Memory Research Is Not a Zero-Sum Game

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Some reflections on the debate between representatives of laboratory-based and naturalistic study of memory, as to the relative merits and shortcomings of the two approaches, are presented. The debate, or quarrel, or squabble was not called for; the polemics that have ensued from it are not going to advance science. There is no reason to believe that there is only one correct way of studying memory. What counts in the final analysis is the extent to which the present work, whatever its orientation, shapes the future. The study of memory from different vantage points is not a zero-sum game in which only one side can win.

The debate about the relative merits and shortcomings of laboratory-based and naturalistic study of memory is a genuine tempest in an ersatz teapot. The debate does illustrate the multifaceted nature of human activity that is science, but it does not contribute much to the advancement of knowledge. What is the commotion all about, and why does it really not matter?

The Commotion

In a widely known, widely cited, and widely admired quote, Neisser (1978) said that if something is an interesting or socially significant aspect of memory, then psychologists have hardly ever studied it. In making this observation, Neisser got some things right and some things wrong. I will look briefly at both sides.

Neisser was quite right to claim that there were in 1978, almost 100 years after Ebbinghaus, many questions that could be raised about memory that had been bypassed in traditional laboratory-based studies of the topic. He was right to point out the need for an expanded approach to the study of memory and to discuss his vision of the general nature of the research that the naturalistic approach would engender.

Neisser (1978) deserves full credit for these publicly and forcefully expressed ideas. They did give students of the naturalistic study of memory a sorely needed boost in self-confidence and undoubtedly contributed to the burgeoning and flourishing of the field. The everyday memory research has already, despite its tender age, yielded some interesting and valuable empirical facts, including some that would have been unlikely to emerge from the traditional research. These alone have vindicated the early ideas about everyday memory. Furthermore, it is quite likely that some of the results of the work will eventually lead to theoretical insights. This is in the nature of science. But even if the fruits of the naturalistic approach stayed at the pure "gee-whiz-this-is-interesting" level—like, say, reports of the psychometric memory profiles of cat lovers and dog lovers—they would enrich our knowledge of ourselves and the universe in which we live.

But Neisser (1978) went wrong in the particular manner in which he served notice to the world of the impending revolution. He could have said something like,

> Look, people, I do not really know much about what has been done in the field of memory, but it is my impression that they have overlooked some very interesting problems. Why don't we do something about some of those inexcusable gaps? Why don't we go out, find some really interesting things about memory outside the laboratory, and change the course of memory research for all times to come?

Such a challenge would have been welcomed all around.

Instead, Neisser (1978) felt that the standard approach had to be demolished before the revolution could succeed; the old order had to be replaced, not revitalized. To that end his clarion call included, first, a summary dismissal of just about everything that had been done in the memory laboratory for close to a hundred years, and second, an indictment of the traditional memory researchers for having not studied anything interesting or significant. This is where trouble started.

Traditional memory researchers could probably have lived with Neisser's (1978) first charge. After all, when someone asserts something about a field of inquiry, he tells the listeners at least as much about his knowledge of the field, or lack of it, as he does about the field as such. And people can live with others' admissions of lack of knowledge.

But his second point ("If interesting or significant, then not studied") was a different matter. It did not require any inordinate degree of sensitivity on the part of a typical student of traditional memory to interpret Neisser's (1978) statement as implying that he or she, as well as all of the other psychologists who had ever studied memory, had been and probably still were (a) not very bright, (b) socially irresponsible, (c) lacking in imagination, (d) some of the above, or (e) all of the above. With their personal and scientific integrity thus impugned, it was inevitable that they would react. Banaji and Crowder's (1989) article has turned out to be the tip of the iceberg of that reaction.

Why It Does Not Matter

Memory, like countless other objects of scientific curiosity, can be studied and described at many different levels, from many different perspectives, using many different approaches and methods. There need not be, and there

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usually is not, any conflict between these different approaches and different levels. Normally they are complementary. As has been pointed out by others (e.g., Bruce, 1985; Conway, 1991, this issue), there is no reason why the relation between laboratory-based study of memory and the real-life study of everyday memory should be anything other than that. The approaches may differ, but the objective, the great cause, is the same.

We all have our favorite criteria for judging the success of our activity. Thus, we may insist that what we find should be interesting (Neisser, 1978), or counterintuitive (Banaji & Crowder, 1989), or susceptible to "proof by disproof" (Ceci & Bronfenbrenner, 1991, this issue), or immediately applicable (some memory researchers), or ecologically valid (many memory researchers), or generalizable (everybody). And we may believe that there are right ways and wrong ways to pursue our objective—understanding memory—in a manner that optimally satisfies our chosen criteria.

Yet it is clear that all of these criteria, although admirable, are incomplete. The one complete, ultimate criterion is the effect that what we do today has on what we and others do tomorrow. It is the extent to which our current findings, facts, thoughts, ideas, insights, and concepts will contribute, one way or another, to the eventual *permanent* knowledge base of our subject matter.

Now, there are three things that are known about the facts and ideas that satisfy this ultimate criterion. First, the origin of the fact or idea does not matter. How it was obtained, its particular source, the locus of the activity that produced it, the particular method used, is immaterial. Laboratory or "the field," hard measurement or casual observation, statistical test or mere "eyeballing," "normal" subjects or special populations, a sample of thousands or a single subject—these are all impertinent. Second, pronouncements ex cathedra as to the significance of a fact or an idea, its nature or origin, regardless of how passionate, are inconsequential in determining whether the fact or idea survives. Third, and most important in the context of the issue we are discussing, judgments about what is good and what is bad, what is worthwhile and what is a waste of talent, what is useful and what is less so, are judgments that seldom can be made in the present. They can safely be made only by posterity.

It is for these plain lessons that the debate about laboratory-based versus naturalistic study of memory is little more than a tempest in the teapot. In the long run, debates of this kind are irrelevant. The sound of the thumping of the chests and the sound of the epithets thrown from one side to the other will soon be forgotten. Once the participants' adrenalin level has returned to normal and they return to more productive pursuits, they will realize that the airing of the issue has run its standard course: a lot of heat and very little light generated, but many people nevertheless happy because of the catharsis they experienced.

The science of memory is not a zero-sum game. There is no law that says that good facts or ideas can come out of one type of approach only if some other approach is suppressed. As in other fields of science, there is room for many different kinds of facts and ideas about memory and for many approaches.

So let us stop squabbling and go back to more creative work. Let us stop doing experiments on experiments, or collecting data just because they can be collected. Let us identify genuinely important problems, and let us not worry about whether someone labels them artificial or real. Let us work on these problems in whatever setting is most natural, using whatever methods are most appropriate. If we succeed, we will discover something that we find pleasing and worthwhile; if we succeed beyond our most daring hopes, that something may help to shape the future.

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