

11 The conflict of memories and documents

Dilemmas and pragmatics of oral history

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Oral history interviews can breathe life into the research and writing of recent history. By filling in the nerves and connective tissue (motives, inspirations, fears, obsessions, etc.) that link actions with each other, participant accounts can animate a narrative with details not found in documents, shade it with nuances, and vitalize interpretations with insights into diverse points of view.

Interviews can also help the historian go about studying an institutional or theoretical development.¹ They may provide information that is unrecorded anywhere else or guide the historian through archival collections, adding annotations, and revealing documents, such as letters or notebooks, which had been squirreled away in an attic or cellar, or clippings which were interspersed in personal files. Interviewees sometimes contribute references that the historian could not possibly have found unaided, or help the historian decode agendas that lurk behind documents. It is not uncommon for the interview process to restore lost information by tickling the memories of "living sources," so they are able to recall events that they had effectively forgotten.

One of the most dramatic uses of oral history—about which I will have much to say here—occurs when memories and documents come into conflict. Such conflict remains at the core of the most devastating criticism that has been leveled against using interviews in historical research. Yet in the hands of a skilled oral historian, this conflict can become a powerful tool in a methodology that leads to deeper histories having subtler overtones than can be achieved without the use of interviews.

Before presenting the methodology, I need to define what is meant by oral history and explain the major objections that historians have raised to it.

Definitions and distinctions

The term oral history is commonly used in three different ways. It can refer to the interviews themselves, to the methodology for conducting them, or to histories that were written *using* the interviews. Most of the literature on oral history deals with the first two uses. This chapter will consider as well the histories that interviews enrich.² These histories divide into two basic types: those that are based almost entirely on interviews (e.g. Studs Terkel's *Hard Times: An Oral History of the Great Depression*) and

those (of more interest to me) that depend as much on other sources as they do on interviews.

An important distinction separates "passive" and "interactive" interviews. This distinction turns on whether the historian is a significant influence. Historians differ radically over what they consider the correct degree of influence. The well-known fact that an interviewer's leading questions can cause distortions of memory, or in the worst case implant false memories,³ suggests to many that passive interviews are more reliable than interactive ones and that historians should play as small a role as possible in conducting the interviews. Yet a completely passive interview is impossible, for even in the extreme case when an interviewee speaks directly into a tape recorder, without an interviewer present, the historian still plays a role in shaping the interview, if only in arranging it—as Ron Grele, one of the founding fathers of modern oral history, has noted.⁴ While many guides to oral history emphasize the case for doing passive interviews, I will concern myself mainly with the argument for conducting fully interactive interviews whose content is self-consciously tailored by the historian with the help of questions based on considerable research.⁵ The resulting dynamic dialogues between the historian and interviewee often yield deeper insights.

The case against oral history

Although historians have been using oral sources to help create and present their narratives since ancient times, the methodology of doing oral history is not fully established, even today.⁶ Perhaps it was the invention of the wire and then the tape recorder that caused historians to begin worrying about oral history. For in the very era when these technologies began to upgrade the quality and usefulness of interviews, many historians began raising sharp objections to them.

Historians who dismiss oral history usually focus on one or more of the following: oral histories are inherently biased; human memory is unreliable; oral history deals with present reflections on past events; and oral histories evolve and change, even while they are in the process of being created.⁷ These are all real problems, but none of them invalidate the use of interviews in the writing of history. Indeed the primary objection of bias applies just as well to documents, which may be as distorted as interviews by the agendas that are in place at the time of their creation.

All people rewrite their memories constantly, recalling some sections in detail, others more vaguely, deleting portions, blowing some parts up while shrinking others. While many researchers adhere to the time-honored picture of memories stored like pictures in a photo album, a growing research literature deals with the fact that memory is selective, distorted, and changing.⁸ Most researchers of memory in the fields of cognitive psychology and neuropsychology have come to believe that "we do not store judgment-free snapshots of our past experiences but rather hold on to the meaning, sense, and emotions these experiences provided us."⁹

The fact that interviews suffer from the full range of the distortions that human memory creates is not news to the experienced oral historian. Any scholar who conducts interviews soon learns what the anthropologist and historian of African

societies Jan Vansina once wrote, that "memory is not an inert storage system like a tape recorder or a computer. Remembering is an activity, a re-creation of what once was. It uses for this purpose not just this or that bit of information but everything available in the information pool that is needed in this circumstance, reshaped as needed for this particular re-creation."¹⁰

Human memory may reorder events temporally or move remembered events into other situations or time frames. In a dissertation about the discourse of oral history, Jean Robertson pointed out that "straightforward chronological progression from earliest to latest event is not the dominant temporal form employed within an oral history and is never maintained continuously over the entire account." A person typically reflects on the past associatively, allowing "one memory to trigger another by a process of association, [and] he often will join events that are historically separate into a single topic of discussion."¹¹

