Horizon

Reflections on intersections between cognitive and social psychology: A personal exploration

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Abstract

I appreciate the opportunity offered by the editor to reflect on the relationship between cognitive and social psychology. This topic has interested me my entire professional life, because I was admitted to graduate school to study social psychology and then eventually migrated to cognitive psychology. The organization of this paper is as follows: I first relate my (somewhat puzzling) personal experiences that led me to wonder about relations between cognitive and social psychology. I suggest that, for many topics, the placement of a topic of study in one field or the other is arbitrary. Next I selectively review some common historical influences on the development of both fields, ones that have made them similar. Both grew from common seeds, which include Gestalt psychology as it became applied to a wider array of topics, experimental psychologists becoming interested in attitude change during and after World War II, and Bartlett's famous book on Remembering: A study in experimental and social psychology. Next I review some research from my lab that picked up themes from Bartlett's work and that, in some aspects, combines cognitive and social approaches. I also discuss the issue of memory conformity or the social contagion of memory, and conclude with thoughts about how social and cognitive psychologists might collaborate on an exciting new arena, creating empirical studies of collective memory. Copyright © 2010 John Wiley & Sons, Ltd.

AUTOBIOGRAPHICAL INTELLECTUAL HISTORY

At the significant risk of boring the reader, I will begin this essay with some personal history because it is germane to the topic at hand. My undergraduate education occurred at Washington & Lee University, a small, private liberal arts university in Lexington, Virginia. Its name arose because George Washington initially gave funds to support it (and it was named Washington College in his honor), but after the US Civil War Robert E. Lee (the commander-in-chief for the Confederate forces) became its President (1865–1870) until his death. After his death, the name was changed and has endured until today. I arrived 100 years after Lee, in 1965, and was immediately attracted to the social sciences in my studies. I took many Psychology, Anthropology, and Sociology courses, but was decided that Psychology would be my major area. In those days, there were only three psychologists on the faculty, and they were mostly from the hard-nosed tradition of classic experimental psychology. Many of our courses focused on animal and human learning, as well as physiological psychology (today called behavioral neuroscience), perception and the like.

Somewhere along the way I decided that I wanted to go to graduate school, but in what field or subfield? My research experience was with David Elmes, my undergraduate mentor, on human memory. However, my interests were increasingly broad from taking anthropology and sociology. What to do? Elmes suggested that I should look into social psychology (this was probably early in 1968). However, given the training of the faculty, my department had no course in social

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psychology, so I asked for more advice. He recommended a great new social psychology text, Roger Brown's *Social Psychology* (1965).

I bought the book and read it during the summer of 1968. What a wonderful book! It had a large section on social structure, on personality and society, and a last section on "social psychological processes." I liked the book so much I read parts of it twice, being particularly struck by the language and cognition sections.

During the fall of 1969, I applied to the leading programs in social psychology, where "leading" was defined by advice of my mentors. After all, what did I know? In retrospect, it is a wonder that anyone bothered to consider my application. I had no coursework or research experience in any field of social psychology, no letters of reference from social psychologists, and so on. But, luckily, something about my application appealed to the social psychology faculty at Yale, and I was admitted to that program.

I arrived in the fall of 1969, and found that Yale was a special place to study and learn. The social psychology faculty at the time included Robert Abelson, Irving Janis, Richard Nisbett, Charles Kiesler, David Mettee, and David Hamilton. While I was there, some of these faculty members left and others arrived—Phoebe Ellsworth, Judy Rodin, and William McGuire. I took courses from nearly all of these people and learned much about social psychology. However, one of the biggest lessons came before classes even started. David Mettee asked me why I had decided to apply to graduate school in social psychology. I told him about reading Roger Brown's great book. He looked at me a bit askance and said, "Oh, that is a great book, but it is not really much about social psychology. Only a couple of chapters are really relevant to the research interests of the people at Yale." I eventually discovered that he meant Chapters 11 and 12, which were on attitude change and person perception. Brown's book was much broader than research interests in social psychology, which then (at Yale) were concentrated on those two topics.

I was shocked; here I had applied in social psychology thinking that the field represented something of a merger of my interests in psychology, sociology, and anthropology, only to discover that the field (at least at Yale) concentrated only on a small subset of those topics. (Of course, every program focuses on some subset of issues, but Brown's book was unusually broad for the time). And Mettee's opinion turned out not to be quite true. The courses I took were excellent. In one, we read a draft version of Janis's book on *Groupthink*, debated it and commented on it. He was in the midst of revising during our course, so perhaps he made changes based on students' thoughts. In my first year, a meeting of researchers interested in attribution theory occurred, which led to the book *Attribution theory: Perceiving the causes of behavior* (Jones, Kanouse, Kelley, Nisbett, Valins, & Weiner, 1972). I also read this book in draft form in my courses with Dick Nisbett. However, attitude change and person perception certainly were the chief focus of the faculty in social psychology at the time.

In the meantime, however, I had hit it off with another faculty member, Robert G. (Bob) Crowder, who studied human memory. I took his course on Human Learning and Memory my first semester, and we read Neisser's wonderful book describing the new field of *Cognitive Psychology* (1967). Here I found material on language, memory, and other topics that Roger Brown had treated under the rubric of social psychology.

The fact that language could be covered under both social and cognitive psychology got me to wondering about disciplinary boundaries. Why are some topics considered social psychology and others cognitive psychology? Of course, the whole idea of *cognitive psychology* was then a new intellectual venture, with the field having been named by Neisser's book. In the late 1960s, there were no undergraduate courses on cognitive psychology like the ones so common today. Crowder advised me on publishing a project I had started as an undergraduate (Roediger & Stevens, 1970), and he showed a strong interest in my intellectual development. After a year, I formally shifted into the cognitive psychology graduate program, but I continued taking courses in both fields.

Unlike many students now, I was a course taker, despite the fact that our graduate program had virtually no requirements beyond taking a two-course sequence in statistics. I felt that this was the only chance in my life to take courses from some of the very best people in the field, so I took three a semester for most of my 4 years at Yale. Endel Tulving arrived in cognitive psychology in 1971 and became my co-mentor, with Crowder. But Wendell Garner, Allan Wagner, Robert Rescorla, and others in experimental psychology were also quite influential in my education, as were the aforementioned people in social psychology. I felt blessed to be at Yale at that time.

My graduate training in both cognitive and social psychology has stood me in good stead for my whole career, despite the fact that both fields have undergone seismic shifts in the ensuing 40 years. However, I am still left wondering—why are certain topics included in social psychology and other topics that seem (on the face of it) of a similar nature included in cognitive psychology? For example, attitude change is cognitive in nature; an individual changes his or her attitude, albeit often in response to social forces. The same is true for memory. Yet attitude change is considered a topic of social

psychology and memory one of cognitive psychology. Is it simply historical accident as to where topics will be pigeon holed? Is it merely the affiliation of the people who begin the line of research that determines placement of the different topics? Did the balkanization of topics have to do with the increasing specialization of our fields? Of course, in the early development of these disciplines the case was different—the great researchers tended to be generalists and not specialists—as I review in the next section.

HISTORICAL INFLUENCES

Interestingly, broad swathes of both cognitive and social psychology seem to have arisen from similar historical roots. I will not be inclusive here, but rather point to some prominent examples.

