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When Your Sources Talk Back: Toward a Multimodal Approach to Scientific Biography

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Abstract. Interviewing offers the biographer unique opportunities for gathering data. I offer three examples. The emphatic bacterial geneticist Norton Zinder confronted me with an interpretation of Barbara McClintock's science that was as surprising as it proved to be robust. The relaxed setting of the human geneticist Walter Nance's rural summer home contributed to an unusually improvisational oral history that produced insights into his experimental and thinking style. And "embedding" myself with the biochemical geneticist Charles Scriver in his home, workplace, and city enabled me to experience the social networks that drive the practical events of his career, which in turn helped me explain the theoretical basis of his science. Face-to-face interaction and multisensory experience will shape each biographer's experience uniquely. Recent developments in sensory physiology suggest that the experience of integrating sense data encourages different patterns of observation and reflection. It is reasonable, then, to think that biography based on face-to-face interviews will, for a given author, have a different character than one based entirely on documents. I reflect on how interviewing shapes my own writing and I encourage the reader to do the same.

Keywords: oral history, interviewing, biography, Barbara McClintock, Norton Zinder, Walter Nance, Charles Scriver

Context as Primary Source

Of course, it is possible to research a biography in silence. Mute documents and introspection are ample sources for an insightful, clarifying life story. But those of us who write the lives of contemporaries potentially have access to noisy, rich, complex sources unavailable to biographers of the long-dead: the subject herself and those who have

known her. Their heads are filled with memories of several kinds. Many could be written down but aren't. Some of these, lightly bound, are easily skimmed off consciousness's surface – all that keeps them unwritten is the effort required to narrate them. Others are so impacted they must be tweezed out by a skilled practitioner, armed with a sharp set of questions. But there are other historical data that are not simply left out of the written record by accident. There are data that do not exist until you, the interviewer, come along. Some are memories, some are observations. They do not sit, waiting to be discovered and interpreted, like a letter in a box in an archive. They are made, through the interaction of researcher and interview subject. This is risky territory. To think about interviewing and biography, then, is to reflect on the relation of speaking and listening to writing, and about the observation of the present as a clue to the meanings of the past.[1]

A couple of things make face-to-face presence special. First, there is the orality of the process. As different as reading someone's letters is from reading her memoir, in neither case did your source know that *you* were going to read that document. When I talk to someone face to face, he addresses me personally – even though he may be aware that he is speaking for posterity. My questions, vocalizations, facial expressions, and posture shape the story – and do so differently for each person I interview. If I show interest, the story becomes embellished with detail; if my eyes flick to the clock, the narrative diffuses and dies. I may prompt him toward one interpretation of the story, or trigger details of the story he would not otherwise have remembered. Such influences may be deliberate or not. Without question, this kind of intimacy with one's sources poses risks – foremost being the hazard of sympathy, "feeling with" the subject and thereby losing the disinterest crucial to good scholarship. Yet these are risks, not fatal flaws. As contemporary oral historians recognize, far from making interview material impossible to interpret, this complexity is where the interpretive meat falls from the bone.[2]

And then, this rich source sits in the midst of meaning. The folklorist Alessandro Portelli makes much of the etymology of "interview" (Portelli, 1990, pp. 31–32). He takes it literally, as a "mutual sighting" – an exchange, not a unidirectional downloading of statements from

[1] For a more systematic treatment of the functions of scientific biography, with an emphasis on scientists currently or recently alive, see Söderqvist, 2006.

[2] For an introduction, see the excellent collection Perks and Thomson, 2006. Of particular interest are Thompson, "The voice of the past: oral history" (pp. 25–31); Portelli, "What makes oral history different?" (pp. 32–42; also in Portelli, 1990); and Yow, "'Do I like them too much': effects of the oral history interview on the interviewer and vice versa" (pp. 54–72).

mnemonic wetware to digital recorder. But although our focus in this mutual sighting is on the other person, let us not neglect peripheral vision. The interview setting, the speaker's mood and affect, the temporal or physical distance the speaker has from the events in question – all are interpretable. All can bear on the meaning of the life you are writing. Consider the reverence we have for Darwin's study, preserved with every inkpot, book, and beetle as it was when it inspired him; such precious glimpses into the past are quotidian for the contemporary historian. Further, a mutual sighting implies recursion: your subject sees you seeing him, and conversely. The rapport between historian and source actively creates meaning. Context itself, then, can be a historical source.

