

Archival Publication: Another Brick in the Wall

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Despite the profound changes in scientific publishing wrought by technology, many of us cling to the belief that the archival publication of primary journal articles remains the bedrock of our field. APS publications, and particularly *Psychological Science* with its 1200+ submissions per year, are outstanding journals and among the most impactful in all of psychology. But will such archival journals remain preeminent in the future? Recent developments in the internet bring seemingly unlimited electronic access to everything ever written about a particular topic. The amount of information that can be brought up in an instant on Google Scholar is remarkable, even for the most esoteric bits of psychological jargon. (I just got 41 hits on synergistic ecphory, for example). Such search engines don't distinguish between something written in one of our archival journals from something written in the popular press, and if blogs are better linked to the internet in the future, the democratization of knowledge (as in Wikipedia) may continue. Everyone's opinion on a topic may be seen as counting. As we try to envision the future of scientific publishing, we should ask how (or even whether) the traditional model of archival publication will persist. We might also ask if it has ever worked as advertised in the past.

What is primary archival publication? The APA Publication Manual, in its ponderously helpful way, has this to say on the topic (the quotes are from page 347 of the 5th edition):

Members of the scientific community generally agree that the characteristics of primary, or original, publication are that (a) articles represent research not previously published ... (b) articles are reviewed by peers before being accepted or rejected by a journal, and (c) articles are archival (i.e., retrievable for future reference).

So far so good. I suppose everyone would agree on these characteristics. (Well, let's make that "almost everyone;" this is academia, after all). The writers of the Manual continue, though, with a little simile. We can call this the Pink Floyd theory of archival publication:

Like a wall that is built one brick at a time, the peer-reviewed literature in a field is built by single contributions that together represent the accumulated knowledge of a field. Each contribution must fill a place that before was empty, and each contribution must be sturdy enough to bear the weight of the contributions to come. To ensure the quality of each contribution – that the work is original, valid, and significant – authorities in the subspecialties of a field carefully review submitted manuscripts. The peer-reviewed journals in which the literature is preserved thus serve as "journals of record, that is, authoritative sources of information in their field" (Orne, 1981, p. 3).

In the late 1980s, I served on the APA Council of Editors Committee, because I edited the *Journal of Experimental Psychology: Learning, Memory and Cognition* and all APA editors are members of the Council. Editing a journal is a hard job, perhaps the hardest in academia besides upper administration.

The Council of Editors is composed of all APA journal editors and it meets once a year, sort of like a big T-group for editors. (A digression: T-group may not be a familiar term to all readers, dating as it does from the 1970s. It stands for therapy group, and group therapy was big business back then. As Tom Wolfe pointed out in *The Me Decade* and *the Third Great Awakening*, the T-group permitted the middle and even lower classes to engage in a practice that had previously been reserved only for rich people with their therapists: “Let’s talk about me!”).

During the big Council of Editors T-group we discussed our problems as editors, and we also often discussed primary archival publication. We really bought into it. After all, this is why we were working so hard as editors. We were publishing those primary articles for posterity, for the archives! We were helping authors put those bricks in the Great Wall of Science! (Actually, more often than not, we were keeping bricks out of the wall rather than putting them in, but that’s a detail). The articles in our journals would be there, archived, forever, providing the authors a tiny sort of immortality. We actually sat around and talked earnestly about archival publication at the Council of Editors meeting. I joined right in.

What were we thinking? Were we on drugs? Were we rationalizing the cognitive dissonance from all our hard work? Were we actually reading the Publication Manual? At odd moments in those years I would say to myself: “Wait a minute. When was the last time I looked at an article from the *Journal of Experimental Psychology* (or most any other journal) from, say, 1943 or maybe 1924? After all, those papers were also “primary, archival” publications, valuable bricks in the wall, on which we were now (allegedly) climbing upward and onward, ever higher. How come we don’t know about them now? It wasn’t that the problems that occupied researchers in 1943 have now been solved. How many issues in psychology can we point to and say “Yes, that’s completely understood. No more research is needed on that topic.”?

Here’s one place where the basic premise of the Pink Floyd theory seems flawed. We might be standing on the shoulders of giants, as Google Scholar reminds us on its home page, but the particular shoulders shift as the field moves among topics. Yes, psychology and other sciences do advance, but more in fits and starts and zigs and zags than in the bricks-in-the-wall linear sequence embedded in the quote above and into some methodology textbooks. Sociologists of science know this well and so do we all from observing our own research areas. Each of our subfields has brief enthusiasms of a sort, but after a spasm of research activity in a particular direction, some invisible pendulum will swing us in another direction. (The concept of *zeitgeist*, the spirit of the times, is often used to explain such happenings, well after the fact).

In my own field of study, human memory, the great emphasis when I came into the field was on accurate remembering (two-store memory theories, levels of processing experiments, and the encoding specificity principle, among other topics). Studies were conducted showing how some variable enhanced or decreased retention in some particular paradigm and how that could be explained by someone’s theory. Errors of memory were mostly considered a nuisance and a bother, due perhaps to guessing (though that idea was not well specified). Elaborate corrections for guessing were undertaken to gain more faithful measures of accurate retention by taking errors into account to correct total performance. Rarely were errors of memory considered of interest in their own right.

