inculcating the strategy should be still more exhaustive and systematic canvasses for deficits in longitudinal studies, whose continued barrenness we could view as encouraging, not discouraging, since it is a prerequisite for conducting the sorts of experiments that might unlock the phenomenon and yield a good theory. Another necessary prelude would be self-experimentation and experimentation with other informed adults, however dim the prospects of positive results.

by **Leonard W. Doob**

Institution for Social and Policy Studies, Yale University, New Haven, Conn. 06520

The cross-cultural approach to eidetic images

My emotions and convictions deviate only slightly from Haber's, which he reports so conscientiously. Eidetic images intrigue me, and ten years later I also continue to be haunted by them and by my own inability to locate their social or psychological correlates in African societies. They remain, as I called them then, a bewildering will-o'-the-wisp. Occasionally when a skeptic doubts their existence or importance, I play a tape containing in English the protocol from a young African who replied to my challenge ("you see . . . only what you have seen") with indignation ("no, I see them [the details] now").

In his autobiographically tinged review, however, Haber is not impressed with cross-cultural findings based on a slight modification of his own method. He prefers data from what he continually calls his "laboratory" (a quiet room, often in a school building) where presumably the emerging reports are never, never, or hardly ever "messy." The present commentary, derived from the nine available references, and the only ones he lists under "Cross-cultural studies," would timidly suggest that his conventional bias has caused him to overlook support for many of his theses and promising leads from these very studies.

Most impressive should be the discovery that, in spite of admitted verbal but challenging confusion (which, Haber sometimes forgets, also exists when he and his subjects converse in English), not only children but also numerous adults outside the Western orbit (adults and children in seven African societies, four groups of aboriginal children in Australia, adults in Brazil, children in Iran) report and behave in virtually the same manner as Jaensch's original subjects in Marburg, Germany, more than half a century ago and Haber's in New Haven and Rochester in the United States. Such an apparently universal phenomenon – ranging from saccadic eye movements and the use of the present tense in describing the images to making a spontaneous distinction between "seeing" and remembering – merits more than perfunctory bow. Although some subjects in Ghana may have falsified their reports, and although a Kamba sample in Kenya was influenced by one experimental manipulation but not by others, on the whole the appearance of the images cannot be attributed to the demand characteristics of the social situation. With eyes watering from serious concentration, a young Swahili male apologized, "I keep looking at the screen but, I am sorry, I see nothing." Some Somali nomads reported nothing after being told, "Most people can see something on the screen after I take [the picture] away; you will probably see something too." Hutu subjects in Rwanda, after witnessing a peer being rewarded with a small gift and with profuse praise heaped upon his ability to report eidetic images, more frequently than not stated they "saw" nothing on the easel. A crowd of Ibo spectators in Nigeria kept looking at the blank screen as if a slide were being projected upon it; some voluntarily and spontaneously corrected a false statement by the eidetic subject. Responses to culturally irrelevant drawings (e.g., the "Alice picture") did not differ appreciably from those evoked by culturally relevant pholotgraphs (e.g., Africans milling around a bus). These non-Western individuals also have contributed inaccuracies that debunk the miraculous accuracy previously attributed to eidetic images.

Haber dismisses too blithely the reports of many Africans that their images were not out there but in their heads (pictorial images). After all, traditional peoples as well as Americans do not wander about with an easel and screen in front of their eyes upon which they can "see" images. The Kamba and Swahili informants who claimed to find their images generally useful in their everyday existence and in recalling their deceased kin apparently were prone to "view" such images in privacy.

The hypothesis that there is a negative relation between eidetic images and westernization, modernization, or acculturation, either within a society or between societies, which Haber and others have stated, has not been consistently validated. Perhaps I myself should have quit eidetic research in Africa after publishing the preliminary finding that eidetic incidence was significantly higher among the rural than among the urban Ibo. Then I would not have uncovered a disturbing reverse relation in a Swahili-speaking sample in Tanzania. That hypothesis, however, cannot be dismissed: at the moment it is clear that the postulated relation is evident in some but not in all instances for which we have data; yet the societies are too few, and exceptions (e.g., validation for an aboriginal group and European Australians, but not for two aboriginal groups) challenge us to locate

the mediating circumstances.

I turn now to the promising leads that are suggested by cross-cultural research and that might be useful to explain the reactions of American children. Both within and between societies individuals differ with respect to whether they have previously been aware of their own eidetic propensity: if the images have served some compellingly useful function, why were some persons surprised when they were induced to "see" them and why among the Bororo of Brazil was witchcraft suspected? Among the Kamba a significant relation existed between replies in an interview about images and actual responses three months later in the testing situation. The greater confidence displayed when eidetic or pictorial rather than memory images are reported might be a reflection of the fact that in these traditional societies information could not be stored in the form of writing. The less or the no more accurate recall days or weeks later of the Kamba who allegedly had "seen" images of the exposed stimuli, in comparison with those claiming to lack such images, on the other hand, suggests that images may not provide more efficient storage and is perhaps of greater psychological significance than the relatively high and low reliabilities reported, respectively, in the United States or Australia, and in Ghana. The sharp distinction subjects made between details they did and did not perceive during the original exposure points to the probability that whatever is "seen" in the images depends as much upon attention as almost any other act of perception and hence may lead to omissions and errors ("I wanted to put them in my mind, but then you took the picture away so quickly," a Kamba complained). Since the superimposition of an image upon a stimulus was too difficult for the Kamba, Hutu, and the aborigines, it may not be a valid test.

On the whole, I repeat, I think Haber is right: we must keep searching for the correlates of these perplexing images. Whether we are more likely to find them in the phenomenology he somewhat vaguely proposes for himself and his laboratory colleagues or in irritating, real-life field situations I do not know. Let us keep all doors open.

by **K. Anders Ericsson, William G. Chase, *and* Herbert A. Simon**

Department of Psychology, Carnegie-Mellon University, Pittsburgh, Penna. 15213

Phenomenological reports as data

Haber claims that eidetic imagery is supported by phenomenological reports and proposes that phenomenological evidence should be more widely accepted as legitimate scientific evidence. In this commentary we will first address the general issues of using verbal reports as data; then, in light of this discussion, we will turn to an analysis of the particular phenomenological evidence cited by Haber in support of eidetic imagery.

In their review of general issues associated with using verbal reports of cognitive processes, Ericsson and Simon (1978, 1979) proposed that verbal reports be viewed like any other kind of data; any model that can reproduce them (or their content) is legitimate. A subject who reports using a subgoal for a problem or computed partial results in a mental multiplication need *not* be trusted. The veracity of reports can be assessed by an independant analysis of the task; a model is sought that both regenerates the reported intermediate steps and generates the same solution as the subject does.

In analyzing the broad range of instructions and circumstances under which verbal reports have been solicited, Ericsson and Simon (1978, 1979) found that valid reports are obtained when subjects are asked to verbalize the information they are currently attending to, that is, "to think aloud." Within an information-processing model, we can assume that attended information is directly available for further processing, and thus also for recoding into the verbal form to be reported. Cases of invalid verbal reporting are invariably associated with asking subjects for reports of information not otherwise directly attended; these reports require inferences. There is explicit evidence (Nisbett and Wilson 1977) that, in response to questions about the reasons for their behavior, subjects resort to inferences based on general knowledge – their answers no longer reflect any direct memory trace of the cognitive process.

If we want to use verbal reports as data in support of some proposed cognitive mechanism, we need to propose an explicit model for how the verbalized information is generated by accessing this mechanism. Second, we need to show that alternative hypotheses for how the subjects generate their verbal reports without this mechanism are not pausible given the recorded verbal reports. In short, in evaluating verbal reports, we need to apply the same criteria as for any other type of data.

In reviewing the phenomenological evidence cited by Haber, we raise the issue of whether subjects are making direct reports of attended information or whether they are making inferences. For example, in the composite picture test (Figure 6), do eidetic subjects actually "see" a composite image of a face directly, or do they "figure it out"? Are people classified as eidetic on the basis of their visual imagery processes, or on the basis of their ability to make the appropriate inferences? One unfortunate problem with identifying eidetic subjects is that they cannot be discriminated on the basis of superior memory performance, nor are the subjects in general spontaneously aware of their eidetic ability. Hence, a rather elaborate procedure of instruction and direct questioning of subjects is necessary to identify an eidetiker and to produce the phenomena and verbal reports associated with eidetic imagery (Leask, Haber, and Haber 1969). The specific questions asked by the experimenter leave rather little information for the subjects to report, for example, "Yes, I see X." The small amount of information generated makes it reasonable to consider other hypotheses than Haber's assertion that a distinct eidetic memory is accessed. Furthermore, many of the studies Haber cites were designed to explore characteristics of eidetic memory (such as the location of the image), and used questions and probes that may well have biased the content of the verbal reports. Leask, et al. (1969) were explicitly aware of the demand characteristics of their assessment procedure and the presuppositions in their questions. In fact, they even uncovered at least one subject faking his report of "seeing" the images. Under such conditions we cannot rule out the possibility that reports were generated without access to a phenomenological experience. Instead of resorting to disputes over whether subjects can be trusted, one should devise an experimental procedure so as to clearly eliminate such a hypothesis. For example, Comstock and Kittredge (1922) were able to elicit verbal reports of afterimages from children while explicitly avoiding any suggestion with a general instruction (e.g., "Tell me what you see; tell me all about it."). The verbal reports accurately reflected the existence of complementary colors, intensity of afterimage as a function of the stimulus, and so on, as predicted by a physiologically based theory of afterimages. Any model of the process that generates these verbal reports without accessible afterimages appears implausible.

How, exactly, are the verbal reports used to define eidetic imagery? From the verbal reports of attended and remembered information, Haber has analyzed the reported content of a picture, as well as information suggesting whether the reported content was directly attended or required retrieval from memory. Eidetic subjects could not be discriminated from noneidetic subjects on the basis of *what* or *how much* information was reported. However, eidetic subjects appeared to have some pictorial content directly available, as shown by their use of present tenses of verbs and the greater fluency of their reports. (Differentiation of recall and verbalization of attended information by verb tense has also been used by Benjafield (1969) in comparing retrospective reports to "think aloud.") In response to requests for specific information about the picture, Haber found the tense criterion to be a "powerful" one in discriminating between eidetic and noneidetic subjects. Let us look more closely at the model, which is proposed to account for the differences between eidetic and noneidetic subjects. In explaining why one eidetic subject used past and present tense intermittently while her eidetic image was supposedly available, Leask et al. (1969) suggested that, because of the incompleteness of the eidetic image, some requested information was not available, thus requiring retrieval and the use of the past tense. The other eleven subjects' consistent use of present tense while the eidetic

image was available then seems unreasonable, and was explained by suggesting a bias against switching tenses. The mixed use of tenses by the eidetic subjects after the image was reported to have faded doesn't lend itself to a simple explanation either. Even granted that some subjects actively retain some presented information in attention after the picture has been taken away, there are other consistent models (e.g. Gray and Gummerman 1975) that do not require a distinct eidetic memory.

In conclusion, we find that we can agree with Haber's call for more research analyzing verbal reports, yet the particular evidence cited by Haber does not present convincing evidence for eidetic imagery. Ericsson and Simon (1978) found in their review that studies collecting verbal reports have done so without much concern for control and methodological rigor, or alternative explanations, because the researchers felt these reports could not be considered anything more than suggestive evidence anyway. We agree with Haber's concluding remark that for verbal reports to achieve their appropriate status as an informative and significant data source for cognitive psychology, it is necessary to apply the same methodological rigor and theoretical precision as for other, already accepted sources of data.

by **Charles J. Furst**

Neuropsychiatric Institute, University of California, Los Angeles, Los Angeles, Calif. 90024

The inside and outside of eidetic imagery

Whether eidetic imagery (EI) turns out to be a scientifically interesting phenomenon or a red herring depends on whether an objective criterion can be found. During 1972–1974 I tested over 250 children for eidetic imagery and found 25 who satisfied criteria modeled after Haber and Haber (1964). My frustration in being unable to find strong correlates of eideticism together with an inability to arrive at an objective test for identifying eidetikers led me to become skeptical about EI.

One approach to finding an objective criterion is to examine the accuracy of subjects' verbal descriptions of their EI. Contrary to Haber's data, which gave no evidence for better pictorial recall for eidetikers than others, our results using a scoring procedure for open-ended responses (and thereby allowing for idiosyncratic coding for each *S*), showed that eidetikers tended to have better visual recall than most, but not all, other children. Eidetikers tended to be near the top of the distribution of recall scores, but still they were not off the distribution (Furst, Fuld, and Pancoe 1974).

Haber believes that accuracy of report should not be used as an objective indicator, but if the EI is not dense in information, as visual perception is, then how is it "visual" or different in kind from other forms of memory? Haber says that EI may be fragmentary, but if so, shouldn't eideticism imply something about the grain of the fragments?

One implication might be Haber's eighth criterion – a test of the ability to fuse two separate pictures by superimposing one (eidetically remembered) onto another (visible) to form a third or target image not present in either image alone. A strict fusion test, most people would agree, would form an objective demonstration of the reality of EI, since it would be beyond ordinary abilities. Despite Haber's report that four of twenty-three eidetikers could fuse his Figure 6, this stimulus does not quite satisfy the fusion criterion, since the target pattern is visible in the first component alone. The fact that his eidetikers could identify it may represent no more than that they are talented at visuospatial or pictorial cognition. This talent is suggested by the data on recall accuracy and also by other unpublished data I have collected showing that eidetikers are superior to other children in the Kohs Block-Design, a putative test of right-hemisphere cognition (Furst and Fuld, 1975).

My own efforts to devise an objective test for EI using the imagefusion principle were unfruitful, eidetikers and other children alike proving unable to fuse any of a variety of component images to find a figure revealed in their superposition. Gummerman, Gray, and Wilson (1972) were also unsuccessful in having eidetic children demonstrate their abilities with a fusion test.

Another approach I took was to devise a pictorial recognition task that might favor accurate visualization of picture fragments and that would not require verbalization of response (verbalization is said to inhibit the persistence of an EI). In this test, subjects were required to point to the spatial location of a sample piece from a large complex scene, following a standard procedure for inducing an eidetic image of the scene. We found that eidetic subjects were no better at this than other children. As a final stab, I tried to see if EI is related to an unusually persistent visual "icon." It wasn't.

In the absence of an objective criterion, it is difficult to see the utility of the notion of eidetic imagery. If EI does not *make* a difference, then it is reasonable to view it as not different in kind from ordinary visual memory, but merely a difference in the way some people construe some mnemonic experiences. When Haber says that the critical distinction is the visual-nonvisual one, he blurs the issue of whether EI differs from run-of-the-mill visual images, the kind most people have. That these ordinary "pictures in the head" are visual is clear, not only on phenomenological grounds but also from Shepard's demonstrations of structural isomorphism between this coding system and visual perception (e.g., Shepard and Chipman 1970).

The localization of an ordinary visual image inside one's head may be as arbitrary as placing it anywhere else – being based on a metaphor related to prevailing theories of the locus of mind in the brain. It could be argued that it makes about as much sense to say that a mental event occurred in one's head as to say it occurred in Cleveland: mental events, as Descartes was fond of saying, are "unextended substance" which don't occur, strictly speaking, in any place at all.

It is true that the phenomenology of EI, as described by Haber, is intriguing – that's what got us all interested in the first place – but these phenomenological reports may only represent the fanciful elaborations of suggestible subjects in a domain for which there are few rules of discourse. Wittgenstein (1953) and others (e.g., Sarbin 1968) have convincingly argued the socially determined nature of constructions of mental experience.

Some of Haber's "converging evidence" that an eidetic child actually *sees* EI externally located could be viewed merely as logical extensions of the situational demands. If a child has reported seeing an image on the easel in front of him, then it is reasonable for the child, when questioned, to elaborate by saying that the image falls off the edge when he tries to move it. Other bits of "evidence" can be viewed as properties equally applicable to ordinary visual memories – for example, reversals of Necker cubes (try imagining a Necker cube), fading of parts, quality related to recency of scanning, and so on.

Particularly troublesome for those who try to make sense of EI is the failure to find support for the developmental hypothesis. My own data contradict the assertion that EI declines with age. In fact, over second through fourth grades in our sample, we found an increasing age trend, adding to the puzzle presented by Haber's finding in a longitudinal study that only one of twelve eidetikers lost that classification with age. These findings are contrary to the hypothesis that EI is a primitive cognitive ability displaced by more analytic strategies, but they are consistent with the view that the behaviors of an eidetiker represent compliance with the considerable demand characteristics of the testing situation – compliance that would be expected to increase with socialization. Also, having in the past acceded to the experimenter's suggestion, a child would on retesting be motivated to make EI reports that were consistent with earlier ones. By this view, EI is not found among adults because there is something unconvincing about the testing situation for them, and they have more firmly acquired the prevailing metaphor for visual images, which is "in the head," rather than "out there." This would also explain the reported prevalence of adult eidetikers among certain African societies (Doob 1964, 1965), cultures that may be isolated from the "in the head" metaphor.

The assertion that EI is phenomenologically distinct from other images, but not distinct by any objective criterion, is unsatisfying from a scientific point of view, as Haber is good to point out. If there are no objective, operational means of distinguishing a true eidetiker from a subject who is merely compliant, then eidetic imagery has questionable utility for a science of mind, much like hypnotic age-regression,

dermo-optical perception, and other psychological wills-o'-the-wisp, which may very well have reality to those who experience them but which seem to make little difference on the "outside."

by **Alastair Hannay**

Department of Philosophy, University of Trondheim, 7000 Trondheim, Norway

Eidetic imagery: theories and ghosts

Ghosts and anomalies. Out of context Haber's concession to Gray and Gummerman about eidetic ability not being an anomaly might be seriously misleading. In a sense now familiar from discussions on the nature of scientific development, initiated by Kuhn (1962, p. 82), Haber's position is precisely that eidetic imagery does constitute an anomaly. It is not an anomaly in the specific sense, intended by Haber, of being a deviation from some "normal" function – a sense that allows the phenomenon to be saved, or the "ghost" exorcised, by being brought within the scope of the accepted theories of that function, as is similarly done when a phenomenon is viewed as an extreme case of a normal function. It is, rather, an anomaly in the more general sense of being a deviation from the natural (in this case psychological) order as such, a theoretically displaced phenomenon that can only be "saved" by some adjustment, more or less radical, in our theoretical grasp of the whole area, including whatever appears to be normal in it.

Haber's metatheoretical and methodological remarks make it quite clear that he does regard eidetic imagery as an anomaly in Kuhn's sense. He says (in his "Personal preface") that the going theories of perception "have no way to describe, let alone account for, this imagery." It is less clear perhaps that in his concluding advice to "ask more questions about the phenomenological indicators of perception, not less," he is advocating that kind of more or less radical revision of the current theoretical framework that Kuhn claims is the eventual outcome of the recognition of a theoretically recalcitrant phenomenon. But the advice at least opens the way to the sort of revision of what counts as acceptable evidence for theories of perception (and memory) that would make the more radical revision of the theoretical framework itself possible. It is this aspect of Haber's paper that I shall comment upon.

Theories and phenomenology. Fear of phenomenology in science seems to have two distinct roots: on the one hand the strictness of current requirements of proof and objectivity and on the other the difficulty of fitting the objects of phenomenological descriptions into the current scientific world view. Though they are distinct, these two roots are obviously connected. Any relaxation of the requirements that brings the objects of these descriptions (what I have called "proximal" events, etc., Hannay 1977) out of the shade immediately makes more glaring the difficulty of fitting them into the conventional unispatial conception of the universe, and a fortiori into the theories of perception that conform to that conception. Such theories are of two general kinds: those that say we say have direct visual access to a common public environment and those that say that perception is an indirect response to that environment from a location within it (i.e. not in some private space).

I have two comments. One is that despite its oddity, eidetic imagery is really only one of a range of phenomenologically distinguishable phenomena that call in question the adequacy of the current theoretical framework. Along with it must be included other forms of mental imagery, for example, memory images (according to Haber's somewhat misleading terminology these are not "visual," while eidetic images are, but in a sense of course they are and we must distinguish between noneidetic visual memory and memory unaccompanied by any visual imagery at all), deliberate visualizing, hallucination, qualities of perception not derived from environmental stimuli, and so on. What may be peculiar to eidetic imagery (though perhaps it shares this peculiarity with hallucination) is its apparent unsortability, being, as Haber concludes, neither perception nor memory (though he accepts that it is a "memorial" phenomenon in a minimal sense, "just as is any verbal description of the content of a previously seen stimulus"). But the fact that some function or phenomenon or other is sui generis does not in itself constitute a threat to the theoretical framework in which we try to describe it; any framework must allow for the possibility of irreducible kinds. The critical problem is how to account for the possibility of the phenomenon, however we choose to describe it, and that problem arises for the whole range, not just for eidetic imagery.

The second comment qualifies the first. Perhaps eidetic imagery is, after all, the most fruitful of these phenomena for purposes of theory construction, not just because it has been more thoroughly researched than the others in the range – though that is one factor – but also because it combines problems general to the range with a number of special puzzles of its own. Why, for example, is it found as a rule only in children? What accounts for variations in eidetic ability from subject to subject? To what extent, and in what sense, is it more a "visual" than a "cognitive" function? And so on. Therefore I would like to append to this comment a suggestion as to the kind of theory that might effectively exorcise the ghost of eidetic imagery, not by denying its existence (some scientists try to do that by appealing to the requirements of proof and objectivity, but that is a matter of bad faith), but by providing a theoretical basis for its existence from which it is also possible to derive testable explanations of its special features.

According to Haber's conclusions (whose tenability I am not qualified to doubt) a theory of eidetic imagery would have to explain (1) how eidetic images can be scanned, (2) how they can be shifted, inverted, and altered in size, (3) how some details of the withdrawn stimulus can be omitted, others added, and others moved, (4) why blinking, looking away from where the stimulus was, and shifting one's gaze to a new stimulus are routinely effective ways of terminating the image, (5) why "active, cognitive rehearsal of the stimulus" is a routinely effective way of preventing an eidetic image's occurrence, and (6) why as a rule only, but then again not all, children have eidetic ability.

The primary puzzle is surely how a perception (of a stimulus) can become, without appreciable alteration in the subject's visual experience, a kind (though "qualitatively different kind") of memory representation of the same stimulus. But if perceptual experience in general were related to the stimulus world as visual effect to *non*visual cause (Haber's stimulus is of course a visual one), and cause and effect were interpreted as logically and temporally distinct, eidetic imagery *could* be treated as a deviant form of perception, in other words as an anomaly in Haber's sense, and thus relieved of its theoretical recalcitrancy. It could be construed (nonanomalously in Kuhn's sense) as a special case of a visual effect of a nonvisual cause (or rather causes), the peculiarity being that here the subsequent causal chains emanating from the relevant parts of the stimulus world (e.g. those corresponding to the empty easel) fail to reach the visual stage, fail to oust the visual effect of the previous chain(s) as they do in normal perception. (Here there would be more point in calling eidetic imagery a perceptual phenomenon than a memorial one, though there may well be cases of eidetic imagery that come closer to memory because they are unconstrained by the kind of experimental setup described by Haber, which links the image to perception via the stimulus.) The question would then be, what inhibits these subsequent chains before their visual completion? Not a very mystery-laden question. In these general terms it would even make sense to conjecture that the inhibiting factor was once a "natural" feature of perception and eidetic ability the rule rather than the exception. It might be further conjectured that the ability as we now know it is a case of ontogenetic playback, the survival during a fairly short period in an individual's development of a function once prevalent throughout an individual's life. Perhaps it once served an evolutionary purpose, but altered circumstances, or the development of better means for serving that purpose, put it out of fashion. My point is not, of course, that such conjectures are plausible, but only that within a framework of this kind they make sense.

Although Haber's paper is not concerned with the construction of alternative theoretical frameworks, it obviously enjoins that topic on those who stand by the theories to which they are more or less happily wedded. His call for greater attention to the experimental psychology of phenomenology (with all the methodological adjustments that implies) is surely justified, as is his claim that this is a more fruitful approach than the continued search for correlates among functions

measurable by the conventionally accepted methods. But if the difficulty in finding satisfactory correlates is due to an inadequate theory of these functions, it may be that once a better theory comes along the search will be more successful. Even so, and before one looks for correlates, it is obvious, indeed it *should* be so obvious as to be a platitude, that the phenomena should be allowed to have their say first.

by **Klaus Heinerth**

Institut für Pädagogische Psychologie, Universität Frankfurt/Main, D-6000 Frankfurt/Main, W. Germany

Autochthonous and phenomenal eidetic capacity

Haber's target article invites certain objections. He has evidently performed exactly the same experiments I have (as in his typical child-experimenter dialogue, typical behavior of the Ss, typical phenomenology), but he comes to different conclusions. I shall briefly summarize my experiments and the conclusions arising from extensive statistical analysis (Heinerth and Nickel 1975) and intensive deterministic research (Nickel, Heinerth, and Bittmann 1975) in the form of theses and hypotheses.

1. Studies of eidetikers have an element of direct, personal involvement (in Haber's studies as well as my own).

2. Although essential, this direct involvement is dangerous: it introduces bias in spite of all precautions. This bias is clearly visible in Haber's convictions, which are based on interpretations, as opposed to imperative facts. At first, I was inquisitive, then an ardent exponent of eidetic imagery, and finally a skeptic, unwilling simply to accept evidence but seeking rigorous demonstrations (such as Stromeyer and Psotka's 1970 successive stereoscopic images).

3. Suggestibility plays an important role, not only with subjects (Ss) but also with the experimenter, who considers the "typical" verbal and nonverbal expressions of Ss convincing. I have myself been seduced by subjective testimony, and I have insisted upon strict criteria.

4. Eidetic function does not exist independently of criteria. It is useful to differentiate between two kinds of eidetic capacity: autochthonous and phenomenal.

