8 Life at the Center

Donald A. Norman and Willem J. M. Levelt

In Chapter 7, Jerry Bruner tells of his vision of the Center for Cognitive Studies, one that complements ours in this chapter. His is the view from the top, the view of a founder and director who knew the context, the goals, the aspirations. Ours is the view from below, the view of the young researchers let loose to do as they would. The global context and purposes were foreign to us, not to be realized until much later (and for some things, not until now, when we read them in Bruner's chapter). Bruner states that "the intellectual life of the Center revolved around the seminars, the Thursday lunches, and the weekly colloquia." Perhaps. But that is not our memory. For us, the intellectual life was in the routine daily activities, in the offices and halls, in the labs late at night, and in the social interactions. The excitement was in the personal interaction and the private discussions and arguments. The formal seminars and lunches and colloquia were, well, formalities: the public display of the refinements. Formal presentations of ideas and results after much of the initial excitement had been polished, prettied up. We were the saplings, struggling to become trees. It is no wonder that we did not perceive the forest, or, for that matter, perceive how or even that we had been planted, nurtured, and cultivated. Such is the view from below.

The Harvard Center for Cognitive Studies. Ah, the good old days. The heady days of the cognitive revolution - when we were all much younger. But, as any modern psychologist will tell you, the good old days were never quite the same as memory would have them be. As for the revolution, there were really two different revolutions in progress, one in psycholinguistics, one in information processing, cognitive psychology. Both were probably revolutionary from the local, American perspective, but not so from the more global, historical perspective.

It is always difficult to know what goes on in another person's mind, especially when the person is George Miller. Quiet, tactful, reticent. A thoughtful thinker and writer, not given to quick responses and dashes of rhetorical comeback. That role was reserved for others. Actually, it was quite easy to know what was on some people's minds. Well, perhaps not to know, but to be told. Thus for Noam Chomsky, we always knew. Not that Chomsky said that much, but there were always people eager to tell us what he meant or what he would have meant had he said something. The prototypical lunchtime seminar - or at least, prototypical in our memory - is of everyone assembled around the large wooden

seminar table with an active, young cast of protagonists (perhaps Mehler, Bever, Fodor, and Katz), each paraphrasing and explaining to the lunchtime audience what the one had tried to explain to us what another had just said what yet another had just previously said that Noam would have said in retort to whatever the issue was, at the time. All the time, Chomsky sitting and listening to the others explaining his mind. George did not go for flocks of interpreters. He stood on his own. One had to read his writings, attend his lectures, and re-create his thinking to figure out what was being thought.

In retrospect, what was accomplished at the Center was quite remarkable. At the time it seemed natural, nothing special. Now, some twenty-five years later, we can evaluate the impact of the Center from a distance, with some understanding of the historical perspective. Psychology was in a period of rapid change. Chomsky had started his work on transformational grammars, changing the face of linguistic theory. The field of artificial intelligence had just begun. Mathematical psychology was blooming. The concepts of information processing were pervading everything: computation, philosophy, communications, engineering, biology, linguistics and, of course, psychology. In the midst of these happenings, the Center for Cognitive Studies gathered together a vibrant group of people with unconventional knowledge and interests, stuck them together in one place, gave them excellent research, meeting, and support facilities, and then allowed what was to happen to happen. There were frequent meetings and seminars, a continual stream of visitors, and, for the members of the Center, no responsibilities. All of the ingredients were present: facilities, people, an active spirit, and critical mass. The Center offered a true demonstration of the critical mass theory of research, the notion that work proceeds best when there are enough people interested in the same or closely related topics so that there is always an audience for new ideas, an audience that can criticize in depth, suggest, and help generate the next generation of ideas.

