grading a picture in a fashion to tempt wrong hypotheses with a likelihood of their receiving low-grade confirmation. I recall two students, Gerry Davidson and Phil Daniels, trying me out as a subject in the “ambiguitor” that moved a picture gradually into focus. The picture was a reproduction of a Renoir painting of a mother and daughter in an opera box flanked by red drapes. I was full of premature hypotheses. The picture came fully into focus: I could not make it out for nearly a minute. After much more research, Molly Potter and I published a little paper in Science on the effect. We received an inquiry shortly after from the head of the union of American airline pilots; they had observed that accidents due to human error often started with precisely such a history of wrong hypotheses under low-grade visual conditions. The general idea, in its broader implications, was of course picked up by Thomas Kuhn in his intriguing book on scientific discovery, noting how inappropriate theoretical paradigms prevent discovery of what is right under the nose. The work on perception that followed had principally to do with the study of eye-movements. But let me pick up that subject in a later section where it can be in context.

For a psychologist interested in intellectual development in the 1960s, the starting point was Piaget. Rightly so, for his structuralist reformulation truly “reconventionalized” and modernized psychology in our day. But his was a needlessly quiescent account of development. As I read more Piaget, three points of dissatisfaction began to emerge. The first was his indifference to the specific history of an organism’s experience. Whatever the child did, whatever he encountered within very broad limits, the stages unfolded in their timeless, logical way. The environment, indeed, was simply aliment, which, in slang, is like our publum. Second, the operative filter to experience was always stage-specific. Nothing could get past it save what conformed to the rules of organization of the particular “stage.” Was there no way of tempting growth? Was there, in Marxist lingo, no liberating principle? And third, Piaget’s central concept of equilibrium controlling the unfolding of stages struck me as so much hanky-panky. It seemed to me that there were forms of environmental “nutriment” that had more effect on growth than others and these somehow got through or
under the filter. Equilibrium, or lack thereof, did not seem a sufficient explanation.

I recall a visit at about this time from Alexander Romanovich Luria. I discussed my doubts with him. He then propounded a theory of action based on Vygotsky in which man operated on and transformed his environment by the use of a language-based Second Signal System. I read Luria’s little book on the pragmatic function of language in development. It seemed to me that the burden of evidence was on the side of linguistic recoding as a major factor in the child’s development. In Vygotsky’s sense, there was a “zone of potential intelligence” that could be brought into play by the use of socially transmitted concepts and language. And we do not know the depth of that zone!

There followed a lively period of exchange between Geneva and Harvard, financed by Ford. Bärbel Inhelder spent a year at the Center and then there were visits back and forth with seminars. Piaget presided over his seminars and discussions with the somewhat slow and determined direction of a glacier. Data were presented by an assistant, always framed in a fashion to fit the logically defined topic of the year, and each year had its topic. “Le Patron” would then set forth the interpretation in a magisterial fashion comparable to a flawless Landowska performance of Bach’s Art of the Fugue. It was breathtaking and brooked no major contradiction. If fundamental objections were raised, questioning the general viewpoint, they were handled with a rather withdrawn respect and put aside as out of the spirit of the logical framework. If within the framework, Piaget is a superb and totally engaged protagonist. His response is never “oppositional”; it is invariably to show, in ever more “logical” detail, how his argument handles all the phenomena necessary to it. It is a magnificent single-mindedness and, I think, lies at the root of his singular systematic thrust. But it is not easy to live with theoretically. He ingests everything offered, and it comes out more Genevan than ever.

I think the research on which I was then at work reflected, perhaps too much, the struggle to come to terms with the corpus of Piagetian work, and the strong bonds of the theory in which it was interpreted. Jackie Goodnow, I recall, commented that I had let myself get drawn too far off the functional base of means–ends analysis on which my theory of hypotheses had been based; I centered, instead, on the problem of representation—how to characterize the nature of the models in terms of which information was stored and transformed, with too little emphasis on the use of models. Enactive, iconic, and symbolic representations were seen as ends rather than as means in problem solving. Like Piaget, we were asking ourselves how levels of representation were attained, not how they were used. There were fruitful research spin-offs—particularly as regards the role of language in development, the role of conflict in stimulating development, and, particularly, the role of cultural factors in shaping the course of cognitive growth (principally, Patty Greenfield’s contribution). But I feel that the theoretical thrust of the work as a whole came from Piaget. We dedicated the book to him. I recall well the lunch at the Ukrayina Hotel in Moscow at the International Congress in 1966 when we presented it to him officially. It was rather a stiff lunch, for he did not like the book. Roman Jakobson eased the occasion with a remarkable wit that can make light of any intellectual heresy. Piaget and I have not been very close since, though I keep a close working contact with Inhelder, Sinclair, and others there. For all that, the research and the book that came from it have had useful reverberations.