Thus, interviewing is more than a means of acquiring data. It is a method of thinking and learning about a biographical subject. Looking someone in the eye, reading body language, improvising questions, observing the subject's context – all of these become tools for interpreting the text of the interview and the subject's life. Three examples from my own experience – one, an interview for my biographical study of the geneticist Barbara McClintock, and two interviews for the Oral History of Human Genetics Project, a collaboration with Edward McCabe, Marcia Meldrum, and Andrea Maestrejuan at UCLA – illustrate ways in which the face-to-faceness of the interview setting can create meaning.

Immediacy

One argument against using interviews is that people are prone to anachronism. Stories about the past are inevitably colored with the present in which they are told. How then can we trust what someone tells us long after the fact? On the other hand, history is made possible because of the converse: the past inevitably colors the present. Witnesses do store in their minds – granted, in complex ways – impressions from real events and relationships that may litter the archives, once you know to look for them. Sorting out the anachronistic from the unrecognized in interview data is challenging, not least because historians, too, fall prey to anachronism. We bring to our work impressions, meanings, and political agendas that can lead to judging the past by the standards of the present. Even conscientious scholars can be blind to their own prejudices. Sometimes it takes being face-to-face with an insistent, emphatic source to shake us from the conceit that we understand how our actors saw things.

Near the beginning of my thesis research on the Nobel laureate corn geneticist Barbara McClintock, I set out to collect memories of the 1951 Cold Spring Harbor Symposium. This conference had legendary status in the McClintock lore. It was at this meeting, all the stories said, that McClintock presented her most famous discovery, genetic transposition, for the first time in public (not strictly true, as it turns out). Everyone knew that McClintock's paper was met with "stony silence" (Keller, 1983, p. 139). No one believed her when she said she had found bits of chromosomes that could move around. This reaction has been widely interpreted as an example of the dogmatism and chauvinism of the male-dominated genetics community.[3] Beginning here, at the epicenter of the McClintock legend, I tracked down scientists who had attended the meeting and heard her speak.

One of the closest and most convenient Symposium participants to me was Norton Zinder, a gregarious microbial geneticist at the Rockefeller Institute, in New York City; he had been a graduate student in 1951. Zinder is a warm, enthusiastic, even garrulous man, with a voice hoarse and passionate, who speaks with his hands as well as his tongue. Every point he refutes seems an abomination; every assertion a matter of life or reputation. I sat down with Zinder and my tape recorder and said, "Tell me what you remember about the 1951 Symposium." His blood pressure rose as he thought about the alleged stony silence surrounding McClintock's paper:

> NZ: So the idea that we went to the '51 Symposium and didn't understand McClintock – what we didn't understand was what she was saying! That's because she didn't *speak* clearly, her talk was ambiguous as any talk could be…
> NC: She didn't annunciate clearly, or her talk wasn't organized clearly?
> NZ: It wasn't *organized* clearly! She was not a good speaker. And moreover, she really didn't want to speak about transposition. She was talking about regulation and *development*. Her whole theme was – and that's what she was pissed off at, and *nobody* would take that seriously. *Everybody* took seriously the transposition! – what she wanted was that transposition was the regulatory control of development. And *nobody* there – or at least the very bright geneticists in the room – could understand how stochastic processes could really be involved in something as organized as developmental regulatory stages. (Zinder, 1996)

He spoke as though he was jabbing me in the chest with his fingertips, backing me against a wall. He certainly got my attention, but he made me a little defensive. I was tempted to reject him as a bully, but I

[3] I document the development and interpretation of this myth in Comfort, 2001.

stopped short because I did not understand what I was being bullied into. What did he mean, "She really didn't want to speak about transposition?" It seemed contrary to everything everyone knew about McClintock. I had read published quotes from her that seemed to say she talked about transposition and no one believed her. If I could not believe McClintock herself, who could I believe? Was Zinder simply exonerating the scientists, mostly male, from the insinuation that they had rejected a Nobel-worthy discovery because it was presented by a woman? Possibly. Nevertheless, the claim was checkable and worth trying to verify. I set out to learn what he meant with all the talk of "stochastic processes" and "developmental regulatory stages."