As noted above, in 1932 Bartlett published *Remembering: A study in experimental and social psychology*, a great work that is claimed as a source of inspiration by both fields. In social psychology, excerpts were published in classic books of readings in social psychology (e.g., Proshansky & Seidenberg, 1965) and the method of serial reproduction led to Allport and Postman's (1947) techniques used in analyzing *The psychology of rumor*. Although it took a while for Bartlett's messages about perceiving and remembering to penetrate researchers investigating those topics, his work was cited warmly by Neisser (1967) in his classic text and went on to guide much research from the 1970s to the present in memory. Remembering, in Bartlett's view, occurred within the individual, but was highly influenced by social factors; hence, remembering represents a prime locus to study the interaction of social and cognitive psychology.

In a different tradition, Carl Hovland was trained as a classic experimental psychologist who conducted research on memory and other topics. As such, he would be considered a forerunner of cognitive psychology (known in the 1950s as human experimental psychology). However, many American psychologists were brought into new research areas because of World War II, and Hovland was no exception. He oversaw a large set of studies on attitudes and attitude change that resulted in a number of volumes published after the war to report his team's experimental research in great detail.

Hovland, Lumsdaine, and Sheffield (1949) published *Experiments on mass communication* that helped to begin research on attitudes and attitude change. It was actually the third volume in the series, and several followed this one during the 1950s. Hovland and his colleagues worked in the Research Branch of the U.S. Army's Information and Education Division, which was further subdivided into smaller divisions (e.g., the Experimental Division, the Survey Division). The experiments reported were carefully conducted and their work opened many issues to study (e.g., chapter 8 reports the first experiments on the issue of whether presenting one side or two sides of an argument produces more attitude change). Hovland then went on to lead many more investigations of attitude change, and the ensuing volumes were published by Yale University Press (e.g., Hovland et al., 1957; Rosenberg & Hovland, 1960).

Many psychologists trained at Yale in the 1950s seemed to thrive on education in both experimental psychology and social psychology. Among the names associated with those books as editors and authors were Milton Rosenberg, William J. McGuire, Robert P. Abelson, Jack W. Brehm, Harold H. Kelley, Timothy Brock, Abraham Luchins, Arthur R. Cohen, Irving L. Janis, Norman H. Anderson, Muzafer Sherif, Harriet Linton, Gerald S. Lesser, and several others. Numerous researchers who went on to fame and fortune in psychology were trained in this milieu. Other people thanked in the preface to the 1949 volume as having made contributions are John Dollard, Paul F. Lazarsfeld, Quinn McNemar, Leonard Doob, and Robert K. Merton!

My point here in recounting all these (to-become-famous) names from publications in the 1940s and 1950s is how readily people could identify with both human experimental psychology and social psychology. While Hovland was leading these efforts, he continued his research in learning and memory and published on these topics, too, until his untimely death in 1961 (e.g., he wrote the chapter on learning and memory in S.S. Stevens' classic *Handbook of experimental psychology* [1951]). In those days it was not uncommon for people to have interests in various topics that today would be considered in different fields. In reading the Yale books on attitude change a decade or more after they were published, I was perplexed as to why attitude change was not as much as (or more than) a topic of cognitive psychology as of social psychology. After all, weren't attitudes within the individual psyche? Was not the study begun by Hovland, affiliated with the field of human experimental (later cognitive) psychology? Yes, attitudes are influenced by social forces, but so are perceptions and memories, and yet those topics were assigned to cognitive psychology.

Consider Loftus and Palmer's (1974) famous experiments on how memory for a scene could be transformed when people later received a question describing the scene in various ways. They showed that simply changing the nature of a verb in one question in a questionnaire could change later recollections for the scene (whether broken glass had appeared in it). Later Loftus, Miller, and Burns (1978) had people watch a traffic accident where a car failed to stop at a sign (shown as either a stop or yield sign to different groups), and then later recollection of the type of sign was altered if subjects had been asked a question that referred to the wrong kind of sign in between the event and the later test. That is, a stop sign viewed in an accident could be transformed in subjects' memories to a yield sign if the yield sign had been suggested in a later question. These experiments would seem to represent an influence of social information on memory, yet these studies were virtually all published in cognitive psychology journals. Attitude change studies, on the other hand, fell in the province of social psychology journals.

Another common source of inspiration for both modern cognitive and social psychology arose from the broad interests and applications of the Gestalt psychologists (e.g., Köhler, 1929). Kurt Lewin, one founder of social psychology, was strongly influenced by Gestalt ideas. As Alfred Marrow puts it in his biography of Lewin, ''Though he was never a completely orthodox Gestaltist, he did become a vital force in the new movement and contributed to it his own special insights. To Lewin, Gestaltism seemed closer to actual experience than did piecemeal analysis, which had prevailed in psychology during his prewar student days'' (Marrow, 1969, pp. 13–14). Lewin and his students applied Gestalt ideas or broadened them to issues of memory, group dynamics, and action research.

Gestalt ideas figured in other theorizing. Fritz Heider, a close colleague of Lewin, proposed a balance theory of social phenomena that was derived from Gestalt ideas of good form (albeit the Gestalt psychologists were writing about perception and Heider about personal relationships; Heider, 1958). Heider's theory applied to cognitions (keeping attitudes and beliefs in harmony, as in various cognitive consistency theories proposed later; Abelson, Aronson, McGuire, Newcomb, Rosenberg, & Tannenbaum, 1968). These balance theories influenced Leon Festinger's (1957) theory of cognitive dissonance—how mind deals with conflicting conceptions and brings them into harmony—an idea which fuelled much research in the 1960s and 1970s. Again, we might ask why cognitive dissonance theory was considered social psychology. After all, the competing cognitions exist in the mind of one person who had to change his/her behavior to fit the cognition or, more likely, change the cognition to fit the behavior. Heider's ideas also helped spark attribution theory, and Kelley (1967) credited Heider's insights in his important chapter.

Of course, Gestalt psychology was a huge force in the development of modern cognitive psychology, too—in theorizing about higher-level vision, remembering, and thinking. Once again, we see common historical influences in the two fields. Two famous effects in memory—the von Restorff effect and the Zeigarnik effect—arose from students of Köhler and Lewin, respectively (von Restorff, 1933; Zeigarnik, 1927). The former refers to the fact that a distinctive or isolated item encoded into memory against a background of similar items (e.g., a picture embedded in a list of words) is well remembered; the latter refers to the phenomenon of better retention of incompleted tasks than completed ones (although this effect is in some dispute). Both phenomena were originally interpreted with Lewin's ideas.

The foregoing examples, which merely scratch the surface, reveal some common influences in the development of cognitive and social psychology. Although the term ''social cognition'' was created in the 1970s to refer to work squarely rooted in both fields, the topic is really much older. Cognitive and social psychology have long run intertwined courses in terms of ideas (about the organization of mental life), in terms of research methodologies, and even in terms of pioneers like Bartlett, Lewin, Hovland and Festinger who conducted research in both domains.

My quest to understand why some topics belong to social psychology and others to cognitive psychology continues. However, a promising lead was provided to me in the 1980s by Reid Hastie. He gave an excellent talk at Purdue University on his interesting work on person memory (e.g., Hastie & Kumar, 1979), work that was almost exclusively published in social psychology journals. Reid and I were friends in graduate school at Yale, and he did his graduate work with both William McGuire in social psychology (for his masters thesis) and Endel Tulving in cognitive psychology (for his Ph.D.), so it was natural that he was one of the pioneers of social cognition in the 1970s and 1980s. After his talk, I asked him: ''Why, if you are studying memory, is your research considered social psychology rather than cognitive psychology?'' Without missing a beat, he said ''Because I lie to my subjects.''