**Man: A Course of Study**

I left off the discussion of educational reform at virtually its beginning. It continued through the 1960s. There came into being in Cambridge, Massachusetts, yet another offshoot from the fertile tree of Jerrold Zacharias. This time, instead of physics, it was to be a social-science curriculum. Zack shared with Franklin Roosevelt the charming but devious practice of setting up or encouraging lots of competing seedlings to see which would grow fastest—doubtless the sign of a generous and optimistic heart. Cambridge was suddenly full of social-
science curriculum seedlings—some serious, some bizarre. It was the age of curriculum projects, and Cambridge (with its nonprofit but wealthy Educational Development Corporation under Zack) had the means. Soon a “little seminar” was called together to sort out the confusion. Eventually, there grew out of the discussions a plan for “K to 12,” as comprehensive curricula from kindergarten to the end of high school had come to be called. The upper end, on major themes in social thought leading up to the present day (evolution, technology, etc.), was to be presided over by the philosopher Morton White; below that there was to be a unit on the transformation of the idea of the American, from subject to citizen, supervised by the political scientist Franklin Patterson; and below that, for children aged ten to twelve, was to be a unit on man, on the nature of our species looked at biologically and culturally. That was the unit I was to be in charge of, with the aid of anthropologists and zoologists like Irven de Vore, Robert Trivers, and Asen Balikci, with Sherwood Washburn as consultant. Elting Morison was to preside over the entire enterprise. The psychological principles that were to guide us were very much along the lines set down in The Process of Education. Considerable grants were obtained from the government, and what was to be the most ambitious school–university collaboration in the making of social-science curriculum got under way.

Putting together Man: A Course of Study (MACOS) was an extraordinary experience in combining psychological ideas about pedagogy to fit not only the major ideas emerging in the human sciences, but also the demands of different media—print, film, graphics, “kits,” and a variety of exercises for pupil and teacher alike. The germ of the idea for the course had come from “Soc. Sci. 8, Conceptions of Man,” a year-long course in General Education at Harvard that George Miller and I gave together for some six years—including the years when MACOS was being constructed. For those of us involved, the experience was extraordinarily rich—intellectually, artistically, indeed over the whole range of the Allport–Vernon–Lindzey scale of values! I lured Peter Dow from the “Quaker Kremlin” of Philadelphia, Germantown Friends, to keep us in touch with the reality of schools, and in time we had a staff of teachers, artists, scholars, and lively graduate students that made for a very yeasty five years from 1962 to 1966.

We decided early on to avoid the format of a textbook cum teacher’s guide. The course should be framed around “obvious” questions about man—what is distinctive about him, how he got that way, and how he could become more so. To answer any of these questions required inquiry into knowledge that, while not well established, was at least well investigated by scholarly research. The pupils were to get a direct experience of the data from which inferences were drawn—film records, field notes, behavior records, whatever. We turned to Lévi-Strauss’ human exchange systems: affiliation, goods and services, and symbols, as defining human society. For the emergence of man, we turned to primate studies. How did we choose other peoples and other species to examine in depth? Luck helped. De Vore had already begun his long-term studies of free-ranging baboons in East Africa. His films and field notes were superb. We needed an accessible culture whose technology and way of life contrasted sharply with Western culture. We found Asen Balikci, a Canadian anthropologist who knew the Netsilik Eskimo of Pelly Bay, and we managed to get the Canadian Film Board to do a series of films on them under his guidance. Historical good luck was with us. Decades earlier, the Danish explorer–anthropologist Knut Rasmussen had visited the Netsilik. His journals and his transcriptions of their tales and myths were splendid. And when the time came to translate them into felicitous English, the poet Philip Booth turned up.

The experience with film was intriguing. With the help of Kevin Smith, we made “sound” films that had no commentary: the message had to be carried by the action, its sights and sounds. There is one film, indeed, in which an old man in the winter igloo tells a hunting tale in Netsilik, his auditors spellbound around him. It is beautiful in its directness. Smith quite justifiably carried off all manner of prizes, and pupils could look and figure out for themselves. Charles Eames advised us, “Better to fail well than succeed poorly.”

Perhaps the most exciting medium of all was the classroom. We tried teachers, actors, scholars, even psychologists; we used seminars, lectures, “open plan.” The chemistry
of classrooms still eludes me—whether it produces leaden gloom or total and concentrated involvement.