Was there a cognitive revolution? Norman points out that he didn't even know what "cognitive" meant. "I remember asking Miller one day while we were out walking just what the word *cognitive* meant," recollects Norman. "Whatever it was that he replied, it didn't make much of an impression. My memory is that he told me a few things it was not, and concluded by saying that it meant whatever one wanted it to mean." Ah, the Center for Cognitive Studies, populated by folks who didn't even know what the name meant. The point probably was that the Center at Harvard was not set up to be *for* anything in particular; it was set up to be *against* things. What was important was what it was not: not psychophysics (at the time, a major, mainstream activity in psychology), not animal studies, certainly not Skinner's operant psychology (whose world headquarters were just down the street). Basically, not contemporary American 1950s psychology. Late 1800s or early 1900s psychology, perhaps, but certainly not

the contemporary American psychology of the 1950s. The enemy was the present. Whatever we were to do, it was not to follow contemporary trends. The innocence of ignorance.

The common enemy was an especially interesting creature for somebody brought up in a European psychological tradition. Levelt remembers:

> When I came to the Center, it was quickly made clear to me (but not by George Miller) that, by and large, all psychologists are behaviorists - even if they might deny that themselves - and that the Center was in the business of demolishing behavioristic doctrine and replacing it by a mentalistic approach. The polemic excitement was, of course, largely lost on someone who had been educated on an eclectic mixture of gestalt psychology, phenomenology, psychophysics, and ethology. All of these are either mentalistic or nativistic or both, and behaviorism had been so completely absent from my horizon that I didn't even know the difference between classical and operant conditioning. What had been the unmarked nativist background of most psychology that I had learned at Leyden University and with Michotte in Louvain now became the marked foreground issue. It was somewhat like experiencing the American excitement over Heineken beer, which I had always thought to be just beer.

Both Miller and Bruner were well aware of the historical and European roots of psychology, but that awareness was rather more the exception than the rule at Harvard. For sure, some of the major landmarks of European psychology were known and discussed: Piaget, Broadbent, Vygotsky, Craik, Luria. European psychophysicists were widely known and admired, but that seemed different, not really relevant to cognitive functioning. The fact is that much thought directly relevant to the interests of almost the entire crew at the Center was conveniently ignored. In retrospect, the excitement about the common enemy can be understood only if one takes into account the loss of continuity that had taken place in American academic psychology. A loss that took place even within Harvard itself. Art Blumenthal, who decided to use his time at the Center to study the history of psycholinguistics, discovered one psycholinguistic classic after another in Harvard's own Widener Library: major works by Preyer, Wundt, the Sterns, Buehler, Guillaume, and many others. And according to the library slips, basically none of these books had ever been loaned out before. At the time that Roger Brown and his co-workers were doing their magnificent work on the psycholinguistic development of the two children they called "Adam" and "Eve," the Widener Library contained the complete Stern Archives, including the most detailed and extensive longitudinal records of child language development ever made. Yet these records had never been consulted in the almost thirty years they had been there. It was therefore easy for Eva (not a pseudonym this time!), one of the Stern children, to convince the library to give the Archives to the Hebrew University of Jerusalem on permanent loan.[1]

Miller's scholarship wasn't sufficient to beat this ignorance. He was careful not to dominate, not to impose his views upon others. He provided the catalyst

and the facilities. It was up to us to do our own work, to make our own discoveries.

The Center started in a pleasant old house on Kirkland Street, separated from all other parts of Harvard. Later the Center was to move to William James Hall, which combined in one building all of the heretofore disparate groupings of the two psychological departments at Harvard: Psychology and Social Relations. A sleek white building: modern architecture in full folly. Cutting up research groups into arbitrary groupings determined by floors and, worst of all, by the speed of response of the elevator, a speed so slow that it led to debates and experiments about response times, trade-offs (stairs versus elevator), and much discussion. On the middle floors, the Psychology Department with its three reigning professors: Skinner, Stevens, and Miller. Boring and Newman on the sidelines. Von Bekesy down below, existing on research grants, just a quiet-spoken psychophysicist, interested in prehistoric art, studying the details of the most sound-sensitive device in existence, next to the ventilation equipment for the entire building. No faculty position, no tenure. No students. Just a Nobel Prize. Soon to retreat to the University of Hawaii. Social Relations upstairs, a combination of social and clinical psychology, anthropology, and sociology. Social Relations and Psychology were separate departments at the time because of an academic feud that predated our existence. (Recombined again once the major protagonists had passed from the scene, but that was after our time, after the active days of the Center.) The Center had its roots in both departments - Miller was from Psychology, Bruner from Social Relations - and its location in the building was symbolic of these roots, with Psychology just below, Social Relations just above. The location did serve a purpose, easing communication with those just above and those just below. The course of science controlled by architecture and elevators.