The first thing I did was to reread the published version of McClintock's 1951 Cold Spring Harbor paper (McClintock, 1951). McClintock won the Nobel Prize in 1983 "for the discovery of transposable elements," and this was the breakthrough paper on transposable elements, so that must be what this paper was about, no? Well…no, actually. Transposition is in there, but it is sandwiched between a long introduction detailing the background on chromosome breakage and a long discussion speculating on gene regulation and development. I didn't understand it all, but the paper is clearly more than an announcement of mobile genetic elements. There was a large, complex argument there that needed to be contextualized. It dawned on me that I had been reading McClintock's work in light of its current interpretation. It had never occurred to me, or any other historian, apparently, that the meaning of McClintock's mobile elements might have changed over time. This was a testable historical hypothesis, interesting to me on several levels from the technical to the historiographical, and one I had ample resources to examine.

Fortunately, almost all of my research lay ahead of me. I used five types of sources: published papers, including McClintock's annual reports to the Carnegie Institution; her correspondence, and other scientists' correspondence with and about her; her own research notes – since she worked alone, she often "thought on paper"; further interviews I conducted with people who knew her and her work; and interviews I collected that others had conducted with McClintock herself. These ranged from the late 1970s until just before her death.

Soon there was no question that Zinder was right: McClintock indeed had been talking about the genetic control of development. Aided by patient and generous geneticists who took me into the field, into their labs, and into McClintock's experiments, I deciphered and translated an arcane and wildly original argument that was rooted in long-forgotten scientific debates of McClintock's day. At bottom was the following

conundrum: Given that all cells in the body have the same set of genes, how do you get different tissues? What makes one cell turn into an eye cell and not a kidney or a bone cell? By studying the patterns of pigmentation in corn plants, McClintock thought she had figured it out. Little pieces of chromosomes – not genes, but something else – hopped around in a coordinated way, reversibly silencing the genes. Like a player piano in reverse, these mobile elements "played" the genes, not by activating them but by muting them. The composition in this case was the developmental program, the instructions for how to build an organism. McClintock had called these mobile elements "controlling" elements because she thought they controlled the action of the genes.

My road-to-Damascus moment came when reading the transcript of an interview with McClintock conducted by William Provine and Paul Sisco in 1980, and graciously furnished to me by Provine and Lee Kass. McClintock's work was receiving renewed interest – it was, in retrospect, the beginning of the run-up to her Nobel Prize – and Provine and Sisco asked her how she felt about the fact that other researchers were finding transposition in their organisms. "People have said how pleased I must be that all of this transposition is going on and I said, 'Don't take that for granted'. The real point is control. The real secret of all of this is control. It is *not* transposition" (McClintock, 1980).

I realized that the term "transposable elements" was anachronistic; in fact, it was a substitute for McClintock's term, "controlling elements," which geneticists had never liked. Most scientists believe that the movements of mobile genetic elements are random, not coordinated as if by a cosmic programmer.[4] This, then, was what Zinder had been referring to with his cryptic talk of "stochastic events" and "developmental regulatory stages." It was "controlling elements" that people didn't understand or didn't believe. In the 1970s, a later generation of geneticists stripped away the connotations of developmental control and converted "controlling elements" into "transposable elements." McClintock's Nobel was awarded for a later interpretation of the meaning of transposition, one different from the meaning she herself ascribed to it.

With my new synchronic perspective, I felt I was flexible enough to begin to comprehend McClintock's notoriously idiosyncratic reasoning. When Zinder said "she didn't speak clearly," he was not being churlish. Even McClintock's published work is famously difficult to understand. Professional maize geneticists tried to wave me off my project, saying even they could not understand her papers. For example, near the end of a 1950 article in PNAS, she posed a series of questions:

[4] See *The Tangled Field*, "A molecular appendix."

> Is this transposition of heterochromatin? Is it a reflection of a process that normally occurs in nuclei? Is it responsible for controlling the rates and types of exchange that occur between nucleus and cytoplasm? Is it usually an orderly mechanism, which is related to the control of the processes of differentiation? (McClintock, 1950, p. 354)

This is the sort of elliptical, digressive discussion that makes McClintock's papers impenetrable to most readers. These questions read like hypotheses. Some were testable with available methods, others were not. Previously, I had read this passage as a speculative discussion of future directions in her research. Since then I had grasped what she had been trying to say for the previous 2 years, and I had listened to and read many hours of recorded interviews in which I got to know her vocal style. In my mind's ear, I could hear her peculiar variant of a blue-blood East Coast accent, hammering home these words. They were rhetorical questions. The passage was in fact a set of conclusions – they summarize the theoretical framework she had built around her data. I began to think of McClintock as in fact a very good writer. She was a *transparent* writer. Her writing faithfully reproduces the intricacy and idiosyncrasy of her thought.