5. Autochthonous eidetic function is extremely rare (<1%). It should be regarded as present only if testing criteria compellingly demonstrate a phenomenon sui generis (so far this has only been achieved by Stromeyer and Psotka). Such a rare phenomenon really should not be regarded as an object of general psychology. All Haber's other criteria seem to be obvious, but they are insufficient, even the eighth one. It does not have the rigor of Stromeyer and Psotka's method. Haber discredits the stereoscopic method as being overly restrictive for testing children. I cannot accept this: as long as this method is the only one that convincingly demonstrates the existence of eidetic capacity, it should be refined but not rejected.

6. Any other eidetic endowment should be regarded as occurring only at the phenomenal level. It differs from afterimages but not from visual concepts which form a continuum with eidetic images.

7. The objections raised by Haber against such a continuum are plausible but not conclusive. As long as there are no opposing reasons, one's interpretations should be conservative: phenomenal eidetic capacity is not an additional endowment but an extremely good capacity for visualization, closest to guided daydream ("catathymically" experienced images in the sense of Leuner 1970).

8. Daydreams differ from eidetic images by degree: their images are generated by phantasy, while eidetic images constitute memories of stimuli. Manipulation of the images as well as training is possible in both cases.

9. Eidetic function is a ubiquitous phenomenon experienced by children. It fades during cognitive development if it is not further trained (children are amazed to be told that adults cannot "product images"). The fact that no correlations between cognitive development and eidetic capacity have been found does not mean that there are none. (The fact that no needle has been found in a haystack does not prove that there is none.)

10. I prefer to remain conservative in considering the phenomena of occurrence and age. Eidetic function tends to be opposed to normal cognitive development. It is highest at the time of the smallest cognitive rapport of Ss (during the earliest childhood) and lowest when the rapport is tight. (This makes experimentation quite difficult.) Longitudinal studies may obscure correlations between eidetic function and age. Continuing testing trains performance, but eidetic ability itself seems to remain stable.

11. Phenomenological research is indeed called for. There has been enough extensive research: intensive work is necessary, if better testing methods are to be developed. My own hypothesis is that imagination can be trained, and even adults may be able to learn to visualize eidetically.

12. As for the rest: eidetic function is a problem for noneidetics only!

by **Dennis H. Holding**

Department of Psychology, University of Louisville, Louisville, Ky. 40208

Does being "eidetic" matter?

For most people, the exciting feature of the eidetic hypothesis has been the possibility of revealing vivid detail in imagery. Casual familiarity with the field probably conjures up the Allport (1924) illustration of the introductory text, with children magically deciphering "Gartenwirthschaft" from an incomprehensible visual display, and perhaps the even more dramatic Stromeyer and Psotka (1970) demonstration of delayed synthesis of Julesz patterns. Unfortunately, as Haber emphasizes, these feats are quite atypical of eidetic performance.

Haber's eight criteria are of several kinds. The first three, though phenomenological, are viewed as the primary distinguishing features of eidetic performance; they deal with the subject's report of an externally referred image of relatively long duration. Criterion 4, concerning use of the present tense, is clearly implied by the earlier criteria. Criteria 5 and 6, dealing with eye movements, are "technical" in nature and only provide corroborative evidence if the phenomenon is already established. Criterion 7, regarding accuracy of report, has been discounted by most researchers and is often achievable by noneidetic subjects. Criterion 8, requiring the synthesis of decomposed stimuli, seems potentially the strongest, but this too must be abandoned if eidetikers are to be regarded as a homogeneous group. Thus, for instance, the 2 out of 270 subjects identified as eidetic on the "seeing it out there" type of criterion, both retardates, were unable to carry out either of Gummerman, Gray, and Wilson's (1972) superimposition tasks, and Haber's own data include few subjects with both types of ability. In fact, the overall impression gleaned from the paper is that we must doubt whether all of the eight criteria are selecting the same population. This impression is strongly reinforced by the Paivio and Cohen (1977) analysis, which shows that the "eidetic" factor is loaded neither on accuracy of report nor on the ability to synthesize percepts.

It appears, therefore, that we have essentially to deal with the group of criteria based on the finding that the eidetic subject somehow ascribes his image to the real world, locating it outside rather than inside his body schema. But this seems almost trivial, unless further consequences can be demonstrated. Where people locate their images and percepts may not be completely arbitrary, but this type of ascription is certainly manipulable. At the level of auditory localization we have examples like Jackson's (1953) demonstration that a whistling kettle is referred to the observed vapor source rather than the actual sound source. At the level of images versus percepts there is the classic demonstration by Perky (1910), whose subjects thought they were imagining objects which, in reality, had been faintly projected onto a screen by the experimenter. Segal (1971), who has repeated and considerably extended this work, points out that it complements an earlier experiment by Külpe (1902). The earlier work shows a reversed Perky effect. Subjects who were shown a dim and fluctuating test stimulus, such as a red square, often developed hallucinatory percepts traceable to autokinetic, entoptic, and other subjective phenomena, *but thought that their percepts were real.* In fact, it was the apparent lability of the decision whether percepts were classed as imaginary or as generated by the experimenter that suggested that the

Perky (1910) experiment might be viable. Finally, Segal (1971) concludes on the basis of her own work that "the decision concerning reality is essentially a probability decision" (p. 96).

One might further argue that attempting to discover where subjects locate their images is of no great interest for most purposes. What one needs to know is whether people classed as eidetic on these grounds can do anything that distinguishes them in other areas of performance. At the moment, the answer seems to be that they cannot. If this area of research is to make any progress, something approaching a new paradigm is needed. The further consequences of being eidetic, if any, must be made to yield significant hypotheses for experimental test. Haber has presented a good deal of the accumulated data on individual differences, the confusing discrepancy between the prominence of eidetic phenomena in childhood and the absence of longitudinal change, the lack of relation with developmental deficits, and the dubious validity of cross-cultural studies. However, he has not proposed any further experimental procedures. If it could be predicted and verified that eidetic subjects can supply their own unmasking of metacontrast, or that they generate different slopes of information gain from noisy histoforms, or if any other solid consequences were indicated, the distinction between eidetic and noneidetic subjects would be noteworthy. However, nothing of the kind is foreshadowed in the paper, nor is the suggestion made how that ascription of "reality" to images might be further explored. As things stand, one is led to conclude that the eidetic hypothesis is relatively sterile.

by **Ian M. L. Hunter**

Department of Psychology, University of Keele, Staffordshire, ST5 5BG, England.

The easel procedure and eidetic characteristics

My comments concern the easel procedure and the preconceptions governing its use and interpretation. The procedure was devised by Jaensch to explore his theory about afterimages, eidetic images, and memory images. And Haber's own interest in the procedure was prompted by a possible physiological mechanism, namely, a sensory aftereffect that persists for some minutes and manifests itself autonomously as eidetic imagery, provided there are no masking or distracting circumstances. Such a mechanism is not inherently implausible: consider McCollough Effects, also those long-lasting visual aftereffects that can follow a day spent fishing in rippling water. However, if I read Haber correctly, he proposes to abandon this notion, at least in the context of the easel procedure. I agree with this proposal. But although Haber may reject the notion, it still colours his discussion. Also the notion, or something like it, permeates the easel procedure: witness the elicitation of afterimages and the manner of interrogating subjects.

Let me make my own assumptions explicit. First, remembering (and also imagining) can be more or less eidetic in its characteristics; that is, the remembered (or imagined) scene can be treated as if it were more or less perceptually present. Haber lists some of these characteristics which can vary in strength and also vary independently of each other. So, we should not classify people as all-or-none eidetikers but, rather, describe remembering performances and the ways in which they are eidetic. Second, when eidetic imaging occurs in the easel procedure, we are witnessing not an autonomous sensory aftereffect, but a remembering that is rich in various eidetic characteristics.

These two assumptions cast the easel procedure and its findings in a fresh light. They make plain that the procedure is not reliably free from demand characteristics and interpretations which may fluctuate from one investigation to the next. They accommodate the otherwise anomalous finding that some subjects "revive eidetic images" at later times. They make sense of the finding that the presence or absence of eidetic characteristics does not necessarily relate to the amount and accuracy of what is remembered. They also raise the question: is the procedure not a niggardly and chancy way of eliciting remembering? In other words, may the procedure militate against the occurrence of remembering which may, on occasion, turn out to have eidetic characteristics?

I pursued this question by imagining myself as a subject. Following preliminaries involving afterimages, I am shown a picture to examine. Now comes the critical point. The picture is removed, and I am asked to continue looking at the easel and report what I see. My answer would be "nothing" or "a grey surface." Thereupon I would probably be classed noneidetic (the literature says little about the reactions of subjects so classed). But suppose, on removal of the picture, I were asked to imagine, to the best of my ability, that I still see the picture and that I should describe this imagined picture. I would probably conduct myself so as to be classed eidetic.

I have proceeded to conduct some rough-and-ready experiments with four colleagues who happened to have been available: a technician specializing in audiovisual aids and three university lecturers. I explained that what I was about to ask of them might seem slightly crazy. I placed a coloured picture (the cover of a magazine *The Great Outdoors*) on the table. I would ask them to look at it and, ignoring the printed wording, consider what the picture showed, note the things in it, their colours and how they related spatially to each other. Then I would cover the picture with plain cardboard and ask the subjects to imagine they could still see the picture underneath.

When I covered the picture and asked, "What are the main things in the picture?," subjects replied, in the present tense, by mentioning contents. The technician spontaneously accompanied his descriptions by tracing them on the blank surface, and the lecturers resorted to pointing when I asked, "Where is that?" Subjects answered supplementary questions like, "What is the colour of the rucksack?" and "Is there anything on the ground behind the hiker?" When I pointed to different parts of the surface and asked, "What is there?," answers were appropriate. Any eavesdropper would suppose we were talking about a perceptually present picture. The only hint that we were not so doing was the occasional past-tense comment "I didn't notice that" when I asked about some detail such as, "Is the hiker wearing a wristwatch?"

I do not suggest that these subjects evinced eidetic imagery as traditionally conceived. But their remembering did have some eidetic characteristics and provided a sympathetic start to exploring how fully and strongly they were remembering eidetically. As it happened, two lecturers reported, one more vehemently than the other, that their remembering was based on verbal encoding and involved nothing describable as "actual seeing." The remaining lecturer suspected he was merely persuading himself that he was actually seeing. The technician furnished a much fuller repertoire of eidetic characteristics with comments like, "You'd think the cardboard was transparent," and, "You could draw round the shapes."

Surveying the literature about the easel procedure, we meet implicit preconceptions, some of which may work against the appearance of eidetic remembering. We meet a historical trend toward tightening the criteria by which remembering is classed "genuinely eidetic." We meet confirmation of the finding that intrigued Galton (1883), namely, children's remembering tends to be more readily and fully eidetic than that of most, but not all, adults. Galton, and later Werner (1940), sought to account for this finding by two plausible hypotheses: the demands of "intellectual" pursuits lead people to encode verbally rather than visually; and the demands of self-analytic reflection lead people to differentiate subjective and objective phenomena (see also Piaget 1929).

Finally, Galton suggested that when adults undertake pursuits that, like drawing, demand more careful attention to visual properties, their remembering becomes more eidetic, at least in some contexts. Assuming a merit in exploring human functioning by starting with highly accomplished real-life instances of it (Hunter 1979), Galton's suggestion is a worthwhile opening for future studies of eidetic remembering, its ecology and its uses.

by **Julian Jaynes**

Department of Psychology, Princeton University, Princeton, N.J. 08544

Palaeolithic cave paintings as eidetic images

I propose the hypothesis that the well-known cave paintings and engravings at Lascaux, Altamira, and in about a hundred other caves in southern France and northern Spain, dating somewhere between

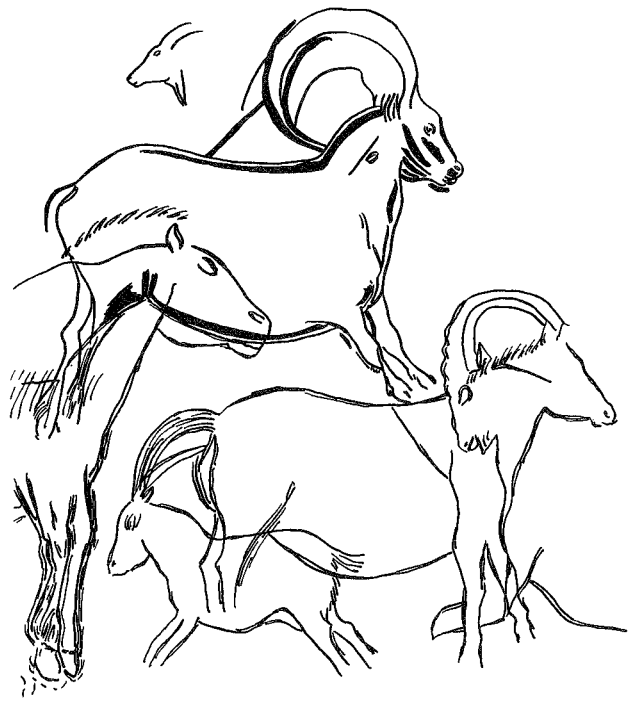

Figure 1 (Jaynes). Engravings on a fallen slab of stalagmite from the cave of La Mairie at Teyjat. Note the seeming double vision of the head of the bison as if the eidetic image has slipped (after Breuil 1952; artwork by Judith Economos).

20,000 and 10,000 B.C., are tracings of eidetic images rather than art in its usual sense. The purpose of this note is to discuss whether Haber's thorough and welcome revival of the topic sheds any light on this issue, and conversely whether consideration of such a hypothesis suggests new ways to study the still perplexing and inconsistent phenomena of eidetic imagery.

The arguments in favor of the hypothesis are as follows: (1) the cave paintings cannot be art in our sense of being meant for public display or ritual observance since they are usually located in the most difficult and inaccessible positions inside the caves; (2) if they were drawn by artists in our sense, we would expect to find preliminary atttempts, as if learning to draw, but such are not found; (3) if drawn by artists in our sense, we would expect a homogeneity of skill regardless of subject, but we do not: the paintings or engravings of animals (about 90% of the total) are of an astonishing sweep, beauty, and likeness, while those of humans are almost entirely sticklike and not likenesses at all; and no scenery is ever attempted; (4) the most prominent reason to think of these paintings and engravings specifically as tracings of eidetic images is their common superposition one over the other, as if each animal was projected on the cave wall regardless of what was there in the first place.

Particularly because of (4) – which I find difficult to understand otherwise – I think we should take the eidetic hypothesis of the origin of these paintings seriously. But if we do, we are assuming much stronger and longer-lasting properties of eidetic imagery in Cro-Magnon man than we find in laboratory studies of the phenomenon today. We must posit that an individual out on a hunt with his group, perhaps stared into the dusk waiting to see the hunted animal; and then finally, either as the animal was at bay or as it was killed, registered the eidetic representation, which, after returning to the deep interior of his cave, he then by torchlight traced out with charcoal and colored ochres on the wall (or where the surface was softer, engraved with a pointed stone) – and all in disregard of what had been drawn there before. (I assume here that the deeper engraving and scraping techniques which are sometimes found were reworkings of previous paintings – for which there is considerable evidence.) Such a picture of the Cro-Magnon painter is full of new assumptions that seem implausible from what we know of eidetic imagery in the laboratory.

It assumes that an eidetic image under late Pleistocene conditions is not "blinked away" as in Haber's studies, nor does the image "fall off" when transferred to another background. But we should recall that at least some eidetic children today can shift images to any surface at will, superimposing them over any subject, and can even change their size as they wish (Leask, Haber, and Haber 1969). The last characteristic is consistent with the lack of relative size constancy in the cave paintings.

It assumes that from a brief exposure, the Palaeolithic eidetic image can be maintained over longer periods than is commonly found in laboratory studies. But the duration in modern studies outside the laboratory can be longer. One investigator reported what seem to have been spontaneous eidetic images in Tanzanian children, which often lasted twenty minutes or more (Doob 1970).

A further difference is that we usually think of eidetic imagery as being a scene, whereas the cave paintings are of solitary animals without any vegetation or scenery. But one thing that all eidetic researchers agree on is that the only way for a subject to get a complete picture is to look at each part of it for enough time; parts omitted are not seen, though they may be remembered on questioning. Hence in the Cro-Magnon situation, it is only the emotionally salient target animal and sometimes only a part of the animal that is stared at and registered, not other hunters or scenic background. (The poorly drawn human figures may indeed be attempts to draw noneidetic memory images, or they may have been added a century or a millennium or more later, as I suspect most of the nonpictorial slashes and graffiti were.)

Whatever these somewhat tenuous comparisons suggest, the huge difference between the Palaeolithic and the contemporary situation is the emotional salience of the image. The animals on the cave walls and ceilings are life and death matters. They are those hunted for food in the desperations of the last glacial age or else dreaded predators such as lion or bear. The importance of such targets to the Cro-Magnon hunter and the waiting dangerous excitement connected with his vision of the animal is in high contrast with the bland picture-book stimuli in the usual eidetic study.

While there is no necessary reason to think that the brain of a modern eidetic child and that of a Cro-Magnon adult 15,000 years ago demonstrate the same processes in so esoteric an area, nevertheless discovery of the pertinence of these variables in the modern condition, particularly of emotional salience, would be some support for the hypothesis.

These possibilities could easily be studied by using pictures of stronger emotional and personal impact, such as food displays with hungry subjects, photographs of the subject's family in various settings after some separation as in children's hospitals, or highly desired objects such as a bicycle or other toy that we know (by pretesting) the child impatiently desires, and then comparing the results with control pictures. The waiting expectancy could be simulated by using projections slowly brought into focus or intensity in dim light and compared with the same pictures presented for the same duration in the usual way. Moreover, dark adaptation of the subject might in itself produce a longer lasting eidetic image, since a bright surround may actually wash out the image. In the study of Tanzanian children with eidetic images of long duration, such images were usually seen one or two feet in front of the eyes *in darkness* as the child lay down for sleep.

The most interesting of Haber's results to me is that nearly all eidetic children can (and commonly do) prevent an eidetic image from forming by naming the items in the picture while seeing it. This immediately suggests that in the usual procedure, the naming of the objects in the testing for imagery (see Haber's protocols) may have hastened the fading of the image. It would thus seem that a nonverbal technique of testing for the image would be preferable, such as asking the child to trace the eidetic image on the blank easel with a felt pen or crayon [cf. *Neisser,* this commentary] – even perhaps as did his Upper Palaeolithic ancestors in a different way and context.

One of Haber's conclusions can, I think, be questioned in part. This

is that fidelity of the eidetic image to the stimulus is no better than memory, a conclusion that has a considerable history in eidetic research. My difficulty has to do with how we can compare what are almost two different modalities. It does not seem to me correct to compare the verbal replies to analytic questions about numbers of items and details, since two such different procedures are involved: the eidetic where the subject is not to verbalize, and memory where analytic verbalization is encouraged. What we really want to know here is the accuracy of depiction, not accuracy of report. Because of the verbal erasing factor, this could be better studied by again comparing tracings by eidetic children with memory drawings of the same picture by noneidetic children. I would suspect that such a study might return the accuracy criterion for eidetic imagery to its former popular favor.

To sum up, I have suggested that if the hypothesis that cave paintings are eidetic images is correct, we might find that the strength, duration, and accuracy of modern eidetic images could be increased by using stimuli of greater emotional salience after an expectancy period in dim light and asking the subject to trace out the image on a blank easel instead of verbally reporting about it. If none of these new variables proved pertinent, I think the hypothesis would be weakened.

And if one appreciates the total situation and the number of individuals involved, one might conjecture that the hypothesis would not have to be completely abandoned. We commonly call these paintings Palaeolithic art with the unthinking connotation that the paintings were made over ten thousand years or more of the Late Pleistocene as a cultural tradition. That this is false can be seen from the fact that there were at best fewer than 100 paintings in any one cave, which (if it were a cultural tradition over this period) would work out to be only one painting every century or two. I think this is absurd, particularly when we remember that primates and particularly human primates tend to continue and even teach a learned activity of this sort that is suddenly discovered and found pleasurable. It is thus more plausible to think of all of the paintings and engravings in these one hundred caves as being done over only a few decades by only a few Cro-Magnon persons, a unique few whose strong eidetic imagery may have been more similar to the extreme of Luria's adult mnemonist (Luria 1968) than to the usual eidetic child.

by **Bela Julesz**

Bell Laboratories, Murray Hill, N.J. 07974

Random-dot correlogram test for eidetic imagery

Questions as to the existence and nature of eidetic imagery are of great theoretical importance to psychobiology. Haber's target article, in not letting researchers' interest in this enigmatic problem wane, makes a real contribution to this field.

Although I have never worked on eidetic imagery myself, I have consented to be a commentator, since in my book, *Foundations of Cyclopean Perception* (1971), an entire chapter is devoted to the Stromeyer and Psotka (1970) study. These investigators tested an unusually able eidetic subject with the technique of random-dot stereograms. I thought some of my colleagues might be interested in my present opinion on this problem, almost a decade later, which Haber's excellent review helps me to formulate.

In my commentary the emphasis will be on Haber's "eighth criterion," that of the subject's (S's) being able to build up a composite image from partly eidetic, partly physical, components. I regard this criterion alone – particularly in the case of random-dot stereograms and correlograms – as objective enough to establish and quantify eidetic imagery.

Before I go into detail, let me outline an "unfakable" test for eidetic imagery and then discuss how close some investigators have come to such a test.

1. An ideal test for eidetic imagery. As I pointed out in my book, my main interest in the Stromeyer and Psotka study was the use of random-dot stereograms, which I still regard as an "unfakable" test for eidetic imagery, provided the cyclopean (monocularly nonexistent) information is known to S (and preferably to the experimenter, too). Indeed, if one generates a *single* random-dot stereogram half-pair

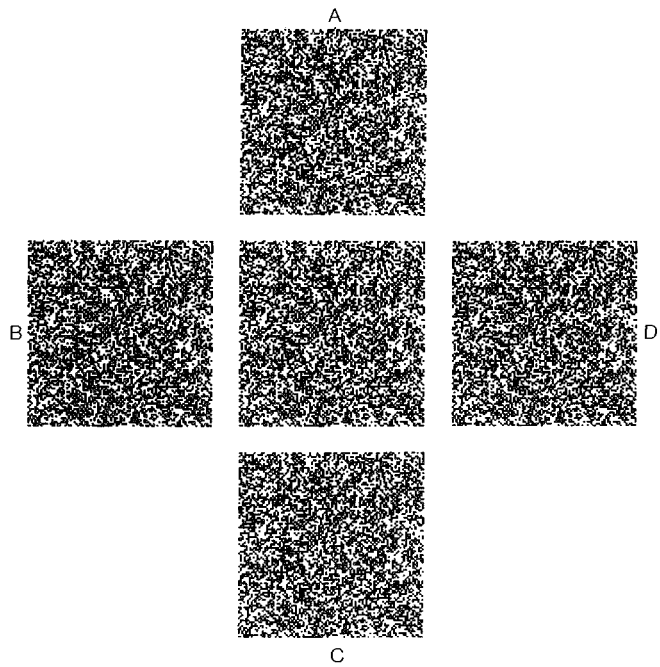

Figure 1 (Julesz). Test random-dot stereograms for eidetic imagery. Center matrix is inspected until an eidetic image is built up. When the surrounding matrices are viewed, fusion with the eidetic image should reveal a square (A), cross (B), diamond (C), or disk (D).

(presented, say to the left eye) and a *series* of corresponding stereo half-pairs (shown to the other eye), then when the left image is fused one by one with the right images, a series of cyclopean forms can be successively obtained that, a priori, are unknown to either subject or experimenter. In my book (Figs. 8.1-1, a,b,c, and d; reproduced here as Fig. 1) a common random-dot left image is printed, which, when fused with four corresponding right images, portrays a square, cross, diamond, and disk hovering in vivid depth above the background. These random-dot stereograms are typically 100 × 100 picture element arrays, and the cyclopean shapes contain many random black and white dots. However, a random-dot stereogram containing only a few hundred dots (picture elements) can portray rather complex cyclopean forms. Ideally, a 100 × 100 random-dot stereogram can portray highly perceptible 3-digit alphanumeric characters in depth.

Now, the subject is asked to view the common half-image with one eye, and to build up an eidetic image by inspecting it for long periods, allowing it to be refreshed any time it seems to fade. The corresponding half-pairs are presented in succession after a fixed latency to the other eye, after the image to the first eye is occluded. Only if S has eidetic imagery (and functional stereopsis) can the hidden cyclopean messages be reported. The advantage of this method is that only a single random-dot array has to be stored by S throughout an entire session, while many independent tests can be performed with this same eidetic image.

A variant of this method is to use random-dot *correlograms* instead of random-dot stereograms (Julesz 1971). Correlograms are similar to stereograms except that the cyclopean shapes are not the result of several binocular disparities; instead the left random-dot array is identical to the right array (i.e. has zero disparity) except for some uncorrelated (or negatively correlated) areas. When binocularly viewed, the identical areas give rise to binocular fusion, while the uncorrelated (or negatively correlated) areas yield binocular rivalry. In a typical test one can use a 100 × 100 randomly black and white dot array for the common stereo half-image, while the other half-image is identical to the first, except for, say, a 3 × 3 dot array that is its negative (i.e. negatively correlated). The position of this small rivalry-inducing patch will vary in successive tests, and the subject has to

report which quadrant contains this patch. A noneidetic subject can guess correctly on each trial with probability = 0.25. The odds, however, of guessing, say, 20 successive patches' positions correctly is infinitesimal. Obviously, the coordinates of the uncorrelated patches can be randomly generated by a computer, so neither subject nor experimenter knows the answer at the time of the test. (Present computer technology permits the generation of a 525 × 525 random-dot stereogram in 33 msec.) Changing the patch size might permit the quantification of the spatial resolution of the eidetic image, provided one could discover anyone who could perform such a series of tests.