We had an interesting failure. We “tried out” one summer in a school that had been made available to us by Newton, Massachusetts, with a few dozen eleven-year-old volunteers. The summer school was a huge success. But we failed to get children (save a very few) interested in the nature of language—“baby linguistics” as an approach to the human symbol system, “baby Chomsky” more precisely. David McNeill and Roger Wales presided. They even had the actor-linguist Paul Schmidt working his magic. The children did not lack for interest in the literary uses of language: Their renderings of the myths or the “guessed dreams” of the Netsilik were extraordinary. Was “linguistic awareness” missing? Or was it the separation of structure from use? The difference between a well-formed sentence and an ill-formed one, “meaningless” or “meaningful,” moved them little. I think in retrospect that, had we concentrated on speech acts—felicity and sincerity conditions on speech—we might have been more engrossing.

What I realize now, and did not then, is that the proper target for a curriculum is not the pupil but the teacher, equipping a teacher for informed dialogue about matters that are not cut-and-dried, about which the teacher maintains a sense of conjecture. Such success as Man: A Course of Study has had, whether in America or England or Australia, comes from its tonic value for teachers. We succeeded modestly, and failed well! There have been prizes, honors, and such—even widespread adoptions. The course also inspired, in 1972, a vicious campaign against it from the extreme Right of American politics—the John Birch Society. It raised too many tender questions about man and human society. It was attacked as antireligious, as “evolutionary”(!), even as advocating wife-swapping as an alternative (a prurient misinterpretation of an Eskimo legend). A member of the House of Representatives from Arizona even charged that MACOS was a subversive conspiracy of those left-wing Harvard intellectuals, B. F. Skinner and J. S. Bruner! The National Science Foundation had its funds cut off by Congress for a few weeks until that body, badly advised in the Nixon era, agreed to pass judgment on the suitability of all future curriculum material prepared under their auspices. Only in America. In no other country where it is used has there been anything but murmurs of appreciation.

There was one other attack, a worthy one. Richard Jones wrote a book attacking the course as being guilty of too much intellect, not enough affect. He believed, and still believes, that a course of study should serve a self-realizing, somewhat therapeutic function. And while working on MACOS, he certainly enlivened the course by keeping us true to the affective, impulse-ridden, and anxiety-tortured lives of people who live in a subsistence culture—for example, the Netsilik boy who is not punished for stoning a seagull to death (a very powerful film sequence, again without commentary). How can we know whether Jones is right about what schools should do? His point is an interesting and powerful one, although I would hardly agree that MACOS is victim of the danger of dryness.

By 1966, I was running out of energy. My research and teaching, work on the course, service on the Education Panel of the President’s Science Advisory Committee, Presidency of the American Psychological Society—they all added up to the famous eighty-hour week that may be easier to sustain in one’s forties and fifties than when one is twenty-two, but that leaves one worn nonetheless. I recall so vividly the sense of relief, sailing off to Bermuda with our little gang in a friend’s yawl. We were bashed around for a day or two just off the island, but it seemed so self-contained. Perhaps contrast is all!

Return to Research

What a calm contrast to return to almost full-time work at the Center. But there were changes in the air. Studies in Cognitive Growth had just come out. I felt the letdown one does feel after a cycle of work is done. I wanted to get beneath the kinds of phenomena we had investigated in that book. The field of cognitive studies was itself in process of change. Detailed studies of information processing were to the fore—filter theories, masking phenomena, studies of meta-contrast. The
work was interesting within its limits. I lectured on it, even tried my hand at it a little with the gifted Danny Kahnemann, but I was not grabbed. My interest in developmental problems was still very much in force. I started some research on young infants, not being completely sure about what I wanted out of it, save possibly that it might provide me a run-up into the accomplished psychological functioning we had found in older children in the course of the previous cycle of work. And at the same time, true to form as Henri Tajfel wrote me, I picked up the thread of the perceptual work where I had left it a few years before, going back to perceptual recognition under degrading viewing conditions, this time using the technique of eye-movement recording in collaboration with that marvelously persistent analyst of eye-movements, Norman Mackworth. We were later joined by Elaine Vurpillot, who came to visit us for a year from the Sorbonne.