The value of Zinder's remarks lay neither in their reliability or their uniqueness, but in their immediacy. His aggressive – though with me, always genial – tone both got my attention and provoked my skepticism. Having Zinder in my face helped me overcome the anachronistic view that the debate over McClintock's findings concerned transposition. Zinder did not provide me with an answer; he provided me with a question – one I had not thought to ask. Since most biographies are written about famous people, most biographers face the same dilemma: how to escape the post-hoc, mythologized version of the subject's life, in order to understand her as she was understood at the time – or even as she understood herself. It was having Zinder look me in the eye, having him tell *me* in particular, in his hoarse and emphatic voice, that drove home the message, and prompted me to follow the thread that spun out into the main argument of my biography of McClintock.

Improvisation

I used to keep a list of questions I would have asked McClintock had I had the chance. In my current research, on the history of human genetics, I often do have the chance to query the principals. In conducting interviews for the UCLA-Johns Hopkins Oral History of Human Genetics

project, I spend 40–70 h in preparation, reading the subject's curriculum vitae and published work, as well as other background materials.[5] Often times, the face-to-face conversation can solve a mystery that emerges from reading the paper record. Such was the case with the human geneticist Walter Nance.

I interviewed Nance at his summer home in rural Tennessee. He is solidly built and bullet-headed, his movement stiffened by childhood polio, his voice a growly bass, his manner gruff but cordial. Nimble is not a word that springs to mind at first meeting, although that is now how I think of him. He and his wife were my attentive and thoughtful hosts for two-and-a-half days. In between recording sessions, we walked in the woods, had cocktails, dined, and socialized; I also had hours to myself, to read and to think. During those days, both Nance and I devoted nearly our full attention to the interview. The formal interview blurred into informal conversation – often, we would bring up over dinner a question we had discussed during the recording session and chew on it together with his wife. At times, this relaxed quality could be frustrating. I had to be mindful about which topics had vanished unrecorded into the Tennessee mountain air. And Nance's narrative dodged and meandered, from stories about childhood to descriptions of scientific papers to his college advisors, and back again. The fuzzy boundaries between the personal and the professional, between youth and maturity, between the social and the intellectual created a spirit of improvisation in our interview. We relied on each other and on the rapport we were quickly developing. After an hour or so, I set down my prepared questions and trusted that I knew his history.

Nance is best known for his work in two fields: twin studies and the genetics of deafness. But he has published in many subfields of human genetics, including biochemical genetics, cytogenetics, population genetics, and clinical genetics. He has also published extensively on genetic counseling, which over the span of his career has splintered off of medical genetics to become an allied but independent specialty. In preparing to talk with him, I became impressed by both the breadth of subject matter and the large number of different collaborators on his various papers. While he was at the University of Wisconsin-Madison, he published with more than half the faculty in his department. Going into the interview, I wondered what it was in his intellectual style that led him to be so scientifically promiscuous. Among historians of dead people, such questions are mere speculation. But interviewing can provide ways to get at them.

[5] The Oral History of Human Genetics Project is online at http://www.socgen.ucla.edu/hgp/index.htm.

Two clues came mid-way through our first interview session, when Nance talked about his experimental style. A physics professor in college, a Dr. Petrie, once told him that he ought to keep a notebook to write down all of the unusual ideas that came to him. ''I used to ask all sorts of crazy questions in class, that he couldn't answer,'' Nance growled proudly. He never kept the journal, but the remark stuck with him. Dr. Petrie, he said, ''perceived that I sort of put things together in a different way than a lot of the other students'' (Nance, 2006). He repeated the remark a minute later. In other parts of the interview he related some amusing anecdotes that illustrated his ''crazy'' ideas, including trying to extract blood from wild marmosets (to study their twinning patterns) and wheeling patients across campus to the physics department to try using a magnetometer to measure hemoglobin content in the blood. I elicited many of these stories with unplanned questions, in which I was simply riffing off of his previous response.