2. How close have experiments come to the ideal one? Let us now examine the few existing tests that have emulated to some extent the ideal tests outlined above. The first such test, proposed by Leask, Haber, and Haber (1969) based on monocularly superimposing two part images (one eidetic, one physical) in a composite one, is described in Haber's target article. This technique of composite pictures has two disadvantages. The images typically used are not random-dot arrays, so in theory S could encode the contoured figures in some other form, making quantification difficult. But, more important, as Haber himself points out, S might be eidetic, but unable to perform the demanding task of moving, in registration, (i.e. with corresponding coordinates exactly aligned), two images in the same eye. On the other hand, for subjects with functional stereopsis (98% of the population), the alignment of the left and right image (if presented within Panum's fusional area) is accomplished automatically by the neural machinery of stereopsis. So, if the ideal test using cyclopean stimuli is presented such that S is asked to scan one stereo half-pair in the vicinity of a fixation marker, and then the second stereo half-image is presented in close spatial registration, the large identical backgrounds are fused, coalescing in the two eyes' views.

The fact that only four of Haber's twenty-three eidetic children, and none of his larger group of children and adults, could perform the composite task attests to the difficulty of this test. I only wonder whether, if he had presented the composite images *dichoptically,* with a large identical random-dot frame around them to facilitate superposition, better results could not have been obtained.

Now, let us turn to the Stromeyer and Psotka (1970) report. The subject used – a twenty-three-year-old, intelligent young woman – had remarkable eidetic capabilities. Her performance on pictures and reading tests (while measuring her alpha rhythm) has been independently reported by Pollen and Trachtenberg (1972). That she could store 1,000 × 1,000 random-dot stereogram half-pairs and correctly report complex surfaces emerging even after hours of delay is a most remarkable feat.

It is a pity that Stromeyer and Psotka used only my published stereograms, limited to a dozen or so cyclopean shapes – the same stereograms I used in my many talks at universities and scientific meetings. Using such a limited and widely known set of stereograms has many obvious pitfalls. I only hope that if another "supereidetiker" is found, the random-dot correlogram test (outlined above) will be applied.

It is important to stress that Stromeyer and Psotka's finding was recently corroborated by Walker, Garrett, and Wallace (1976), who used three 100 × 100 random-dot stereograms from my book (portraying a T-shape, a triangle, and a Necker cube). Twenty adult subjects were hypnotically regressed to seven years of age, and after a 60-sec delay between the eidetic and physical image presentations, two subjects were able to report the three cyclopean shapes correctly. The same subjects could not perform this task under normal waking conditions or in neutral hypnosis (without age regression). "During the postexperimental interview, the two eidetikers both mentioned that as a child (7 or 8 years old) they recalled being able to look at a picture and later reproduce a clear image of it," Walker et al. wrote in this astonishing report.

3. Discussion. The Walker et al. (1976) experiment comes close to the ideal test proposed above, but is still unnecessarily strict. The random-dot correlogram test is simpler and might give a better "yield," although their 10% success rate (2 out of 20) is similar to that of Haber and Haber's (1964) 8% (12 out of 179) eidetikers among children. The important contribution of Walker et al. is to make it possible to increase markedly the number of eidetikers in the available population by using hypnotic age regression. This, combined with the random-dot stereogram test (or perhaps improved by using random-dot correlograms), opens up the scientific study of eidetic imagery.

Of course, I can imagine how many of my colleagues who were skeptical before might increase their disbelief when hypnotic age regression, another enigmatic phenomenon, is added to the elusive eidetic phenomenon. If eidetic imagery were just an oddity, I would agree with the skeptics: why bother? However, eidetic imagery, particularly with the high resolution and long persistence some of the supereidetikers exhibit, is an "existence proof" that the central nervous system is capable of storing a huge amount of information for long periods. Whether this detailed storage of texture is for minutes, hours, or days is an intriguing question and should be pursued by any reproducible means available.

When we are confronted by some extremely rare mental phenomena, exhibited by a select few individuals, their implications for brain research are perhaps more important than the phenomena in themselves. The music of J. S. Bach, or the theories of Einstein, are great treasures of mankind, but more important, the existence of such individuals illustrates the depth of human creative capabilities. I am in full agreement with Haber that the goal of establishing and describing eidetic phenomena is worth pursuing, since there is hardly any problem more interesting than the structure and detail of visual memory.

by **Israel Lieblich**

Department of Psychology, The Hebrew University of Jerusalem, Jerusalem, Israel

Eidetic imagery: do not use ghosts to hunt ghosts of the same species

We are going to play a game with scientific knowledge. Here on this page I am going to show you a summary of scientific knowledge related to eidetic images. Look at it as long as you wish but I do not want you to stare at it. You may move your eyes. After you finish, go back to Haber's target article, which describes twenty years of haunting eidetic imagery mainly by use of phenomenological indicators, and indicate what if anything is new.

> Some individuals after examining a picture and then being asked to project their image of it on a gray screen, behave as if they were still actually seeing the picture and can describe it in great detail. Such an "eidetic" image is sometimes regarded as a special kind of image, almost photographic. Jaensch (1920) was largely responsible for attracting attention to this phenomenon. It is fairly common in children but rare in adults. It may be related to personality traits. But we know little about the actual nature of the eidetic image, despite a large number of studies; good summaries will be found in Allport (1924) and Klüver (1926, 1928, 1932). At present we cannot tell how the eidetic child establishes his image or whether it is qualitatively different from the ordinary strong visual imagery of many people. (Woodworth and Schlosberg 1954, p. 722)

Following your own comparison, I would like to propose the main thesis of the present commentary. It is high time to try to attach to each research program in modern psychology a "monitor," which would indicate periodically whether the given program develops or degenerates. Lakatos (1970) called attention to such progression and degeneration of scientific programs. I would like to propose, on the basis of Haber's review, that the pure phenomenological program in eidetic imagery is a clear failure, and thus, contrary to Haber's suggestion, less rather than more research on pure phenomenological indicators of perception should be undertaken.

Establishing a monitor for tracking the progression or degeneration of programs needs some utility indicators to be monitored. Following Lakatos's remarks, I have compiled a first-approximation list of such indicators. For progress or degeneration respectively they are: fast or slow progress; consistency or inconsistency of the measurement procedures used; beauty, originality, and empirical success, versus boring, "normal," and negative results; content-increasing explanations versus ad hoc hypotheses; theoretically digested versus undigested anomalies; theory-guided versus hack experimentation. Some

of the indicators might be dependent, but still, passing the phenomenological program of research of eidetic images through all of them might be instructive. As to the speed of progress of the phenomenological program, I consider it to be almost nil. Woodworth and Schlosberg (1954) end their summary by saying, "At present we cannot tell how the eidetic child establishes his image"; 26 years later Haber ends his [shorter] abstract [BBS 2(3) 1979: back cover] by stating "We do not yet know why or how only a small percentage of children possess this [eidetic] ability." This understanding is of course the main aim of the program.

As to the consistency of the measurement procedure, although Haber and Haber (1964) have contributed decisively to the standardization of the measurement procedure of studies following them, they might have introduced some inconsistencies that potentially pose problems of continuity with the studies conducted before them. Woodworth (1938, p. 46) cautions against procedures such as staring with no eye movement, which might induce the young subjects to form afterimages during the test for eidetic images. Haber and Haber (1964) start their experiments by requiring the subjects to stare and stop eye movements, thus effectively training the young subject to form afterimages. Then the subject is required to unlearn this behavior during the test for eidetic images. Afterimages are an excellent source for valid visual information. Why should a young subject relinquish this procedure when it is clear that he is being judged on his visualization abilities? Gray and Gummerman (1975), as Haber describes, have found that eye movements during eidetic image testing were smaller and more variable than the verbal report of the images would predict, which might be related to the peculiar procedure used. I would conclude then, that the consistency of measurement procedures, especially with the older literature, is imperfect, and that even within the later period, confounding with afterimages is enhanced and not reduced by the standard procedure of exposing the young subjects to conditions that maximize afterimage occurrence.

As to the interest, beauty, and originality of the program, I am sad to admit that except for Stromeyer and Psotka's (1970) study in which two decomposed versions of a Julesz random dot stereogram were presented, requiring as a correct response the report of a three-dimensional object, no original or empirically successful results were apparent from the review. On the contrary, most of it is full of negative findings, or at least confusing ones. Stromeyer and Psotka's (1970) methodology is surely far removed from the pure phenomenological program normally used by the experimenters in the field.

As to content-increasing explanations, I did not detect any such explanations. On the contrary, the explanation of the negative correlation of the probability of detecting eidetic subjects with age left me very confused. The review claims that an extensive longitudinal study over the entire span of elementary school found that eidetic potential remains stable, with no relation to thinking styles, reading proficiency, or neurological and functional pathologies. Still the negative correlation of the phenomenon with age exists. I did not detect further hypotheses that might increase content. I am afraid that at this stage of the program the only content-increasing explanation for the sudden drop out of eidetic visualizers older than say fifteen is that this ability is lethal in adulthood. . . .

The main problem of phenomenological research in eidetic imagery rests with the last indicators of Lakatos. There is absolutely no guidance of experimentation by theory. The requirement of the pure phenomenological approach as applied to eidetic imagery is that it use "rich" stimuli that might result in lengthy protocols, such as the one demonstrated in the target article. This requirement precludes the establishment of a theory or even a model for the phenomenon. As a result, we are subjected to experimentation that is not guided by any theory related to visual perception. The discontinuity claims, based, among other evidence, on factor analytic studies, would hinder such theorizing even more. The claim itself is an example of logical problems with the program. Does the discontinuity reflect a typology of children or a typology of visual experiences within a child?

Now to some positive indications. Stromeyer and Psotka's (1970) experiment should, in my opinion, serve as the basis for anybody interested in a theory of visual perception that would also be constrained by the basic data of the research program of eidetic imagery. The absolute lack of any demand characteristics, the strong and unambiguous requirements of the task, the closeness of the stimulus both for theorizing at the neural level, (e.g. Dev 1975) and for the meaningful perceptual product it represents when viewed stereoscopically, could serve as necessary bases for new progress. As to the pure phenomenological approach, I would suggest, on the basis of the review and Lakatos's criteria, that it be abandoned in this domain.

by **Martin S. Lindauer**

Department of Psychology, State University of New York, College at Brockport, Brockport, N.Y. 14420

Exorcising the ghosts in the study of eidetic imagery

Haber's interesting and challenging review of his work on and thinking about eidetic imagery (EI) represents an area in which he has an obvious intellectual (and personal) investment – despite its lack of progress. For that reason, his paper is also an implicit call for help in moving the problem along. It is in the spirit of offering such assistance that this commentary is written.

Haber is to be applauded for championing the cause of phenomenology in a field known for its exquisite dependence on overdesigned, highly technical, rigorously controlled, and reductionistic procedures; and in which the Observer (*O*) is treated as if he or she were nothing but a passive recording instrument. For Haber, phenomenology is the essential indicator of EI, and, as he insists in the conclusion of his paper, the basis for its continuing study. Yet it is possible that Haber is not phenomenological enough.

There is much more his *O*s could tell us about their EI, either under more naturalistic conditions (i.e., at home) or retrospectively (as could their parents). Open-ended questions about eidetikers' thoughts and feelings might be asked on such matters as how often EI occurs, the circumstances of its occurrence, and whether the child feels good or bad (or anxious) about this ability. Interviews might provide more information (or leads for more systematic research) than obtainable under restricted laboratory circumstances. Even there, Haber is not as phenomenological as he could be. For example, it might be useful to know if some pictures are better liked or found more interesting than others; or whether with a choice among stimuli, EI would be more forthcoming. In short, a broader conception of phenomenological analysis might reveal information about EI as a function of the type of stimulus (including whether or not it was voluntarily chosen), the viewing circumstances, and the state of *O* (e.g., whether relaxed or not).

In addition, I do not think it would be unfair to direct a phenomenological inquiry to Haber himself, since he is refreshingly candid about his own phenomenological stake in EI. He is genuinely puzzled over the phenomenon, yet never discloses why he thinks an understanding of EI is essential to a theory of perception. Unless more of a case is made, I'm afraid the problem of EI may well be abandoned, as Haber fears. It seems as if Haber himself, in his reluctance to carry forth the phenomenological method to its limits, is plagued by the ghost of phenomenology.

Reflecting on my own phenomenology, I am troubled by Haber's definition of EI as a visual reconstruction of a memory. This seems to turn upside down the way in which perception and memory have traditionally been treated. That is, it is usually memory that is thought of as a reconstruction of a percept. Haber, like others in the information-processing movement, has not sufficiently appreciated the difficulty of placing memory before perception. Returning to the definitional issue, I do not think there is any real advantage to an overly precise definition of EI at this confused point in our knowledge. To do as Haber does raises the thorny question of how to distinguish between perception and memory, and the even more troublesome question of whether there are different kinds of perceptions ("ordinary" ones and others: eidetic, aesthetic, analytic, verbalizing, etc.). In keeping with a phenomenological perspective, definitions may work against an open mind on EI at this time.

I am also struck by Haber's failure to notice in his own data several

tantalizing hints as to future research directions. Haber reports some differences between eidetikers (for example, in the ability to shift images and to see 3-D images), yet he lumps all eidetikers together. There may indeed be interesting differences between eidetikers. These, if disentangled, might reveal the kinds of differences between eidetikers and noneidetikers not otherwise noted (as well as disclose previously not found correlations).

I also think Haber has too hastily divorced EI from photographic memory and from imagery in general. Haber does not tell us enough about photographic memory to make as hard a distinction as he does. For instance, the question of exactly what kind of images those with photographic memory have, and how they use them, is not discussed. (I am also more troubled than Haber appears to be about the lack of accuracy in EI reports. Which shows more error anyway: perception or memory?) Further, one unpublished report by Paivio and Cohen on the distinction between EI and imagery does not seem enough to discount the possibility of a continuum between the two. In my own work (Lindauer 1969, 1972), interviews with vivid imagizers (adults) strongly indicated to me at least that they were *seeing* an image. There are many parallels between EI and imagery, for example, the dependence on self-reports, the absence of functional relationships (Lindauer 1977). Thus, I suspect Haber may be losing more than he is gaining by considering EI to be so unique a phenomenon. I would hypothesize that were he to compare eidetikers with good imagizers, rather than with randomly drawn noneidetikers, he would find less of a difference between them, if any, than he reports from other comparisons. I would also like to see Haber follow up on his use of perceptual tasks (e.g., the decomposed stimulus, and reversible figures). To the extent that EI acts like a perception – ambiguous figures shift in their figural saliency, and prolonged inspection results in satiation effects – the possibility exists that EI is more perceptual than mnemonic.

In closing, I would point out two oversights in Haber's research program. The first is the neglect of EI in modes other than the visual. There seems to be no inherent reason why eidetikers would not be found in auditory or tactile modes. The second is the scarcity of experimental paradigms, and instead, the favoring of the study of individual differences and the use of correlational designs. Given the beautifully described abilities of eidetikers, can we not make predictions about their performance on subsequent tasks? May we ask Haber to use his phenomenology to predict, for example, how eidetikers would do on visual versus verbal tasks; would they be field dependents or independents on the hidden figure task; would the autokinetic effect show more variability; and finally, would their thresholds for the detection of subliminal stimuli be lowered (as they keep the image in mind and fail to notice its absence in the stimulus field)?

Haber is too good a scientist to give up the ghost when so many researchable questions still remain.

by **David Marks**

Department of Psychology, University of Otago, Dunedin, New Zealand

Eidetic imagery: Haber's ghost and Hatakeyama's ghoul

Dr. Haber's review of eidetic imagery is an important résumé of current fact and fiction concerning this mysterious phenomenon. As one who has long believed in the need for more intensive and rigorous study of phenomenological indicators of imagery, perception, and memory (following in the footsteps of my mentor and colleague Peter McKellar) I give wholehearted support to Haber's plea for more, instead of less, research into the nature and contents of mental experiences as reported by the subject. A substantial amount of research in this area indicates that verbal reports of conscious imagery experience provide reliable and valid indicators of functional, cognitive activity. This evidence, mostly collected by a group of psychologists working in New Zealand and Australia (McKellar 1977; Marks 1977; Sheehan 1972; Richardson 1969) suggests that verbal reports of imagery experience enable strong predictions to be made about objectively measured performance, going well beyond what is possible without reference to those verbal reports.

Preliminary work in our laboratory based on reports by a Japanese researcher (Hatakeyama 1974, 1975) using a phenomenological approach has revealed important and fascinating properties of the eidetic process that would pass unnoticed using other so-called more objective procedures. To Haber's haunting by the eidetic ghost we must now add a ghoulish presence revealed by Hatakeyama. The question is, are Haber's ghost and Hatakeyama's ghoul one and the same?

Experiments conducted by Doris McIlwain and myself support earlier work by Allport (1924) and Klüver (1932), and more recently by Hatakeyama (1974, 1975), in showing that *eidetic images are essentially autonomous and flexible perceptuallike constructions of a central, cognitive mechanism rather than peripheral copies or reproductions of recent stimulus input.* Important properties of the eidetic process are missing from the description implied by the list of defining criteria offered by Haber, although to be fair, Haber concludes his discussion with a constructive concept of eidetic imagery not dissimilar to the definition given here. The point is, though, are we looking in the right places for the ghost that haunts us?

A good technique for the examination of the autonomous, constructive nature of eidetic imagery is the "Open Circle" test used by Hatakeyama (1975). A circle, five centimetres in diameter, on white paper, is placed in front of the subject at a comfortable viewing distance (30 to 45 cm.). The subject is instructed to gaze at the centre of the circle and is told that the experimenter will say the name of a color to him. He then tries to imagine the suggested color inside the circle, and if any color, image, or shape appears, he reports such an appearance. If it changes into anything else, the subject tells the experimenter as it is happening and taps the desk each time the image changes. Several color suggestions are tested in turn, with a minimum of two minutes allowed for each. In our research we have found it highly informative to have the subject draw any emergent images as accurately as possible at the end of each trial.

In one study we compared the "Open Circle" test performance of three adult eidetikers, selected on the basis of Haber's criteria, to that of a control group of sixteen noneidetikers. In addition to meeting Haber's criteria, the three eidetic subjects reported vivid, definite images to four or more of the six suggested colors while the control group produced an average of less than one. There was no overlap between the eidetikers and controls on any measure of the responses obtained, and responses reported by control subjects were always faint or pale in comparison to the strong color reports from the eidetic group.

Perhaps more striking than reports of color responses are the actual images themselves which in all respects show qualitative and quantitative differences between eidetikers and controls. Eidetikers report significantly more emergent form, more changes in form, more movement, and their images last five to twenty times longer. Typical examples of autonomous imagery, elicited by one of our eidetikers (K.L.) to the suggestion "red," are shown in Figure 1A, as drawn by the subject herself. Figure 1B shows her responses to the same test two years previously.

The sequence of transitions in the latter case was *cyclical*, as the image metamorphosized through several stages and then cycled through the same sequence again for as long as the subject concentrated (over three minutes). She reported that she could make the cycles go as fast or as slowly as she liked, but she could not alter the particular images or the progression of changes.

The emergent forms reported are not always simple or recognizable. Yet there seems to be a discernible "nucleus" around which the image sequence develops. Such nuclei usually develop in a natural progression in keeping with Allport's assertion that in eidetic imagery "the range of flexibility is very great indeed, but it does not extend to include the ridiculous or unnatural" (Allport 1924, p. 110. see also Ahsen 1977).

Striking three-dimensional emergent forms may also occur, such as "an orange with pitted, textured skin and a green stalk," or "a pair of velvet trousers" reported as "rising up from the surface" of the projection card.

Note that our subjects were all university students, of high intelli-

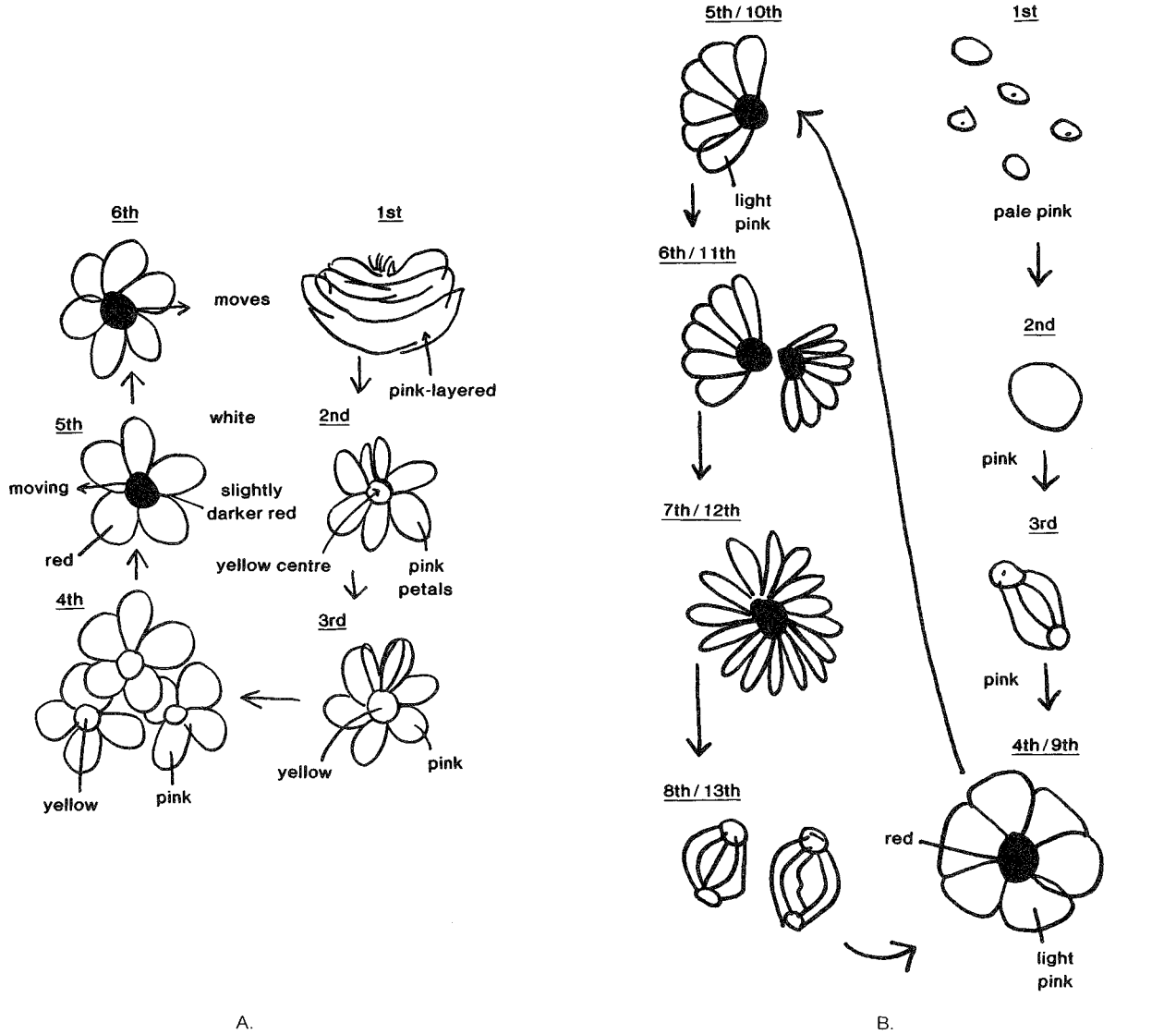

Figure 1 (Marks). A: A sequence of eidetic images produced by subject K.L. to the color suggestion "red." B: A sequence produced by the same subject two years previously to the same color suggestion. Note that in this instance the imagery progressed, and then cycled through a sequence similar to that given in A. (Artwork by Judith Economos.)

gence and normal mental health, not hallucinating psychotics, and the only stimulus for these reports was a circle on white paper and a suggested color.

Spontaneous occurrences of autonomous eidetic images in the absence of suggestion are also quite common. One of our eidetikers reported the spontaneous appearance between two trials of the experiment of "a bishop's head and shoulders," which the subject described in the present tense in great detail as his eyes scanned the blank easel in front of him. This image lasted for 266 seconds. No movement was evident in this image, which was achromatic in tone. McIlwain (1978) reports:

> At first the subject thought it adhered to the "ground" and would not be on the other side of the card, but when the experimenter turned the card over to test this, the image remained. It was rapidly replaced by a second image of two men–one in Elizabethan garb with an elaborate ruffle around his neck. The man in the background carried a large cross. The subject could report details of the location of the men and of the background terrain.

Other examples of autonomous eidetic imagery can be found in Hatakeyama's (1975) study of Y.K. and in the earlier studies of Allport (1924), Klüver (1964), and others.

While our own research has been based on a relatively small number of adult eidetikers, it suggests a strong relation between eidetic ability as defined by Haber's list of criteria and imagery of a radically different nature as reported above and by Hatakeyama (1975). Eidetic images may have peripheral or central origins, and the emergent form seems to represent a dynamic interrelation between these two sets of factors. Nuclear progression, fading, changes in color, position, size, direction, and dimensionality indicate a highly organized, fluid, and mysterious process.

If further research verifies our preliminary data on the content of eidetic imagery, it will prove necessary to expand the currently accepted list of criteria that define eidetic ability. Our studies suggest that eidetic images based on visual picture stimuli are but one manifestation of a far broader and more interesting phenomenon. Autonomous flexibility is a major property of eidetic phenomena, and this fact necessitates a reexamination of the whole area, which remains poorly understood and neglected.

Of Haber's original list of eight criteria for eidetic imagery only six now appear to have empirical support: image reported, in the present tense, in front of the eyes, of substantial duration, the eyes having moved over the stimulus (if there was one), and also over the image. We believe that the reason the two other indicators originally proposed by Haber (greater recall accuracy and the ability to form composites)

do not reliably occur is that the basic nature of eidetic images is constructive and flexible, not reproductive. Eidetic images are autonomous and dynamic, subject to centrally controlled nuclear progressions, not remnants or revivals of stimulus input at a peripheral level. The ghost is also a ghoul.