The members of the Center interacted in groups, not by any purposeful arrangement but by the accidents of time, space, and organizational structure. One arrived, was assigned an office, told where to get supplies, where the lunchtime seminars were held, introduced to whomever was standing around at the time, and that was it. Except that that really wasn't it. The spirit of the Center did not reside within anything one could be introduced to or shown: the spirit came from the intense intellectual climate. Norman puts it this way:

> In my memory there were immediate and vociferous debates from the moment I entered the Center, especially with the people I first encountered: Al Bregman, Paul Kolers, Jacques Mehler, Nancy Waugh. Debates that in every case led to collaborative and productive ideas, experiments, and creativity on language, memory, attention, perception, and thought (although to publishable results only in the case of Waugh). Did it start the first day? My memory has the heated discussions with Bregman starting in the first hour, as he helped me carry my books to our joint office. The experimental studies with Nancy Waugh that led to the work on primary memory started almost immediately after my arrival at the Center.

So it was with many of us. We all did whatever we were interested in. George Miller asked whether we could use a digital computer to control experiments. Norman and Bregman answered "yes," and before we knew what had happened, Miller had written a proposal, shipped it off to Washington, gotten approval, and purchased a DEC PDP-4: 8,000 words of memory, a paper tape reader, and special digital and analog input and output channels. We squeezed it all into a tiny office on the first floor (shared with Dave McNeil). The PDP-4 provided computational power, unheard of in any other psychological lab, and a full 8,000 words of memory. Today even the smallest computer one can purchase has more computational power than that early thing. At the Center, and especially once we moved into William James Hall, the computer worked faithfully. This was one of the first uses of a laboratory computer in psychology.[2]

What really did go on at the Center? The study of language was at the core, but the range of topics was immense. Both Miller and Bruner had wide-ranging interests. This led to an eclectic selection of visitors to the Center and active research that covered more areas than any individual could follow. It led to novel interactions and to continual groupings and regroupings as research interests fluctuated. What were the topics? There was work on memory, perception, concept formation, thinking, developmental psychology, decision theory, and especially the development of language in children. Miller was continually searching for new paradigms, a search that started in psychoacoustics, speech, information theory, mathematical models, computer simulation, and, at the Center, transformational grammars. Each new approach richer and more powerful than the previous, each with its own set of limitations. Miller had a reputation for being the first to show how each approach might enrich our understanding of psychological phenomena, but also the first to point out the fatal flaws.

Was it revolutionary? Chomsky certainly spoke of a revolution, and there can be no doubt that Chomsky intended to create a revolution in linguistics. But was it a revolution for us psychologists? The answer depends upon the perspective one takes. Consider first Miller's psycholinguistics. Historically speaking, its significance and impact are huge, with the only competition coming from Wundt's psychology of language. Around the turn of the nineteenth century, every study in the psychology of language had to take account of Wundt's views, either for or against. Similarly, during the Center's years and long after, every major psycholinguist had to take account of Miller's views or the views of Miller's coworkers and students. There were even public conversions or, in some cases, public announcements that no conversion would be forthcoming.[3]

So, at least in psycholinguistics, the air was revolutionary. But now compare Miller's psycholinguistics to that of Wundt. Three major features characterize Wundt's position: His psychology of language was, first, mentalistic (and quite

Humboldtian, for that matter). It was, second, strongly linguistically inspired (by a group of young turks in Leipzig, the *Junggrammatiker).* And, third, it was nonexperimental (this coming from the father of experimental psychology himself!). How does Miller's psycholinguistics compare? Clearly it shares mentalism, and even a Humboldtian kind of mentalism, with Wundt. Miller's mentalism was pretty revolutionary from the perspective of the then current American scene, but it is not so revolutionary from the wider historical perspective in psycholinguistics.