The second clue came a few minutes later, in a discussion of the poor quality of most mathematics teaching for biologists. Nance described his experimental style as ''abstracting the truth out of numbers,'' an idea he rendered later as ''*ex*tracting the truth out of numbers.'' I was less interested in the math than in quantification as a conceptual unifier. It dawned on me that it might be Nance's way of linking all of these far-flung topics, that his brain might work by making connections among things, ideas, or techniques that strike most people as disparate and collecting large amounts of raw data and performing mathematical analysis. The more conventional way of thinking about the role of math in science is that the numbers are embedded in the phenomena and the scientist extracts the truth. But maybe Nance hit closer to the mark when he talked of *ab*stracting the truth: the numbers represented for him the unifying Platonic principles that he saw himself as trying to discover. Perhaps math connected the marmosets and the magnets.

A third clue was social. The department of medical genetics at Wisconsin was only 5 years old when Nance arrived. He had already earned an MD but wanted a PhD in genetics. The department was full of extremely talented young Ph.D. scientists and it had money but not much space. Nance's status as a credentialed physician gave him connections and independence that other graduate students did not have. With Oliver Smithies, his supervisor, he learned starch-gel electrophoresis, a technique Smithies invented in 1955 that made the separation of the various proteins in a biological sample vastly cheaper, smaller, easier, and more reliable. It was enormously important to human genetics in the late fifties and early sixties. Coming as a visiting physi-

cian, rather than as a regular graduate student, Nance was given extraordinary resources:

> When I went to Oliver's lab with an MD, I had a laboratory with two technicians that I basically directed…and I'm sure I got preferences that a regular PhD [student] wouldn't have gotten. You do it the other way around, I don't think people in a medical school would give a damn if you have a PhD. I mean, that doesn't cut you any slack at all! (Nance, 2006)

At Wisconsin, then, Nance had an essentially independent position almost on the level of the other faculty, but without the constraints of being on the faculty and having to establish one's intellectual independence from one's colleagues. This position gave him the freedom to play.

These disparate topics – childhood anecdotes, stories about his education and training, introspection about his intellectual style – spilling out as they would, jostling up against one another in his narrative, helped me to explain Nance's scientific style. Nance applied Smithies's starch gel electrophoresis to generate data from which he could "abstract the truth." For example, Newton Morton, a population geneticist on the faculty, had discovered a population in northern Brazil that was being relocated. As a consequence, every person in this village was being examined medically; this meant that nearly every member of nearly every family was passing through a single medical clinic. It was a rare opportunity to sample an entire population. Morton set up a program to draw blood from these individuals and screen it for a variety of known biochemical variations. Morton drew the blood and sent it back to Nance and Smithies to separate out the various protein variants. While at Wisconsin, Nance also collaborated with the biochemical geneticist Robert DeMars on the enzyme called G6PD. His dissertation compiled genetic studies of six different blood proteins (Nance, 1967a). He worked with Irene Uchida and the cytogeneticist Klaus Patau on dermatoglyphics, the study of fingerprints. After leaving Wisconsin, he continued to collaborate with colleagues from other departments. Nance's collaborations, then, came from his eclectic sense of where interesting data might come from and his skills as a physician, applied within a scientific context. He too was improvising, picking up conceptual or quantitative leads and playing with them, making connections on the fly.

Nance also exported genetic ideas to the community of physicians. Over the years, he has written many articles for clinical journals in which he gave an overview of recent results in genetics and molecular

biology – the genetic code, the operon, genetic counseling – in an effort to persuade specialists from orthopedics to auditory physiology that genetics was important.[6] He has always hungered for new methods, new types of data to which he could apply his intellectual methods. His research style has been much more intellectual than manual; catholic in method but with an underlying conceptual unity.