Perhaps that provides as close as we can get to a criterion of why some research is considered cognitive psychology and some social psychology. However, some of my own work as a cognitive psychologist, reviewed below, breaks this rule, so I guess that makes me partly a social psychologist, too.

SOCIAL ASPECTS OF REMEMBERING: CONTINUING BARTLETT'S RESEARCH TRADITION

In many ways, Bartlett's (1932) project represented a promissory note rather than a finished product. As I have pointed out elsewhere, despite the fact that the term ''experimental'' figured prominently in the book's title, Bartlett never actually reported a true experiment (Roediger, 1997, 2000). Rather, Bartlett's work thrived on the controlled demonstration, often with only a couple of subjects who were usually tested under rather casual conditions. His research relied on observation, anecdote, and demonstration. He criticized Ebbinghaus's (1885/1913) compulsive and controlled research on memory, and especially his choice of materials, as unnatural (Bartlett, 1932, pp. 2–6). Yet Bartlett then selected the Native American story of ''The War of the Ghosts'' as his own primary material to begin his study of remembering. This story is related to normal English prose about as much as nonsense syllables to words, and many of Bartlett's most interesting findings hinge upon the fact that the story is ecologically invalid (e.g., he comments on subjects' effort after meaning as they attempted to understand the bizarre story by casting it into more familiar forms).

Over the years, I have been drawn to Bartlett's work and tried to answer some puzzles that he left. Strangely, despite the fame of his book and the huge number of psychologists who must have read it, relatively few researchers have systematically explored the techniques and findings that Bartlett reported in his casual studies. Yes, the book has been frequently cited; yes, it has indirectly inspired many later efforts in social and cognitive psychology of remembering: but the actual use of Bartlett's research methods, and strong confirmatory support for his conclusions using those methods, are mostly lacking. This fact represents a strange situation; I do not know a parallel case in which reverence for research is held based on the rather flimsy smattering of fact provided in the book. The plural of anecdote is not data or at least it should not be.

In the remainder of this essay, I review my own work on various projects inspired by Bartlett's research, both on individual and social aspects of remembering. I will not try to review the literature on social aspects of remembering—there is too much—but will draw in some relevant work on occasion. Weldon (2001) and Ross, Blatz, and Schryer (2008) provide excellent reviews of social aspects of remembering.

EFFECTS OF REPEATED TESTING

The most famous technique that Bartlett (1932) used was the method of repeated reproduction. His subjects (usually Cambridge University students) would read *The War of the Ghosts* and be tested a short time later. Then he would test the same person again (sometimes repeatedly) over a period of weeks, months, or even years, with the schedule of testing mostly driven by when he encountered the person on campus. Bartlett did not aggregate his data, but simply provided recall protocols from sample subjects. These examples showed that recall became increasingly poor over time, with students forgetting much of the story and skewing it toward a form more familiar to them, such as a fairy tale (sometimes they even tacked a moral onto the end of their recollections, quite unlike the original).

Bartlett proposed that several processes underpinned changes in recall over repeated tests: leveling (dropping unfamiliar details), sharpening (selecting certain details for embellishment), and rationalization (making the story more rational than it actually was). Borrowing a concept from Henry Head (1920, 1926) and using it in a rather different way than Head did, Bartlett proposed that people encoded and remembered information via schemas, organizational structures that were developed to help store information and to guide its retrieval. Schema-driven processing could introduce errors when students filled in details required by the schema, even if they had never occurred in the story.

Bartlett's reports emphasized how memories became shorter and more error prone with repeated reproduction. However, another British psychologist, P.B. Ballard (1913), had published experiments using repeated testing that had shown the opposite of what Bartlett claimed. After studying poetry and asking people to repeatedly recall it (without further study), Ballard found that the students would often recall lines of poetry on later tests that could not be recalled on earlier tests. Sometimes the total amount of material recalled on later tests exceeded that on earlier tests. Ballard called this phenomenon reminiscence: Recall of material on a later test that could not be recalled on an earlier test. Others replicated the effect (e.g., Brown, 1923), and it received some experimental scrutiny off and on over the years. In the modern era, Erdelyi and Becker (1974) brought the topic back into cognitive psychology in an important series of experiments in which they found that total recall of a set of pictures improved across three successive tests (see too Roediger & Thorpe, 1978).

These facts pose a puzzle: Why did the results of Bartlett (1932) and Ballard (1913) disagree? Both used repeated testing, both used connected discourse (albeit prose and poetry), yet Bartlett emphasized forgetting and distortion across repeated tests whereas Ballard (and later Erdelyi and others) showed improvements in recall over such tests. Why? (A related puzzle is why Bartlett [1932] never cited Ballard (1913) or Brown (1923). Ballard even published in the *British Journal of Psychology*, the pre-eminent British journal of its day.)

I began conducting research on reminiscence and hypermnesia in the mid-1970s and was merrily publishing my articles over the years. However, every year when I taught courses on cognitive psychology or memory, I would dutifully cover Bartlett's repeated reproduction results using *The War of the Ghosts*, because those classic studies made it into virtually all textbooks on cognitive psychology and human memory (and usually introductory psychology texts, too). The juxtaposition of my research (showing improvements of recall on repeated tests) and my lectures (discussing how performance grows worse on repeated tests) aroused enough cognitive dissonance that I decided to conduct research to reduce my dissonance. Around 1990, with the aid of a graduate student named Mark Wheeler, we decided to try to unravel this puzzle.

Wheeler and Roediger (1992) reported two experiments that examined two probable reasons for the discrepancy between Ballard's (1913) and Erdelyi and Becker's (1974) results on the one hand and Bartlett's (1932) results on the other. These were the type of material used in the two types of studies and the length of time between tests. Erdelyi and Becker used sets of picture stimuli and short intervals between tests, whereas Bartlett used *The War of the Ghosts* and relatively long intervals between tests.

In our first experiment, Wheeler and I developed a story that had a reasonable schema or story grammar, but that was a bit unusual in that it mentioned many concrete objects. The story had been written around a set of 60 pictures we had developed, so they could be items referred to in the story. Thus for one set of subjects they heard the story (simulating Bartlett's story condition), but whenever the name of an item in the story was first introduced, a picture representing that item would pop up on a screen for 7 seconds. Subjects were told to remember the picture items (they would have to write down their names later), but they were also told that the story was given to help them remember the pictures and so they should pay attention to the story, too. We referred to this as the Picture + Story condition. A different set of subjects was presented with the set of pictures with no story (as in typical hypermnesia research done by Erdelyi and Becker [1974]) and they heard the name of the picture when it was presented; this condition we called the Pictures + Names condition. Sixty subjects received pictures in one of these two conditions.

In addition, we also varied the number of tests given to subjects after they studied the pictures. Soon after the study phase in each presentation condition, subjects received 0, 1, or 3 tests. Bartlett usually used one test shortly after study, whereas hypermnesia researchers usually provide several immediate tests to examine possible improvements over time and repeated tests. We included a 0 (no test) condition to examine the effects of the early tests on tests that we gave later. In all tests, subjects received sheets of paper with 60 lines and were asked to fill all the lines, guessing if necessary. This forced recall procedure was developed by Erdelyi and Becker (1974) to keep response criterion constant. That is, if improvements in the number of items correctly recalled occurred over repeated tests, one could rule out the possibility that subjects loosened their response criteria and guessed more over repeated tests (but see Roediger, Wheeler, & Rajaram, 1993, for problems with this technique). One week later, all subjects returned to the lab and took three more successive forced recall tests.