With Haber I believe the phenomenology of eidetic imagery provides a fascinating study, alive with possibilities, which will continue to haunt us for many years to come.

by **John O. Merritt**

Human Factors Research, Inc. Goleta, Calif. 93017

None in a million: results of mass screening for eidetic ability using objective tests published in newspapers and magazines

The extraordinarily eidetic subject reported by Stromeyer and Psotka (1970) was discovered quite by accident, and in effect, she discovered herself when she overheard Stromeyer talking with a colleague about Haber's recent research, and said, "I think I can do that." She was not only able to pass all of the criteria suggested by Haber, but far surpassed any previously reported eidetic abilities. Her nearly complete control over highly accurate and detailed images permitted a long series of experiments using methodology from standard psychophysical research.

The general approach was an extension of the "composite formation" method described by Haber, but requiring highly detailed and "photographic" eidetic imagery. I became involved as a colleague in these experiments as the other half of the double-blind presentation of random-dot stereograms. Subsequently, we realized that these exciting initial results would have to be replicated with other subjects in order to make any general or theoretical inferences about the possible mechanisms by which our individual subject was able to accomplish these feats. After being unable to locate other such subjects on campus, we turned to advertisements in the newspapers, again without success. With the idea that eidetic talent sufficiently developed for our experimental procedures must be extremely rare, we published several articles in the popular press (e.g. McBroom 1970; Stromeyer 1970; anonymous 1971; Davy 1971; Ford 1972; Merritt 1973) complete with self-tests for the kind of eidetic ability we hoped to find. Figures 1 and 2 are typical of the tests used; specific instructions for self-testing are contained in the caption of Figure 1. Although our first subject could perform all of our tests with ease, we failed to find even one other subject, out of the millions exposed to the self-tests, who could actually perform in our presence.

About thirty adults and children correctly responded to the various

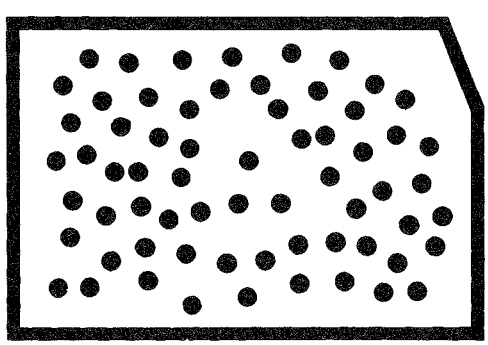

Figure 1 (Merritt). Self-testing instructions that accompanied articles in the popular press: "Carefully examine the dot pattern for several minutes. Move your gaze about to inspect all details. Do not stare at one point. Shut your eyes and try to recall an image of the pattern. If you can build up a good image, turn to the next page and superimpose your eidetic image on the dot pattern at the top. Make the rectangular borders coincide exactly. Do you see any numbers or letters? Each pattern alone is a random array of dots, but when one is superimposed on the other, very clear figures will appear." Reprinted with permission of John Merritt.

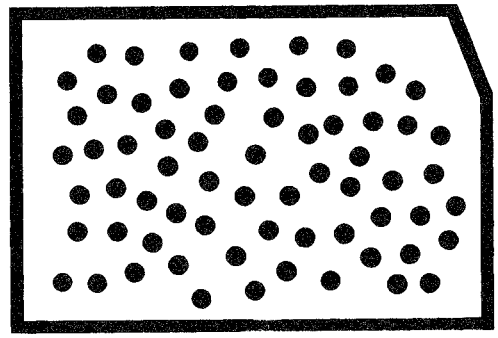

Figure 2 (Merritt).

articles and textbook presentations of the self-tests, but when retested in our presence, none was able to perform any test, including the original self-tests (for which they knew the answers). We do not know how to interpret this failure to perform in our presence the same tests for which they were able to give correct answers when originally reading the article in the newspaper or magazine. It is unlikely that many of the subjects were deliberately constructing an elaborate hoax simply as a means of meeting a real Harvard psychologist in person, although in some cases this seemed to be the motivation. (A sufficiently knowledgeable individual could, with the aid of tracing paper or a copy machine, discover the correct answers to these self-tests; it is our conservative opinion, however, that fraud was very unlikely in most of the cases in which earnest and naive subjects did their best to duplicate their originally successful performance with the self-tests, but failed in our presence.) It could be that our presence simply produced too much motivation and degraded performance of what must for most eidetikers be a very delicate mental achievement. We simply failed to find anyone who could duplicate in any way the achievements of our first eidetic subject, and we can draw no useful conclusions from this negative result.

It does seem important to describe this negative result, however, since it involved the screening of millions of readers, as the sample of references cited earlier suggests. Each of the articles contained at least two self-tests, and instructions for reporting the results to this commentator at Harvard's psychology department. As noted above, about thirty people sent in the correct answers, but those we were able to test in person (about fifteen subjects in the Philadelphia area) uniformly failed to pass any objective tests. Stromeyer and I drove to Rochester to test one of Haber's best subjects (the girl who could move images about freely, even changing size and turning them over). She, too, was unable to pass any of our more stringent objective tests.

by **Ulric Neisser**

Department of Psychology, Cornell University, Ithaca, N.Y. 14853

Tracing eidetic imagery

I can find nothing to disagree with in Haber's thoughtful review. Instead, I will use this space for two other purposes: to report an interesting observation made on one eidetiker at Cornell and to indicate why the notion of a continuum of imagery seems less attractive now than it was a decade ago.

In 1974 Jean Dirks discovered a twelve-year-old eidetiker among the subjects of an experiment on the development of recognition memory (Dirks and Neisser 1977). *B* was a pleasant, mildly retarded girl (she couldn't read) who attended a special school in Ithaca, N.Y. She met all the Haber criteria, and her phenomenal reports of "still seeing" the pictures were utterly convincing. Dirks has reported most of the findings with *B* elsewhere (Dirks 1978); nothing in them suggests that she was much different from other eidetic children. One additional test is worth describing here, however, because the technique may be useful to others. It is an image-tracing procedure [cf. *Jaynes*, this commentary] that we adapted from a suggestion by Dr. Elsa *Siipola* [8.v.] of Smith College. It included three steps. (1) We gave *B* a colorful

picture of an elephant, with a piece of tracing paper superimposed on it, and asked her to trace the outline of the elephant with a pencil. She had no difficulty in making an accurate tracing. (2) We put the elephant picture on the table and aksed *B* to copy it on a separate piece of paper. The result was a stereotyped drawing of the kind that children often produce. (3) We had *B* form an eidetic image of the same picture, slide it down from the vertical screen onto a piece of blank paper (we had previously ascertained that this was possible), and then "trace around the image" with a pencil. She complied with this request cheerfully, as if what we were asking was an easy and natural thing to do. The outcome was clear. The "tracing" *B* made from her image was very similar to the drawing she had made in step (2); it was not at all like the actual tracing in step (1).

The results of the tracing test suggest that *B*'s image was a construct rather than a copy; more like something she was making than something she was seeing. Thus it is consistent with the view of imagery as a constructive process that I suggested in *Cognitive Psychology* (Neisser 1967). In that volume I treated imagining and perceiving as very similar activities, both being "constructive." I have more recently had second thoughts about perception, which seems better described as the pickup of information guided by anticipatory schemata (Neisser 1976). On that view images can be treated as unfulfilled perceptual anticipations (Neisser 1976, 1978a, 1978b); as "constructs" of a particular kind with a particular role to play in normal perceptual acitivty. This hypothesis can successfully account for many of the imagery effects that have been observed in the laboratory; mental rotation, imagery mnemonics, perceptual set, and the like. If I were to maintain the continuum position, I would have to argue that *B*'s eidetic images also represented some sort of perceptual readiness. Such an argument could be made (her anticipations were evidently not very specific), but I am not much inclined to make it. Eidetic imagery is obviously different from what college-student subjects do in standard experiments: why should we try to explain both with a single principle?

The notion that eidetic phenomena are on a continuum with "other kinds of imagery" makes less and less sense as we discover that those other kinds themselves are painfully diverse. Recent correlational studies have made this obvious. Self-rating measures of imagery correlate only moderately even among themselves, and hardly at all with measures of spatial abilities (White, Sheehan, and Ashton 1977; Ernest 1977). Lorraine Bahrick recently reconfirmed this in a study at Cornell: she tested twenty-four subjects on mental rotation, the Space Relations test, the Betts scale, Marks's VVIQ, Slee's VES, visual recall, visual recognition, and Brooks's visual/verbal interference task. Factor analysis suggested the possible presence of a spatial factor, a self-rating factor, and other less easily interpretable components; basically most of the correlations were just low. McGee (1979) has recently reviewed literature that suggests the existence of two different factors in the spatial domain alone. It is noteworthy that these indications of wide diversity have appeared even though many types of imagery have not even been included in the parametric studies. If we knew more about hypnagogic imagery, recurrent imagery (like that many people experience after repetitive visual experiences), dream imagery, and the like, they would certainly complicate the picture further. In the long run "imagery" seems likely to go the way of "personality": instead of a consistent core we will find different processes being used in different situations and described introspectively in different ways. It would be a mistake, then, to generalize too readily about eidetikers. Eidetic imagery is a real, special, remarkable phenomenon that we will just have to study in its own right until we understand it.

by **Alan Richardson**

Department of Psychology, University of Western Australia, Nedlands, W.A. 6009, Australia

Eidetic imagery, occipital EEG activity, and palinopsia

The evidence for a perceptlike experience of the kind so clearly described by Ralph Haber is overwhelming. Its phenomenal characteristics and the usual conditions of its emergence into awareness are qualitatively different from those of the afterimage, the memory image, and the imagination image. The primary distinguishing quality of an eidetic image is that it can be seen, out there, as if the original stimulus on which it is based were still present. However, this image is seldom, if ever, an exact copy, and it is usually assumed that, as with all other memory phenomena, some process of reconstruction is involved.

Almost all investigators have testified to its existence, and yet all are uncertain as to its presumed adaptive function(s), the range and significance of its manifestations in different individuals, and its gross neurophysiological correlates and "causes." While agreeing in general with almost everything written in Haber's target article, I will say a few words on research possibilities both old and new.

Because eidetic imagery stands alone, with nothing to which it can be consistently linked, it is probably a more fruitful policy, at the present time, to encourage all forms of prospecting rather than to imply that certain veins may have been worked out, as, for example, "There is no evidence to support any version of a developmental hypothesis." The language of "no" and "any" is too strong for the present.

Gold is sometimes found even in areas where previous digging has been intensive and unproductive. What we are looking for is too valuable to abandon any approach that might still lead to our goal. Perhaps the developmental hypothesis is still capable of giving direction to eidetic research. What may be needed is a new method of testing it.

One such method is suggested by the finding that eidetic imagery in one adult (Pollen and Trachtenberg 1972) and in four children (Furst, Gardner, and Kamiya 1974), is associated with high amplitude occipital alpha activity. If these results receive further confirmation, it is possible to envisage the study of eideticism from the first year of a child's life. By using current methods for the investigation of visual perception in young babies it would be possible to monitor EEG alpha activity during the inspection phase of looking at an object and subsequently when the object is removed. If a high level of occipital alphas were to be found, it might be inferred, tentatively, that an eidetic image had been present.

The incidence of this inferred eideticism could be determined, and a sample of babies followed through into late childhood. The parents of babies identified as having eidetic imagery could be encouraged to note instances of behaviour that might be interpreted as having an eidetic component. One such instance was reported by Harry Helson (1933) whose four-year-old son had said, "Do you know, daddy, I can see pictures on the wall in here if I look at it and think." When asked if he could see them outside or at night he replied, "No, not outside and not when it's dark. Only when it's not too light or too dark, only when it's just right."

Good evidence exists that weak memory imagery can be made more vivid by training (Walsh, White, and Ashton 1978). Large individual differences have been noted in the vividness, completeness, controllability, colour, duration, and course of eidetic imagery, but little is known about the way in which any of these characteristics might be changed or strengthened. A beginning might be made by recording the eye movement patterns of those who have strong and weak forms of some eidetic characteristic. If discriminable patterns could be found, they could provide the kind of information and understanding from which rational training programmes could be developed.

Last, a word on brain injury and eideticism. While it is unlikely to be a necessary condition for the production of eidetic imagery, the possibility that some form of injury may be a sufficient condition should not be ruled out. Quite apart from the recurrence of the eidetic/brain-injury theme in the psychological literature, an accumulation of reports on a comparable phenomenon (palinopsia) exists in the neurological literature. Bender, Feldman, and Sobin (1968) define palinopsia as "the persistence or recurrence of visual images after the exciting stimulus object has been removed." Meadows and Munro (1977) remark that "the mechanism underlying palinopsia is unknown" but then go on to state that "right-sided lesions cause palinopsia more commonly than left-sided ones." A search for palinopsic patients who would be prepared to undergo an eidetic testing programme might prove to be especially rewarding. Such a search is currently underway, with the aim of establishing whether or not palinopsic patients are eidetic in terms of Haber's criteria.

by **Cynthia Roberts-Gray**

Perceptronics, Monterey Operations, Presidio of Monterey, Calif. 93940

The visualization continuum

Haber's proposal that the concept of visualization is the key to understanding visual imagery is intriguing. There is great appeal in his assertion that the purpose of studying eidetic imagery is to understand and explain how "some memory representations can be visual." In this framework his arguments persuaded me away from my original position (Gray and Gummerman 1975) that "vividness" is the feature that distinguishes eidetic from other types of visual memory imagery. Haber's emphasis on visualization in distinction to vividness or fidelity suggested to me that research should be focused on imagery as a *mode* of representation; that is, one should focus on the functional rather than the structural aspects of imaging. "Vividness" and "concreteness" are structural dimensions. "Visualization" sounds like a process or functional dimension. Indeed, Haber speaks briefly to the "functional role of visual imagery in memory" and to the "functional significance" of eidetic imagery per se. A functional perspective for the study of eidetic imagery is most attractive. It suggests that researchers could leave off asking who the eidetikers are or what the eidetic image is like and begin instead to ask what the eidetiker can do with the eidetic image. But Haber dismisses the functional perspective by pointing out that no correlates (except age) have been discovered for eidetic imagery. Perhaps I misunderstood Haber's intent in emphasizing visualization.

He says that visualization is a continuum and that it is this continuum that we need to study and understand. But he puts his major effort into convincing his reader that eidetic imagery is "distinct from normal perception," that it is a "kind of . . . imagery different from that of other children, and from virtually all adults," and that it "stands alone." The implication of this argument is that the visualization continuum is not really a continuum at all, or that eidetic images are the only images that occur on the visualization continuum. This interpretation seems confirmed by Haber's statements that the criteria for scoring images as eidetic "differentiate eidetic imagery from . . . memory" and "specify visual memory as distinct from some nonvisual memorial process." Such reasoning denies the whole impressive literature which illustrates that persons without eidetic abilities are capable of visualizing. Brooks (1967), for example, has demonstrated that problem solving that requires visualization of an unseen block design is more difficult when the problem is presented in written rather than oral form. Perky's (1910) classic work demonstrated that normal adults can and do confuse objective visual stimulation with the products of their own efforts at visualization. Sheehan (1966) has shown that adults can use a slide projector to create objective matches for the images they are visualizing as memory images of a previous slide presentation. Thus it is not necessary, as Haber implies, to *infer* that noneidetic people visualize. There are numerous data sources to show that eidetikers are not the only people whose memory representations can be visual.

If we are to maintain that the study of eidetic imagery is going to enlighten us about the "continuum of visualization," we must conclude that eidetic imagery is a form of whatever else is represented on the continuum. We cannot isolate it as a phenomenon distinct from other forms of visual imagery. Furthermore, if we are to conceptualize a continuum for study, we need a conceptual scale with which to identify points on that continuum. I am now convinced that we should disallow vividness as the conceptual scale for differentiating eidetic images from other forms on the continuum. Haber has not, however, described any alternative. It is possible that when we have, as Haber suggests, learned more about the components and content of the images and the conditions and stimuli that produce and terminate them, we will discover the conceptual scale(s) that will help us to understand and explain the continuum of visualization. But it seems to me that a very large amount of time and research has already been devoted to describing the contents and conditions of eidetic imagery. Allport's (1924, 1928) and Klüver's (1925, 1931) work is particularly important in this respect.

As an alternative, therefore, I would propose that a direction for future work might be to take up the functional approach, which I thought Haber was going to suggest. I think we should focus on visualization as a *mode* of representation and study eidetic imagery as a specific point on that functional continuum. A likely measure for scaling the continuum would be the frequency with which visualization is exercised as the preferred mode for representing information for further processing. Haber says that "it appears as if eidetic children have two modes of processing visual *stimuli:* a visual mode, which leads to a visual image, and a verbal rehearsal mode, which blocks the image, though it probably aids verbal memory." It is tempting to rephrase the first clause to make it read, "a *visual rehearsal* mode which aids *visual memory.*" Research questions might then be framed to determine what circumstances elicit one mode of rehearsal rather than another.

It seems obvious that a task that requires the person to provide a list or narrative composed of verbal labels to describe the contents of a visual stimulus would ordinarily drive the individual to choose the verbal rehearsal mode. Since this is the type of task employed in the standard imagery research procedure, it is not surprising to find that Haber proposes to abandon the "memory-based" criteria for scoring images as eidetic. But surely there are tasks that could drive the individual to a visual rehearsal mode. Sequential hue or shape discriminations may be examples of such a task. Do eidetikers perform better in such tasks? Or is it simply that they are individuals who prefer the visual rehearsal mode unless forced into a verbal rehearsal mode? The essential question is one about function: are eidetikers able to do special things with their images? Or are their images different from those of other children and adults by virtue of the frequency with which visualization is selected as the mode of memory representation?

While I can agree with Haber's comment that perceptual researchers should not try to circumvent phenomenological indicators of perception, I cannot agree that a greater emphasis on phenomenological indicators is the most fruitful approach for future study of eidetic imagery. Instead of asking yet more questions about the components, contents, and conditions of eidetic imagery, the time has come for asking questions about the functions of visualization.

by **Paul A. Roodin** *and* **Erol F. Giray**

Department of Psychology, State University of New York at Oswego, Oswego, New York 13126

Eidetic imagery is not a ghost

A number of useful conclusions are found in Haber's careful update of eidetic imagery research. Perhaps the most significant is that eidetic imagery exists as a distinct phenomenon separate from other forms of nonvisual memory and afterimages. Interestingly, this conclusion has been arrived at after a review of much of the same general data cited by Gray and Gummerman in 1975. Unlike Haber's current synthesis, however, Gray and Gummerman suggested that eidetic imagery is nothing more than the upper limit of traditional perceptual-memorial processing strategies, that is, a matter of a quantitative not a qualitative distinction.

From our own research on the process of eidetic imagery, it seems clear that we are in support of the conclusion developed from the data by Haber. Eidetic imagery is neither "enigmatic" nor the simple additive extension of traditional visual-imagery processes (e.g. quantitative ones) as suggested earlier by Gray and Gummerman (1975). With the criteria originally outlined by Haber and Haber (1964) one can easily identify subjects whose performance on eidetic imagery tasks is *qualitatively* different from that of others who do not possess this ability. Eidetic imagery is not a matter of degree. As Haber has once again illustrated, these subjects, although by no means common, satisfy all of the criteria in response to all test stimuli while noneidetic subjects rarely meet any of these criteria in a systematic fashion. We have found in our own studies that subjects consistently represent two distinctly bimodal populations with no overlapping of subjects. Eidetic imagery is an "all-or-none" phenomenon and such subjects are clearly discontinuous in a qualitative, empirical, and statistical sense from those who do not show evidence of this ability. On this issue we could

not be more in agreement with Haber.

Eidetic imagery and development. The conclusion that eidetic imagery is unrelated to development seems a bit premature. Although significant improvements and much-needed modification in the measurement of eidetic imagery have occurred, the "developmental hypothesis" has remained essentially unchanged and accepted uncritically for over sixty years. This hypothesis needs to be examined closely to see, for example, that an extremely simplistic model of development has been implicitly assumed: a decrement model. That is, at one stage of development a "primitive" strategy or processing mode is present while at a second stage of development this strategy is said to be absent. Such an inverse relationship can be identified nominally in some research, but perhaps the lack of additional general support stems from a far too narrow and limited conceptualization of development.

Recent theorists suggest that development is multilinear and encompasses the entire life span (Baltes and Willis 1977; Chandler 1976). Thus development may reflect processes that reflect incremental, decremental, and a host of curvilinear relations (e.g. U-shaped and inverted U-shaped functions). Even the meaning of stages in recent theory [see Brainerd: *BBS* 1(2) 1978] suggests a heightened awareness of a broader and more complex definition of development and developmental processes. Some suggest a less absolute or static view of stages and advocate a more dynamic, relativistic, and equilibrium-based model (Flavell 1971). Thus, in considering the relation of eidetic imagery to development, several reformulations and new perspectives need to be addressed. First, there is no necessary reason to postulate that eidetic abilities must be supplanted by more advanced processing abilities: coexistence of both primitive and advanced abilities or states is a fact of modern theories of development. Second, if some developmental processes are represented by curvilinear relations, then tests of the relation between eidetic imagery and development need to be extended systematically beyond the years of childhood, especially to old age. In order to conclude that eidetic imagery and development are unrelated, the full range of potential variance of this relationship needs to be explored (e.g. life-span developmental research). Our initial foray into this area (Giray, Altkin, Roodin, Yoon, and Flagg 1978) offers tentative support for the logic of extending this search across the life span since eidetic imagery did appear with increasing frequency among the aged (70–95 years of age).

Eidetic imagery and neurological development. It is difficult to accept Haber's contention that attempts to link eidetic imagery and neurological development or pathology will prove fruitless. One of Haber's major criticisms of this approach is the lack of a consistent set of data linking eidetic imagery and neurological development or pathology. Surely the problem lies in the methodology employed by past investigators who have defined categories of neurological pathology in rather global fashion and searched for rather unspecified links, often reporting fortuitous positive results. Such generally defined categories can only add error variance, and as Haber correctly notes there is a great deal of inconsistency among the studies in this area in terms of trends, percentages, and support for the relation between eidetic imagery and neurological pathology or development. This speaks to a problem in method not in theory. In fact, the possibility that eidetic imagery is related to neurological pathology or development appears to be the only "new" viewpoint presented in Haber's review. While the search for relations between eidetic imagery and other more mature forms of processing has proved unsuccessful, there appears enough suggestive evidence that neurological studies of this ability are a very worthwhile way to proceed. Just as the "developmental hypothesis" needs further specification and delineation prior to additional testing, we suggest that a careful rationale be developed for those investigators who continue to search for relationships between eidetic imagery and neurological pathology or development. The perpetuation of research in this area without such a rationale or plan seems pointless. We need to think about why the continued postnatal development of the central nervous system might lead to the appearance, suppression, elimination, or reemergence in later life of eidetic abilities.

by **Peter W. Sheehan**

Department of Psychology, University of Queensland, St. Lucia, Qld. 4067, Australia

Eidetic imagery: continuing to be an enigmatic phenomenon

The phenomenon of eidetic imagery is haunting not because we should look upon it as a ghost that ought to be exorcised, but rather because we need to know more about it to be able to live with its idiosyncrasies. The study of eidetic imagery among school children over a ten-year period by Haber and his associates (Leask, Haber, and Haber 1969) is a classic in the imagery literature, and Haber remains one of the most experienced testers of eidetic imagery that we have in psychology today. The essential enigma of the phenomenon is that the search for its correlates has been decidedly unrewarding, and our conviction as to the reality of the phenomenon basically remains tied to our faith in subjective report. As Hebb (1968) suggests, to the skeptic eidetic imagery sounds like an image that has got stuck to the viewing surface. Yet the reports of eidetic subjects are peculiarly compelling; one cannot fail to be impressed, for instance, as the imager talks of the picture he or she is seeing falling off the edge of the easel, or of this or that detail occurring in the upper left-hand side of the picture that exists "out there."

It is true as Haber argues that the procedures of many contemporary investigators of memory imagery tell us virtually nothing about the phenomenology of visual imagery and even less about the individual differences that accompany such subjective states. But, on the other hand, the study of eidetic imagery lacks the methodological rigor associated with much of the research into other forms of mental imagery. Objective test procedures and equipment can determine, for example, if the eyes are actually scanning the stimulus and also the eidetic image that the subject reports, but such measurement is rarely adopted, and investigators ultimately come to trust in what their subjects say.

Recognizing the problems of proceeding on the basis of phenomenological criteria for arguing the reality of eidetic imagery, Haber carefully attempts to discount alternative hypotheses about why subjects report as they do. Eidetic imagery, for example, must be distinguished from simple remembering, and it must not be explainable in terms of the demand characteristics associated with the test situation. The strength of his position lies basically in the extent to which the evidence for the reality of eidetic imagery converges and the degree to which standard test procedures for investigating the phenomenon yield stability of imagery classification. No small part of the frustration experienced by the investigator in this field is the fact that attempts to be objective – such as using the composite picture test – are frequently rendered invalid because the test conflicts with the experience of subjects. Subjects, for example, may have trouble in aligning mental pictures, or their imagery may be too fragmentary for the task at hand.

The methodological demands of eidetic imagery research are challenging, and this provocative paper tends somewhat to underestimate them. As eidetic imagery is a memorial phenomenon, yet one that is distinct from memory imagery and remembering without imagery, the investigator must be able to discriminate confabulation in the subject's report. The statement that the clock "is probably a grandfather clock," for example, offers a comment *about* an experience. Procedures, then, must separate the experience itself from any elaboration of the experience as reported. A problem also exists when parts of the original picture that is imaged eidetically remain in view while other parts do not. This shift from one category of response to another requires test procedures that are sensitive enough to reliably separate seeing a picture from remembering one. Perhaps the largest threat to the phenomenon, however, comes from the criticism that eidetic subjects may be reporting an externally projected image because they are strongly cued to do so. This potential criticism is a major one when it is recognised that Haber focuses on the report of seeing the image externally as the critical criterion for asserting the reality of the phenomenon. When the subject is told to continue to look at the easel,

for example, and to tell the investigator whatever he still sees, the demand characteristics for "external" report are especially obvious. Similarly, procedures that play an important part in familiarizing subjects with the broad concerns of the study (e.g., those that test initially for negative afterimages) themselves serve to accentuate the very response that is the defining criterion. Other measures said to differentiate eidetic imagery raise the possibility of related sources of artifact. Use of the present tense, for example, may serve to partially validate the distinction between current perception and memory of prior perception but may, on the other hand, simply reflect the most obvious and consistent way for subjects to describe something that they are mistakenly reporting as "out there."