Starting in the 1970s, activities both inside the laboratory and outside (especially in legal cases) led

researchers to wonder about memory errors or illusory memories or false memories. New paradigms were developed, old ones were rediscovered, and researchers directed their attention towards the fragility of remembering. Accurate remembering was not necessarily considered an aberration, but was not emphasized as it had been in the past. A huge wave of studies concerning illusory memories swept over the field and has not abated yet. In many cases, the bricks in the wall that had previously been put in place to explain workings of memory (such as the interference theory of forgetting) were neglected and considered irrelevant in the new wave of false memory studies. Yet in fact these older studies and theories were actually highly relevant to understanding the new effects that were being discovered. Interference is a potent force in creating illusory memories. However, our historical amnesia prevented researchers for a time from making the connection between the older work on interference and the newer research on false memories.

Psychologists often like to beat themselves up over the fact that we sometimes rediscover things that our intellectual ancestors knew perfectly well from yesteryear's research. I suspect this does not happen more in our field than others. The nature of graduate education in all fields seems to naturally concentrate on the present scene and to ignore the past. (How many departments still even require a course in the history of psychology for graduate students?) When I was chair of my department, I would often look at graduate syllabi, and it was my impression that graduate education in most any field usually consists of learning about the last 10-15 years of research. Yes, there may be exceptions to this rule, with a nod given to older studies (maybe a paper or two are assigned), but the focus is on recent history and this is probably a natural (and probably good) impulse. I went to graduate school in the late 1960s and early 1970s, so I learned mostly what had happened in my field from the mid-1950s to 1970 or so. We pretended that cognitive psychology started in the 1950s, but most every topic in the field has a much longer history. Sure, I read some things before that time – Ebbinghaus, Bartlett, William James — but not too many, and the things I read were usually not the archival bricks in primary journal articles.

I read the New York Times online each day and one of my favorite features is the “On this day” item at the bottom, which lists an historical event and permits the reader to examine the story that appeared about it in the Times. I also use it as a quiz: Can I think of one paper or book published in the year of the Times’ story that has influenced my work or my field (or that I just know about, never mind its influence)? If the date is in the mid-1950s or later, I can always do it. However, earlier than that, it’s embarrassing how often the answer is no. “On July 18, 1936, the Spanish Civil War began as Gen. Francisco Franco led an uprising of army troops based in North Africa.” Nothing leaps to mind for 1936, but maybe if I worked at it? “On July 15, 1918, the Second Battle of the Marne began during World War I.” I draw another blank. In fact, until I read the story, I didn’t know much about the Battle of the Marne. “On July 19, 1941, British Prime Minister Winston Churchill launched his “V for Victory” campaign in Europe.” Aha! Melton and Von Lackum’s paper on two-factor interference theory came out that year. “On July 21, 1925, the ‘monkey trial’ ended in Dayton, Tenn., with John T. Scopes convicted of violating state law for teaching Darwin’s theory of evolution. (The conviction was later overturned.)” Nope, nothing leaps to mind. One out of four is about par for the course for me for those years before the 1950s.

Perhaps dates are not good retrieval cues for important papers; maybe the information about papers or books is available in my brain but not accessible with a cue like “1918.” Maybe, but I doubt it.

I suspect (but of course cannot prove) that our field is mostly ignorant of the great body of research of

our distant predecessors, even those in our own subfield. The bricks in the wall are effectively underground. Does this mean our science is not cumulative? I have heard arguments supporting that claim, but I suspect that the truth lies somewhere in the middle. We do make progress, but somewhat haphazardly, in a zig-zag rather than linear pattern. I convince myself of progress by looking at specific topics covered in high-level textbooks of the day. Go back to an important textbook in your field from (say) the 1920s or 1950s or 1980s and compare the coverage of a specific topic you know well to that of today. Although differences will exist due to skill of the authors, I usually find that today's books are more sophisticated in their treatments than were analyses of the same issues in the books of yesteryear (although of course this depends on the topic). The textbooks seem to provide clear evidence of progress across time. Yet even assuming our field does progress, it is clearly not in the linear, bricks-in-the-wall fashion envisioned by the anonymous writer in the Publication Manual. Yes, fads and fashions invade our subfields and the focus of scientific attention frequently shifts. We get tired of topics when the pace of progress slows and the problems get hard, so we move to a new area where the questions are fresh and seem more interesting. However, the field will eventually return to important, enduring topics (like interference in memory) even if they are discarded for a time.

If today's scientific world in psychology seems complex and fragmented due to the new methods of publication, we can rest assured of one fact: the situation will be much more complex in the future, when the great controversies and papers of today are relegated to the dustbins of history, just as much work in our field from the 1930s and 1940s seems to be today. So enjoy this time; these might be the last of the good old days, when primary archival publication still matters.