The design and results are shown in Table 1. The numbers on the left side of the table refer to the testing conditions immediately after study and after a week; so, for example, subjects in the 1–3 condition received one test immediately after study and then three tests the following week. The table may appear complex, but the message to be gleaned from it is reasonably straightforward. First, the story did help subjects remember the pictures, as the numbers in the Pictures + Story condition at the bottom of the table are generally greater than those in the Pictures + Names condition at the top. Thus, the story did induce schematic processing. Second, hypermnesia was found in the immediate test conditions, although it was actually greater in the Pictures + Story condition than in the standard (for hypermnesia experiments) Pictures + Names condition (see Roediger, Payne, Gillespie, and Lean [1982] as to why this is so).

The third analysis was of most importance for present purposes: What happened after a 1 week retention interval, especially in the condition in which subjects had been given a single test immediately after learning (i.e., Bartlett's standard condition)? What we found there was great forgetting over the week delay (e.g., from 31.8 items recalled to 23.3 items recalled in the 1–3 condition for the Pictures + Story condition). The same forgetting occurred in the various other conditions where the comparison could be made. Thus, at least in the sense that forgetting occurred, Bartlett's result was replicated. Of course, our conditions and our materials did not permit us to examine the types of errors Bartlett (1932) reported.

Table 1.   Mean number of pictures recalled as a function of presentation context and testing schedule in Experiment 1

	Initial tests				Delayed tests			
Group	T1	T2	T3	T3–T1	T1	T2	T3	T3–T1
				Pictures + Names				
3–3	26.6	27.2	28.4	1.8[*]	25.2	26.3	26.0	0.8
1–3	25.7				20.2	21.7	23.0	2.8[*]
0–3					16.7	17.5	17.5	0.8
				Pictures + Story				
3–3	32.7	35.0	36.4	3.8[*]	31.8	33.0	33.4	1.6[*]
1–3	31.8				23.3	25.0	25.6	2.3[*]
0–3					17.4	17.2	18.4	1.0

Note. All groups took three tests in the delayed session. Group 3–3 received three tests in the initial session. Group 1–3 received one test in the initial session, and Group 0–3 took no memory tests until the delayed session.
[*]These conditions demonstrated reliable hypermnesia across the three consecutive tests as indicated by a simple analysis of variance.

A final point worth noting from Table 1 is the effect of the number of tests given immediately after study on recall a week later. For example, in the Pictures + Story condition, recall on the delayed tests was worst for subjects who had not been tested at all during the first week (17.6 pictures recalled), much better if one test had been given (24.6), and best if three prior tests had been given (32.7). These results dramatically show the power of testing memory as a mnemonic enhancer on later tests, a point of increasing attention in cognitive psychology (see Roediger & Karpicke, 2006).

Although Wheeler & Roediger's (1992) first experiment obtained hypermnesia (on immediate tests) and forgetting (on spaced tests), in a sense reconciling the two previous traditions of work, we did a further experiment using prose materials (a John Updike short story) to replicate and extend this work. We were able to replicate the effects of the first experiment, but we still did not see dramatic distortions of the sort Bartlett reported. We also tried a classroom pilot experiment using *The War of the Ghosts* and we found the same thing—with short delays between tests, there was actually improvement in recalling *The War of the Ghosts* on a second test. However, with a week between tests, we obtained forgetting (but not much distortion). Thus, although our results created some harmony between the two traditions of research begun by Ballard (1913) and Bartlett (1932), we never really obtained results showing the types of errors Bartlett reported, a fact which led some years later to another project.

CAN BARTLETT'S REPEATED REPRODUCTION RESULTS BE REPLICATED?

In 1987 I was at a conference in Toronto with Donald Broadbent, the eminent British researcher who had received his Ph.D with Bartlett. I had been looking into the history of Bartlett's research, trying to find papers in which the work had been replicated, with little luck. I asked Broadbent if anyone had ever replicated and confirmed Bartlett's research that had been rather informal as reported in *Remembering*. Broadbent said "Yes, certainly, that has been done." I asked him for the relevant references, and he said he would have to get back to me when he returned home.

I returned home and waited, but nothing arrived. So I wrote to Broadbent to remind him of our conversation. He wrote back, somewhat sheepishly, saying that he had not forgotten my request, but that he had had trouble locating any successful replications. He now tended to agree with me that perhaps there were none. He mentioned Paul's (1959) work, but admitted that it did not really fill the bill. I had also written to other people, and Dick Neisser directed me to a paper by Gauld and Stephenson (1967). Broadbent did the same, but he said that the paper created more problems than it solved. In fact, Gauld and Stephenson argued that Bartlett's results were probably artifactual. They pointed out that Bartlett's instructions to his subjects, like much of his procedure, were somewhat vague, and deliberately so. Bartlett wrote that "I thought it best, for the purposes of these experiments, to try to influence the subjects' procedures as little as possible" (1932, p. 78). However, Gauld and Stephenson argued that Bartlett did not even make clear whether he actually asked his subjects to remember the story of *The War of the Ghosts*, strictly speaking. Rather, they surmised that Bartlett's request may have been for the students to retell the story rather than remember it. If so, then the fact the subjects deviated so greatly from the original and

introduced fanciful deviations and errors may be understood as part of the creative process in deliberately reconstructing the story. Had they been given strict instructions to remember, the results might have been very different. As they noted:

> Most people who retell a story are unlikely to care very much whether the story they retell is the same, detail by detail, as the story they originally heard. In other words, they are most unlikely to take pains that what they come out with is always what they remember rather than what they guess at or even consciously invent. Now if the changes and inventions in the reproduction of stories . . . are to serve as the foundation for a theory of remembering, [then it should be established that the subjects] were indeed seriously trying to remember, and were not more or less consciously guessing or romancing in order to fill in gaps in their memories'' (Gauld & Stephenson, 1967, p. 40).

Gauld and Stephenson (1967) conducted research to test these ideas, varying the type of instructions given to subjects before they attempted recall. In general, distortions in recall of the sort Bartlett had reported were obtained only under conditions when subjects were told to be lenient in recall—to retell the story—rather than when they were instructed to remember the story and to be accurate. Thus, although they did in a sense replicate Bartlett, it was under not under conditions that would normally be called *remembering*, with an emphasis on accuracy (see too Wynn & Logie, 1998). They were not optimistic that Bartlett's research was really replicable under standard instructions for remembering, with an emphasis on accuracy.

Yet there was an important difference between Bartlett's (1932) original research and Gauld and Stephenson's (1967) replication attempt. The latter gave their successive tests shortly after learning but, as noted previously, Bartlett tended to give one test shortly after learning but then gave later tests spread over weeks, months, and even years. As shown by Wheeler and Roediger's (1992) experiments, this difference may be critical.

In the late 1990s, Erik Bergman and I set out to attempt a replication of Bartlett's (1932) findings under conditions in which subjects were instructed to remember, but with long retention intervals between occasions of remembering. We hoped to simulate Bartlett's (1932) conditions with the assumption that he really did give instructions to remember. Subjects in our experiment read *The War of the Ghosts* twice and then attempted recall of it three times: 15 minutes after reading, then again a week later, and finally 6 months later for as many subjects as we could locate at that point. The experiment and scoring were surprisingly difficult to pull off, which may be one reason why no replications had been published. However, in the final analysis, we were able to replicate Bartlett's results with instructions to recall (and with a warning against "romancing"), contrary to Gauld and Stephenson's (1967) conclusion.