The most provocative aspect of the study of eidetic imagery relates to the question of its theoretical significance. Haber argues forcibly against the view that eidetic imagery represents an offshoot from an early developmental stage or that it is a suggestibility phenomenon; he asserts that eidetic imagery is visual in nature and should be considered independent from nonvisual memory. The discontinuity in its classification tempts Haber to conclude that eidetic imagery is a qualitatively different kind of memory representation from any other kind, and distinct from normal perception. Factor analytic work offers encourating support for this point of view, but Haber relies heavily on the evidence he has gathered to suggest that eidetic imagery is unrelated to lack of symbolic skills or education and that it is actually quite stable over the relatively long period that he has studied. In evaluating the data collected from neurological studies, case studies, and cross-cultural research, he also relies heavily on the assumption that the inconsistencies in the literature represent variations due to the fact that investigators don't apply all of the criteria, and the inability of subjects to understand the test procedures that have been adopted. The fact is, however, that some of that variability suggests the influence of complex mediational factors.

In this commentator's own work (Sheehan 1973), for instance, regularity of eidetic imagery function has been argued on the grounds of within-group consistency of data and the uniform way in which the phenomenon manifests itself for particular groups of subjects, the data suggesting that the observable variability across different testing groups is not necessarily erratic but meaningfully related to specific factors operating within the groups that are investigated. The available data don't entirely support Haber's interpretation of the meaning of eidetic imagery, any more than they lead us to endorse Gray and Gummerman's (1975) alternative hypothesis that eidetic imagery is different in degree rather than kind and represents the vividness of the visualization component in memory. Finally, Haber's emphasis on the stability of eidetic imagery over time represents an especially puzzling aspect of his case. We aren't told, for instance, what turns a stable occurrence among children into a rare phenomenon among adults.

The strength of the evidence on eidetic imagery essentially appears to lie in the extent to which it points to the differentiation of eidetic imagery from other types of mental functioning. Eidetic imagery, for instance, is not just another kind of visual persistence, and it bears little resemblance to afterimagery or other such manifestations of persistence. It is true that if we simply take phenomenological report, many aspects of eidetic imagery are similar to other kinds of imagery, but data on eye movements appear to firmly differentiate eidetic imagery as a distinctive phenomenon. Eye movements, for example, lead to reported instability of the afterimage, but not of the eidetic image; and that finding remains difficult to explain on the basis of demand characteristics, or common mechanisms said to be operating for the two kinds of imagery.

Clearly, the enigmatic nature of eidetic imagery can only be resolved by future research. Search for the correlates must continue, though it would appear that we need to address ourselves not just to one correlate (level of education) versus another (age), but to the influence of correlates considered jointly, so that their mutual contribution can be assessed. Haber is very probably right when he argues that research should address the conditions and stimuli that produce and terminate the phenomenon. Strategies of this kind are far more likely to throw light on the structure of eidetic imagery than detailed analysis of the content of the imagery, or its subjective attributes.

There are a host of intriguing questions to pursue. Why, for example, are the percentages of eidetic imagery elevated in the geriatric population (Giray, Altkin, Roodin, Yoon, and Flagg 1978), and what is the structure of the eidetic imagery that occurs in different sensory modalities? Haber does not address the illustration of eidetic imagery in other than the visual modality. Data from research on these and other issues are urgently needed to clarify why eidetic imagery is so seductive. That seduction exists precisely because the phenomenon seems so distinct, yet the processes underlying it remain obscure.

by **Michael H. Siegel**

Department of Psychology, State University of New York, College at Oneonta, Oneonta, N.Y. 13820

Eidetic imagery: where's the ghost?

Professor Haber has written a clear summary of a cloudy area. He has succeeded in presenting the major problems together with suggestions for further research. My comments are largely related to his emphasis.

Theory. A justifiable criticism of most research in this area is that it lacks a theoretical base. Case studies provide a richness and a flavor that experimental studies lack, and the former need to be continued. Experimental studies certainly do add to our fund of information and should also continue. Both would be made more meaningful, though, by the elaboration of a convincing and comprehensive theory of eidetic imagery. Such a theory could be used to plan a sequence of studies and certainly would lead to dispelling some of the mystery from this area.

Validity. Haber pointed out that research on eidetic imagery virtually disappeared for almost thirty years after 1930. Certainly an important reason for its apparent demise was the insistence on studying and measuring objective behavior. The essence of eidetic imagery is personal and immediate. American psychology was for three decades inhibited by the proscriptions of what became standard, good experimental practice for conducting research whose validity could not have an external check.

Actually it was largely the work of Haber and his associates that helped to repromote interest and research on eidetic imagery. By specifying standard rules of stimulus presentation and criteria for classification as eidetic, Haber made the entire testing procedure easier to reproduce.

Still, even with what has become standard testing, the essence of eidetic imagery remains personal and immediate. The distinction between imagery and other forms of memory rests *largely* upon verbal reports and has to do with accuracy of detail, use of the present tense in reporting the image, and the fact that the subject can or must scan the image: four of the seven criteria restated by Haber in the target article. Although the verbal report is objectively measured and the duration of the eidetic image can be timed, none of these criteria permits anything but a weak inference of eidetic imagery.

Perhaps even more important than his standardization of the eidetic testing situation was Haber's description and use of an eighth criterion. Subjects looked successively at two visual stimuli. If they could preserve a complete image of the first and align and combine it with the second, they would be able to detect and describe a composite image that had never been presented. Regrettably, not all subjects classified as eidetic using the other seven criteria can produce this composite image successfully, so this procedure should not be the sole "indirect" test; nevertheless, the use of a technique such as this is critical. One important task for researchers in this area is to develop a variety of "indirect" tests of this unusual imagery ability. Only with such procedures can validity be assured.

Nonvisual modalities. One of the more humbling exercises, for someone involved with current and original research, is to review literature usually considered ancient, only to discover surprisingly modern ideas. As early as 1927, Griffitts worked on individual differences in imagery across modalities. One of the debates then, and perhaps now, was about a general imagery factor, one not limited to a single sense modality.

Image perseveration certainly occurs in sense modalities other than

vision. There have been many fine studies of absolute pitch, for example. A strong case could be made for these illustrating eidetic imagery. In his presentation Haber devoted only a paragraph to absolute pitch. The space reserved is probably appropriate, but it seems clear that whatever the eidetic ghost is, it may materialize in any modality.

Analogs to visual eidetic imagery and absolute pitch in other senses probably occur, but have not been a popular or an easy topic for research. It would be of some interest to look at the issue of superordinate imagery that transcends a single modality. Demonstrations of its presence might serve to shed some light on a physiological system for image preservation and might provide some clues about the purpose and the evolution of such abilities.

by **Elsa M. Siipola**

Department of Psychology, Clark Science Center, Smith College, Northampton, Mass. 01060

The search for neurological correlates of eidetic imagery

Haber's conclusions on the present status of the search for valid correlates of eidetic imagery puzzle me. He does not seem open-minded since he overlooks promising leads provided in the references. Instead of being a penetrating analysis of the research in a given area, his final judgment is often based upon taking a poll of how many are for or against the variable investigated. If the results are inconsistent, the proposed correlate is branded as unpromising. No attempt is made to discover sampling or procedural differences that might explain the inconsistency. This basic weakness applies to several areas, but illustrations here will be confined to the section on neurological pathology.

The early research of Siipola and Hayden (1965) was based upon two assumptions, both of which were modified by the results obtained. The developmental theory was not supported since the frequency of eidetics was low in the familial retardate group. A generalized neurological deficit theory was strikingly supported. But since the results were inconsistent across diagnostic categories of brain damage, a specific locus of neurological damage was suggested as the critical variable related to eidetic images.

The task since then has been to find the specific neurological correlate. This assignment is a difficult one, since even today one cannot identify directly the presence and locus of subtle neurological aberrations in human subjects. The time-honored solution to this obstacle has been that of selecting for study clinical cases with well-documented neurological damage in specific locations.

The empirical studies summarized by Haber have also approached the problem indirectly by selecting samples of retarded children (classified as familial or brain damaged) in the hope of finding clues to the neurological substrate of eidetic images. But unfortunately, accurate differentiation of the brain-damaged from the familial retardate is also impossible. The diagnosis of brain damage indicates only possible neural damage in some unspecified area; it includes such heterogeneous diagnoses that one could hardly expect the subjects to have identical loci of neural damage. Inaccuracy also applies to the classification of familial retardates diagnosed as such merely because crude medical diagnoses indicate no obvious brain damage.

The inconsistency in the research findings may, then, be largely attributed to these difficulties, which have resulted in dissimilar compositions of the small experimental groups. For example, Giray, Altkin, and Barclay (1976) noted an important sampling difference. Hydrocephalic subjects, 78% of whom were found by them to be eidetic, were missing from the brain-damaged samples in two projects yielding negative results. They were also deliberately excluded from the miscellaneous brain-damaged sample of Giray et al. (1976).

There is also the problem of defining a proper control group. Should it consist of familial retardates some of whom may actually be brain damaged? Or should it consist of Haber's so-called normal children? The final interpretation and poll count necessarily depend upon the composition of the experimental groups and the type of control group selected for comparison. Thus, one can even find a majority (rather than a minority) of positive findings supporting a neurological deficit theory, if one is inclined to do so.

In addition to these sampling problems, wide variability in the procedures for identifying eidetics has also magnified inconsistency in the results obtained in the neuropathological area. Although Haber's development of a standard procedure has been a valuable contribution, researchers do not follow his method literally. Such procedural deviation is inevitable since a method designed for school children in New Haven has to be adapted to ensure similarity in understanding of the task for subjects varying in age from childhood to senility, from normality to severe pathology, and from highly acculturated groups to aboriginal samples. However, for a given sample the adaptations can be very similar. For example, the procedural adaptations made by Siipola and Hayden (1965) and by Giray et al. (1976) to handle the retardate's difficulties in communication were almost identical.

Despite all of the above obstacles, the recent research has led to different theories of the specific neural deficit that might account for eidetic imagery. Giray et al. (1976) have proposed structural damage in a particular area of the visual information-processing area. They hold that such damage might be caused by a number of factors other than hydrocephalus. Richardson and Cant (1970) favor the occipital lobe as the locus of damage. Freides and Hayden (1966) have suggested that the locus may vary among eidetics, and that the clue in an individual case might be obtained by determining whether the eidetic image is monocular or binocular. Both types were found by them in their clinical and institutional population.

The promise of a neuropathological approach is that it provides a method for discovering the necessary and sufficient neural condition giving rise to eidetic images and the possibility of finding a more valid technique to identify the phenomenon. The critical neural correlate may turn out to be a minor deviation from the norm, a localized deficit like color blindness. However, a neurological theory is not intended to supply the complete answer to all the mysteries of eidetic imagery. The following important questions would also need solution. Is there a genetic factor involved? Are there different types of eidetikers? Does the condition limit or enhance cognitive or artistic abilities or the enjoyment of life? Do the answers to some of these questions depend upon cultural factors?

Why does Haber find the neurological approach so unpromising? Actually most of his theses are congruent with a neurological deficit theory, which, if adopted, would integrate his stand on several issues. The discontinuity principle is supported by considering eidetic imagery as an anomaly. The same is true of the stability principle. Also, Haber's rejection of the standard developmental theory requires a substitute to fill the void.

In summary, a more penetrating and open-minded analysis of the research data could have produced a more positive picture of the present search for correlates. Instead, most of the possible approaches and leads suggested by others are overlooked or dismissed. This leaves Haber with nothing to contemplate except the vivid phenomenal eidetic image itself, which gradually fades into just a mysterious haunting ghost.

by **Gudmund Smith**

Department of Psychology, Lund University, S-223 50 Lund, Sweden

The need for strict differentiation between eidetics and noneidetics

One of the more disturbing features of the literature on eidetic imagery is the inconsistency among various studies with regard to the reported frequency of eidetic subjects. As Haber points out, students of this phenomenon have used "eidetic" criteria with varying degrees of strictness. However, some studies have also used indirect criteria. This accounts for the high incidence of eidetic subjects in early German studies.

For E.R. Jaensch, for instance, a size-constant afterimage (an afterimage not changing in apparent size with the projection distance) was a sign of an "eidetic disposition." An eidetic child's afterimage

was thus regarded as part of a joint personal reaction – deeply embedded in the person, to use William Stern's terminology – not an isolated retinal reaction, as in noneidetic adults. Consequently, the afterimage of an eidetic child did not follow the sensory laws of perception.

Jaensch's explanation is unsatisfactory for many reasons. One of them is that you can easily explain the size-constant afterimage by referring to the cognitive immaturity of the child, his inability to distinguish clearly between self and nonself factors in the visual world, and his concomitant tendency to view afterimages as part of outside reality (Smith and Danielsson 1978). Afterimage size can, moreover, be influenced by the afterimage theory presented to the subject, regardless of age (Smith and Sjöholm 1974).

Using the Jaensch school as a background, it seems safe to follow Haber in his discussion of eidetic imagery knowing that his criteria are strict and reliable. The very strictness of the criteria may, however, be one reason there are no in-between subjects in his samples, no subjects who are only partly or vaguely eidetic. To be sure, Haber's arguments for a sharp distinction between eidetics and noneidetics seem well-founded. I would nevertheless like to see him pursue the continuity hypothesis a little further. As an isolated curiosity, eidetic imagery might be very difficult to explain psychologically.

Talking of explanations, there is an intriguing sentence in Haber's paper: "The main prevention technique described by nearly all eidetic children is active, cognitive, verbal rehearsal of the stimulus." The verbal mode of memorizing thus seems to exclude the eidetic mode. This observation should be considered in the light of Doob's findings that the eidetic ability in illiterate subjects disappears when they learn to read. Even if messy and difficult to control, Doob's data may be worth analyzing more carefully, and to be cross-validated, if possible. After all, a partial explanation of eidetic imagery could very well be found along these lines.

Finally, I fully agree with Haber that eidetic images should be studied phenomenologically. At the same time, however, eidetic children's way of memorizing, of handling visual afterimages, of dreaming, and so forth, should be studied in the same manner. Maybe one reason no correlations have been found between eidetic ability and other psychological variables is that the latter have been crude, irrelevant, and nonphenomenological. Jaensch was not only a manic and unreliable scientist but also a creative one. Some of his ideas about the eidetic personality could be worth reexamining – using tools that are both reliable and sensitive.

by **Benjamin Wallace**

Department of Psychology, Cleveland State University, Cleveland, Ohio 44115

Eidetic imagery need not haunt us: a supportive example for the use of phenomenological reports

Unlike Ralph Norman Haber, I entered the realm of investigating eidetic imagery very recently (Walker, Garrett, and Wallace 1976). Also, unlike Haber, I have not studied such imagery in children, per se. Rather I have perhaps added more controversy to an already controversial area by using hypnotically age-regressed adults as my target population. Of course, there is a sound, methodological reason for this which I will elaborate upon here. Before doing so, however, I wish to agree with one conclusion reached by Haber and others, namely that eidetic imagery (whatever it is) is a very rare phenomenon. But exactly how rare is this ability to scan a pictorial scene or stimulus and report perceiving a visual image? According to many sources cited by Haber, the incidence of such imagery ranges between 8% and 20% of a mostly young population (between the ages of 6 and 12). The modal age for its appearance or discovery is around 7 years. Unfortunately, for reasons unknown to us, the ability to use eidetic imagery seems to disappear around the age of 12, never, it seems, to reappear. However, if adult subjects can be found who are highly hypnotizable and who can be regressed to the age at which eidetic imagery is most prevalent in children, it may be possible to study this phenomenon in such a population. This has been one of my endeavors for the last five years.

In my first experiment (Walker, et al. 1976), I employed random-dot stereograms in a very conservative and rigorous test of eidetic imagery abilities. College students, who had been found to be highly hypnotizable, were regressed to 7 years of age and were shown 3 different stereograms, in a random order, so that in composite form, they produced either a triangle, a T-shape, or a cube. The paradigm employed was similar to that used by Stromeyer and Psotka (1970) and is briefly described by Haber in his target article.

Of 20 subjects tested in my first experiment, 2, or 10% correctly identified all 3 stereogram composites. In a postexperimental interview, it was also learned that both of these subjects had some recollections of having some eidetic imagery abilities as children. As a result, my most recent study (Wallace 1978) dealt with this important clue as a possible method for identifying adults who could be hypnotically age-regressed to demonstrate eidetic imagery abilities. Unfortunately, the endeavor was only partially successful. Of 24 subjects who reported having had some memory of eidetic abilities as children and who could be successfully hypnotically age-regressed, only 2 were able to identify stereogram composites correctly using the paradigm of my 1976 study.

The small number of subjects able to demonstrate eidetic imagery during hypnotic age-regression points to a major problem in conducting research in this area, a problem that does not appear to have been addressed by Haber in a meaningful fashion. At least with my paradigm, it appears that 2 requirements are necessary for demonstrating such imagery in adults: (1) the ability to be hypnotically age-regressed and (2) a history of childhood eidetic imagery. As this combination represents a very small portion of the general population, conducting large-scale studies would be extremely difficult. For example, in my 1978 study, I was able to find 63 highly hypnotizable subjects from a sample of 482. To find subjects who recalled having some eidetic imagery abilities as children, the number was further reduced to 26. Fortunately, 24 of those subjects volunteered for my 1978 study. As such, the percentage of subjects who actually demonstrated eidetic imagery abilities in a hypnotically age-regressed situation was 8.33%, or 2 of the 24 subjects who met the supposed requirements for such. However, this percentage constitutes 8.33% of the top 10% to 15% of the range of hypnotic susceptibility and not of the general population. In other words, the actual figure for the incidence of the phenomenon may range from 0.83% to 1.25%, not 10% as I concluded in my 1976 paper.

As a result, I have reached two conclusions: (1) it is possible to restore eidetic imagery abilities for some former childhood eidetikers with the aid of hypnotic age-regression, and (2) the percentage of subjects with whom this manipulation can be performed successfully is extremely small, in fact so small that of the theoretical 8% to 20% of children who may have been eidetikers, most of these cannot be identified in adulthood, let alone be made to retrieve the eidetic ability, even with the aid of hypnotic age-regression. Therefore, even though my methods for probing into the mysteries of eidetic imagery are unique and might eventually help define the process, a patient group of scientists must emerge who are willing to spend many months or even years to study a phenomenon or process that gives us very few clues as to its identity. And because such clues are few in number, the extremely conservative tests I and others have employed for testing the existence of eidetic imagery may play against us in solving the mysteries of this phenomenon. Although in the end I believe the rigorous methodology that I imposed will be necessary, I and others may have acted prematurely in imposing this rigor, especially when eidetic imagery is such a rare bird to capture. In agreeing with Haber, I feel that unless the rigor is relaxed somewhat, eidetic imagery will be haunting us for a very long time.

Finally, because of the relatively few individuals who can be identified as eidetikers, either as children or with the aid of hypnotic age-regression, I believe it may be futile to try to locate great numbers of eidetikers who may not even exist. A more fruitful experimental strategy would be to conduct as many experiments as possible on subjects identified as eidetikers to try to improve our understanding of what the phenomenon is or to what it is related. I agree with Haber that it is different from Sperling's visual image, but that does not help to

identify what it is. I also agree with Haber that it would be a mistake to stop conducting research on this very interesting, albeit controversial, area of perception. However, since Haber did not include a warning for future scientists in this area, I shall do so: you may have to run many subjects before you find an eidetiker, but if you are sufficiently patient, you may find one or two!

Author's Response

by **Ralph Norman Haber**

Department of Psychology, University of Illinois at Chicago Circle, Chicago, Ill. 60680

Eidetic imagery still lives, thanks to twenty-nine exorcists

While there may be a substantial sampling bias in terms of who was invited to comment, and who accepted that invitation, I feel quite gratified that only two of twenty-nine eminent scientists (about the same percentage as that of eidetic children in the population of children) suggested that work on eidetic imagery was so sterile or unproductive as to be unworthy of further effort. While each of the remaining commentators suggests many criticisms and problems, they all accept, at least implicitly, the premise that the enterprise is appropriate or even valuable. This acceptance is critical because, from 1935 to 1965 at least, eidetic imagery must have ranked only marginally ahead of ESP and well behind hypnosis (to choose at random two other somewhat dicey subjects) as a legitimate topic for research in psychology.

Since I have been somewhat personally identified with whatever recovery of professional interest there is in eidetic imagery, my initial reaction in reading the twenty-nine commentaries was to classify them into pro and con, feeling smug about the pros and ready to heap carefully reasoned invective on the cons. As I made such a classification, however, I began to see the great diversity in perspective represented by these commentaries. Further, I saw that on a topic with as checkered a history as this one, there cannot yet be any perspectives that are either right or wrong, or any clearly correct or incorrect directions for future work.

Redefinition of eidetic imagery. Since more than half of the commentators referred to some aspect of differentiating eidetic imagery from other facets of mental life, I am forced to reconsider the definition I offered in the target article.

Are there visual images that would not be called eidetic, or are the words "eidetic" and "image" synonomous? These questions are raised by BLANC-GARIN, ROBERTS-GRAY, and especially highlighted by AHSEN, who extends the term "eidetic" to cover virtually any form of re-creation of memory (Ahsen 1977).

I would like to restrict the term "eidetic imagery" to those cases the respondent reports as modality-specific, and for which some converging operations are available to support the specificity of the modality. An image differs from a percept in that the latter occurs in the presence of stimulation whereas the former occurs when there is no stimulus present. Eidetic images differ from other images, or from memory more generally, in that the former are represented in a specific sensory modality, such as vision, audition, touch, and so on. In most instances the modality of the eidetic image is the same as the modality of the original stimulation, but there is no necessity for this correspondence. What is critical is that the subject says (and acts as if) he is currently seeing (or hearing, etc.) something that is not presently stimulating him. If the report is not explicitly anchored in a specific modality, it should not be included as eidetic imagery, but treated more generally as nondescript imagery, or thinking, or fantasy, as the case may be.

ROBERTS-GRAY, in the introductory paragraph of her commentary, clearly reflects my stress on visualization and differentiates visualization from vividness. I wish to reject Gray and Gummerman's (1975) proposal that vividness be a criterion for eidetic imagery. To say that the subject "sees" his image involves no claim that his image is vivid, only that it is visible. Some eidetic images may be very vivid and others pale. We need to study this dimension of vividness in eidetic imagery as we do in other forms of imagery, but vividness should not be a criterion.

AHSEN's instructions for the Eidetic Parents Test (Ahsen 1977) also pertain to my concern about visualization. In that test, the subject is asked the following: "Picture your parents in the house where you lived most of the time with them, the house which gives you the feeling of a home. Where do you see them? What are they doing? How do you feel when you see the images?" Such instructions, when given under the appropriate conditions, can certainly elicit a substantial verbal output, an output that undoubtedly reflects memories of prior experiences. But there is no demand that the responses reflect specific prior experiences, nor that the responses be anchored in modality-specific content. Thus, some of these responses will have nothing to do with seeing (or reseeing), or with hearing, tasting, or touching, even though they do represent descriptions of some kind of mental phenomena.

AHSEN's commentary is a request to broaden the definition of eidetic imagery to cover reports of experience that are in no way dependent upon an immediately prior external stimulus, but are "in essence internal evocations." BARBER makes a similar request, since he reports that some experimental subjects, when asked "if they can see in the room an object that is not present," do describe images that are visual in content. However, I see a fundamental difference between the expansions proposed by Ahsen and by Barber, in that while both ask for an internal evocation rather than induction by an immediately prior stimulus, only Barber demands that the response meet criteria for being modality specific (usually visual). It is this difference that I feel is critical and should form the basis for distinguishing eidetic imagery from other forms of mental activity. Thus, eidetic images need not be restricted to those induced by an immediately prior exposure of a stimulus, but can be evoked by instructions to form an image, in a particular modality, of any event or action known to the subject. The definition does exclude reports that are simply descriptions of memories without reference to modality. Therefore, some, but not all of the reports elicited by Ahsen's instructions would meet this criterion.

Perhaps, since eidetic imagery is a very old concept – see Ahsen (1977) for a good review – it might be best to abandon it to the historians and invent a new set of terms: one for each modality. Thus, my own research has concerned visual imagery elicited in the absence of a present visual stimulus, a *visual stimulusless vision*. BARBER's work, referred to in his commentary, would also fall into this category, as would most of the examples suggested by ASHTON. Excluded would be reports of memories or hallucinations that were not specifically visual (or in some other sensory modality), or for which it was not possible to present converging evidence that the respondent was indeed seeing something at the time he made his report.

Both ROBERTS-GRAY and LINDAUER, and to a lesser extent HANNAY, question why eidetic imagery should be differentiated from any other kind of vivid imagery as long as the subject's report that he is *seeing* an image is convincing. According to the redefinition offered here, no differentiation should be made – vivid imagers, as assessed by Lindauer (1969, 1972, 1977), or Sheehan (1972), or Richardson (1969) would all be displaying eidetic images as long as the images were anchored in vision (or some other sense). The critical issue is whether the subject is seeing a scene or object that is not concurrently present in the visual field of view. The picture-induction procedure is a convenient and fairly well controlled method to elicit such modality-specific images, but instructions to form an image may be just as useful. It is an empirical question, yet to be tested, as to whether subjects classified as eidetic by the picture-induction procedure also give responses classified as eidetic when those responses are elicited by other procedures.

One hint of a negative answer comes from finding that the classification of subjects by the picture-induction procedure is almost

always bimodal (see ROODIN & GIRAY), whereas instructions to form images are most likely to yield a continuous distribution of self-reported images (Sheehan 1972). Of course, in the latter case, there are usually no converging operations to ensure that the subjects are reporting images that they were actually seeing. If those are included, I expect that the subjects who can self-generate visual images that they can see may turn out to be a small and discontinuous subset of all subjects who are good imaginers.