The oral historian also learns that the memories expressed in interviews draw on information from the present as well as the past in its constructive process and that memories are colored by the interests, images, and values of the period in which the memories are expressed.¹² In a perceptive essay about oral history, the historian Michael Frisch noted that Terkel's *Hard Times* is as much about people "trying to live in and understand the 1960s" as it is about life in the 1930s.¹³ Memories adapt and can shift their content to fit the prevailing values of communities.¹⁴

Interviews are affected by the interests, knowledge, and experiences of the historian who will shape her narrative to fit the interests of her audience. She has the power (and often draws on it) to pattern the historical fabric she weaves. Even the way the historian selects and arranges materials from the oral and written accounts is affected by her ideas, interests, and accumulating knowledge derived from many sources and studies.

In this essay I will explain why, for the case of oral history, I feel this is as it should be. The injected feedback of historians can bring interviewees to usefully rethink, refine, and, in some cases, alter their recollections when they conflict with documents.¹⁵ The process of historical research using interviews may be compared with studying the kind of system (ranging from the quantum-mechanical to the psychological) in which the process of observation changes what is being observed, thus injecting a degree of indeterminism. Interviews can be seen as dialectics that operate between historian and interviewee, between present and past, and between interviews and every other kind of source (Figure 11.1).

The usefulness of suspicion: the mask

"I have done that," says my memory. "I cannot have done that," says my pride, and remains inexorable. Eventually—memory yields.

(Friedrich Nietzsche)¹⁶

To use interviews to best advantage, the historian needs to be aware of how a person's interests, beliefs, values, aspirations, and full range of emotions (including pride, shame, fear, pity, and love) affect their memories, and how a person's memories are



Figure 11.1 Historian Lillian Hoddeson with Hans Bethe at Los Alamos in 1979, preparing for an interview about its Theoretical Division during the Second World War.

Source: Los Alamos photo.

shaped and reshaped to support his present self-image. In studying the transmission of traditions in African culture, Vansina developed a useful way to model this process. He identified the public account, the one people reveal readily, as a cover story which he calls the "mask." This mask is "built up in terms of roles and statuses, values and principles—the noble mask of oneself."¹⁷ Behind this mask is the hidden portrait, or "face," the authentic account. The face is "much less often limned and reveals traces of doubt and fear as quite contradictory experiences are remembered." The distinction between mask and face, Vansina explains, varies from culture to culture, from one discipline to the next, and from one profession to another. Both mask and face are important objects of study for the oral historian; exploring their relationship helps her to understand how interviewees see themselves in relation to their culture. But to construct a deeper history, she must go farther and dislodge the mask, to discover the face.

The masks of executives, politicians, and other public figures tend to be more firmly attached than those of scientists or craftsmen; for the glue that attaches mask to face hardens through repeated tellings of the stories.¹⁸ My least successful interviews have been with individuals who deal regularly with the public. I have also found that mask-related problems intensify when interviews are conducted in places that reinforce the masks. Thus my 1970 interview with James Fisk, a former Bell Labs president, suffered from my mistake of interviewing him in his executive office. The result was indistinguishable in tone and content from Fisk's articles in the *Bell Labs Record*. I might have had better luck had I interviewed him in a taxi or a pub—an environment that could weaken, rather than support, the hold of his mask.

The historian's principal tool for examining the mask is her set of questions. The questions must be incisive and compassionate at the same time. Good interviews have a tension that derives from the uncomfortable probings of both the historian and interviewee, who are each trying "to understand the historical position from which the other speaks," as Grele described it.¹⁹ The historian keeps the interview tense with efforts to explore and elucidate the story. "When did that happen?" "Were you aware of his work on that subject?" "Why have you left this part of the story out?" "Who else was there?" "What was her response?" "Why does your account differ from X's in this way?" Such questioning can be disturbing, even embarrassing. But the historian cannot allow the tension to amplify so much that it threatens the continuation of the interview.

In my interviews I routinely use documents as additional tools. I typically bring along quite a collection. The trick is to bring out relevant materials at just the right time. Consider my interviews in May 1978 with Robert R. Wilson, the founding director of the high-energy physics laboratory in Illinois known as Fermilab (Fermi National Accelerator Laboratory).²⁰ As the facility's creator and chief executive, Wilson gave many public lectures and press conferences. In the process, he developed his standard account of why he built the laboratory. This was a mask I needed to probe. He was happy to present it to me during our first session, which was fairly relaxed and revealed little beyond the written literature, but it allowed us to make contact. Now I could prepare my strategy.

I came to the next session equipped with a pile of carefully selected documents about Fermilab's origins. As Wilson told his story, I used the materials to gently correct him, adding dates, names, and other details. The interview went smoothly until I produced a particular document that conflicted with one of his favorite stories—as it happened, just as he was about to tell it. He became visibly upset and we were both embarrassed. But not for long, for the session was interrupted by an urgent phone call. He ran out and I was left wondering whether I ought to pack up.