Figure 1 serves as a reasonable summary of the results. The data on the left are from subjects who participated in all three sessions, which involved testing them soon after hearing the story, 1 week later, and then again 6 months later. The
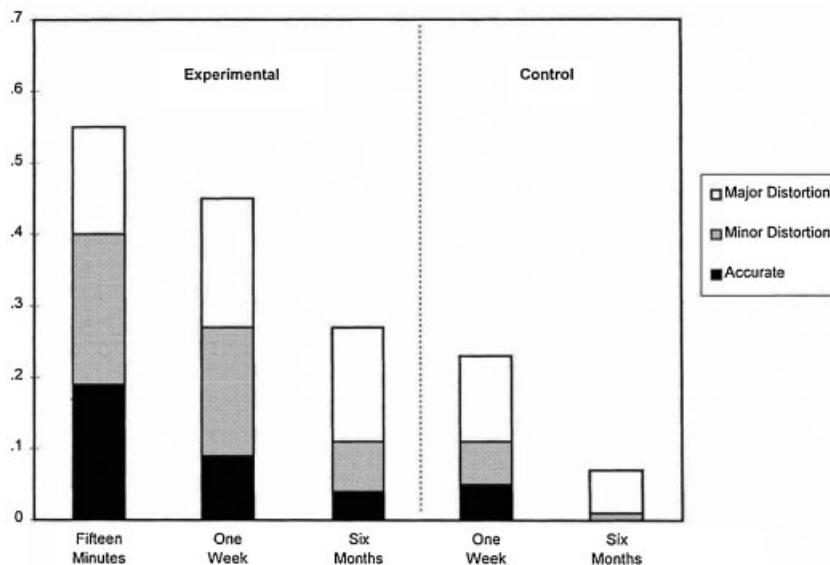
Figure 1. The proportion of items recalled accurately, with only a minor distortion or with a major distortion. The experimental conditions on the left differed from the control conditions on the right only in having a first test that occurred 15 minutes after the story had been read. After Bergman and Roediger (1999)

data on the right are for subjects from a control condition in which the immediate test was omitted and only the 1-week and 6-month tests were given, to examine the influence of that first test. The dependent measure was the number of idea units recalled where an idea unit, as the name implies, is a basic idea in the story. (Our scoring was based on Mandler and Johnson's [1977] system that decomposed *The War of the Ghosts* into 42 idea units). We scored units as to whether they were essentially correct, introduced a minor distortion, or were distorted in a much more serious way (major distortion). We can consider major distortions to be the sort of dramatic errors that Bartlett's subjects reported.

The results from the Experimental conditions on the left (with all three tests given) show that immediately after presentation, subjects could recall 40% of the idea units accurately or with only a minor distortion. This figure dropped to about 27% after a week and to 11% after 6 months. Forgetting happens. Note that the 1 week figure is inflated by the initial test given just after recall, because if the comparison is made to the subjects who did not receive an immediate test in the Control condition, the comparable level of more or less accurate recall was only 12%. Similarly, at 6 months the control condition that did not have the immediate test got almost nothing right.

For the Experimental condition, the amount of major distortion did not seem to change much over repeated testing, with about 15% major distortions on all tests. However, the data in Figure 1 uses what Koriat and Goldsmith (1996) call input-bound scoring; that is, performance is scored by what percentage of the items studied (or potentially input into memory) can be recalled. For situations where performance changes greatly over conditions (such as in this experiment), they advocated another way of examining recall data, output-bound scoring. This procedure is especially useful with long retention intervals where performance is greatly depressed. Output-bound scoring answers two basic questions: What percentage of the material that the subject recalls is correct? What percentage is distorted? The idea is to use the number of items recalled as the denominator in computing a percentage of accurate and erroneous recall, given the amount recalled.

Using output-bound scoring, we can re-interpret Bartlett's hypothesis (involving increasing forgetting and greater distortion over time and tests) as stating that the proportion of recollections involving a major distortion should increase over repeated tests. Our results bore out this prediction. The proportion of recall that involved major distortions increased from .27 after 15 minutes to .40 after a week to .59 after 6 months! In examining the data on the right, we can also see that Bartlett's procedure of giving his subjects a test soon after learning actually reduced the proportion of major distortions in material, because without the nearly immediate test, .52 of subjects' idea units recalled involved a major distortion after a week and the figure was .75 after 6 months. The act of immediate recall produced a testing effect, ''freezing,'' as it were, some memories in accurate form (see Kay, 1955).

In conclusion, the work by Bergman and Roediger (1999) verified the main conclusions of Bartlett's (1932) claims in more careful research: over time, not only do people forget more (hardly a surprise), but the proportion of their recall (at least of the War of the Ghosts) becomes more error prone and once errors creep in, they seem to remain from repeated recall. One can wonder why it took nearly 60 years before a paper replicating Bartlett's casual research was published, and I do not have an answer, but better late than never. Gauld and Stephenson's (1967) surmised that Bartlett's results cannot be replicated under instructions to remember is also overturned by this work.

SERIAL AND REPEATED REPRODUCTION

Another of Bartlett's (1932) famous results arose from comparing serial and repeated reproduction. The latter procedure, already discussed, involves subjects repeatedly retrieving the same information. On the other hand, serial reproduction involves a series of subjects recalling material, but with each studying the previous subject's recall and then trying to recall from that. That is, subject 1 (just as in repeated reproduction) studies and recalls some target material, then subject 2 studies what subject 1 recalled and tries to recall that, subject 3 studies the protocol produced by subject 2 and then recalls it, and so on. This procedure resembles the children's game variously called ''rumor'' or ''telephone.''

Bartlett was interested in social transmission of ideas and what happens to them over successive transmissions, and serial reproduction represents an analog of that process. On the other hand, repeated reproduction represents how memories change as the same person repeatedly retrieves them. Bartlett again did not do careful experiments to compare the two techniques, but rather gave sample protocols of each. He observed that serial reproduction produced greater forgetting and distortion than repeated reproduction (although he never did a quantitative direct comparison). Chapter VII of his book reports his research on serial reproduction and it is one of the most interesting in the whole book. He used *The War of the Ghosts* as material, but he also used several other stories and passages, both fiction and nonfiction. The changes

were often astounding, and Bartlett concluded that ''In every single case, except that of cumulative stories, the final result, after comparatively few reproductions, would hardly ever be connected with the original by any person who had no access to some intermediate versions'' (1932, p. 171).

Serial reproduction was used in other studies after Bartlett's, perhaps most famously in Allport and Postman's book on *The Psychology of Rumor* (1947), which itself spawned further work. However, no studies exist that directly compare serial and repeated reproduction. My students and I thought it was worth the effort to correct this oversight, and we conducted an experiment to do just that.

Roediger, Meade, Gallo, and Olson (2009) used eight lists of words that are known to induce false memories (DRM lists; Deese, 1959; Roediger & McDermott, 1995). The words involve the 15 strongest associates (*bed, rest, awake, tired, dream*...) to another word (the critical item) that is not presented with the list (*sleep*, in this case). Subjects tend to recall and/or recognize the nonpresented critical word with roughly the same frequency as words actually presented. Although these materials might have been dismissed by Bartlett as artificial, they do permit careful measures of both accuracy and inaccuracy, which is difficult with natural discourse.