But what about ROBERTS-GRAY's comment that there is impressive evidence that subjects exist who are presumably without eidetic imagery, yet still capable of visualizing (for example, Brooks 1967; Perky 1910; Sheehan 1966). As I noted before, we badly need data on the similarities of these different kinds of visualizing tasks, just as we need to know whether the picture-induction and the instruction-to-visualize procedures produce comparable results. In none of the references just given was the subject required to identify his images as visual. Brooks did not even assess imagery directly, so he did not know what his subjects were doing, let alone seeing. I think it quite likely that there are some subjects who are good at visualizing who yet fail to do so in the picture-induction test for eidetic imagery. We need to know how these various tasks differ.

It is useful to limit eidetic imagery to modality-specific reports because these reports pose the more interesting theoretical problems. Theories of visual perception are concerned (at least in part) with explaining how excitation of the photoreceptors can elicit an awareness of a visual world in front of the eyes. Research on eidetic imagery suggests that we can at times be aware of a visual world even when that world is not actually in front of the eyes. To examine this form of awareness with any hope of explaining it, we have to separate it from non-sensory descriptions that are more properly called thoughts, feelings (in the nonhaptic sense of the word), wishes, musings, ideas, cognitions, or fantasies. When any of these last terms refer to a visually present sensory experience, then we should include it as eidetic, but not otherwise. I am not saying that these other phenomena are uninteresting or unimportant for a science of psychology. It is just that they are not explicitly visual (or auditory, etc.). When the question is posed as to whether a person can see something that is not presently stimulating his receptors, we need to insist both that he is seeing, and that there be converging evidence to give us confidence in that claim.

This redefinition of eidetic imagery provides a testable set of predictions about the relation of eidetic imagery to other forms of imagination and to memory, as well as predictions about the efficacy of different types of elicitation procedures. Finally, it firmly anchors eidetic imagery close to perceptual processes in general, so that it becomes important for perceptual theory to explain the basis of awareness of perceiving, irrespective of whether a stimulus is present (a percept) or not (an eidetic image). This haunting connection between eidetic imagery and general processes of perception explains its fascination for me – to answer LINDAUER and HANNAY.

NEISSER's provocative commentary indicates the importance of the distinctions just made. He wants to distinguish eidetic images from other kinds of imagery precisely because he feels that the latter represent a widely diverse set of phenomena that may not have much in common with each other, let alone with eidetic imagery. He cites a wide range of correlational and experimental evidence to support the existence of this diversity among types of imagery and imagining abilities. Thus he too recommends that we study eidetic imagery in its own right and not force it into a preconceived mold with other forms of mental activity.

Are eidetic images reproductions of some prior stimulus or constructive? Historically, the accuracy criterion for the presence of eidetic imagery was based upon the assumption that such images were faithful reproductions of the inducing picture and would therefore be perfectly accurate – a kind of photographic memory. Virtually every investigation of eidetic imagery has shown this assumption to be false. Not only are the reports of eidetic imagers often fragmentary, but they often contain details or arrangements not present in the stimulus. It is for this reason that I have referred to eidetic images, as well as to perception in general, as constructive and not reproductive (see Haber 1978; and Haber and Hershenson 1980 for further details).

NEISSER makes this same point, from somewhat the same theoretical perspective. He illustrates this with an example from drawings made by an eidetic twelve year old, in which the drawing of an image of an elephant resembles the way elephants look to her and not necessarily the way they are depicted in the stimulus picture. In the same vein, MARKS describes how eidetic subjects (selected according to picture-induction procedures) can be induced to visualize images on blank surfaces, which then grow, move, and are transformed as the subject continues to look at the surface. These images are still eidetic, in the sense of being visual, but they certainly possess a dynamism not possessed by photographic reproductions.

When good imagers are asked to form an image of a familiar object or person, their images are usually nonspecific with respect to irrelevant details. Thus, while I can "see" an image of my wife, in which she is wearing shoes, I cannot see what kind of shoes, unless I am specifically asked. In other words, I construct the image according to my cognitive state at the moment, and while it is visual – I see her – it is not necessarily complete or even accurate.

The expectation that images will be photographically realistic is probably the same one that leads most of us (including specialists who should know better) to think of the perceptual apparatus as being like a camera, the retinal image like a picture, and perceiving like taking a picture. The eye-camera metaphor has been most mischievous in perceptual theories (see Braunstein 1975, for the best treatment of the history and misuses of this metaphor), and accounts for the mistaken expectation of fidelity in imagery, too.

Are the only eidetic images visual ones? Eidetic imagery is almost invariably defined in the experimental literature as a form of visual imagery, as distinct from auditory, olfactory, or other modality-specific forms. Thus, we describe eidetic responses as reported visual images of visual stimuli that are not presently in the visual field to view. Does this mean that there are no auditory eidetic images, and that somehow auditory images have some other property (besides modality) that makes them noneidetic? My comment on perfect pitch at the end of my target article was picked up by SIEGEL, who suggested that this might be considered a form of auditory eidetic imagery, and SHEEHAN, LINDAUER, HOLDING, and others have discussed imagery in other modalities. While I had implicitly restricted eidetic imagery to the visual modality in the target article, I now see no theoretical reason to do so; it should be possible to demonstrate such imagery in other modalities. All that is required is careful attention to definitions and operations. If such imagery is found in other modalities, it is then an empirical matter to determine the relationships among the different kinds of imagery within the same subjects.

Comments on the picture-induction procedure. Several commentators (AHSEN, BARBER, ASHTON, SIEGEL) mentioned that the picture-induction method followed in my research, and in most other recent studies, may be a narrow-minded procedure. Historically – that is prior to 1935 – researchers made use both of pictures (Ahsen's typographic eidetic) and of instructions simply to imagine (Ahsen's structural eidetic). When I began my work in the 1960s, I selected the former procedure only because I could more easily adapt it to the rigors of the laboratory without losing the phenomenon I sought. I see nothing intrinsic to eidetic imagery that requires one method rather than the other, except that the problems of control are so much more difficult in the instruction-to-imagine procedure.

The picture-induction procedure, however, is specifically criticized by HUNTER, JAYNES, and SHEEHAN, with some telling points. Hunter, for example, objects to the form in which the questions are asked. He assumes that remembering and imagining are on a continuum in which eidetic characteristics are more or less present. For example, he says that, with the traditional easel procedure, asking the subject whether he can *see* anything after the picture is removed reduces the likelihood of eideticlike responses, especially as

compared to asking the subject to imagine that he can still see the picture and to describe the imagined picture. Hunter reports on four subjects with whom the latter version of the instructions was used; these "data" do indeed include more eideticlike responses in all four subjects. However, the most critical datum is absent in at least three of the four – the subjects said they were remembering a verbal encoding of the picture and not actually seeing it where it had been before. Thus, Hunter does not need a converging operation – the subjects themselves deny the visual component of the remembering. This is fortunate, because otherwise switching the instructions from "see" to "imagine" opens the door to a far wider range of potentially mischievous responses. Any subject who is able to see his images should be able to do so just as easily with the "see" instructions as he can with the "imagine" instructions, whereas the less rigorous "imagine" instructions run the risk of eliciting responses that appear to be eidetic without being visual at all. Therefore any instruction procedure should stress seeing and not just imagining.

HUNTER, JAYNES, and ROBERTS-GRAY raise a different and more interesting objection to the picture-induction procedure, one that I think does call for a major change in method. All three note the findings from Leask, Haber, and Haber (1969) that verbalizations by an eidetic subject during the inspection of an inducing picture erase what would otherwise be a subsequent eidetic image of that picture. This was a robust finding which applied to every eidetic subject on which it was tested. Given this finding, is it not also likely that verbalization during description of the subsequent image reduces the duration, completeness, and accuracy of that image? All three commentators suggest as an alternative procedure that instead of making a verbal response, the subject should draw or trace what he sees, without any attendant verbal description. NEISSER describes one example of the use of a tracing procedure with a twelve-year-old eidetic subject, and shows how he obtained different and better results than he would have done using only a verbal report indicator.

The general success of this procedure is an empirical matter, which can be established by testing some eidetic subjects with and without verbalization responses. As a further test, subjects could even be trained to suppress implicit verbalization during the inspection of an inducing picture, to see if this increases the quality of eidetic responses. ROBERTS-GRAY offers some specific suggestions about possible procedures.

If empirical evidence suggests that verbalization during the response interferes with eidetic imagery, then we will have further demonstration of verbalization-visualization antagonism, beyond what Leask et al. already reported. Leask et al. very loosely linked this antagonism to a developmental hypothesis of eidetic imagery, suggesting that simultaneous visual and verbal encoding of the same stimulus is very difficult, and that younger children are better at or more likely to perform the former whereas older children and adults rely more upon the latter. In the target article I was very critical of the developmental hypothesis (see more below), but the visual-verbal opposition can be present without any developmental implications. It may be that visualizing and verbalizing are two naturally interfering ways of representing stimulation or memory at all ages, and that the degree of interference does not change much with age or cognitive style. The interference of the two modes has been amply demonstrated in adults by Allport, Antonitis, and Reynolds (1972) and by Brooks (1967), for several kinds of simultaneously performed tasks, and appears as a basic opposition in the literature on lateralization of cerebral hemispheric function (see especially Kinsbourne 1978). For these reasons I see the tests suggested by ROBERTS-GRAY, HUNTER, and JAYNES as critically important. They may even provide us with more powerful tests of eidetic imagery than the picture-induction procedures.

AHSEN, JAYNES, LINDAUER, and SIEGEL each mention another objection to the picture-induction method. They suggest that eidetic responses will be more likely to occur if richer, more important, or more emotional material is used as the basis for induction, or if a more personal relationship exists between tester and subject. Each commentator pointed to the relative sterility of the pictures typically used, such as Alice in Wonderland and other old-fashioned scenes. The argument in this context is entirely empirical and can be answered by experiment. I know of no evidence that the incidence or any other characteristics of eidetic imagery varies as a function of the emotionality of the material or transference of the testing situation, but there is no reason such evidence could not be collected. DOOB mentions some negative evidence in that African subjects found Western stimuli just as imagery-inducing as stimuli from their own cultures. If such evidence of differences among pictures is forthcoming, it would suggest a new direction for theorizing rather different from any explored in the target article.

I have reviewed a number of comments about the testing procedure. While all of my work has used a picture-induction procedure, I acknowledge that instructions to visualize something not presently in view can be just as useful. What is critical with such a procedure, however, is that the instruction be to visualize rather than merely to imagine, or to describe, or to think out loud; and that there be included some kind of converging operation to differentiate these different kinds of reports.

Alternative induction procedures. In addition to picture inductions and instructions to visualize, at least two other techniques are mentioned in the commentaries: composite pictures or stereograms and hypnotic age regression. JULESZ describes the stereogram procedure in some detail and properly calls it "unfakable," and superior in this respect to the composite picture versions I developed (Leask et al. 1969). Julesz himself should even be credited with that composite picture test (though not held responsible for it). After my first report appeared (Haber and Haber 1964), he sent me several sets of stereograms, suggesting that I try them. After considering them, I decided, without even trying them on any of my subjects, that such a test would be too difficult. But I used his idea and proceeded to make up a number of composite picture sets, the results of which have been described. Fortunately, Stromeyer and Psotka (1970) were less cautious, with startling success on their one subject. WALLACE also reports several instances of success using stereograms with eidetic subjects. On the other hand, MERRITT describes a million failures, and there are undoubtedly other unreported failures of eidetic subjects to do this difficult task.

JULESZ provides several reasons why stereograms should be easier to fuse than the composite pictures. For any successive or dichotic stimulus presentation task to work, the subject has both to align an image of one stimulus with either an image or a perception of the other, and to have sufficiently complete images to produce the fusion necessary for the stereopsis or combination. Julesz's claim that alignment is automatic with binocular vision, but problematic with pictures is undoubtedly correct. However, the likelihood of an incomplete image with a stereogram is much greater than with a simple line drawing. As SHEEHAN also points out, any type of composite picture is unusual for eidetic (let alone any other) subjects. Finally, as I noted earlier (see Leask et al. 1969), all of our eidetic subjects had much greater trouble achieving and maintaining images of abstract or meaningless stimuli. Since even the separate halves of the composite pictures are fairly abstract, and of course a random-dot stereogram half is as meaningless as possible, all versions of this testing procedure pose exceedingly difficult demands upon eidetic subjects. It is for this reason that I ignored Julesz's suggestion originally, trying instead to develop more meaningful pictures. In any event, while I agree that the test is "unfakable," I still feel it is so difficult and restrictive that it can only be used with already identified "supereidetic subjects," as Julesz calls them.

WALLACE also used random-dot stereograms, but with hypnotically age-regressed subjects. In two separate studies, he found four subjects who could correctly report the stereoscopic pictures – the most ever reported who could achieve this rigorous criterion. His subjects were adults who were highly hypnotizable and who had some memories of eideticlike abilities from their childhood. He selected these subjects because of the evidence that eidetic abilities, while rare in childhood, were even rarer in adulthood.

WALLACE's work is a very promising, though exhausting, approach. However, given the great conservatism of the stereogram fusion test,

I hope he tries some of the more traditional tests under his conditions. I realize that the demand characteristics surrounding interpretation of verbal responses under hypnosis are even more complex than under normal conditions, but until we try, we do not know what can be found.

Demand characteristics and phenomenological reports. Five commentators (BUGELSKI, ERICSSON et al., FURST, HEINERTH, and HOLDING) raise serious objections to all the research done on eidetic imagery because they feel there has been insufficient control over the demand characteristics in the testing situation, especially those that induce suggestibility. In general, each of these commentators questions in some form whether there is any reliable evidence that the subjects are experiencing a visual image projected before their eyes when making their reports.

There are two threads to these comments: are the images seen projected out into space (as are normal percepts of visible stimuli), or are they inside the head or wherever else subjects feel their images to be; and are the reports about visual images at all, or are the reports merely verbal metaphors for a nonvisual memory of previously seen stimuli?

The first of these threads, stressed by FURST and HOLDING, arises because some experimenters (including myself, I am afraid) simply assumed that eidetic images would be projected out rather than internalized as "pictures in the head," to choose one possible different location. Therefore I set up a projection surface, induced an afterimage on it in nearly all subjects, and then asked them if they could also project other kinds of images on the same surface. Most, but not all of the eidetic subjects we tested said their images were on the projection surface, but a few said their images were inside their heads (Leask et al. 1969). Doob (1965) reports that many of his subjects saw their images inside rather than outside, even with the use of a projective surface. Would I have found more "insiders" if I had used an instruction-induction procedure without any reference to a projection surface? I do not know, and it would be interesting to find out, for it might give us better data on the anatomical geography of imagery. But while the projection surface may produce a demand for projected rather than internalized images, it does not in itself force subjects to say they can see something somewhere when they cannot.

The second thread, touched on by all five commentators, is much more important: do the reports reflect something that the subject is seeing, even though the stimulus for it is not in view, or do they reflect only a nonvisual memory of what was previously seen? Although I discussed at some length parallel observations and converging operations designed to make the visual interpretation more imperative, these five commentators were apparently unconvinced. I will consider their concerns one by one.

Did the afterimage test (which induced afterimages in nearly all subjects) induce a few of them to continue to use a visual metaphor in their subsequent reports (BUGELSKI, FURTH, ERICSSON et al.)? We used the afterimage test in our screening initially because most young children simply giggled when we asked them whether they would see anything on the easel after we removed the picture. This was to convince them that the question was reasonable. We knew we were creating a demand, but it was one that was needed in order to proceed at all. Note, however, that even using the nondirective questioning form suggested by Ericsson et al., nearly all the afterimages, and all of them for those subjects subsequently classified as eidetic, were negative in color. Yet all of these eidetic children gave positive color reports in their reports about the pictures. If we set them up to use visual metaphors by the afterimage test, then we should have found the same kinds of responses in both tasks. Further, much of our subsequent screening did not use the afterimage test at all, nor did much of the published research from other laboratories. So even that demand is absent.

Did the way in which the subject was asked to tell us what he saw demand that he use visual metaphors regardless of whether he had visual imagery (ERICSSON et al., BUGELSKI, HOLDING, FURST)? Since nearly all investigators using the picture-induction procedure begin by asking whether the subjects can "see" anything there (after the picture is removed from the easel), the subject is certainly given a hint that the experimenter would be willing to accept any affirmative response. I see little prospect of avoiding such suggestions with this method. Therefore, the inference of visualization has to come from the nature of the content of the responses, or from other converging operations. I listed fourteen such instances in the section on criteria in the target article. For example, consider items 4 and 5 on that list. In their reports, eidetic subjects reliably make distinctions between what they can see and what they can remember (see Leask et al. 1969, for details of these tests and data). When an eidetic subject is tested on a picture he has seen before, but is given an inadequate amount of time to view it on this subsequent occasion, he reports having an image of part of it (the part he just looked at) but can describe the other parts just as accurately from his memory. He says he is remembering those parts from last time, not seeing them now. It is this shifting of report back and forth between memory and image, correlated with the induction of the image but not with a difference in memory, that makes me confident that when a child says he is seeing something, that is more than a visual metaphor for nonvisual memory. The longest part of my 1969 monograph is devoted to an examination of such converging operations, and I present a variety of relevant data. My point here is that just because the instruction to the subject may induce a demand does not mean that the method is no good or that the results are inconclusive. Rather, when a particular demand is inevitably induced, other measures must be examined to determine how to interpret the data. I feel that some of these commentaries focused only on the source of the demands and did not carefully consider the analyses of the data.

Is any measure based only upon verbal report really useful (HEINERTH, LIEBLICH, FURST)? The plea here is to depend only on the JULESZ random-dot stereogram fusion test, because it alone is immune to all demand characteristics and virtually forces a visual image interpretation. As I said earlier, any subject who passes this test can be comfortably called eidetic. But what about those subjects who fail it, yet meet other criteria? Do we reject them out of hand? I hope not. The Julesz test is too powerful and rejects some subjects who do not have sufficient lability of imagery to meet its demands. I hope that those investigators who find subjects who succeed on the Julesz test will contrast them with subjects who fail, in order to examine those restrictive demands. Such contrasts can add enormously to our knowledge about the different tests, and especially about the role of the variables that supposedly make one test more restrictive than another.

Are subjects hypersuggestible because of the testers' prestige (BUGELSKI)? None of my experiments was ever conducted in a laboratory; they were run in classrooms, corridors, or closets at elementary schools. Occasionally, the testing was done at the subject's house, since in some studies we also tested the parents and siblings of our eidetic subjects. Therefore, we engendered no additional demands beyond those any tester creates in a school setting.

Does repeated testing train eidetic imagery (HEINERTH and FURST)? In the longitudinal sample described in Leask et al. (1969), each subject was tested a number of times over a span of years. Only one subject ceased to remain eidetic, and he was the poorest of the sample initially. I tend to agree with Furst and Heinerth (and said so in 1969) that these subjects may have retained their eidetic abilities because of the repeated testing sessions, and that perhaps fewer of them would have been eidetic if they had only been tested once at older ages. Other than this one instance of perhaps inadvertent training, I do not know of data on whether eidetic abilities can be enhanced or created, by practice or training. If Furst and Heinerth are correct, this might help resolve the discrepancy between the general downward trend in eidetic imagery as age advances, and the stability of eidetic abilities in this one sample.

Finally, I want to respond to the general theme of the commentary by ERICSSON et al. They have been working for a number of years on information-processing models of problem-solving behavior that depend in part on verbal reports for their input. While Ericsson et al.

have not been faced with distinguishing between visual and nonvisual reports, they use reports of information to which the subjects are currently attending, as compared to inferences based upon general knowledge – knowledge that is not a reflection of any direct memory trace of their cognitive processes. This powerful program of research has created independent models of the task at hand that can generate the same data, including the same intermediate steps, as those produced by the subject. In their commentary, they suggest that the same criterion be applied to a research program on eidetic imagery – that is, the research should be validated by a model that will generate the same data as the subjects do. If this can be done, then any argument about the nature of the reports will be resolved, because the properties of the model will describe the nature of the reports.

I wish I felt we had as much understanding of visual imagery as we have of problem solving. Precisely because we lack detailed knowledge about how imagery functions, I have appealed for more research on the phenomenology of imagery. I see a close parallel between the present status of work on imagery and Simon's research over the past fifteen years, in which some of the early stages required detailed examination of lengthy protocols from subjects as to how they thought about each problem while they worked on it (see Newell and Simon 1974). Such examinations gradually lead to specific parts of models and then to full-fledged simulations. I do not feel apologetic about being behind in model building. I think I am right in noting what we are still missing and how we should go about it. Simon's is a good model to follow!

Several commentaries express concern with some aspect of interpretation of data or of theory in my target article. I will consider each of these in turn.

Cross-cultural studies. In one of the most useful comments, DOOB gently takes me to task for my rather brusque treatment of eidetic research outside of American elementary schools. My review of that work was entirely within the context of its relevance to the developmental hypothesis for eidetic abilities: samples from traditional, especially nonliterate societies should show a greater incidence of eidetic subjects. Since they do not, I used that work to cast further doubt on the developmental hypothesis.

But DOOB is entirely right when he claims that I missed the most important point in the cross-cultural data: across a wide range of subjects, cultures, ages, and testing conditions, both children and adults provide the same type of responses as found in the more controlled laboratory research in the United States and Germany over nearly a century. In this sense, eidetic imagery, as measured in this manner, is a universal phenomenon. Further, I slighted the concern of cross-cultural research with the demand characteristics of the task and the converging operations used to help provide confidence in the visual character of the responses. I am very pleased to have Doob's commentary co-appear in this treatment, thereby allowing me to correct these omissions from my target article.

With respect to the hypothesized negative correlation between Westernization and eidetic imagery, DOOB admits the inconsistency among the various published findings, but says that to dismiss them all is to ignore any chance to understand what may account for high correlations in some samples and low ones in others. Both Doob and SMITH argue for more work here.

Neurological correlates of eidetic imagery. SIIPOLA, RICHARDSON, ROODIN & GIRAY, and BRIDGEMAN all feel I was overly negative in my treatment of the research reporting differences in the prevalence of eidetic imagery in populations with various neurological deficits. In reading their commentaries, I realize I should have made a slightly different distinction, one between the quality of the present evidence and the validity of the pursuit.

With respect to the evidence at hand, SIIPOLA and ROODIN & GIRAY add nothing to contradict my summary that the present data are too inconsistent to allow any conclusion about possible neurological correlates. In fact, Siipola especially describes the kinds of problems that typically beset any such research program with humans in whom it is virtually impossible to specify the locus of any particular deficit. Therefore, at this point I still see a neurologically based explanation of eidetic abilities as only a hypothesis, with little data to support it.

Does this mean we should abandon the hypothesis? ROODIN & GIRAY correctly point out that for the hypothesis to be useful, it needs to provide a far better rationale than it does now. Which specific aspects of normal neurological development and abnormal development or damage would be expected to account for the presence or absence of eidetic abilities? At present, I see the hypothesis as only an offshoot of a developmental one. Its proponents have not worked out in theoretical terms how neurology and eideticism should covary. BRIDGEMAN's intriguing commentary outlines one example of how such a theory might be developed, but only with such specificity can useful predictions be made and tested. I am delighted that Bridgeman was willing to expose his speculations in his commentary because, as ROODIN & GIRAY note, his ideas stand out sharply in an area marked by a paucity of theory.

On the other hand, there are the isolated findings of higher than typical percentages of eidetic subjects among certain clinically defined samples. RICHARDSON, ROODIN & GIRAY, and SIIPOLA all urge that these be treated as important leads to be pursued, and that they not be simply abandoned because of their isolation, inconsistency, or poor sampling specification. Richardson also notes the literature in clinical neurology on palinopsia, in which certain brain-damaged individuals have trouble suppressing visual images of no-longer-viewed stimulus objects.

The bases for my lack of enthusiasm have been the difficulty of defining the locus of deficit, the absence of a good model relating neurological development and damage to eidetic abilities, and, most important, the apparent normality of nearly all subjects identified as eidetic. It is the last finding that is difficult to reconcile with a neurological deficit theory. Perhaps, as RICHARDSON says, neurological deficits may provide a sufficient condition for eidetic imagery even if not a necessary one. If he is right, we may find that there are two kinds of eidetic subjects, one in which the eidetic abilities are the result of some neurological abnormality and the other in which there is no such etiology. The next question would be whether the two groups were also different in their eidetic skills. To determine this we need to pay much closer attention than we have in the past to the content of the eidetic responses of the subjects tested in the studies on neurological deficits.

Developmental correlates of eidetic imagery. The last third of my target article questioned the evidence reported to account for the changes in the prevalence of eidetic imagery with age. Only HEINERTH and FURST directly challenged my concerns, but two others – DENNETT and ROODIN & GIRAY – offered some important comments on the nature of developmental theory as it has been applied to eidetic imagery. They both question the adequacy of traditional developmental models to account for eidetic, as well as many other phenomena. Without repeating their arguments here, let me merely add that I concur with their assessments. When we fully understand the processes underlying eidetic imagery, we will undoubtedly find that younger subjects are more likely to use these processes, or are better at them than older ones. It is to be hoped that by that time we will have far more sophisticated theories of the (multiple) courses of developmental change and the multiplicity of causes that account for those changes.

Homogeneity of eidetic imagery abilities. Most investigators assume that all subjects classified as eidetic are basically the same in their abilities. Several commentators have questioned this assumption. LINDAUER and, to a lesser extent, SMITH and HOLDING, suggest that when we find that eidetic subjects differ in their abilities to do specific tasks (for example, to perceive a three-dimensional image, to turn their images upside down, or to move their images to a different surface), we should at least entertain the possibility that eidetic abilities are not homogeneous. I certainly agree, though I have more simply assumed that these differences are more likely to

reflect quantitative differences in facility rather than qualitative differences in kind of imagery or kind of subject. It is certainly worth testing more explicitly.