The call upset him further, although I did not learn why until later. It was the notification that Wilson's resignation as Director of Fermilab, provisionally accepted in February 1978 by the board of Fermilab's managing organization (the Universities Research Association), was now official. Wilson had not expected this turn; his resignation had been but a gesture in his attempts to increase funding for the laboratory.

When Wilson returned to the interview, he continued his story, but there was a noticeable difference. The conflict that I had engineered, between memory and documents, had loosened his mask. The phone call's further blow to his public persona had pried his mask wide open. He offered a revised account that not only fit the documents but was also much richer in detail. The two jolts—one planned, the other coincidental—had helped him to expand his memory. I also believe that our interview, which was based on a developing friendship and genuine interest in his Fermilab career, helped Wilson adjust to his own cold reality. For the historical era we were probing in the interview had suddenly ended.

The encounter had an additional important consequence for my work. Wilson enlisted as a helpmate in my historical study of Fermilab. From that point on, he and I could act as a team in reconstructing Fermilab's history. When an historian can

achieve this kind of collaboration with an interviewee, the possibility opens of reaching new levels of historical re-creation that are not possible using memory or documents alone.

Horace Judson tells a related story about the adaptation of memory to fit historical inquiries and the subsequent opening of research opportunities. The story comes from Judson's experience interviewing Francis Crick, with papers and correspondence in place before them. About ten months after a particular interview, Judson gave Crick a manuscript to read based in part on the interview. Crick noticed an inconsistency. "Did I really tell you that? Well it wasn't quite like that at all." At this point, Crick offered a refreshed memory that fit the full body of evidence.²¹

Let me now turn to some examples from my research on the wartime and postwar history of Los Alamos National Laboratory. Just before an interview on building the hydrogen bomb (H-bomb), Edward Teller confided in me: "Lillian, I have a terrible problem." He was working on his memoirs.²² A year or so earlier, I had helped him to gain access to his Los Alamos papers from the 1940s and 1950s. "What's your problem, Edward?" "The problem is simple," he replied.

Then, throwing up his arms, he exclaimed with considerable emotion: "The documents don't agree with my memory!" He was referring to the story of the development of the Ulam-Teller design of the fusion bomb system. Teller's account was significantly shifted—in his favor—from the one told by Hans Bethe, an account that *did* fit the documents.²³ Having thus warned me, Teller then proceeded to offer his memories, which did not agree with the documents.

I am fully convinced, however, that Teller was not trying to invalidate or supercede the documents. His memories were what he *could* offer. By sharing his distress about their disparity with the documents, he had made *his* problem also become *my* problem. And by pointing to this problem openly and honestly at the time of our interview, Teller spared me the embarrassment of calling the disparity to his attention. His perception of the disparity as a "terrible" problem then opened the door to several highly productive interviews that we had subsequently about the history of the H-bomb—interviews in which Teller's mask was indeed partially dislodged.

How the conflict between documents and memory more typically operates is illustrated by an interview that Gordon Baym and I conducted in Pasadena in 1979 with the theoretical physicist Richard Feynman, then 61. I brought to that interview a large, recently declassified, stack of wartime theoretical documents that had been discovered at Los Alamos in an old safe. I thought that the documents, many in Feynman's own hand, might help him recall his wartime work. Unfortunately, Baym and I had scheduled this interview too soon after the first of Feynman's major surgeries; the operation clouded his memory. Blankly turning page after page, Feynman grew increasingly frustrated by his inability to recall anything about them. He blurted out angrily: "You know what you oughta do with these documents? Throw 'em in the garbage!" The interview remained uncomfortable—indeed anguished—for at least half an hour.

Then Feynman discovered in the stack several interesting blueprints that caused his memory barrier to lift. These blueprints had figured large in one of his favorite stories, told on many occasions and eventually published several times.²⁴ In his anecdote, "Little Richard" was on a wartime mission at the Oak Ridge laboratory. At one

point, several Oak Ridge engineers were describing the safety precautions built into one of their chemical plants. They had gathered around a "loooooong table" covered with the enormous blueprints. As they spoke, Feynman realized that he didn't know the meaning of a certain symbol, a cross inside a circle.

As far as he could make out, the symbol referred either to a valve or to a window. But at that point in the discussion, to simply ask what the symbol meant would have been embarrassing. Feynman had waited too long. So he ventured a guess. Bracing himself, he put his finger on one of the symbols and asked, "What happens if this valve gets struck?" He fully expected the Oak Ridge engineers to say, "That's not a valve, sir, that's a window!" Instead, according to his version of the tale, the engineers began to move their hands quickly up and down over the blueprints, which had been spread out over several tables. They continued for a time, only to end up staring at each other in shock. Feynman had pointed to the crucial weakness that could have caused the plant to blow up. The engineers concluded (as in many of Feynman's stories) that he was a genius.