My interest in eye-movements had been kindled by reading and discussing with Mackworth the work of Yarbus and Luria, and of Vurpillot, particularly that aspect of their work that looked at eye-movements as external indicators of an internal search program. I had known Mackworth at the University of Cambridge. We had even wondered in those days whether the tracking of eye-movements might not be the road to understanding more fully and continuously the nature of unfolding perceptual hypotheses. Now we would look at differences between children of different ages and adults. We took as our displays some of the same stimuli that Molly Potter and I had used in our studies of blur-interference. What we learned was that, even down to age three, humans do in fact deploy their line of regard in a fashion consonant with the information value of features of a picture as judged independently by other judges. Big transport saccades take them to high-yield areas, and small saccades are used to search around within the area. Obviously, major features of the display not at the center of regard are being processed peripherally, judging by the manner in which regard shifts to them after a prior area has been exhausted. And indeed, there turns out to be very little difference in the pattern of ordinary eye-movements (uninstructed) between adults and children. One of the major ones of these is in the ratio of small to large saccades, the former being closer inspections around a highly marked informational feature. Young children do less close looking linked to the big patterns of movement. But I think I am not exaggerating when I say that it takes a quite experienced eye to distinguish the child's from the adult's record. Indeed, when one sets instructed tasks for subjects—as when Vurpillot in her elegant work asks children to judge whether two displays are alike or different—one can certainly find differences in the overall program, what it is that the child compares in making his judgment rather than anything intrinsic about the oculomotor system. Research in this promising field continues and, indeed, becomes possible now by virtue of new techniques not only of recording, but also of analyzing data. Indeed, along with a research student of mine, Peter Coles, I am in the midst of just such studies at Oxford. That will have to wait until a later installment!

The work on early infancy, started on my return to the Center, also continues now. I knew nothing about infants—aside from the direct experience I had had of my son and daughter and nephews and nieces. The incomparable Berry Brazelton, Harvard pediatrician and (along with Richard Held) an old tennis partner, took me in hand. He must have been appalled at my initial ignorance, but I am not shy with infants and read speedily. We did clinic rounds, observed infants in various tasks, looked at research films together. I trotted off to Bill Kessen in New Haven and Lew Lipsitt in Providence to search for procedures. My research assistant, Ilse Kalnins, was doing her Ph.D. at Totonto, stranded in Cambridge by her postdoctoral zoologist husband. She knew far more than I did (though I ended up as her cosupervisor). Soon we had a group going—Colwyn Trevarthen, Alistair Mundy-Castle joined from Europe, and Hanus Papousek in flight from Czechoslovakia. Tom Bower came later, and Jerry Kagan was also on the scene, though his interests were somewhat different.

I was back on my own street, searching out the roots of inference. And in the six years from then till my departure for Oxford in 1972, we managed quite a few variants on that
theme in a rather unsystemic way, ending up studying the
growth of skills. I never dreamed then then that “infancy”
would become all the rage!

It soon becomes apparent, when one studies infants, that
there is far more there than meets the unaided eye. What one
needs are behavioral procedures by which the infant can make
his information processing accessible for analysis. We—Ilse
Kalnins and I—began with sucking. Drew Marshall helped us
develop a highly sensitive device for its registration. In success-

tive studies, we discovered the extent to which the infant could
regulate his sucking as part of a self-comforting system in re-
sponse to changing environmental disrupters, could alter the
burst—pause pattern to suit the requirements of an operant-
conditioning task with milk as a reward (Don Hillman’s thesis),
could suck in order to bring a picture into focus or desist
when sucking defocused it (and then reverse when the condi-
tions were reversed, all the while coordinating his sucking with
his viewing of the picture). Meanwhile, Brazelton and Main,
and then Trevarthen and Richards, were showing the degree
to which mother and infant coordinated their joint attention
and joint arousal in face-to-face contact. And a few floors
below us, Bower was getting on with his studies of the percept-
tual precocity of infants, and Kagan with his studies of the
sensitivity of the infant to changes from habitual presentation
patterns. Marshall Haith, then fresh from Yale, was demon-
strating that infants in the dark with minimal stimulation ac-
tively searched out their domain and were anything but pas-
sive. My own further work with Barbara Koslowski showed us
that infant skill developed in a quite predictably rational fash-
ion, subroutines being mastered for particular tasks and, once
mastered with the consequent freeing of processing capacity,
combined with other subroutines to achieve more complex
goals. Mundy-Castle and Anglin were showing the extent to
which infants could follow a hidden trajectory of a vivid
stimulus that disappeared up in one window and then down in
another. And the great bulk of the research was done on in-
fants in the first half of their first year. At later stages,
Greenfield and Smith were examining the grammatical com-
binning of holophrase with gesture and context in the one-word
utterances of infants.