We tested subjects in groups of four, but staggered when they started reading the material. (They played video games while they waited). The basic design is shown in Table 2, where each row represents a type of repeated reproduction experiment: subjects study material and then recall it four times. The serial reproduction part of the experiment is represented in bold and cuts diagonally across the figure: the first subject studied a 60 word list (4 DRM sublists, in which words were blocked by the theme of the list, so all the sleep-related words were presented together then all words from another list, and so on). That subject recalled the material once, and then after short breaks recalled the material three more times without any further study. The second subject studied the first recall protocol of the first subject, then recalled that material four times. The third subject studied the efforts of the second subject's first recall attempt and then recalled it, and so on for the fourth subject (who studied the third subject's first recall). Although the main comparison of interest is in the top row of the table (repeated reproduction of the subject who studied the entire list) and the diagonal (serial reproduction across four subjects), we went ahead and filled out the other cells with repeated reproduction by each subject so we could examine how their repeated recalls might change when they begin at different levels of completeness. However, for present purposes, we consider only repeated reproduction of the original subject (the top row in the table) relative to serial reproduction across the four subjects (the diagonal in bold).

Veridical recall is shown in Figure 2, scored with two different criteria (Koriat & Goldsmith, 1996). Figure 2(a) shows input-bound scoring (in which the denominator is the 60 items subjects studied). As can be seen, the repeated reproduction subjects showed little forgetting across four tests, whereas the serial reproduction subjects showed dramatic forgetting

Table 2.   Design for comparing repeated reproduction (the rows) with serial reproduction (the diagonal in bold)

**Veridical Recall
Proportion of 60 List Items Recalled**

**Veridical Recall
Proportion of 60 List Items Recalled**
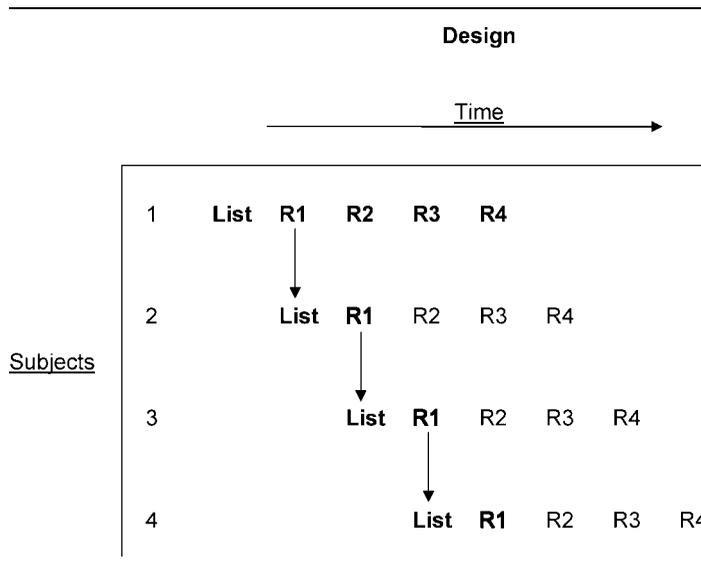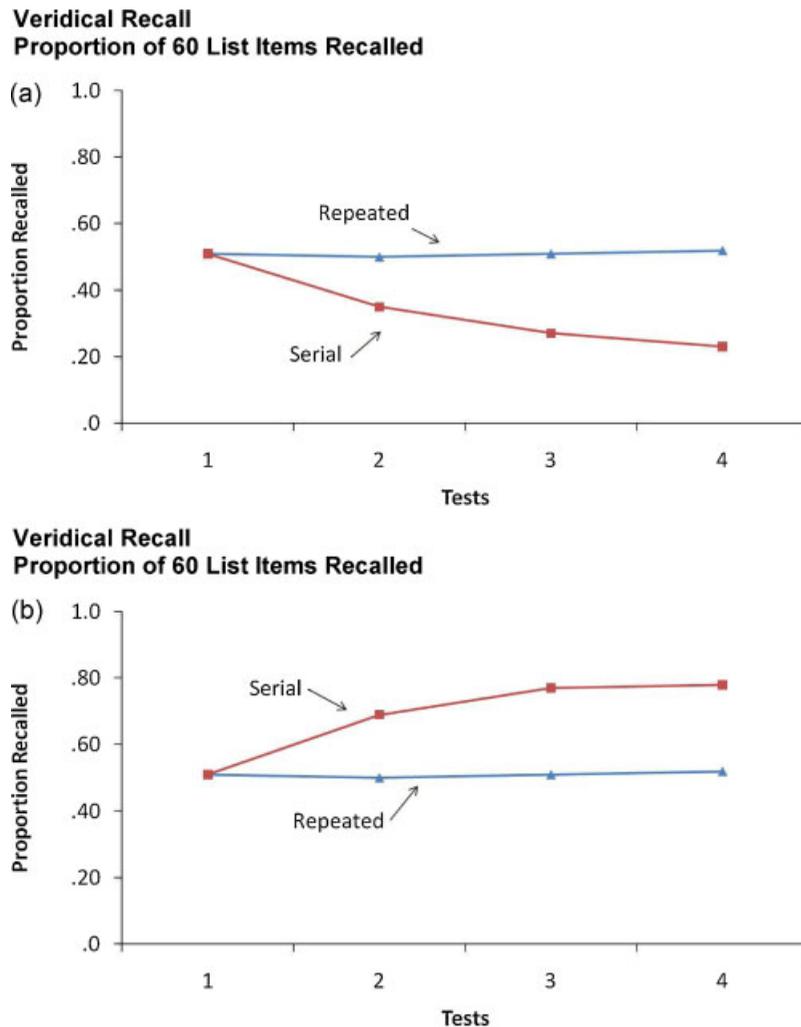
Figure 2. Veridical recall measured with input-bound scoring (Figure 2(a)) and output-bound scoring (Figure 2(b))

(from .50 to about .20 words), so that by the fourth subject they recalled only 40% of material accessible to the repeated reproduction subjects. The problem is not really that repeated reproduction subjects were ''poor rememberers,'' because when scored by output-bound measures they actually outpaced the repeated reproduction subjects, as is shown in Figure 2(b). That is, the material that serial reproduction subjects studied grew increasingly small with their position in the series—each subject studied to previous subjects' recall—so it was natural that they could remember a greater proportion of the smaller amounts of material. This represents the standard finding in the list learning literature about list length and retention (Murdock, 1962): the shorter the list, the higher a proportion of items that can be recalled from it (even if the number of items recalled is less with shorter than with longer lists).