Miller's psycholinguistics was, like Wundt's, deeply affected by a revolutionary kind of linguistics. In fact, this feature was probably the most distinguishing characteristic of Miller's psycholinguistics: a "transformational psycholinguistics." From a historical perspective, the fact that the psychology of language was affected by linguistic theory was not new. What was new was the transformational grammar and the effort to test its "psychological reality" by experimental means.

This brings us to the third feature: Miller's psycholinguistics was experimental. This marked a departure from the work of Wundt. But, of course, this was not seen as revolutionary. Psychologists were supposed to do experiments, so it was only natural that the psychologists of language would do experiments with verbal materials. There was no disagreement here with the tradition of behavioral psycholinguistics, except that the unit of study was now the sentence, not the isolated word or meaningless nonsense syllable. Still, from the historical perspective, this *was* a pretty revolutionary feature of Miller's work. More precisely, it was Miller and his co-workers who, for the first time in the history of psycholinguistics, developed at any scale experimental procedures for studying the mental processes underlying sentence parsing, memory, and generation. It was exactly these kinds of mental processes that Wundt had declared to be inaccessible to experimental study, and before Miller, there had been no more than scattered and occasional attempts to approach these issues experimentally. In this particular case, the experiments defeated the linguistic theory: It was precisely the psychological reality of transformations that could not stand experimental tests. But whatever will survive of the psycholinguistic theories from the Center (such as the derivational theory of complexity and the coding hypothesis), the methodology has become the foundation of modern experimental psycholinguistics.

And what of those of us not in psycholinguistics? Did we perceive the Center as revolutionary? No. We simply did our work, pushing forth in whatever direction seemed most promising. To most of us, the work wasn't revolutionary; we were simply working on new areas of research, areas in which nobody else was working. We were studying how the mind operated, what its structure was, what

the nature of information processing might be, what the flow of processing looked like, and what mechanisms might be involved. It is true that the work was novel to the then contemporary psychology, but it followed its own rich heritage.[4]

From the time of the very first computational machines constructed of gears and cogs, scientists have tried to determine how machines might be made to imitate humans. Developments in the mid-1900s gave major impetus to this work, which to a large extent was summarized in the various conferences on "thought processes" held in the late 1950s. Information theory and theories of computation gave a formal mathematical structure to the work, as did the evolution of a formal model of computational approaches, from developments in formal logic to McCullough and Pitts, who showed how it might all work with neurons, to Shannon, who applied it to computing machinery. And to Lettvin, Maturana, McCullough, and Pitts, who showed how specialized circuits might be implemented in the frog's eye. Rosenblatt's work on the perceptron was in the air. Minsky and McCarthy were beginning at MIT. Newell, Simon, and Shaw had started their work on problem solving, following a long European tradition going back to Selz and de Groot (a heritage that has now been explicitly acknowledged).

The research directions being explored at the Center were natural follow-ups to the thrust of information and computational sciences. To those of us who had been brought up on this approach, nothing was more natural than to pursue the question of how the human brain processed information. In this sense, from this historical perspective, we can say that there was nothing special about the application of ideas about information processing and computation to the understanding of psychological issues: It followed a long tradition. The work could be considered revolutionary only from the perspective of American psychology of the time. From a more global, more historically oriented perspective, the work was evolutionary, not revolutionary.

Note that the term "evolutionary" can be applied in two senses. First, the work was evolutionary in that it had developed in a natural way from a rich heritage. The basic ideas could each be traced to their natural, logical predecessors. Second, the work was evolutionary in the sense that when a new species takes over a niche that has not yet been occupied, it drives out the inhabitants of the surrounding niches. This is a somewhat revolutionary style of evolution. And this is what did happen at the Center. We occupied ground that others in psychology did not care about. Contemporary psychologists did not perceive us as revolutionaries. They probably thought we were irrelevant. But because we succeeded in our endeavors, the work came to dominate psychology, driving out much that had come before. (Driving out the good as well as the bad, but such is the way with evolution.)