Feynman was disappointed to observe that the real blueprints were much smaller than the supersize ones featured in his story. They had grown through many tellings. But he was delighted all the same to see the original documents after many years. And he was fascinated to read some of the other documents relating to this Oak Ridge visit, documents that had been filed along with the blueprints. They offered, among other things, the names of the engineers involved, and these names further tickled his memory.

The crisis was over. Now Feynman was eager to again tell his story of how he saved the Oak Ridge plant. And then to tell other stories. The blueprint and its related documents had jolted Feynman's memory back into action. It is of utmost importance for the oral historian to become skilled at using such jolts.

Jolts can be incorporated in the oral historian's manner of questioning. In 1987, the pioneering oral historian Charles Morrissey published a classic paper, which has become part of the standard literature on doing oral history. In it he described a method of questioning that employs the jolt. He called the method the "two-sentence format."²⁵ In the first of the two sentences (which can be a group of sentences), the historian lays out the agreed-upon knowledge, including some features of the mask. In the second, he probes the mask using a question based on the response of the interviewee to the first sentence. In further rounds of questioning, the historian probes deeper and deeper, interlacing further questions resulting from answers to earlier questions.

I can illustrate Morrissey's technique by appealing again to my Los Alamos interviews with Teller. In one interview I was trying to learn Teller's side of why Bethe, who was serving as head of the Theoretical Division, moved Teller out of the Division in June 1944. Bethe's account, substantiated by his colleagues Robert Bacher and Carson Mark, held that Teller had been moved out because he stopped working on his implosion bomb assignment and was spending his time instead studying the H-bomb.

Drawing on Morrissey's system, I began by outlining a few facts, saving the main question for later. I showed Teller an organization chart that revealed the new H-bomb group established in June 1944 with Teller in charge. Discussing this well-documented, and noncontroversial, event brought Teller back into the period and

gave him the chance to tell his story—that is, to articulate his mask. I encouraged him to develop the picture in detail. “Does this indicate that there was an increased effort at Los Alamos to study the hydrogen bomb?” My question brought Teller to object and present his well-known view that Oppenheimer had placed far too little emphasis on the H-bomb during the Second World War.

My next step was to gently probe the mask: “Bethe told me that he moved Rudolf Peierls into your position as leader of the implosion theory group because you were working too much on the H-bomb.” That gave Teller a space in which to counter Bethe’s story with a more detailed version of his side. He explained that he was just not good at doing the intricate hydrodynamics calculations which were then required in the theoretical work on implosion. This point, suppressed in the standard account, made everything fit.

But was it merely a retrospective reinterpretation? I could answer this question only through more research. I would need to go back into the Los Alamos documents and see whether they shed any light on Teller’s ability to do such calculations. I would probably need to speak with others familiar with the wartime implosion calculations and with the nature of Teller’s particular talents. I would need to reinterview Bethe and anyone else I could find who had served in the Theoretical Division at Los Alamos during the Second World War. (Peierls and Mark were by then no longer available.) After that, I could return to Teller with more informed questions and more evidence with which to test his recollections. Doing repeated interviews interspersed with periods of document research typically generates accounts that become increasingly responsive to the existing evidence, especially as the historian or the interviewee add relevant points that had been left out of the discussion or resolve conflicts in the account. Interviews can become even more reflexive if budgets allow the transcription of interviews and editing of the transcripts by both the interviewee and interviewer.²⁶

Whether I continue to probe this particular issue of why Teller left the implosion program will depend largely on whether the answer is important in my future research. A historian has to judge whether pursuing a question is worth the effort it takes to address it—and when to stop any particular inquiry.

The need to suspend suspicion: the dilemma of trust

In tension with the need for suspicion and confrontation is the need to establish a relationship of trust and collaboration between the historian and the interviewee. The oral historian must always keep in mind that the accounts she is offered depend on the willingness of her living sources to share their stories. We are not likely to dislodge the mask of an interviewee who mistrusts our sympathy with her role in the history.²⁷ Thus Teller would not have offered me his problem had I not previously established a trusting relationship with him. Nor would Crick have agreed to Judson’s extensive interviewing had there not been trust between them. The point, as Vansina once put it, is that interviews are “social processes of mutual accommodation during which transfers of information occur.” If there is no social relationship (as with the administration of questionnaires), the information transfer will be minimal.²⁸

The dilemma the oral historian faces is to simultaneously maintain both critical detachment and a relation of trust. Solving this dilemma calls for compromise in which the subtlest part is negotiating between the opposing positions of confrontation and trust.²⁹ The oral historian is caught in a bind. To maneuver she must surrender part of her or his objectivity. She must bend her professional commitments just enough to establish a genuine, trusting relationship with the interviewee. In the interest of gathering the best information, she may have to (at least for a time) become a participant in the very system she is studying.