The critical issue is what happens to false recall of the four critical items. Figures 3(a) and 3(b) supply the answer. Even when using input-bound scoring of all four items (Figure 3(a)), subjects engaged in serial reproduction showed greater false recall than did repeated reproduction subjects, and false recall in both procedures tended to grow with repeated recall attempts. Recall of the nonstudied critical items such as *sleep* increased over recall attempts. Figure 3(b) shows the even more dramatic effect with output bound scoring, with false recall in the serial recall subjects becoming increasingly great as more subjects are added to the series. Bartlett often tested 10–12 subjects in serial reproduction whereas we tested only 4. If we carried the procedure forward and extrapolated from the function in Figure 3(b), we might eventually wind up with

False Recall
Proportion of 4 Critical Items Recalled

False Recall
Proportion of Critical Items Recalled as a Function of Total Items Recalled
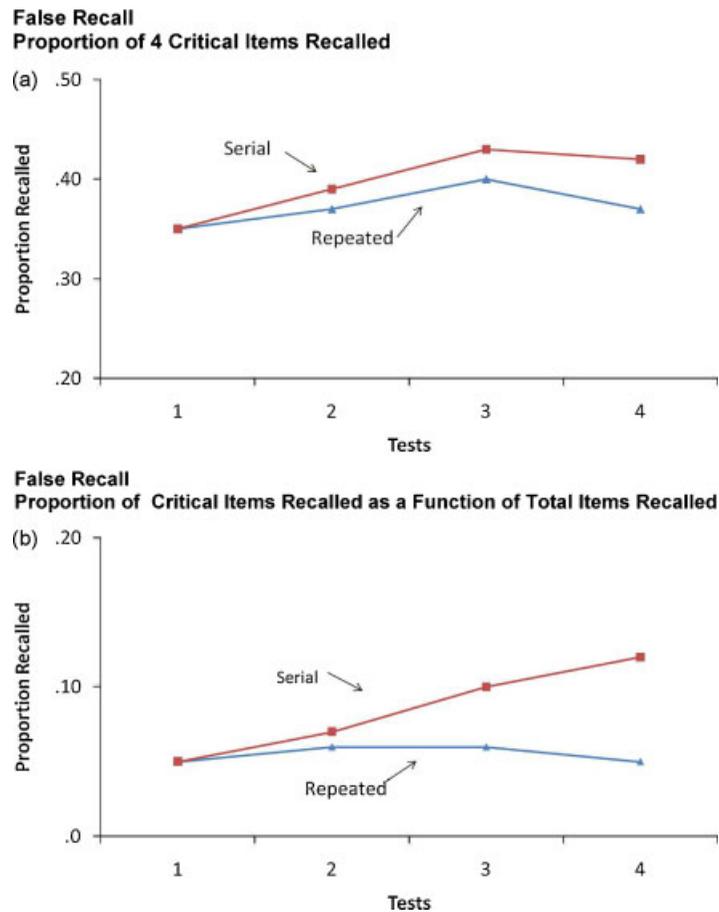
Figure 3.   False recall measured with input-bound scoring (Figure 3(a)) and output-bound scoring (Figure 3(b))

subjects recalling only the items that had not been presented. Of course, that is mere speculation at this point; it may be that the false recall function will level off at some point (as happens with repeated study; McDermott, 1996).

Our experiment confirms Bartlett's more casual analyses of serial and repeated reproduction. Serial reproduction gives rise to greater forgetting over recall attempts and introduces greater distortion. Note that our materials—word lists— might even be more ''inert'' than other types of materials, so if anything they could underestimate the potential for serial reproduction (relative to repeated reproduction) to reveal changes with repeated tests.

This section on extending Bartlett's legacy answers questions that have been left dangling ever since 1932, firmly ignored for many years. Yet the results are not just of historical interest, or so I would hope. The techniques used here show that the difficult but interesting analyses of memory phenomena that Bartlett undertook can be made experimentally tractable. The basic conclusion is that although both repeatedly recalling the same material oneself and social recall (from friend to friend) are error-prone processes, the latter tends to lead to greater errors than the former (just as Bartlett (1932) proposed).

SOCIAL CONTAGION OF MEMORY

Although the study of social influences on remembering has historically been neglected, contemporary researchers are rapidly changing this state of affairs. Social studies of memory are sprouting in many labs; symposia abound on these topics; and journal pages are filled with relevant studies. The area of social remembering is enjoying a boom, and it

represents an important collaborative effort among social and cognitive psychologists. I report here just one example of this sort of research, which my collaborators and I have called the social contagion of memory (Roediger, Meade & Bergman, 2001; Meade & Roediger, 2002), although (as discussed below) others are engaged in similar studies called by different names.

The paradigm we developed represents a melding of two classic paradigms, that of Asch (1956) in studying conformity and that of E. F. Loftus (e.g., Loftus et al., 1978) in studying eyewitness memory. Asch's studies involved a situation (ostensibly a perception experiment) in which a subject had to pick out which of several lines matched in length that of a target line. The critical manipulation was whether subjects responded alone (when they were nearly always correct) or whether other subjects (actually, experimental confederates) took turns before them and picked the wrong line. In this latter case, the subjects would then often pick the wrong line, too, to conform to the group. A long history of research has been devoted to understanding conformity using the Asch paradigm (see Bond & Smith, 1996, for a review and meta-analysis).

Loftus et al.'s (1978) eyewitness memory (or misinformation) paradigm was discussed briefly in previous pages. It involves a subject viewing a scene (usually an accident or a crime) and then either reading a description of it or being asked about it later. Some of the description or questions provided later are erroneous, and the issue of interest is whether subjects' memories are altered. Does the post-event misinformation reshape recollections of the actual scene? The answer, of course, is *yes*, and the effect is powerful: Using Tulving's remember/know technique to assess phenomenological experience, subjects frequently report that they vividly remember the misinformation as part of the original event even though it was not there (Roediger, Jacoby, & McDermott, 1996).

In the Loftus paradigm, there is an implied social presence. The misinformation is delivered through questions (created by the experimenter) or from a narrative that is often described as being the account of another subject. However, unlike the Asch (1956) paradigm, no person or people are actually present to deliver the misinformation.

We blended these two paradigms to study conformity processes in memory or what we called social contagion. Two subjects came to the lab, although one was actually a confederate of the experimenter. They were told that they were participating in a memory experiment and that they would study scenes and be tested together. They then viewed six typical scenes from an apartment, such as a kitchen, a closet, etc. Some scenes were viewed for 15 seconds and others for 60 seconds. After viewing all six scenes, subjects' memories for objects in each scene were tested in a collaborative fashion in which they took turns recalling items. They were cued with the name of the scene (e.g., kitchen scene) and asked to recall 12 items that had appeared in the scene, six by each subject, with the two subjects taking turns. For half the scenes, the confederate recalled all items correctly, but for the other half he made two errors. One item recalled was an item that would typically be found in the relevant part of the apartment but had not appeared in the scene (e.g., a toaster in the kitchen scene). The other item was one that could fit into the scene and not look out of place, but that would be less expected (e.g., oven mitts).

In the final phase of the experiment, subjects were separated and taken to individual cubicles. Now the true subject was given a recall test in which he or she was cued with the name of each scene and asked to recall as many items from the scene as possible, with a warning to recall only items he or she was reasonably sure were in the scene (i.e., to use a high criterion).

The basic results are shown in Table 3 and all the numbers represent proportions of errors made. The top row of the table shows recall of the "contagion" items, those the confederate had reported and that the subjects had incorporated into their own later recollections. The row below indicates the proportion of the same errors made when subjects had not been supplied with misinformation from the confederate (control). Note that these numbers from control subjects are not zero, which indicates that subjects have some tendency to recall the missing items even when they are not suggested, especially

Table 3.   Mean proportion of high- and low-expectancy items falsely recalled when each scene was presented for 15 and 60 seconds

Items	15 seconds		60 seconds		
	Low	High	Low	High	*M*
Contagion	.17	.41	.05	.24	0.22
Control	.03	.11	.00	.08	0.06
Difference	.14	.30	.05	.16	0.16

for the items that are expected to be in such a scene (like the toaster in the kitchen scene). This sort of error occurs because the items are schema relevant (Miller & Gazzaniga, 1998). The primary results are the difference scores at the bottom of the table, which reveal the social contagion effect or the tendency for subjects to recall the erroneous items produced by the confederate more than in the control condition. Thus, following the contagion metaphor, the confederate's recollections infected the subject's memory. Note also that the contagion effect was larger when subjects had little time to view the original scene (15 seconds) relative to the longer time (60 seconds). This outcome makes sense—the better encoded the scene, the less susceptible is the memory to outside infection by misinformation.