The approach of the Center, focused as it was on doing new things, things

thought never to have been done before, led to insularity and arrogance. Insularity in that, since it was assumed that nobody in psychology had ever had these ideas before, we could afford to ignore all else that was going on. Arrogance in part because we assumed that these were the only ways to approach psychological issues and in part because this was a long-established Harvard tradition. Full professors did not speak to junior professors. Junior professors were told at the time of hiring that "Harvard is a good place to be from," being promised from the beginning that they had no future at Harvard (unless they had received their Ph.D. there). And everyone at Harvard was either a competitor or irrelevant. It actually wasn't all that bad, but this description captures the spirit.[5]

The success of the Center

The Center brought together the leaders of a new generation of psychologists. It fostered an atmosphere of creativity. It helped to establish the cognitive revolution in psychology. Miller's influence was pervasive. His quiet presence was a powerful force on the work. First, in his own writings and seminars he pushed back the boundaries of psychology. Second, by the policy of letting people alone, free to push their work in their own directions, he helped to create the excitement at the Center. And finally, he served as a role model of the research scientist.

For some, he had another influence: subtle education about the rest of the world. He achieved this gently, without force, without direct confrontation - these are not Miller's ways. But by gentle conversation, subtle hints and suggestions, and through the examples of his own writings. Norman describes the impact upon his own development this way:

> I arrived at Harvard relatively untutored in psychology. Yes, I had a Ph.D., but it was in a very specialized area of mathematical models, with a thesis in psychoacoustics (working on a problem discovered by von Bekesy, then developed by, among others, Stevens and Miller - for that matter, a topic upon which Neisser had done his doctoral dissertation). Most of my training had been in electronics, and my three years in the Psychology Department at Pennsylvania had not done much to broaden my views: Psychoacoustics and mathematical models of learning were, after all, very much in the engineering tradition. 1 guess I fitted in well at the Center, for I was one of the arrogant, thinking that the work we were doing was unique, that there was no need to read what others had done.
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> Miller showed the falsity of that. He produced two books while I was at the Center that demonstrated how wrong this view was. One, a review of mathematical psychology *(Mathematics and Psychology,* 1964), the other an introduction to psychology *(Psychology, the Science of Mental Life,* 1962). Both revealed the literature. But it was most especially the latter book, *Psychology, the Science of Mental Life,* that opened my eyes. It introduced me to the early literature of psychology, showed that there was life in the field, that there were ideas from the past that still exceeded the present capacity to bring them to fruition. My fascination with William James started then.

Consider the impact of Miller's book *Psychology, the Science of Mental Life.* Today, although it is still a very nice introduction, it does not seem courageous

or particularly special in its choice of subject matter. Consider the title - *The Science of Mental Life*. The title is nothing to wonder at. You would have had to be there to understand. At the time, such a title was unheard of. Skinner, just down the street, exerted an amazing influence. He had banished the word "mental" from the vocabulary of psychology. Therefore, Miller's choice of title was a political statement. Yes, the title was borrowed from William James; that was just the point. The behaviorists had thought that they had banished mentalism from psychology forever, especially William James. And here it was back again, right under their own noses. We don't know what battles Miller and Bruner must have gone through to create the Center, what problems it caused for them in their departments. It must have been difficult.

The Center worked because it created a high-tension atmosphere of creative, ambitious people coupled with good research support. The interaction was intensive, critical, powerful. It was not a relaxed place. One always had to produce ideas and then defend them against the onslaught of critical, informed opinion. It didn't matter how good your work was last year or even last week: What have you done today? Such an atmosphere will not work for all, but for the particular group assembled at the Center, it did. Mostly. And at this particular time in American psychology, a time when behaviorism still dominated everything, the Center turned out to be a critical force.