Role of case studies of eidetic subjects. Only SIEGEL suggested that we need more case studies. I was rather offhand in my treatment of published case studies, being concerned about their sampling, the presence of pathology (see AHSEN), and the lack of control over the conditions producing the observations. Typically I see case studies as a source of hypotheses rather than as a data base from which to test hypotheses. Since there are so many case studies reported in the literature, what we need now is someone to provide a detailed analysis of them, something on the order of what Von Senden (1960) did in summarizing all the published studies of recovery of perception following recovery of vision. It is particularly important in such an analysis to include as much information as possible on the sampling and testing variables, so that proper weight can be given to the observations.

Functional significance of eidetic imagery. LINDAUER and ROBERTS-GRAY list a number of important suggestions as to other sources of information that should be pursued. They suggest that we try to find out more about the functional ecology of eidetic responding: when does the subject use it, how does he feel when he uses it, are others, such as parents or teachers, aware of his ability, can he recall instances of eidetic abilities from earlier in life, what circumstances or types of stimuli make eidetic images easy or difficult to elicit, and so forth. For this, they suggest a number of other tests, not necessarily to find correlates but to develop an inventory of a constellation of skills that go with eidetic abilities.

ROBERTS-GRAY (as well as HANNAY) goes on to stress the need to ask questions about the functional significance of visualization as a mode of representation, especially as compared to verbalization. When do subjects visualize rather than verbalize, and what benefit does it provide them? Roberts-Gray is right in thinking I am interested in such questions, though she is also right that I do not always maintain a functional point of view. Her ideas of visual rehearsal are intriguing, especially in the context of looking for tasks that favor one type of representation over another.

Should we abandon eidetic imagery? Both LIEBLICH and HOLDING say, by implication, that we should stop studying eidetic imagery; the other twenty-seven commentators plus the author say no. Lieblich proposes a scientific monitor who would determine the scientific value of further work on a topic such as eidetic imagery, and he reviews evidence on relevant criteria. In general, I am concerned by a kind of scientific imperialism that such a monitor might create, but on this topic I simply disagree with his assessment of each criterion. While Holding is less imperious, he too feels the research program on eidetic imagery is sterile, and shows little sign of revitalization.

Both LIEBLICH and HOLDING save their strongest criticism for the lack of a conceptual or theoretical base to guide experimentation and model building. They, of course, are not the only commentators who are distressed by the lack of well-developed theories of eidetic imagery, but I feel that they are placing more stress on such lacks than is appropriate. Rather, as most of the commentators say, and as I have repeated many times in my target article and in this response, much of the domain of eidetic imagery is yet to be mapped. We still need to know more about the circumstances under which it is produced, the variables that control its strength, vividness, or frequency, and so forth. Clearly, some of these questions could be answered more easily if we had a good theory of eidetic imagery, but good experiments can be constructed and interpreted without theories or models to guide them.

In addition, I have tried to focus on some of the older theories, especially the developmental one, and to question the form of the theory and the likelihood of its being correct. To be able to reject a well-entrenched theory is probably an even more important contribution (and certainly a more difficult one) than to provide positive support for a current theory.

Therefore, we need more data. But we are also ready to try some theories that transcend any particular experiment. Instead of trying to pull eidetic imagery aside and study it in isolation, we should now treat it as part of a more general model of perception. My detailed discussion in the section of this Response entitled "Redefinition of eidetic imagery," is designed to begin this process, and NEISSER's recent work is an even broader attempt to do the same. For thirty years research on this topic ceased. We have now had nearly twenty years of substantial research activity resting on a much firmer and more reliable set of procedures. I started my target article by admitting to being haunted. The commentators have been superb exorcists, at least for my personal ghosts. With this *BBS* treatment as a summary of those twenty years, we can be ready for the model building and the breakthroughs that should follow.

REFERENCES

The literature on eidetic imagery spans nearly a century, several languages, and many topics; I have attempted to collect a complete bibliography of works written or published between 1900 and 1978. I have subdivided it into five headings: primary sources of empirical data on eidetic imagery, individual case studies of subjects with eidetic imagery, theoretical and review articles on eidetic imagery and related topics, cross-cultural studies of eidetic imagery, and studies on eidetic imagery using neurologically impaired subjects. In the article I have referred to a few theoretical papers on other topics, and these have been included in a final category of general references. Of course, many of the articles reporting new data also have theoretical discussions, but I have not listed them twice. Within the big categories of data and theory, I have divided the references appearing before and after 1964, as this date reflects a substantial change in methodology.

Empirical studies of eidetic imagery – pre-1964

Ahsen, A. (1959) Hemispheric experiments on eidetic images of parents. Paper presented at the Pakistan Science Conference. [AA]

(1962) Eidetic images: a study of imagery steps for the evocation of mental structures. Paper presented at the Psychological Society, Rawalpindi. [AA]

Allport, G. W. (1924) Eidetic imagery. *British Journal of Psychology.* 15:99–120. [AA, RNH, DHH, IL, DM, CR]

(1928) The eidetic image and the afterimage. *American Journal of Psychology.* 41:418–25. [RNH, CR]

Bonte, T. H. (1933) Über die Suggestibilität von Eidetikern und Nichteidetikern (The suggestibility of eidetikers and noneidetikers). *Zeitschrift für angewandte Psychologie.* 44:161–92. [RNH]

Freiling, H. (1923) Über die raumlichen Wahrnehmungen der Jugendlichen in der eidetischen Entwicklungsphase (The spatial perception of the young in eidetic development). *Zeitschrift für Sinnesphysiologie.* 55:69–132. [RNH]

Gengerelli, J. A. (1930) Some quantitative experiments with eidetic imagery. *American Journal of Psychology.* 42:399–404. [RNH]

Jaensch, E. R. (1922a) Über die subjektiven Anschauungsbilder (On subjective imagery). *Bericht über den Kongress für experimentelle Psychologie.* 7:3. [RNH]

(1922b) Die Volkerkunde und der eidetische Tatsachenkries (Anthropology and eidetic phenomena). *Zeitschrift für Psychologie.* 91:88–111. [RNH]

(1930) *Eidetic imagery and typological methods of investigation.* Translated by O. Oeser. London: Kegan Paul, Trench, Trubner; New York: Harcourt Brace. [AA, RNH]

Jaensch, E. R., and Jaensch, W. (1921) Über die Verbreitung der eidetischen Anlage im Jugendalter (On the incidence of eidetic ability during the years of youth). *Zeitschrift für Psychologie.* 87:91. [RNH]

Johannesson, B. (1939) Eidetische Untersuchungen in Island (Eidetic studies in Iceland). *Zeitschrift für Psychologie.* 146:161–81. [RNH]

Karger, P. (1925) Die eidetische Anlage der Jugendlichen in ihre Bedeutung für die Klinik und die Schulleistungen (The eidetic ability of youth and its importance for the performance in clinic and school). *Klinische Wochenschrift.* 4:22–47. [RNH]

Katz, G. G. (1956) An investigation of eidetic imagery in conditions of hypocalcemia, with and without latent tetany. Doctoral dissertation, New York University. [RNH]

Klüver, H. (1925) Description of eidetic phenomena. *Psychological Bulletin.* 22:331. [RNH, CR]
(1926) An experimental study of the eidetic type. *Genetic Psychology Monographs.* 1:71–230. [RNH, IL]
(1928) Studies on the eidetic type and on eidetic imagery. *Psychological Bulletin.* 25:69–104. [RNH, IL]
(1930) Fragmentary eidetic imagery. *Psychological Review.* 37:441–58. [AA, RNH]
(1931) The eidetic child. In *A handbook of child psychology,* ed. C. Murchison. Worcester, Mass.: Clark University Press. [RNH, CR]
(1932) Eidetic phenomena. *Psychological Bulletin.* 29:181–203. [RNH, IL, DM]
Krellenberg, P. (1922) Über die Herausdifferenzierung der Wahrnehmungs und Vorstellungswelt aus der originaren eidetischen Einheit (The development of perception and imagination from a primary eidetic state). *Zeitschrift für Psychologie.* 88:56–119. [RNH]
Kroh, O. (1922) *Subjektive Anschauungsbilder bei Jugendlichen.* Göttingen. [AA, RNH]
(1924) Die eidetische Anlage bei Jugendlichen (The eidetic ability of young people). *Zeitschrift für Kinderforschung mit besonderer Berücksichtigung der pädagogischen Pathologie.* 29:63. [RNH]
(1952) Die Eidetik in neuer Beleuchtung (Eidetics in a new light). *Psychologische Rundschau.* 1:257–65. [RNH]
Leven, D. (1926) Die eidetische Anlage der Jugendlichen (Eidetic ability in young people). *Klinische Wochenschrift.* 5:271. [RNH]
Liefman, E. (1928) Untersuchungen über die eidetische Veranlagung von Schulerinnen einer hoheren Mädchenschule. Anhang: Einzelne Untersuchunger an Erwachsenen (Research on eidetic abilities in a girls' high school. Appendix: Individual research on adults). *Zeitschrift für angewandte Psychologie.* (Beihefte) 43:111–96. [RNH]
Meenes, M. (1933) Eidetic phenomena in Negro school children. *Psychological Bulletin.* 30:688–89. [RNH]
(1934) The relationship of the eidetic phenomenon to the after-image and to the memory-image. *Psychological Bulletin.* 31:739. [RNH]
Meenes, M., and Morton, M. A. (1936) Characteristics of the eidetic phenomenon. *Journal of General Psychology.* 14:370–91. [RNH]
Morsch, J. E. (1942) Further investigation of "eidetic imagery." *Psychological Bulletin.* 39:611–12. [RNH]
Morsh, J. E., and Abbott, H. D. (1945) An investigation of after-images. *Journal of Comparative Psychology.* 38:47–63. [RNH]
Nickols, J. (1962) Eidetic imagery synthesis. *American Journal of Psychotherapy.* 16:76–82. [RNH]
Peck, L., and Hodges, A. B. (1937) A study of racial differences in eidetic imagery of preschool children. *Journal of Genetic Psychology.* 51:141–61. [RNH]
Peck, L., and Walling, R. (1935) A preliminary study of the eidetic imagery of preschool children. *Journal of Genetic Psychology.* 47:168–92. [RNH]
Purdy, D. M. (1936) Eidetic imagery and plasticity of perception. *Journal of General Psychology.* 15:437–54. [RNH]
Quercy, P. (1925) Les eidetiques (The eidetikers). *Journal de Psychologie.* 22:801–11. [RNH]
(1926) Recherches sur l'eidetisme (Research in eideticism). *Journal de Psychologie.* 23:702–22. [RNH]
Smith, G. (1949) *Psychological studies in twin differences: with reference to afterimage and eidetic phenomena as well as more general personality characteristics.* Lund: C. W. K. Gleercup. [RNH]
Teasdale, H. (1934) A quantitative study of eidetic imagery. *British Journal of Educational Psychology.* 4:56–74. [RNH]
Traxel, W. (1962) Kritische Untersuchungen zur Eidetik. *Archiv für die Gesamte Psychologie.* 114:260–336. [RNH]
Tripp, E. (1926) Observations sur l'image eidetique (Observations on the eidetic image). *Archives de Psychologie.* 20:53–72. [RNH]
Urbantschitsch, V. (1907) *Über subjektive optische Anschauungsbilder* (On subjective visual images). Leipzig and Vienna: Deuticke. [RNH]
Wachtel, J. (1934) Studjum eidetyzmu wsrod dorostych (Studies of eideticism in adults). *Kwartalnik Psychologiczny.* 5:394–401. [RNH]
Zaworski, T. (1947) Akustyczne wyobrazenia eidetyczne (Acoustical eidetic imagery). *Kwartalnik Psychologiczny.* 13:156–203. [RNH]
Zimmerman, M. (1931) *Eidetik und Schulunterricht* (Eidetic imagery and school instruction). Langensalza: Hermann Beyer & Sohne. [RNH]

Empirical studies of eidetic images – post-1964

Ahsen, A. (1964) Eidetic images of parents, their relationship with symptoms and character formation. Paper presented at the Psychological Society, Rawalpindi. [AA]
Anonymous. (1971) Do you have a photographic mind? *Current Science.* 56:6. [JOM]
Cohen, M. W. (1975) Eidetic imagery and oral comprehension. Master's thesis, University of Western Ontario. [RNH]
(1976) Eidetic imagery vs. language skills. *Journal of Social Psychology.* 99:143–44. [RNH]
Davy, J. (1971) *The Observer.* (London) Supplement, 21 March. [JOM]
Didlo, J. H. (1970) Operant conditioning procedures applied to eidetic imagery: a preliminary investigation. Doctoral dissertation, Colorado State College. [RNH]
Dirks, J. (1978) Spatial memory of two eidetic subjects. Paper presented to the American Psychological Association, Toronto. [UN]
Ford, B. (1972) You may have a photographic memory. *Science Digest.* pp. 56–71. [JOM]
Freides, D., and Hayden, S. P. (1966) Monocular testing: a methodological note on eidetic imagery. *Perceptual and Motor Skills.* 23:88. [RNH, EMS]
Furst, C. J., and Fuld, K. (1975) Verbal and spatial abilities of eidetikers. Unpublished paper. [CJF]
Furst, C. J.; Fuld, K.; and Pancoe, M. (1974) Recall accuracy of eidetikers. *Journal of Experimental Psychology.* 102:1133–35. [AA, CJF, RNH]
Furst, C. J.; Gardner J.; and Kamiya, J. (1974) Posterior alpha-wave characteristics of eidetic children. *Psychophysiology.* 11:603–6. [RNH]
Giray, E. F.; Altkin, W. M.; Roodin, P. A.; Yoon, G.; and Flagg, P. (1978) The incidence of eidetic imagery in adulthood and old age. Paper presented at American Psychological Association meeting, Toronto. [RNH, PAR, PWS]
Giray, E. F.; Altkin, W. M.; Vaught, G. M.; and Roodin, P. A. (1976) The incidence of eidetic imagery as a function of age. *Child Development.* 47:1,207–10. [RNH]
Gummerman, K.; Gray, C. R.; and Wilson, J. M. (1972) An attempt to assess eidetic imagery objectively. *Psychonomic Science.* 28:115–18. [CJF, RNH, DHH]
Haber, R. N. (1969) Eidetic images. *Scientific American.* 220(4):36–44. [RNH]
Haber, R. N., and Haber, R. B. (1964) Eidetic imagery: I. Frequency. *Perceptual and Motor Skills.* 19:131–38. [CJF, RNH, BJ, IL, POR]
Hatakeyama, T. (1974) The process of having identified an adult eidetic person and her eidetic experiences in daily life. *Tohoku Psychologica Folia.* 33:102–18. (RNH, DM]
(1975) The constructive character of eidetic imagery. *Tohoku Psychologica Folia.* 34:38–51. (RNH, DM]
Heinerth, K., and Nickel, H. (1975) Reproduktionsleistungen von 10–14 jährigen Schülern und Studenten nach kurzzeitiger Darbietung einer Bildvorlage: Anschauliche Vorstellungen oder subjektive Anschauungsbilder? (Reproductions of 10–14 year olds and university students after a short-term presentation of a picture: memory or eidetic images). *Psychologie in Erziehung und Unterricht.* 22:148–60. [RNH, KH]
Lamb, H. L. (1974) Eye movement in eidetic imagery. *Dissertation Abstracts International.* 35:548. [RNH]
Leask, J., Haber, R. N., and Haber, R. B. (1969) Eidetic imagery in children: II. Longitudinal and experimental results. *Psychonomic Monograph Supplements.* 3:25–48. [AA, KAE, RNH, JJ, BJ, PWS]
Leuner, H. (1970) *Katathymes Bilderleben.* Stuttgart. Georg Thieme Verlag. [KH]
Lin, C-S. (1972) An experimental study of visual eidetic imagery of Chinese school children. *Dissertation Abstracts International.* 33:1,025. [RNH]
McBroom, P. (1970) Is your memory photographic? *Philadelphia Inquirer.* November 8, Sec. 2, p. 1. [JOM]
McIlwain, D. J. (1978) Eidetic abilities: a study of spontaneous and post-picture imagery. Honors thesis, University of Otago. [DM]
McMains, M. J. (1975) The recall of geometric and random figures by children with eidetic memory under conditions of interpolated interference. *Dissertation Abstracts International.* 35:3,620. [RNH]
Merritt, J. (1973) Do you have a photographic memory? *Informative Articles.* pp. 1–6. [JOM]
Nickel, H.; Heinerth, K.; and Bittman, F. (1975) Gibt es wirklich Eidetiker? (Eidetic ability: fact or artifact?) Untersuchungen mit stereoskopischen Vorlagen an Schulkindern und Studierenden. *Psychologie in Erziehung und Unterricht.* 22:259–73. [RNH, KH]
Onizawa, T., and Seki, Y. (1974) Personality characteristics of the change type and non-change type of eidetic images. *Tohoku Psychologia Folia.* 33:88–92. (RNH]
(1975) Recall of eidetic children. *Tohoku Psychologica Folia.* 34:33–37. [RNH]
Paivio, A., and Cohen, M. W. (1977) Eidetic imagery and figural abilities in children. Paper presented at a symposium on current directions in imag-

ery, eidetic and otherwise, at the annual convention of the American Psychological Association, San Francisco. [JB, RNH, DHH]
Pierro, P. S. (1966) An investigation of visual form perception and eidetic imagery in students who read well and spell poorly. Doctoral dissertation, Northern Illinois University. [RNH]
Pollen, D. A., and Trachtenberg, M. C. (1972) Alpha rhythm and eye movements in eidetic imagery. *Nature*. 237:109–12. [AA, RNH, BJ, AR]
Stromeyer, C. F., III (1970) Eidetikers. *Psychology Today*. 4:76–80. [RNH, JOM]
Stromeyer, C. F., III., and Psotka, J. (1970) The detailed texture of eidetic images. *Nature*. 225:347–49. [BB, DCD, RNH, KH, DHH, BJ, IL, JOM, BW]
Wallace, B. (1978) Restoration of eidetic imagery via hypnotic age regression: more evidence. *Journal of Abnormal Psychology*. 87:673–75. [BW]
Zelhart, P., and Johnson, R. C. (1959) An investigation of eidetic imagery. Paper presented at Western Psychological Association meeting, San Jose, California. Cited in *Fundamentals of child development*, ed. H. Munsinger. New York: Holt, Rinehart & Winston, 1971. [RNH]

Case studies of eidetic abilities – pre-1964

Antipoff, H. (1926) Un cas d'image eidetique spontanée (A case of spontaneous eidetic imagery). *Archives de Psychologie*. 20:73. [RNH]
Bousfield, W. A., and Barry, H., Jr. (1933) The visual imagery of a lightning calculator. *American Journal of Psychology*. 45:353–58. [RNH]
Helson, H. (1933) A child's spontaneous reports of imagery. *American Journal of Psychology*. 360–61. [AR]
Hunter, I. M. L. (1962) An exceptional talent for calculative thinking. *British Journal of Psychology*. 53:243–58. [RNH]
Jaensch, E. R., and Mehmel, H. (1928) Gedächtnisleistungen eines schwachsinnigen Eidetikers (Memory performance of a feeble-minded eidetic man). *Psychiatrisch-neurologische Wochenschrift*. 30:101–3. [RNH]
Jelliffe, S. E. (1928) On eidetic psychology and psychiatric problems. *Medical Journal and Record*. 128:80–83. [RNH]
Kao, C. C., and Lyman, R. S. (1944) The role of eidetic imagery in a psychosis. *Journal of Nervous and Mental Disease*. 100:355–65. [AA, RNH]
Luria, A. R. (1968) *The mind of a mnemonist*. Translated by L. Solotaroff. New York: Basic Books. [RNH, JJ]
Miller, E. (1931a) The eidetic image – an undertone of psychosis. introduction to a future inquiry. *Proceedings of the Royal Society of Medicine*. 24:1,223–30. [AA, RNH]
(1931b) Eidetic imagery. *Journal of Neurology and Psychopathology*. 12:1–13. [RNH]
Miskolczy, D., and Schultz, G. (1929) Eidetik und Schizophrenie (Eideticism and schizophrenia). *Monatsschrift für Psychiatrie und Neurologie*. 72:354. [RNH]
Mota, A. (1950) Eidetismo e alucinacoes; correlacoes dinamicas (Eideticism and hallucinations; dynamic correlations). *Anais Porugueses de Psiquiatria*. 2:359–75. [RNH]
Neymeyer, H. (1956) Über pathologische Eidetismen (On pathologic eidetic images). *Psychiatrie, Neurologie und medizinische Psychologie*. (Leipzig) 8:234–57. [RNH]
Rauth, J. E., and Sinnott, J. J. (1937) A new eidetic phenomenon. *Child Development*. 8:112–13. [RNH]
Saltzman, S. S., and Machover, S. (1952) An inquiry into eidetic imagery with particular reference to visual hallucinations. *American Journal of Psychiatry*. 108:740–48. [AA, RNH]
Schilder, P. (1926) Psychoanalyse und Eidetik (Psychoanalysis and eidetics). *Zeitschrift für Sexualwissenschaft*. 13:56. [AA, RNH]
Stratton, G. M. (1917) The mnemonic feat of the "Shass Pollak." *Psychological Review*. 24:244-47. [RNH]
Weiskrantz, L. (1950) An unusual case of after-imagery following fixation of an "imaginary" visual pattern. *Quarterly Journal of Experimental Psychology*. 2:170–75. [IMLH]
Wertham, F. (1930) Eidetic phenomena and psychopathology. *Archives of Neurology and Psychiatry*. 24:809-21. [RNH]
Ziolko, H. U. (1953) Zur Bedeutung spontan-eidetischer Erscheinungen in der Psychiatrie (Concerning the significance of spontaneous eidetic imagery in psychiatry). *Zeitschrift für Psychotherapie und medizinische Psychologie*. 3:171–78. [RNH]

Case studies of eidetic abilities – post-1964

Ahsen, A. (1965) *Eidetic psychotherapy: a short introduction*. New York: Brandon House. [AA]
(1968) *Basic concepts in eidetic psychotherapy*. New York: Brandon House. [AA]
(1977) Eidetics: an overview. *Journal of Mental Imagery*. 1:5–38. [AA, RNH, DM]
(1977a) *Psycheye*. New York: Brandon House. [AA]
Coltheart, M., and Glick, M. J. (1974) Visual imagery: a case study. *Quarterly Journal of Experimental Psychology*. 26:438–53. [RNH]
Dolan, A. T., and Sheikh, A. A. (1977) Short-term treatment of phobia through eidetic imagery. *American Journal of Psychotherapy*. 31:595–604. [AA, RNH]
Gengerelli, J. A. (1976) Eidetic imagery in two subjects after 46 years. *Journal of General Psychology*. 95:219–25. [RNH]
Gummerman, K., and Gray, C. R. (1972) Recall of visually presented material: an unwonted case and a bibliography for eidetic imagery. *Psychonomic Monograph Supplements*. 4:189–95. [RNH]
Hill, A. L. (1975) An investigation of calendar calculating by an idiot savant. *American Journal of Psychiatry*. 132:557–60. [RNH]
Schale, F. C. (1972) Exploring the potential of the monocularly blind for faster reading. *Academic Therapy*. 7:401–10. [RNH]
Sheehan, P. W. (1968) Color responses on the TAT: an instance of eidetic imagery? *Journal of Psychology*. 68:203–9. [RNH]
Sheikh, A. A. (1976) Treatment of insomnia through eidetic imagery: a new technique. *Perceptual and Motor Skills*. 43:994. [AA, RNH]
Siegel, M. H. (1977) Visual and auditory imagery: a case study. Paper presented at American Psychological Association meeting, San Francisco. [RNH]
Walker, N. S.; Garrett, J. B.; and Wallace, B. (1976) The restoration of eidetic imagery via hypnotic age regression: a preliminary report. *Journal of Abnormal Psychology*. 85:335–37. [AA, RNH, BJ, BW]

Theoretical and review articles on eidetic imagery – pre-1964

Barber, T. X. (1959a) The after-images of "hallucinated" and "imagined" colors. *Journal of Abnormal and Social Psychology*. 59:136–39. [AA, TXB, RNH]
(1959b) The "eidetic image" and "hallucinatory" behavior: a suggestion for further research. *Psychological Bulletin*. 56:236–39. [TXB, RNH]
Bonte, T. H. (1934) *Die eidetische Anlage und ihre Bedeutung für Erziehung und Unterricht*. (Eidetic ability and its importance for education). Leipzig: Barth. [RNH]
Carmichael, L. (1925) Eidetic imagery and the Binet test. *Journal of Educational Psychology*. 16:251–52. [RNH]
Cramaussel, E. (1926) Images eidetiques (Eidetic images). *Journal de Psychologie*. 23:1,003–10. [RNH]
Dimitrow, M. (1931) Psychologie des images eidetiques spontanées (The psychology of spontaneous eidetic images). *Annuaire de l'Université de Sofia, Faculté Historico-Philologique*. 27:150. [RNH]
Duker, H. (1965) Hat Jaenschs Lehre von der Eidetik heute noch Bedeutung? (Is Jaensch's concept of eidetic phenomena still of significance?) *Psychologische Beiträge*. 8:237–53. [RNH]
Feiman, G. (1935) *Eideticism and school age*. Moscow: Biomedgiz. [RNH]
Gressot, M. (1950) Essai sur l'image eidetique et le problème de la vision (Essay on the eidetic image and the problem of vision). *Schweizer Archiv für Neurologie und Psychiatrie*. 66:86–122. [RNH]
Hansen, W. (1929) Über den gegenwartigen Stand der eidetischen Forschung (The present status of eidetic research). *Vierteljahrsschrift für wissenschaftliche Pädogogik*. 5:347–83. [RNH]
Heller, T. (1930) Eidetik und Psychologie der Aussage (Eidetics and the psychology of testimony). *Zeitschrift für angewandte Psychologie*. 35:210–12. [RNH]
Henning, H. (1923) Starre eidetische Klang-und Schmerzbilder and die eidetische Konstellation (Rigid eidetic sound and pain images and the eidetic constellation). *Zeitschrift für Psychologie*. 92:137–76. [RNH]
(1924) Die neuentdeckte Erlebnisklasse der Eidetik, die Urbilder und der Konstitutions-typus (The newly discovered phenomenon of eidetic imagery, archetypes, and body type). *Deutsche Zeitschrift für Nervenheilkunde*. 81:180. [RNH]
Herbertz, R. (1930) Was ist "Eidetik"? (What is eideticism?) *Psychologische Rundschau*. 1:35–358. [RNH]
Hofe, K. vom. (1926a) Beitrag zur Frage der eidetischen Anlage (Contributions to the questions of eidetic ability). *Archiv für Ophthalmologie*. 117:40. [RNH]