A similar bind arises for the historian who studies institutions or societies. In the 1970s, when I needed access to source materials about the invention of the transistor, it helped to have earlier held a student research position at Bell Laboratories. Without that “in,” I do not believe that I would have been granted access to the relevant primary source materials. Analogous insider connections have also helped in my studies at Fermilab and Los Alamos. These links had costs, for I now bear the dueling responsibilities of respecting the interests of the laboratories while I struggle also to uphold the standards of my profession. Similarly, a historian who acquires a security clearance to gain access to classified materials is obliged (in fact legally required) to respect classification restrictions. She may be required to withhold from publication some of the information she uncovers.

This historian’s dilemma of being both on the inside and outside of a system she or he is studying is similar to the situation faced by the social historian or anthropologist who enters a community she wants to study. Joining the community may be necessary for gaining access to materials, but it always entails a loss of objectivity, no matter how much anthropological distance (or “strangeness”) one tries to preserve.³⁰

If the historian succeeds in handling this insider-outsider dilemma, further difficulties are in store. A trusting interviewee may reveal historically valuable information that is potentially damaging to the interviewee or to someone else, perhaps another interviewee with whom the historian is in a relationship of trust.³¹ Because oral historians work within an unwritten code that requires them to protect the words of interviewees who have confided in them, the historian cannot disclose this information. Furthermore, because an interview is jointly owned by interviewer and interviewee, neither are free to disclose material in an interview that both have not explicitly released. One way for the historian to get around an ethical problem that arises is to show the interviewee how he plans to use the sensitive information, explaining the importance and asking the interviewee for his opinion. However the interviewee responds, the historian is obliged to respect his wishes.

The nuts and bolts of oral history: using iteration to achieve convergence of a story

I have argued that doing oral history requires the historian to be at once confrontational and collaborative, objective and personal, and suspicious and trusting. She must also maintain her commitments to her professional community and to all her interviewees. How can this be managed?

Pragmatically. We must operate in whatever mode works at the time. Over the course of many related interviews, a balance between suspicion and trust can usually be reached and particular distortions will tend to average out. We move in stages toward convergence, to a stable and robust interpretation that is the end of our historical research.³² While I can well imagine situations in which convergence is not possible, I have never in my own work encountered one.

I can best illustrate this pragmatic procedure by calling again on my experience of writing a technical history of the first atomic bombs.³³ When I began my Los Alamos study in 1978, I faced thousands of memoranda, technical reports, data books, program reports, and the like. It was impossible to read them all in the span of a decade or so and also write this history up.

Over the next several years I created a small research team with which to divide the work, a team that included two history graduate students, the Los Alamos archivist, and a group of other historians, another archivist, and a number of scientists who moved in and out as consultants.³⁴ We began by familiarizing ourselves with the received view, conducting about fifty initial interviews on various aspects. To prepare for the initial round, the team gathered a subset of documents on which to base the first stages of the work. But we did not initially know the history well enough to make the best selections.

The key to progress was interviews—many interviews. The first documents we gathered, most of which were not used or even referred to in our final book, helped us prepare questions for the first round of interviews. Many of them also served us during the interviews, by helping the interviewees step back into their past and by delivering the message that their stories would be carefully checked. Almost all the interviewees were curious to examine papers they had created years earlier and not been allowed to see since.³⁵ Most of the interviewees were careful to qualify which points in their recollections they remembered only vaguely.

Inviting our interviewees back to Los Alamos for their interviews proved simpler and less expensive than having large numbers of documents declassified. We found that it also helped them to trigger their memories. For Proust it was the madeleine that did it.³⁶ For the Los Alamos scientists, it was the mesas, the canyons, and even the security guards that revived memories our team could now capture with a tape recorder.³⁷

The group we interviewed at Los Alamos clearly welcomed the chance to unpack the complex emotions that were still entwined with their memories of the intense wartime period. Typically in their seventies, these interviewees would speak with us or study documents in the archives for hours at a time. One interviewee—Bernard Waldman, who had measured the blast at the Trinity test and had flown on the observer plane into Hiroshima—made it a point to come to Los Alamos and be interviewed by our team, as he had only two weeks left to live. Dying of cancer, he told us that if he did not make this effort now, no one would ever know his unique story.

The rough account that emerged from the first round of interviews served as a guide to selecting further documents. Subsequent research exposed gaps and inconsistencies. Now we could dig deeper and refine the emerging picture through further interviews, further archival work, even more interviews, more studies in the archives, and so forth.