Or such was my hunch. Sitting in his study atop a Tennessee mountain, I had an opportunity to test it. Suddenly, while Nance was finishing a response, an image came to me. Hours before, I had begun the interview with my usual first question: ''Describe for me your family background and childhood.'' Nance's curriculum vitae said that he had grown up in the Philippines. His grandparents, he told me, had been Methodist missionaries in China and his father, a surgeon, had been a kind of medical missionary in the Philippines. Now, talking about his research career, his efforts to teach doctors about genetics struck me as a kind of proselytizing. I asked:

> NC: Did you see yourself as a kind of a genetics missionary?
>
> WEN: Absolutely…I have felt throughout my career that I've been a kind of a missionary, spreading the gospel to the uninitiated. (Nance, 2006)

He answered without hesitation; the immediacy is audible in the recording. This suggests that I hit upon a word he had used himself in thinking about his role in the field. The orality of the interview provides confirmation in a way that text cannot; even email or text chat suspend time, however slightly, and therefore lose information. From there, the conversation evolved into directions I hadn't at all prepared for directly, but which tied into what by this time was years of research into human genetics. He gave me insights into the human genetics community at the time, the research styles of various other researchers, and the evolution of the field in the 1960s. All of which fed back into my developing understanding of Nance himself and his role in that community.

My understanding of Nance's intellectual style and professional role emerged out of the improvisational nature of the interview context: the spontaneous creation of a historical document that idiosyncratically juxtaposes the personal, professional, and intellectual. The meandering of the interview, its jumps and doublings-back, provided opportunities for thematic connections that might not have occurred through more

[6] For example: Nance, 1959, 1967b, 1971a, b; Nance et al., 1965; Nance and Engel, 1967.

systematic research. It encourages a metaphorical style of interpretation: the scientist as missionary. Metaphorical reasoning – understanding the unknown by comparison with the known – is a fundamental method of understanding, although we tend to underrate it (Lakoff and Johnson, 1980; Root-Bernstein, 1989). Such thinking is made more difficult for the historian when episodes are imposed on an arbitrary linear scale such as a timeline or the alphabet, or when emotions and digressions are filtered out of written documents in the name of economy. No paper filing system could map a person's memory. An interview can do no more than take a transect through someone's memory, but, as ecologists know, that is a reliable way to sample a complex environment.

Embeddedness

Three o'clock in the afternoon on a brilliant early fall day in southern Quebec. I settled myself in Charles Scriver's small conference room, on the seventh floor of the Montreal Children's Hospital, an old building by the standards of American academic hospitals – this wing was built in the 1950s. I set up my recording equipment and began this oral history interview with one of the variants of my usual first question: "Tell me when and where you were born, and about growing up." Scriver's voice is soft, without burr or edge, and his erudition is masked by the folksy, unpretentious air of his Canadian accent. He smiles gently when he speaks. "It's a very unglamorous history compared to how people move around the world today," he said. "I was born in Montreal, I was educated in Montreal, I've worked in Montreal, and I'm going to die in Montreal" (Scriver, 2006).

A seemingly inauspicious start, perhaps. Nance grew up in the Philippines, China, and New Orleans; I have interviewed the children of Depression-era bootleggers and Holocaust refugees. By those standards, Scriver did not promise to be as exciting an interview. In fact, though, it turned out to be one of the most rewarding I have conducted. Scriver's remark proved to be a touchstone of the interview. His life and work are saturated with an extraordinary sense of place – one I never could have appreciated had I not interviewed him in person and in his natural environment. His opening remark expressed how embedded he is in his environment – something I could only have appreciated by interviewing him in his place. Besides explaining much about Scriver's life, this embeddedness turned out express the essential core of his intellectual work.

Scriver is among the most distinguished living human geneticists. He made his reputation in the field of biochemical genetics, working on amino-acid disorders. His work on the biochemistry of rickets led him to champion the addition of vitamin D to Canadian milk. His development of a genetic test for a number of metabolic diseases led to his organizing and carrying out the first large-scale genetic screening program in Canada, in the 1960s and 1970s, which was coupled to a pioneering program in genetic counseling. Since then, he has become a strong partisan for the notion of genetic individuality and so-called personalized medicine. He is fond of social network theory, as developed by researchers such as Duncan Watts and Albert-Laszlo Barabasi, and has written extensively on the application of network theory to genetics (Watts, 1999; Barabasi, 2002). Networks of genes, some believe, form natural, functional clusters that undergird our physiology.

