
The Bankruptcy of Everyday Thinking

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Several disagreements with Banaji and Crowder's (1989) work on everyday memory are presented. Psychology is no longer a young science. The progress of research in memory is being impeded not by the everyday memory movement, but by an excessively restricted theoretical base. First, the key notion of generalizability is being applied over data, rather than over mechanisms. Second, theories of memory take insufficient account of other mental functions.

Whenever I see psychology described as "our young endeavor" (Banaji & Crowder, September 1989, p. 1185), I know I am in the presence of a losing argument. Viewed from where most of the scientific action is at the moment, 100 years and more is aged indeed. So what is going on? The plea of youth seems to be a plea for more time. Banaji and Crowder feel they are on the right track and that they will get there in the end. Energy is being diverted from a serious enterprise by doctrinaire, superficially attractive, but essentially flippant activities.

The key term is *generalizability*. Banaji and Crowder (1989) want data to generalize. They actually would not mind it generalizing to real-world situations, but because of the "immense variability from one real-world situation to another" (p. 1189), generalization between real-world situations is impossible. On the other hand, one is supposed to be able to get generalizations from laboratory studies to real-world situations. Indeed, from the way Banaji and Crowder write about the applicability of "general principles" one expects to be able to subscribe to the Yale University Correspondence Course in Examination Study Techniques right now. In contrast, the only memory courses with pretensions to a psychological basis that are commercially available in the United Kingdom are based on mnemonic systems, some of which do go back to the ancient Greeks.

"Principles of memory that cannot be discovered using the scientific method may indeed exist, and emergent principles may someday be discovered using everyday memory methods," wrote Banaji and Crowder (1989, p. 1190). The way of formulating this patronizing concession (e.g., the use of *indeed* and *someday*) illustrates the gap. But "the scientific method" is actually a way of thinking, not a method of deriving data. It is a way of thinking that relies on hypotheses and, in the psychology I value, that relies on theories. Theories do not arise solely from data, although generalizations might. Theories arise from other processes, most that involve hard thinking. Generalizations are fragile, however. After 10 years of research, Underwood (1961) concluded that facilitation by distributed

practice in verbal learning "occurs only under a highly specific set of conditions, and the magnitude of the effect when it does occur is relatively small" (p. 230). It is easy to forget the promising beginnings of that line of very laboratory research.

I will briefly discuss an effect mentioned by Banaji and Crowder (1989) as a prime example of a laboratory-generated, counterintuitive result that is discussed at length by Conway (1991, this issue) as an example of the profitable influence of real-world studies on classical theory: the recency effect. What is amusing about this example is that Conway's discussion is actually in the mainstream; both sides of the discussion share the belief that you generalize across data. The goal of modeling brain processes—the core of the cognitive movement—gets scant regard. My own encounter with recency was in the context of serial recall and the role of Precategorical Acoustic Storage (PAS). PAS was not a theoretical device dreamed up to account post hoc for the effects of an acoustic suffix on serial recall (Crowder & Morton, 1969). Rather, the modality-specific suffix effect was predicted on the basis of PAS, which had been dreamed up (in the context of a model of lexical access) to account for the differences in serial recall curves between acoustic and visually presented lists (Morton, 1968, 1970). These were laboratory studies, carried out with all the professional regalia one could muster, but that did not stop the two experimental papers from being rejected by the first journals to which they were submitted, essentially on the grounds that they violated some spurious canons of generality. In fact, the theory explicitly rejected the idea of generality. Certain circumstances yield one result, and other circumstances yield other results. It was certainly very clear in the theory that as far as the cause of recency was concerned, serial recall and free recall had little or nothing to do with each other (Morton, 1970). So it is very odd to find Banaji and Crowder failing to distinguish the two (with their seemingly generalizable effect), as it is odd to find Conway, supposedly in rebuttal, claiming that Crowder and Morton's position "became untenable when auditory recency effects were found in distractor paradigms" (p. 23). The study in question used free recall, and the auditory recency effect referred to involved up to 40% errors on the final item, compared with less than 5% errors in the serial recall experiments. Is a better case for two recencies needed?

The philosophy underlying the PAS approach is that the nature of the task is crucial. It doesn't really matter whether the tasks are in the laboratory. But it is even stranger to find Conway (1991) imagining that the excellent Baddeley and Hitch (1977) study on rugby games

had anything to do with either serial or free recall when he refers to "convergence of findings across research traditions." (p. 22) It is possible that Banaji and Crowder and Conway are both confusing properties of the medium with properties of memory. However, if one is looking for the properties of the medium, then even the rat is, perhaps, too complex an organism. The *Aplysia* might be thought to be the better model for learning. As it seems likely that the mechanism underlying memory findings (i.e., the human brain) has a variety of functions and a flexible architecture, it is wise to accept that recency, like other superficial characteristics of memory data, can be created in a number of ways depending on the task. What might be an uneconomical, theoretical step when considering a narrow range of memory data may seem more reasonable with a framework encompassing speech, language, problem solving, and perception.

I don't want to legislate how people should carry out research, but I do like the pursuit of theory and I do like the resulting theory to survive reality checks. I would like my chemistry principles to say something about rusty armor (I studied chemistry up to university level, and it took Primo Levy's *Periodic Table* to bring it to life). This is, of course, what the rush to naturalistic memory settings is really all about, even if this is not always apparent. Generalizations that do not extend outside the restricted environment in which they were bred are not of much use, irrespective of their beauty. Other populations and other situations give us a feel for how far we have gone

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and whether the direction is likely to be useful. However, as far as I can see, it is not the case that studies of amnesia help researchers develop memory theory but, rather, that memory theory helps them understand amnesia. The utility of much of current memory theory in understanding amnesia is well illustrated in the chapters in Cermak (1982; see also Morton, 1985). Cermak himself referred to "the skimpy theoretical base on which we are now operating" (p. 374). Whether the necessary theories arise from laboratory studies or everyday memory contexts, they will have to account for both. The correct generalizations will be at the level of mechanisms, not data. This will require thought, not observation.

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