In many cases, the interviews offered clues that helped us interpret documents. For example, in one committee report I noticed a reference to the eminent French experimental physicist Frédéric Joliot. Initially I thought the reference indicated that the Los Alamos concern about spontaneous fission (a nuclear splitting that is not triggered by neutrons but occurs of itself) stemmed from findings by the Paris group. But when Emilio Segrè told me in an interview that no one at Los Alamos trusted Joliot's work, I deduced that Joliot was mentioned here simply to justify the experiment being proposed by the Los Alamos physicists.

As the team accumulated more interviews and studied more documents, the distortions of particular interviews came to matter less. With a stronger base of information we could cope better with the conflicting interpretations and better interpret the biased documents. And we could more effectively draw useful historical conclusions from our slanted sources. Over a period of many months (the whole project took fifteen years from start to publication), the dialogue between documents and interviews yielded a series of successively improved models of the history. They eventually converged on a story that remained stable during the past three years of our work, the story we published in 1993. The process we had undertaken is analogous to computer modeling—indeed to the methodology of any empirical project, including traditional historical research.

I have drawn on my experience of writing the Los Alamos history to illustrate how the historian can work to resolve the conflicts between memory and documents, between trust and suspicion, and how, with the use of many interviews and much research in documents, a point can eventually be reached where the composite account stabilizes. At this point, further interviews and further archival research produce only negligible change in the historical reconstruction. We have found the face behind the mask, the story on which all the partial accounts converge. At some future time this face may destabilize in response to new materials or a more compelling interpretation, but for the time being it is the best we can do.

Acknowledgment

This chapter is based on two talks: the first was delivered in 1992 at "The Relation of Oral and Archival Sources to Writing History and Biography" workshop, a session of the History of Science Society, organized by Ronald E. Doel; the second was presented on April 28, 1994 at a Stanford University workshop on "Interviews in Writing the History of Recent Science," organized by Horace Freeland Judson to whom I'm grateful for much useful editorial help in shaping this piece.

Notes

- 1 Allan Nevins told Saul Benison, when the historian of medicine was embarking on his first oral history project, "The people who you interview are going to become your teachers . . ." Saul Benison, "It's Not the Song, It's the Singing: Panel Discussion on Oral History," in *Envelopes of Sound: The Art of Oral History*, 2nd edn, ed. Ronald J. Grele (New York: Praeger 1991), 54.
- 2 Many hard questions about oral histories as historical sources are raised critically by Soraya de Chadarevian, "Using Interviews to Write the History of Science," in *The Historiography*