Scriver's opening remark suggested a "small world," á la Duncan Watts – a small network of intimately and multiply connected individuals. Born on November 7, 1930, Scriver was the only child of two distinguished McGill University physicians. His father, Walter Scriver, was chair of the department of medicine at the Royal Victoria Hospital, associated with McGill University's school of medicine. His mother, Jessie Boyd Scriver, was a pediatrician and one of the first women full professors at McGill University Medical School. For college, Scriver too attended McGill. He applied nowhere else. He recalls no pressure or particular expectations from his parents as to career choice, and yet after college he applied only to McGill medical school. McGill accepted him. He took his Medical Doctor and Master of Surgery (MDCM) degree in 1955. The next year, he married his girlfriend of 10 years, who goes by the nickname "Zipper." They are still married. He did an internship and residency in medicine at McGill, a second residency in pediatrics, also at McGill. His only extended time abroad was a second pediatric residency at Harvard, 1957–1958, and a 2-year fellowship at University College Hospital, London, 1958–1960. He returned to Montreal to join McGill's faculty in pediatrics and has remained there ever since. He is a man profoundly unaffected by restlessness.

Privileged but not snobbish, sheltered but not naïve, Scriver lives comfortably nestled within concentric rings of affection, geography, and culture. He was surrounded by family when he was at work. He loves Montreal above all other cities. His office is filled with indigenous Canadian art. His choice of research problems, his scientific and clinical methods, and his vision of biology are all strongly shaped by these influences. Like many of my interview subjects, Scriver attributes many

of his choices and opportunities to chance. But in his case, these moments of serendipity often stem from a personal connection.

Scriver's principal coauthor on the genetic screening and counseling work was Carol Clow. She was a classmate in high school. But where Scriver had graduated and gone on to college and medical school, Clow had dropped out, married, and raised a family. Then, years later, during his residency in pediatrics, he says, his mother – the chief of pediatrics – reintroduced them, under sober circumstances.

> I was introduced to Mrs. Clow and her son who was dying of leukemia. And my mother said to me, 'Mrs. Clow will teach you about the dying child.' And Carol looked at me and I looked at her and I said, 'We haven't seen each other for quite a while.' (Scriver, 2006)

Fast forward several more years, and Mrs. Clow enters his life once again: a foster Eskimo child she was raising had died of Sudden Infant Death Syndrome. Scriver was talking with the distraught woman when he recognized his mother's footsteps coming down the hall. He invited her in and asked her advice. Scriver's mother said Mrs. Clow should not blame herself and should get out and do some productive work.

Scriver was at that time thinking about assembling a large pilot study for genetic screening in Quebec. It would involve testing 40,000 families; it would require a huge amount of repetitive work. No one in Scriver's group was interested in the project, so he offered it to Carol Clow. She accepted, and spearheaded the pioneering and successful project. In following up with the patients, she developed an innovative approach to genetic counseling, which became the prototype for a province-wide genetic counseling program, the first in Canada. Thus, Clow is one of the architects of genetic screening and counseling in Canada (Clow et al., 1969; Clow and Scriver, 1971; Scriver et al., 1978).

Scriver was so impressed with Clow that he proposed her for an unusual employment stream at McGill, whereby people without the normal credentials may be admitted to the faculty. Although she had not even finished high school, she retired as an associate professor. Scriver told this story with evident pride. At the end of the day's interviewing, he drove me across town to his house, where Zipper was making dinner for us. Driving through his neighborhood, he stopped outside a small grocery, saying he needed to pick up something for Zipper and I could just wait in the car. I waited quite a while, because as he left the store, a passerby stopped him. They chatted and chatted. When Scriver returned to the car he apologized for my wait and

explained that he had been talking to Carol Clow's daughter-in-law. He was so connected to Clow that years later he would keep an out-of-town guest waiting for 20 min while he chatted with her daughter-in-law. This was unselfconscious proof of the intimate, personal connection Scriver described in his relationship to Clow as well as to others in his professional life. The wait was worth it.

Thus, the research continues after the recorder goes off. Moving with Scriver through his neighborhood, the hospital, and the city provided continual evidence of connection. When Scriver and I walked out the main entrance of the hospital to eat our lunch, he greeted a young man at the security desk who hailed him warmly. "Who was that?" I asked as we stepped outside. Scriver's experiences setting up genetic screening and counseling programs in Canada began with phenylketonuria (PKU) in the 1960s, but expanded to include various other diseases, including Tay-Sachs and other hereditary diseases (Scriver et al., 1968; Scriver and Clow, 1980a, b).[7] Scriver attributes the success of his program to his building support from the bottom up: he began with his local community, with individuals who knew him, rather than with lobbying and legislation on a provincial scale. Students and teachers promoted the program to the local schools, which adopted it. The man at the security desk, Scriver told me, was a long-time survivor of a severe hereditary anemia, and had been one of the advocates for genetic screening in the high schools.