From the point of view of the young researcher, everything seemed perfect. The continual intellectual challenge, the research facilities, even the lack of responsibilities, save to create and produce. The administrative burden of running such a large operation was borne completely by George Miller and Jerry Bruner, their efforts invisible to the rest of us. Perhaps too invisible. Getting research facilities seemed so easy. If we wanted a computer, George would get it. Give him a week to write a grant proposal. If we wanted a this or a that, it would happen. Now that we are out in the world getting our own support, we can see what a tremendous amount of work must have been involved. In retrospect, it was amazing that Miller and Bruner got any of their own work done.[6]

What was bad? The insularity, the narrowness, the naivete and arrogance. But maybe that was necessary, necessary to push the field into uncharted waters. To move psychology from the dark ages of behaviorist traditions into the modern age, in which "mental" and "mind" and "consciousness" were words that could be used without apology, without quotation marks.

Notes

1 The Stern records have now been transcribed and stored in the computer files at the Max-Planck-Institut in Nijmegen. They are available in the United States at CHILDES, the Carnegie-Mellon/Max-Planck Child Language Archives.

2 As far as we can tell, this was the second digital computer to be used specifically for the control of psychological experiments, the first in a university setting. The first laboratory computer for psychology was probably the DEC PDP-1 used by Rubenstein, Hayes, Nickerson, and others at the Air Force Decision Science Laboratories at Hanscom Field in Bedford, Massachusetts. (See Miller, Bregman, and Norman, 1965.)

3 An example of a conversion is the 1969 article by Deese, "Behavior and Fact." An example of the public refusal to convert is Osgood's (1968) article "Towards a Wedding of Insufficiencies."

4 It is important to note that we are concentrating upon the work done by the people with whom we interacted, which means primarily those centered around George Miller. There was another major focus of the Center, that of infant perception and motor development, centered around Jerry Bruner. Our efforts focused upon the linguistics and information processing developments. The resulting lack of involvement in the work of those studying infants means that we cannot speak of the nature or impact of their work upon what is today the active field of cognitive development.

5 Weekly colloquia were places where one displayed one's cleverness. The first question was always asked by a senior faculty member, always standing up from the front row, facing the audience. After all, the speaker was only the vehicle; the important folks were the ones in the audience. A long first question, with a twinkle in the eye, displaying wit, erudition, and insight. Who else dared match that question? One of us (D.A.N.) remembers one occasion where a young Center member poked him toward the end of the talk and whispered, "I can't think of a question; can you think of one for me?" As for the answers to the questions - irrelevant.

6 The writing of this chapter has caused us to recognize yet another powerful influence of the Center upon each of our lives: as a role model for our own development, both as research scientists and as proponents of similar research centers. Thus, Norman has built up a similar, although more limited, enterprise jointly with Dave Rumelhart at the Institute for Cognitive Science at the University of California, San Diego. Levelt has done a similar thing on a somewhat larger scale at the Max-Planck-Institut fur Psycholinguistik, Nijmegen, The Netherlands. Students, postdoctoral fellows, visiting scientists. Provide good facilities. Set up a climate for the holding of frequent seminars and discussions. Take away the administrative burden from the younger workers. And leave people alone.

References

Deese, J. (1969). Behavior and fact. *American Psychologist, 24,* 515-22.

Miller, G. A. (1964). *Mathematics and Psychology.* New York: Wiley.

Miller, G. A. (1962). *Psychology, the Science of Mental Life.* New York: Harper & Row.

Miller, G. A., Bregman, A. S., & Norman, D. A. (1965). The computer as a general-purpose device for the control of psychological experiments. In B. Waxman & R. W. Stach (Eds.), *Computers in Biomedical Research.* New York: Academic Press.

Osgood, C. (968). Towards a wedding of insufficiencies. In T. R. Dixon & D. L. Horton (Eds.), *Verbal Behavior and General Behavior Theory.* Englewood Cliffs, N.J.: Prentice-Hall.