(1926b) Über eidetische Anlage beim Erwachsenen (About eidetic ability in adults). *Medizinische Klinik*. 22:1361. [RNH]
Hofmarksrichter, K. (1931) Visuelle Kompensation und Eidetik bei Taubstummen (Visual compensation and eidetic imagery in deaf mutes). *Archiv für die gesamte Psychologie*. 82:329–402. [RNH]
Husen, T. (1946) *Studier rorande de eidetiska fenomenen* (Studies concerning eidetic phenomena). Stockholm: H a Kan Ohlssons Boktryckeri. [RNH]
(1952) Studier rorande de eidetiska bildernas fenomenologi, II (Studies concerning the phenomenology of eidetic imagery, II). *Lunds Universitets Arskrift*. 48:1–85. [RNH]
Jaensch, E. R. (1921) Ueber psychophysische Konstitutionstypen. *Monatsschrift für Kinderheilkunde*. 2:22. [RNH]
Kiesow, F. (1925) Zur kritik der Eidetik (Criticism of eidetics). *Archiv für die gesamte Psychologie*. 53:447–84. [RNH]
Kirek, H. (1926) Die Bedeutung der sensoriellen Veranlagung für die Bildung von Objektvorstellungen insbesondere bei Eidetikern (The significance of a person's sensory type for his ability to imagine objects, with special consideration of this ability in eideticism). *Zeitschrift für die Behandlung Schwachsinniger und Epileptischer*. 46:121–23. [RNH]
Klüver, H. (1926) Mescal visions and eidetic vision. *American Journal of Psychology*. 37:502–15. [RNH]
(1928) *Mescal: the divine plant and its psychological effects*. London: Kegan Paul. [RNH]
(1932) Eidetic phenomena. *Psychological Bulletin*. 29:181–203. [RNH]
Luther, F. (1931) Die probleme der Eidetik Erich Jaenschs und seiner Schule (The problem of eidetics for Erich Jaensch and his school). *Zeitschrift für Menschenkunde*. 6:401–19. [RNH]
Mata de Gregorio, J., and Poleo, M. A. (1958) El fenomeno eidetico (Eidetic phenomena). *Cuadernos Psicologia (Caracas)*. 1:8–11. [RNH]
O'Neill, H. E., and Rauth, J. E. (1934) Eidetic imagery. *Catholic University of America Educational Research*. 8:22. [RNH]
Shipley, T. (1958) The horopter in eidetic subjects. *Psychological Bulletin*. 55:171–75. [RNH]
Wilhelm, F. (1927) *Die Bedeutung der eidetischen Forschung für Erziehung und Unterricht* (The importance of eidetic investigation for instruction and education). Leipzig: J. Klinkhardt. [RNH]

Theoretical and review articles on eidetic imagery – post-1964

Ahsen, A. (1972) *Eidetic parents test and analysis: a practical guide to systematic and comprehensive analysis*. New York: Brandon House. [RNH]
(1973) *Basic concepts in eidetic psychotherapy*. New York: Brandon House. [RNH]
Ahsen, A., and Lazarus, A. A. (1972) Eidetics: an internal behavior approach. In *Clinical behavior therapy*, ed. A. A. Lazarus. New York: Brunner/Mazel. [AA]
Blakemore, C.; Braddick, O.; and Gregory, R. L. (1970) Detailed texture of eidetic images: a discussion. *Nature*. 226:1,267–68. [RNH]
Blanc-Garin, J. (1974) Recent research in mental images: their role in the procedures of perceptive and cognitive processes. *Année Psychologique*. 74:533–63. [JB, RNH]
Dolan, A. T. (1977) Eidetic and general image theory of primary image objects and identification processes. *Journal of Mental Images*. 1:217–27. [AA, RNH]
Dolan, A. T., and Sheikh, A. A. (1976) Eidetics: a visual approach to psychotherapy. *Psychologia*. 19:200–9. [RNH]
Giray, E. F.; Altkin, W. M.; Roodin, P. A.; and Vaught, G. M. (1977) The enigmatic eidetic image: a reply to Gray and Gummerman. *Perceptual and Motor Skills*. 44:191–94. [RNH]
Gray, C. R., and Gummerman, K. (1975) The enigmatic eidetic image: a critical examination of methods, data and theories. *Psychological Bulletin*. 82:383–407. [TXB, KAE, RNH, IL, CR, PAR, PWS]
Hebb, D. O. (1968) Concerning imagery. *Psychological Review*. 75:466–77. [RNH, PWS]
Panagiotou, N., and Sheikh, A. A. (1974) Eidetic psychotherapy: introduction and evaluation. *International Journal of Social Psychiatry*. 20:231–41. [AA, RNH]
Richardson, A. (1969) *Mental imagery*. New York: Spring. [RNH, DM]
Sheikh, A. A. (1978) Eidetic psychotherapy. In *The power of human imagination*, ed. J. L. Singer and K. Pope. New York: Plenum Press. [AA]
Standing, L.; Bond, B.; Hall, J.; and Weller, J. (1972) A bibliography of picture-memory studies. *Psychonomic Science*. 29:406–16. [RNH]
Taylor, J. G. (1973) A behavioral theory of images. *South African Journal of Psychology*. 3:1–10. [RNH]
Tharp, V. K. (1972) Mnemonic techniques, visual imagery and brain function. *Biological Psychology Bulletin*. 1:10–20. [RNH]
Twente, G. E.; Turner, D.; and Haney, J. (1978) Eidetics in the hospital setting and private practice: a report on eidetic therapy procedures employed with 69 patients. *Journal of Mental Imagery*. 2:275–90. [AA]

Cross-cultural studies of eidetic imagery

Ayman, T. (1973) Cross-cultural and predictive aspects of eidetic imagery: a comparative investigation. *Dissertation Abstracts International*. 33:6,168. [RNH]
Doob, L. W. (1964) Eidetic images among the Ibo. *Ethnology*. 3:357–63. [CJF, RNH]
(1965) Exploring eidetic imagery among the Kamba of central Kenya. *Journal of Social Psychology*. 67:3–22. [CJF, RNH]
(1966) Eidetic imagery: a cross-cultural will-o'-the-wisp? *Journal of Psychology*. 63:13–34. [RNH]
(1970) Correlates of eidetic imagery in Africa. *Journal of Psychology*. 76:223–30. [RNH, JJ]
Feldman, M. (1968) Eidetic imagery in Ghana: a cross-cultural will-o'-the-wisp? *Journal of Psychology*. 69:259–69. [RNH]
Kearins, J. (1976) Skills of desert Aboriginal children. In *Aboriginal cognition*, ed. G. E. Kearney and D. W. McElwain. Canberra: Australian Institute of Aboriginal Studies. [IMLH]
(1978) The visual spatial memory of desert Aboriginal children. *University News* (University of Western Australia) 9(2):8–10. [IMLH]
Levak, M. D. (1969) Eidetic images among the Bororo of Brazil. *Journal of Social Psychology*. 79:135–37. [RNH]
Sheehan, P. W. (1973) The variability of eidetic imagery among Australian aboriginal children. *Journal of Social Psychology*. 91:29–36. [RNH, PWS]
Sheehan, P. W., and Stewart, S. J. (1972) A cross cultural study of eidetic imagery among Australian aboriginal children. *Journal of Social Psychology*. 87:179–88. [RNH]

Eidetic imagery and neurological pathology

Ahsen, A. (1978) Eidetics: neural experiential growth potential for the treatment of accident traumas, debilitating stress conditions, and chronic emotional blocking. *Journal of Mental Imagery*. 2:1–22. [AA]
Bender, M. B.; Feldman, M.; and Sobin, A. J. (1968) Palinopsia. *Brain*. 91:321–38. [AR]
Critchley, M. (1951) Types of visual perseveration: "palinopsia" and "illusory visual spread." *Brain*. 74:267–99. [PAR]
Essler, K. R., and Rowe, C. J. (1946) A note on eidetic imagery in a case of obsessive neurosis. *Journal of Clinical Psychology*. 2:286–88. [AA, RNH]
Flournoy, H. (1926) Eidetisme chez une debile (The eideticism of a mental retardate). *Archives de Psychologie*. 20:73–74. [RNH]
Floyd, L. G. (1956) Eidetic imagery in normal and psychotic cases: a study in the incidence and characteristics of eidetic imagery in normal and psychotic cases, and a comparative analysis of the groups. Doctoral dissertation, New York University. [AA, RNH]
Furst, C. J.; Gardner, J.; and Kamiya, J. (1974) Posterior alpha-wave characteristics of eidetic children. *Psychophysiology*. 11:603–6. [AR]
Giray, E. F.; Altkin, W. M.; and Barclay, A. G. (1976) Frequency of eidetic imagery among hydrocephalic children. *Perceptual and Motor Skills*. 43:187–94. [AA, RNH, EMS]
Giray, E. F., and Barclay, A. G. (1977) Eidetic imagery: longitudinal results in brain-damaged children. *American Journal of Mental Deficiency*. 82:311–14. [AA]
Lessell, S. (1975) Higher disorders of visual function: positive phenomena. In *Neuro-ophthalmology*, vol. 8, ed. J. S. Glaser and J. L. Smith, pp. 27–44. St. Louis: C. V. Mosby Co. [PAR]
Meadows, J. C., and Munro, S. S. F. (1977) Palinopsia. *Journal of Neurology, Neurosurgery, and Psychiatry*. 40:5–8. [AR]
Richardson, A., and Cant, R. (1970) Eidetic imagery and brain damage. *Australian Journal of Psychology*. 22:47–54. [RNH, EMS]
Siipola, E. M., and Hayden, S. D. (1965) Exploring eidetic imagery among the retarded. *Perceptual and Motor Skills*. 21:275–86. [AA, RNH, EMS]
Symmes, J. S. (1971) Visual imagery in brain-injured children. *Perceptual and Motor Skills*. 33:507–14. [AA, RNH]

General references

Allport, D. A.; Antonitis, B.; and Reynolds, P. (1972) On the division of attention: a disproof of the single channel hypothesis. *Quarterly Journal of Experimental Psychology*. 24:225–35. [RNH]

Baltes, P. B., and Willis, S. L. (1977) Toward psychological theories of aging and development. In *Handbook of the psychology of aging*, ed. J. E. Birren and K. W. Schaie, pp. 128–54. New York: Van Nostrand Reinhold. [PAR]

Barber, T. X. (1964a) Hypnotically hallucinated colors and their negative after-images. *American Journal of Psychology*. 77:313–18. [TXB]

(1964b) Toward a theory of "hypnotic behavior": positive visual and auditory hallucinations. *Psychological Record*. 14:197–210. [TXB]

(1970a) Hypnosis, suggestions, and auditory-visual hallucinations. In *Origin and mechanisms of hallucinations*, ed. W. Keup, pp. 167–80. New York: Plenum Press. [TXB]

(1970b) *LSD, marihuana, yoga and hypnosis*. Hawthorne, N. Y.: Aldine. [TXB]

(1971) Imagery and "hallucinations": effects of LSD contrasted with the effects of "hypnotic" suggestions. In *Imagery: current cognitive approaches*, ed. S. J. Segal, pp. 101–29. New York: Academic Press. [TXB]

Barber, T. X., and Calverley, D. S. (1964) An experimental study of "hypnotic" (auditory and visual) hallucinations. *Journal of Abnormal and Social Psychology*. 63:13–20. [TXB]

Barber, T. X.; Spanos, N. P.; and Chaves, J. F. (1974) *Hypnosis, imagination and human potentialities*. New York: Pergamon. [TXB]

Baxt, N. (1871) Über die Zeit welch nötig ist, damit ein Gesichtseindruck zum Bewusstsein kommt und über die Grösse (Extension) der Bewusstseins Wahrnehmung bei einem Gesichtseindrucke von gegebener Dauer. *Pfuger's Archive gesammelte Physiologie*. 4:325–36. [RNH]

Benjafield, J. (1969) Evidence that thinking aloud constitutes an externalization of inner speech. *Psychonomic Science*. 15:83–84. [KAE]

Betts, G. H. (1909) *The distribution and functions of mental imagery*. New York: Teachers College, Columbia University. [RA]

Braunstein, M. R. (1975) *Depth perception through motion*. New York: Academic Press. [RNH]

Breuil, H. (1952) *Four Centuries of Cave Art*. Montignac, Dordogne: Centre d'Études et de Documentation Préhistoriques. [JJ]

Bridgeman, B. (1973) Receptive fields in single cells of monkey visual cortex during visual tracking. *International Journal of Neurosciences*. 6:141–52. [BB]

Brooks, L. R. (1967) The suppression of visualization by reading. *Quarterly Journal of Experimental Psychology*. 19:289–99. (RNH, CR]

Chandler, M. J. (1976) Social cognition and life-span approaches to the study of child development. In *Advances in child behavior and development*, vol. 11, ed. H. W. Reese, pp. 225–39. New York: Academic Press. [PAR]

Comstock, C., and Kittredge, H. (1922) An experimental study of children as observers. *American Journal of Psychology*. 33:161–77. [KAE]

Dev, P. (1975) Computer simulation of a dynamic visual perception model. *International Journal of Man–Machine Studies*. 7:511–28. [IL]

Dewey, J. (1891) How do concepts arise from percepts? *Public-school journal*. November:128–29. [AA]

Dirks, J., and Neisser, U. (1977) Memory for objects in real scenes: the development of recognition and recall. *Journal of Experimental Child Psychology*. 23:315–28. [UN]

Drever, J. (1964) *A dictionary of psychology*. Revised by H. Wallerstein. Baltimore: Penguin Books. [AA]

Erdelyi, M. H. (1976) Coding modality vs. input modality in hypermnesia: is a rose a rose a rose? *Cognition*. 4:311–19. [RNH]

Ericsson, K. A., and Simon, H. A. (1978) Retrospective verbal reports as data. C.I.P. working paper no. 388. [KAE]

(1979) Thinking aloud protocols as data: effects of verbalization. C.I.P. working paper no. 397. [KAE]

Ernest, C. H. (1977) Imagery ability and cognition: a critical review. *Journal of Mental Imagery*. 1:181–216. [UN]

Flavell, J. H. (1971) Stage-related properties of cognitive development. *Cognitive Psychology*. 2:421–53. [PAR]

Forisha, B. L. (1978) Mental imagery and creativity. Review and speculations. *Journal of Mental Imagery*. 2:209–38. [JB]

Galton, F. (1883) *Inquiries into human faculty and its development*. London: Dent. [TXB, IMLH]

Garner, W. R.; Hake, H. W.; and Eriksen, C. W. (1956) Operationism and the concept of perception. *Psychological Review*. 63:149–59. [RNH]

Gordon, R. (1949) An investigation into some of the factors that favour the formation of stereotyped images. *British Journal of Psychology*. 39:156–67. [JB]

Griffitts, C. H. (1927) Individual differences in imagery. *Psychological Monographs*. 37: number 172. [MHS]

Haber, R. N. (1978) Visual perception. *Annual Review of Psychology*. 29:25–59. [RNH]

Haber, R. N., and Hershenson, M. (1980) *The psychology of visual perception*. 2d ed. New York: Holt, Rinehart & Winston. [RNH]

Ham, M. W., and Spanos, N. P. (1974) Suggested auditory and visual hallucinations in task-motivated and hypnotic subjects. *American Journal of Clinical Hypnosis*. 17:94–101. [TXB]

Hannay, A. (1972) *Mental images – a defence*. London: Allen & Unwin; New York: Humanities Press. [AH]

(1977) Proximality as a mark of the mental. In *Contemporary aspects of philosophy*, ed. G. Ryle. London and Boston: Oriel Press. [AH]

Hebb, D. O. (1972) *Textbook of psychology*. Philadelphia: W. B. Saunders Co. [AA]

Horowitz, M. J. (1970) *Image formation and congition*. New York: Appleton-Century-Crofts. [AA]

Hunter, I. M. L. (1979) Memory in everyday life. In *Applied problems in memory*, ed. M. M. Gruneberg and P. E. Morris, pp. 1–24. London: Academic Press. [IMLH]

Jackson, C. V. (1953) Visual factors in auditory localisation. *Quarterly Journal of Experimental Psychology*. 5:188–97. [DHH]

Jaensch, E. R. (1920) Zur Methodik experimenteller Untersuchungen an optischen Auschauungsbildern. *Zeitschrift der Psychologie*. 85:37–82. [IL]

Julesz, B. (1971) *Foundations of cyclopean perception*. Chicago: University of Chicago Press. [BJ]

Kagan, J., and Havemann, E. (1972) *Psychology: an introduction*. 2d ed. New York: Harcourt Brace Jovanovich. [AA]

Kinsbourne, M. (1978) *Asymmetrical function of the brain*. Cambridge: Cambridge University Press. [RNH]

Kosslyn, S. M. (1975) Information representation in visual images. *Cognitive Psychology*. 7:341–70. [JB]

Kosslyn, S. M.; Pinker, S.; Smith, G.; and Shwartz, S. P. (1980, in press) On the demystification of mental imagery. *The Behavioral and Brain Sciences*. [RNH]

Kuhn, T. S. (1962) *The structure of scientific revolutions*. Chicago: Chicago University Press. [AH]

Külpe, O. (1902) Über die Objectivirung und Subjectivirung von Sinneseindrucken. *Philosophische Studien*. 19:508–36. [DHH]

Lakatos, I. (1970) Falsification and methodology of scientific research programmes. In *Criticism and the growth of knowledge*, ed. I. Lakatos and A. Musgrave. Cambridge: Cambridge University Press. [IL]

Lazarus, A. A., ed. (1972) *Clinical behavior therapy*. New York: Brunner/Mazel. [AA]

Lindauer, M. S. (1969) Imagery and sensory modality. *Perceptual and Motor Skills*. 29:203–15. [RNH, MSL]

(1972) The sensory attributes and functions of images and imagery evoking stimuli. In *The function and nature of imagery*, ed. P. W. Sheehan, pp. 131–47. New York: Academic Press. [RNH, MSL]

(1977) Imagery from the point of view of psychological aesthetics, the arts, and creativity. *Journal of Mental Imagery*. 1:343–62. [RNH, MSL]

McGee, M. G. (1979) Human spatial abilities: psychometric studies and environmental, genetic, hormonal, and neurological influences. *Psychological Bulletin*. 86:889–918. [UN]

McKellar, P. (1977) Autonomy, imagery and dissociation. *Journal of Mental Imagery*. 1:93–108. [DM]

McPeake, J. D., and Spanos, N. P. (1973) The effects of the wording of rating scales on hypnotic subjects' descriptions of visual hallucinations. *American Journal of Clinical Hypnosis*. 15:239–44. [TXB]

Marks, D. (1972) Individual differences in the vividness of visual imagery and their effect on function. In *The function and nature of imagery*, ed. P. W. Sheehan, pp. 83–108. New York and London: Academic Press. [DM]

(1977) Imagery and consciousness: a theoretical review from an individual differences perspective. *Journal of Mental Imagery*. 1:275–90. [DM]

Martin, L. J. (1915) Ghosts and the projection of visual images. *American Journal of Psychology*. 26:251–57. [TXB]

Müller, J. (1826) *Über die phantastische Gesichtserscheinungen*. Coblenz: Holscher. [AA]

Natsoulas, T. (1965) Converging operations for perceptual defense. *Psychological Bulletin*. 64:393–401. [RNH]

Neisser, U. (1967) *Cognitive psychology*. New York: Appleton-Century-Crofts. [UN]

(1976) *Cognition and reality*. San Francisco: Freeman. [UN]

(1978a) Anticipations, images and introspection. *Cognition*. 6:169–74. [UN]

(1978b) Perceiving, anticipating, and imagining. In *Minnesota studies in the philosophy of science*, vol. 9, ed. C. W. Savage. Minneapolis: University of Minnesota Press. [UN]

Newell, A., and Simon, H. A. (1972) *Human problem solving.* Englewood Cliffs, N.J.: Prentice-Hall. [RNH]

Nisbett, R. E., and Wilson, T. D. (1977) Telling more than we can know: verbal reports on mental processes. *Psychological Review.* 84:231–59. [KAE]

Orne, M. T. (1962) On the social psychology of the psychological experiment: with particular reference to demand characteristics and their implications. *American Psychologist.* 17:776–83. [TXB]

Paivio, A. (1975) Perceptual comparisons through the mind's eye. *Memory and Cognition.* 3:635–48. [RNH]

Parish, E. (1897) *Hallucinations and illusions, a study of the fallacies of perception.* London: Scott. [TXB]

Pavlov, I. P. (1941) *Conditioned reflexes and psychiatry.* Translated and edited by E. Horsley Gantt. New York: International Publishers Co. [AA]

Peck, C., and Schlag-Rey, M. (1979) Spatial coding of visual information in cat's superior colliculus. *Investigative Ophthalmology and Visual Science.* Supplement: 101–2. [BB]

Perky, C. W. (1910) An experimental study of imagination. *American Journal of Psychology.* 21:422–52. [RNH, DHH, CR]

Piaget, J. (1929) *The child's conception of the world.* London: Routledge and Kegan Paul. [IMLH]

Piaget, J., and Inhelder, B. (1966) *L'image mentale chez l'enfant.* Paris: Presses Universitaires de France. [JB]

(1968) *Mémoire et intelligence.* Paris: Presses Universitaires de France. [JB]

Pylyshyn, Z. W. (1975) Do we need images and analogues? In *Theoretical issues in natural language processing.* Cambridge, Mass.: MIT Press. [JB]

Richardson, A. (1977) The meaning and measurement of memory imagery. *British Journal of Psychology.* 68:29–43. [JB]

(1969) *Mental imagery.* London: Routledge and Kegan Paul. [RNH]

Russell, Bertrand. (1921) *Analysis of mind.* New York: Macmillan. [AA]

Sarbin, T. R. (1968) Ontology recapitulates philology: the mythic nature of anxiety. *American Psychologist.* 23:411–18. [CJF]

Schlag, J., and Schlag-Rey, M. (1977) Visuomotor properties of cells in cat thalamic internal medullary lamina. In *Control of gaze by brain stem neurons, developments in neurosciences,* vol. 1, ed. C. H. Baker and A. Berthoz. Elsevier: North-Holland Biomedical Press. [BB]

Schneider, A. M. (1967) Control of memory by spreading critical depression: a case for stimulus control. *Psychological Review.* 74:201–15. [RA]

Segal, S. J. (1971) Processing of the stimulus in imagery and perception. In *Imagery: current cognitive approaches,* ed. S. J. Segal. New York: Academic Press. [DHH]

Sheehan, P. W. (1966) Accuracy and vividness of visual images. *Perceptual and Motor Skills.* 23:391–98. [RNH, CR]

(1972) *The function and nature of imagery.* New York: Academic Press. [RNH]

Shepard, R. N. (1978) The mental image. *American Psychologist.* 33:125–37. [RNH]

Shepard, R. N., and Chipman, S. (1970) Second-order isomorphism of internal representations: shapes of states. *Cognitive Psychology.* 1:1–17. [CJF]

Singer, J. L., and Pope, K. S., eds. (1978) *The power of human imagination.* New York: Plenum Press. [AA]

Smith, G. J. W., and Danielsson, A. (1978) Psychopathology in childhood and adolescence as reflected in projected afterimage serials. *Scandinavian Journal of Psychology.* 19:29–39. [GS]

Smith, G. J. W., and Sjöholm, L. (1974) Can our theory of reality influence our perception of it? *Psychological Research Bulletin,* Lund University. Monograph Series 14(1):1–27. [GS]

Spanos, N. P., and Barber, T. X. (1968) "Hypnotic" experiences as inferred from subjective reports: auditory and visual hallucinations. *Journal for Experimental Research in Personality.* 3:136–50. [TXB]

Spanos, N. P.; Ham, M. H.; and Barber, T. X. (1973) Suggested ("hypnotic") visual hallucinations: experimental and phenomenological data. *Journal of Abnormal Psychology.* 81:96–106. [TXB]

Spanos, N. P.; McPeake, J. D.; and Carter, W. (1973) Effects of pretesting on response to a visual hallucination suggestion in hypnotic subjects. *Journal of Personality and Social Psychology.* 28:293–97. [TXB]

Sperling, G. (1960) The information available in brief visual presentations. *Psychological Monographs* 74: number 11. [RNH]

von Senden, M. (1960) *Space and sight.* Translated by P. Heath. London: Methuen. [RNH]

Walsh, F. J.; White, K. D.; and Ashton, R. (1978) Imagery training: development of a procedure and its evaluation. Unpublished research report, Department of Psychology, University of Queensland. [AR]

Werner, H. (1940) *Comparative psychology of mental development.* New York: Harper. [IMLH]

White, K.; Sheehan, P. W.; and Ashton, R. (1977) Imagery assessment: a survey of self-report measures. *Journal of Mental Imagery.* 1:145–70. [UN]

Wittgenstein, L. (1953) *Philosophical investigations.* New York: Macmillan. [CJF]

Wolfe, T. (1936) *The story of a novel.* New York: Scribners. [TXB]

Wolpe, J. (1969) *The practice of behavior therapy.* New York: Pergamon Press. [AA]

Woodworth, R. S. (1938) *Experimental psychology.* New York: Holt and Co. [IL]

Woodworth, R. S. and Schlosberg, H. (1954) *Experimental psychology.* 2d ed. New York: Holt and Co. [IL]