Networking, long integral to Scriver's research and clinical style, has become a central concept in his vision of biology. Network theory provides Scriver and others with a means of understanding how slight genetic variations condition an individual's response to medication, nutrition, or other environmental variables. They hope such data will provide a means for understanding, as Scriver says, "Why this person has this disease now." This is a fascinating area of human genetics and genomics, rich with social and ethical implications for scholars to chew over. As a historian of genetics, one of my interests is "Why this scientist had that set of beliefs then." My interview with Scriver in his place helped me understand how this individual clinician-scientist with this particular background and in these particular surroundings came to champion this set of ideas. Although he might have said many of the same things had I interviewed him in a neutral setting – say, if I had caught up with him at a professional conference – the impromptu, un-selfconscious reinforcements of his core beliefs that I received simply

[7] Scriver et al., 1968; Scriver and Clow, 1980a, b. On PKU and genetic screening, see Paul, 1998; Lindee, 2005; Cowan, 2008.

by walking and driving with him through his world gave his statements a reliability and depth they would not have had in any other location.

Synaesthesia

When we research a biography in silence, we work in a monosensory world. Indeed, the traditional research experience often seems designed to strip away all sensory experience. Documents in an archive are organized and filed, abstracted from their context. Often one can view only one box, folder, or even letter at a time – each text under analysis is surgically isolated to prevent cross-contamination. The only input is the eyes on the page. The pleasure and the beauty in such work lies in the active, imaginative task of recreating a life from such sensually remote textual traces. The examples above show that sensory perception, too, is an active cognitive process, not at all a simple matter of passive reception.

Each sensory modality is a unique thinking tool. We prefer vision for analytical discrimination – think of dials, meters, and oscilloscopes, which transduce various stimuli into pictures. Odors are famously powerful triggers of memory, although for Proust it was the taste of a madeleine and for others it is music (Cady et al., 2008). Moreover, psychologists distinguish separate attentions for each modality (Spence and McDonald, 2004, p. 21). Many people like to knit, for example, while listening to a seminar or watching television. Exploiting these different modalities can therefore be a powerful way to gain multiple perspectives on a biographical subject – giving us insight, perhaps, into the subject's emotional life or triggering memories that might otherwise remain deeply buried.

Further, the senses interact in complex ways. If a mosquito lands on my arm, I focus my visual attention there. I look up involuntarily from my book when the dog barks. A visual cue transiently causes us to hear and feel more acutely in that direction if we look – or even prepare to look – toward it (Spence and McDonald, 2004). Thus, multisensory experiences have enhanced intensity and force, as Norton Zinder's voice and gesticulations amplified his passionate statements about Barbara McClintock. Also, recognition – an incredibly complex process and arguably one of the key activities in an empirical, mostly subjective discipline such as biography or history – is heightened by multisensory integration (Newell, 2004, p. 123). A taxonomically indeterminate bird looks more like a duck if we hear it quack. In this way, my being

embedded with Charles Scriver aided my perception of the relationships in his networks and drove home the conceptual importance of networks in his own research. Finally, speech itself has been analyzed as a multisensory process (Fowler, 2004). This could help explain how serious conversation – a style of thought involving speech, listening, body gestures, and often reading and bodily contact – can lead to insights one would not reach by sitting alone staring at documents. Hence Walter Nance and I arrived at an understanding of how his life and work fit together through the rambling, improvisational process of conversation.

Such data do not exist before the interview, and they persist afterward only in the traces left in a notebook or on a memory card or magnetic tape. They are novel creations of the interview process itself. And they change the way we think about our subject. The ways that a multisensory approach will alter a particular biographer's thought are undoubtedly unique to the biographer. In my own case, it has led me to a great interest in motivation and to developing empathy for my subject without sympathy. By that I mean understanding what the other person is feeling without feeling it myself – my shorthand for the effort to circumambulate the scholarly hazards of this sort of intimacy. But each relationship between biographer and subject is a unique partnership – messy, dynamic, and, if done well, mutually rewarding.

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