- of *Contemporary Science and Technology*, ed. Thomas Söderqvist (Amsterdam: Harwood Academic Publishers, 1997), 51–70. For more on the debate over whether interviews should count as “history,” see the exchange between Dennis Tedlock and Alice Kessler Harris in “It’s Not the Song,” 50–105. According to Harris, interviews are raw data, not “history.” To qualify as history, she feels, a piece of writing needs to address the question, “What does all this mean?” See p. 90.
- 3 For a psychologist’s account of the implantation of false memories, see, for example, Elizabeth Loftus and Katherine Ketcham, “Lost in a Shopping Mall,” in *The Myth of Repressed Memory* (New York: St. Martin’s Griffin, 1994), 73–101. For an historian’s acknowledgment of passive interviews, see, for example, Dennis Tedlock in “It’s Not the Song,” 91–92.
 - 4 Remarks by Grele at “Interviews in Writing the History of Recent Science” workshop, Horace Freeland Judson, organizer, Stanford University, April 28, 1994 (hereafter cited as Stanford workshop).
 - 5 This is, in fact, the kind of interview one most often encounters in research-oriented oral history collections, such as the one maintained by the American Institute of Physics Center for History of Physics, College Park, Maryland.
 - 6 Spoken historical testimony dates back at least to Herodotus, Thucydides, and the writers of the Gospels. Recorded oral history became possible after the advent of the wire recorder. On that basis, Allan Nevins established the first modern oral history program at the Columbia University Oral History Research Office in 1948. Large numbers of historians began to employ interviews in the 1960s, when tape recorders became widely available.
 - 7 Today the objections to oral history, while often voiced, are rarely committed to print. One article that includes a number of the objections is J. L. Heilbron, “An Historian’s Interest in Particle Physics,” in *Pions to Quarks*, ed. Laurie M. Brown, Max Dresden, and Lillian Hoddeson (Cambridge: Cambridge University Press, 1989), 47–56.
 - 8 The literature on memory distortion has grown to enormous proportions in the last decades. A sampling of sources about memory distortions in the field of cognitive psychology can be found in Daniel L. Schachter, *Searching for Memory: The Brain, the Mind, and the Past* (New York: Basic Books, 1996). See, esp., the references for chapter 3, “Of Time and Autobiography”; chapter 4, “Reflections in a Curved Mirror: Memory Distortion”; and chapter 5, “Vanishing Traces: Amnesia and the Brain,” 72–161.
 - 9 Schachter, *Searching for Memory*, 5.
 - 10 Jan Vansina, *Oral Tradition as History* (Madison: University of Wisconsin Press, 1985), 176, 147–148.
 - 11 Jean Ellis Robertson, *Language in Oral Histories: The Shape of Discourse About the Past*, (Ann Arbor, MI: University Microfilms International, 1986), 14–16. See also Joseph C. Miller, “Listening for the African Past,” in *The African Past Speaks: Essays on Oral Tradition and History* (Folkestone, England: Archon, 1980), 13–15; Elizabeth Tonkin, *Narrating Our Pasts: The Social Construction of Oral History* (Cambridge: Cambridge University Press, 1992), 103–121; and John P. Dean and William Foote Whyte, “How Do You Know if the Informant Is Telling the Truth?” in *Elite and Specialized Interviewing*, by Lewis Anthony Dexter (Evanston, IL: Northwestern University Press, 1970), 122.
 - 12 See, for example, Ronald J. Grele, “Can Anyone Over Thirty Be Trusted? A Friendly Critique of Oral History,” in *Envelopes of Sound*, 1991, 206–207.
 - 13 Michael Frisch, “Oral History and *Hard Times*: A Review Essay,” in *A Shared Authority: Essays on the Craft and Meaning of Oral and Public History* (Albany, NY: State University of New York Press, 1990), 7.
 - 14 Vansina, *Oral Tradition*, 94.
 - 15 At the 1994 Stanford workshop, William Provine was among those who argued strongly for extensive preparation before interviews; Judson countered that an interviewer’s ignorance can sometimes help coax out a more complete (rather than telegraphic) description from the interviewee. For other points of view on this issue, see Susan Allen McGuire, “Expanding Information Sets,” *Oral History Review* 15 (Spring 1987): 61; Mary Stuart, “And How Was it for You Mary? Self, Identity and Meaning for Oral Historians,” *Oral History* (Autumn 1983): 62; Frisch, *Shared Authority*, esp. 188–189; and Eva M. McMahan, *Elite Oral History Discourse: A Study of Cooperation and Coherence* (Tuscaloosa, AL and London: University of Alabama Press, 1989), 5.
 - 16 Friedrich Nietzsche, *Beyond Good and Evil: Prelude to a Philosophy of the Future*, trans. W. Kaufmann (New York: Vintage, 1966), 80, par. 68.
 - 17 Vansina, *Oral Tradition*, 8. This notion of the mask is referred to by many other scholars. We find it, for example, in Lorraine Daston’s notion of “scientific personae” and in Leon Edel’s compelling distinction between the public and private myth in biography, summarized by Nathaniel C. Comfort in *The Tangled Field: Barbara McClintock’s Search for the Patterns of Genetic Control* (Cambridge: Harvard University Press, 2003), 2–4.
 - 18 The psychoanalyst George Moraitis pointed out at the 1994 Stanford workshop that a person’s mask can shift. For example, when a subject that had earlier been kept hidden is used later to help a person avoid a different subject, the mask becomes part of the interviewee’s defense system.
 - 19 Grele’s remarks at the 1994 Stanford workshop.
 - 20 These interviews were among the earliest in a series that led to Lillian Hoddeson, Adrienne Kolb, and Catherine Westfall, *The Ring of the Frontier: The Rise of Magascience at Fermilab 1967–1989* (Chicago: University of Chicago Press, forthcoming).
 - 21 Horace Judson, private communication to Lillian Hoddeson, December 18, 1999.
 - 22 These memoirs are now available as Edward Teller and Judith Shoolery, *Memoirs: A Twentieth-Century Journey in Science and Politics* (Cambridge, MA: Perseus, 2001).
 - 23 These different accounts appear in interviews that Hoddeson conducted with Bethe and Teller at Los Alamos during July 1995. They are held by the Los Alamos National Laboratory Archives, Los Alamos, New Mexico.
 - 24 See, for example, Richard Feynman, “Surely You’re Joking, Mr. Feynman”: *Adventures of a Curious Character* (New York: W. W. Norton, 1985): 120–125.
 - 25 Charles Morrissey, “The Two-Sentence Format as an Interviewing Technique in Oral History Fieldwork,” *Oral History Review* 15 (Spring 1987), 43–54, esp. 51; also Morrissey, “On Oral History Interviewing,” in Dexter, *Elite*, 112.
 - 26 At the 1994 Stanford workshop, Ron Grele made the distinction between the research interview and the archival interview; the two are aimed at different audiences. Obviously, editing has little value when the goal is to maintaining the “purity” of an interview. Most archival interviews were created, however, for research purposes, and for these, editing to clarify the content and sense intended by the interviewee can be very valuable.
 - 27 Some eminent historians of science (e.g. Thomas S. Kuhn) have been spectacular failures as oral historians because they could not put aside their own programs and focus on what their interviewees were in the best position to tell them. See, for example, Kuhn’s interview with Niels Bohr in the Archive for History of Quantum Physics, held at AIP Niels Bohr Library, College Park, Maryland, and elsewhere.
 - 28 Vansina, *Oral Tradition*, 63.
 - 29 Morrissey, “Two-Sentence,” 47. Also Ronald J. Grele, “Private Memories and Public Presentations: The Art of Oral History,” in *Envelopes of Sound*, 260–261.
 - 30 The concept of strangeness in anthropological studies of scientific practice is discussed, for example, Bruno Latour and Steve Woolgar, *Laboratory Life: The Construction of Scientific Facts* (Princeton, NJ: Princeton University Press, 1986), chapter 2; and Sharon Traweek, *Beamtimes and Lifetimes: The World of High Energy Physics* (Cambridge: Harvard University Press, 1988), Prologue.
 - 31 Vansina tells a story in which a young oral historian learns about a murder in one branch of a family committed by a member of the other branch. Jan Vansina in section II, “It’s Not the Song,” 68.
 - 32 This concept of robustness has been discussed by a number of philosophers, for example, William Wimsatt, “Robustness, Reliability, and Overdeterminism,” *Scientific Inquiry and the Social Sciences* (San Francisco, CA: Josey-Bass Publishers, 1981).