Later research using the social contagion paradigm confirmed that the effect held even when subjects were warned, just before the final test, that the other subject had made errors and that his responses should be viewed with caution (Meade & Roediger, 2002). Other studies in the same series showed that even when source monitoring tests were given to subjects explicitly to alert them to the possibility that an item on the test had been produced by the other person, the effect was still strong. Finally, as in the Asch (1956) paradigm, Meade and Roediger (2002) showed that the more confederates who made the error, the greater was the likelihood that the subject would recall it later. Finally, in a last experiment, they showed that using actual confederates produced somewhat larger social contagion effects than using virtual confederates (subjects said to be in other rooms, responding over linked computer systems but with only their responses appearing to the actual subject). If confirmed, this last finding indicates that there is something especially powerful about having real people deliver the misinformation.

Interestingly, others working at about the same time reported results similar to ours (e.g., Gabbert, Memon, & Allen, 2003; Wright, Self, & Justice, 2000) and such studies continue. Others have called the effect ''memory conformity'' rather than ''social contagion,'' and that name is perhaps more appropriate. After all, the contagion metaphor makes it sound as if the transmitted memories are a disease. This may be an accurate characterization in the case of implanting false memories. However, if we ask the functional question of why people so readily accept information from co-witnesses to a scene, it is probably because doing so usually has a positive effect and is adaptive. That is, if a person's own memory for an event is poor, for whatever reason, she may fill in the gaps by listening to another person's account. In fact, in our social contagion studies, when we examined recall of items that confederates correctly recalled on the other subjects' later recollections, we found that these items also were well remembered (a kind of vicarious testing effect).

COLLECTIVE MEMORY

Another interesting interdisciplinary development in memory studies is represented by the topic of collective memory. This umbrella term refers to a form of memory that transcends individuals and is shared by groups, so is quintessentially social psychology (Pennebaker & Banasik, 1997). We may think of each important group to which we belong—our family, our profession, our city, our country, and many others—having shared memories that constitute part of our identity. The term collective memory is used in many different ways across various fields (Wertsch, 2002, 2008a), but all uses have in common this core idea of shared memories that help provide self-definition. Both social and cognitive psychologists will be important contributors to the emerging disciplines of collective memory and, more broadly, to memory studies (Roediger & Wertsch, 2008).

Collective memory is a term introduced by the French sociologist Maurice Halbwachs (1887–1945) and has been adopted by scholars in a large number of disciplines in the humanities and social sciences. English versions of Halbwachs two major works are *On collective memory* (1980) and *The collective memory* (1992), which are compilations of his writings from the 1920s to 1940s. Halbwachs' writing is expansive and many writers have been inspired by his work, but they have used the material in many different ways. A core idea is the fact that every group has a collective memory and they are often willing to defend their version of the past as being the true version over rival claims. Consider the differences between Turks and Armenians over the expulsion of Armenians from Turkey in 1915. Armenians (and many others) refer to the events of that year as genocide, but Turks refuse to accept that term and discuss events as troubles or wars (which would point to some fault on the part of the Armenians). Another case of genocide, the holocaust from World War II, probably represents the largest single focus to date for studies of collective memory (e.g., Young, 1993).

One tension in studies of collective memory is with the field of history. Isn't collective memory just another name for the study of history, albeit the kind of history that each of us carries about in our heads? The answer is *yes* in the sense that

both are about the past, but *no* in the more important sense that collective memory serves different purposes than history. Historians seek an objective record of what happened in the past, what forces were at work, as much as such an objective account is possible (and that issue is much debated by historians). Collective memory, on the other hand, serves as part of an identity project, defining who we are. Because collective memories shape identity, conflicting evidence of facts with the way the group wants to remember the past may be minimized or rejected (as the Turks reject characterization of their forbears as having committed a genocide in 1915). To put the matter another way, "History's aspiration to present an objective account of the past often comes into direct conflict with collective memory's simplifying, subjective approach that serves an essential role in identity formation. In a nutshell, one could say that history is willing to change a narrative in order to be loyal to the facts, whereas collective remembering is willing to change information (even facts) in order to be loyal to a narrative" (Wertsch & Roediger, 2008, p. 324).

The huge majority of studies of collective memory have to date been qualitative in nature. The role social and cognitive psychologists could profitably play in this emerging field of study is in developing testable hypotheses that may be studied empirically through correlational and experimental studies. For example, in interviewing Russian high school students, Wertsch 2002; 2008b) discovered that their collective memories of the main events of World War II differed radically from events that Americans recall as critical. The difference begins even with the name of the war. Although known as World War II in much of the world, the same event is called The Great Patriotic War in Russia.

Wertsch asked a group of Russian high school students to describe five great events in the war. He discovered that the set of events that the Russian students described (e.g., the Battle of Stalingrad) generally do not overlap with events that Americans remember (the bombing of Pearl Harbor, D-Day). The only event in common on most lists was the dropping of atomic bombs on Hiroshima and Nagasaki, which finally ended the war. In a very real sense, Russians and Americans remember different wars.

Of course, the Russian and American narrative templates (Wertsch, 2008b) describing World War II are only two of many; surely the English remembrance of the war would differ from these, as would memories of the French, the Germans, the Danes, the Italians, the Poles, the Japanese, the Chinese, the Norwegians, and so on. The idea of collective memory (or memories) of a people remembering the "same" event are made salient in this way.

We (Zaromb, Butler, & Roediger, in preparation) are just making a start empirically on studies of American recollections of World War II, but others have already gone further. Berntsen and Thomsen (2005) examined memories of Danish people of the events of the day the Germans invaded Denmark in 1939 and the day they withdrew 6 years later. Remembrance of the events were quite accurate (even the weather occurring that day) and their recollections met criteria for flashbulb memories. On a different front, Pennebaker, Paez, and Rimè (1997) edited a book on *Collective memory of political events* (1997) that discussed many interesting issues arising from around the world. Still, work in this field is just beginning (see too essays in Boyer & Wertsch, 2009).

I mention the topic of collective memory here, because I think this would be fertile ground for collaborations among social psychologists in Europe, the US, and around the world in helping to understand the force that collective memory has in shaping today's world. Many of the great political debates currently facing the world have their roots in the deep past and how it is remembered differently by people of various nations and ethnicities. Think of the remembrances of Jews and Arabs in the Middle East, of Protestants and Catholics in Northern Ireland, or of Shiite and Sunni Muslims in Iraq, as just a few examples.

CONCLUSION

My essay began with reflections on the relationship of cognitive and social psychology. I began with my personal experiences from undergraduate and graduate school and described some research that can be seen as a melding of cognitive and social psychological approaches. Much of contemporary social psychology seems to me as aptly described as cognitive social psychology, but cognitive psychologists like me are also branching out into social psychology. I ended by looking to the future: the study of collective memory represents promising intellectual terrain for collaborations between cognitive and social psychologists (with others—historians, political scientists, sociologists). Together we can try to understand the complicated forces at work that shape personal and national identity and that can bring countries to war based partly on their differing senses of history.

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