- 33 Lillian Hoddeson, Paul, W. Henriksen, Roger A. Meade and Catherine L. Westfall, *Critical Assembly: A Technical History of Los Alamos during the Oppenheimer Years, 1943–1945* (New York: Cambridge University Press, 1993).
- 34 The history graduate students were Catherine Westfall and Paul Henriksen; the Los Alamos archivist was Roger A. Meade; the consulting historians were Robert Seidel and Richard Hewlett; the consultant archivist was Alison Kerr; and the scientists were Gordon Baym, Robert Penneman, and Leslie Redman. Behind the scenes were strong administrative allies at the laboratory, including Gilbert Ortiz, L. M. Simmons, and David Sharp.
- 35 Donald Kerr, the director of the Los Alamos laboratory (and later Kerr's successor, Sigfried Hecker) signed a paper allowing former Los Alamos scientists under proper conditions to see their own classified wartime documents while they were being interviewed in secure quarters at Los Alamos.
- 36 Marcel Proust, *Swann's Way* (New York: Random House, 1934), 33–36.
- 37 The association of place with memory has been extensively employed over the ages as a mnemonic device. The "method of loci" is said to have been used in 477 BC by the Greek orator Simonides. See F. A. Yates, *The Art of Memory* (Chicago, IL: University of Chicago Press, 1966), 1–26.

12 Reading photographs

Photographs as evidence in writing the history of modern science

Ronald E. Doel and Pamela M. Henson

In the late 1940s, G. C. Heron sat at his desk, frustrated. The photograph archive he directed at New Zealand's National Library, the Alexander Turnbull Library, was actively soliciting photograph collections. At the same time individuals throughout the country, seeing little value in them, were tossing images away. Heron found this deeply troubling. Photographs "preserve scenes, impressions and faces of bygone days and form an historic record as surely as does any manuscript, diary or printed work," he wrote, but few historians were doing anything with them.¹

During the last half-century photographic archives have burgeoned: over 8 million images fill the still photographs stacks of the US National Archives alone.² Yet historians remain hesitant about embracing photographs as evidence in the way Heron desired. Many only seek photographs at the end of research projects. Then they use them like potted plants, hoping to illuminate stories based on written archives. Keith McElroy has rightly observed that historians use photographs "primarily as illustrations, and frequently their content has contradicted the thesis of a publication that was derived from literary sources." Yet few historians pause to consider what images might teach them.³

Photographs are nevertheless becoming more important to scholars of modern history. The total number of books that directly address photographs as an aspect of historical methodology remains astonishingly small—just twenty-five have been published, all since 1973. But a growing number of historians are drawing on intense interest in contemporary visual culture to "read" photographs as evidence, as historians Alan Trachtenberg and Robert M. Levine have urged.⁴ In recent years social and cultural historians have employed photographic collections to analyze the living standards and cultural expectations of lower-class citizens of late nineteenth century Brazil, urban life in turn-of-twentieth-century America, the practice and ideology of the British Empire, rural life in the New Deal era, the cultural world-view of editors at the National Geographic Society, the ambitions of planners and financiers who promoted the first world's fairs, and the practice of medicine in post-Civil War America.⁵

Historians of science have moved more slowly than colleagues in other fields to incorporate analysis of photographs of the social, technical, and institutional practices of science into their writings. Sustained by the rich textual and archival sources of a privileged and highly verbal elite, few historians of modern science have felt the