I think that it would not be parochial to say that, in those
five or six years at Harvard, work at the Center and in the
group around it helped turn the concept of “helpless infancy”
a little on its head. Virtually all the players—and I have only
named half of them—are still at it elsewhere, and it is still too
early to say where it leaves things. We are all, I suspect, much
too close to know.

For all the productivity, however, this was not a happy
period at Harvard and in America. Vietnam, with all its des-
pperate inhumanity, was beginning to tear the nation apart. It
also released something extraordinarily parricide in the youth
culture that started out in the innocence of “flower people”
and led eventually to the Weathermen. For the first time in my
academic life, I felt active hostility from some of the postdoc-
toral fellows, veterans of wherever they had been involved in
the Vietnam protest or in university uprisings. I was strongly
opposed to the war. My son Whit was in Vietnam in the Army.
He was no supporter, but he was there as were many
thousands of others. And beyond Vietnam, domestic issues of
inequality had finally been made incomodiously clear by the
Blacks in America. It was a matter with which I had become
deeply involved in Washington.

The Revolution of Rising Expectations

In the atmosphere of the latter 1960s, research felt almost like
an escape from the miseries of the world—from those miseries
close by, as in auctions in the universities; from those at a
middle distance, as in the squalor of American ghettos; and
from those at a distance, in Vietnam. Earlier in the 1960s, I
had been somewhat involved in getting Head Start under way.
By 1968, there was already a good deal of discussion about the
“irreversible” loss in cognitive development in children with
insufficient early opportunity for stimulation. The “depriva-
tion model” was very much the leading metaphor. Various
friends, notably Urie Bronfenbrenner, and I had lobbied to-
gether in Washington for the improvement of preschool pro-
vision, particularly, for “deprived” children.
One episode gives a sense of the times. Another good friend, Adam Yarmolinsky, a brilliant young lawyer and one of Robert MacNamara’s “whiz kids,” had just become Sargent Shriver’s principal aide in the new Office of Economic Opportunity, established by President Johnson to lead the “war on poverty.” When I told Yarmolinsky about the currently circulating ideas for something like a “head start” for deprived children, he arranged for me to see Shriver, whose wife and I had served together on the Board of the John F. Kennedy Center in Nashville. I broached the idea. Shriver’s response was: “Are you seriously proposing that the federal government get into nursery schools when we hardly dare to poke a nose into ordinary public education in the States? It’s political dynamite.” I departed for the Boston plane.

About a week later, at cocktails before dinner, the phone rang. It was Shriver asking if I could come down to talk more about “that idea.” The conversation was curious the next day. I had mentioned “pilot projects” to explore how such a program might be put together, having very much in mind the models that had been tried out by Susan Gray and Nick Hobbs in Tennessee and by Bettye Caldwell in Syracuse. Shriver was urging that in a matter as important as this one, pilot projects were “too fastidious.” If the need is there, one plunges in. In the American political perspective of that time, I suspect he was right. So Head Start came into being. Julius Richmond and Nick Hobbs did an enormous job of attempting to give it shape. Meanwhile, a number of us worked in a White House committee, on a more comprehensive child-care bill to be guided through Congress by Representative Brademas and Senator Mondale. I recall a dark, wintry Sunday evening I spent with Urie Bronfenbrenner in the Executive Offices overlooking the White House, drafting part of a speech we hoped President Johnson would give, supporting the proposal for comprehensive child care. He did give it, but by the time the bill had worked its way through the Congress, Nixon was in the White House. He vetoed it. The New York Times invited Bronfenbrenner and me to write one of their “op-ed” guest editorials, which we did in the form of an open letter to the President on America’s short-sightedness toward children. The ebullient Daniel Patrick Moynihan, then in transit from Harvard to President Nixon’s circle of advisers, visited me in my office shortly after. Would I support the President’s new proposal for an Office of Child Development? I told him I would anytime there was a sign that it was more than a paper promise. There never was such a sign. Instead, there was the Westinghouse Report alleging that Head Start (hardly off the ground) was a failure—even though, for example, it showed that almost 90 percent of the parents interviewed felt the program had helped their children (a finding that, it seemed to me, suggested that, surely now, steps could be taken to involve parents more in the program). Fortunately, Head Start survives, and it may take many years before such an institution takes a shape suitable to its task.

By 1968, Harvard erupted much in the same pattern as other universities. It seemed a unique event as it came upon us, but it was a familiar scenario. I had always been close to undergraduate life at Harvard, as a teacher, as a member of Winthrop House, and eventually as comaster of Currier House along with my wife. In one respect, I welcomed the new political consciousness among the Harvard undergraduates. Far better than the Eisenhower years! But I disliked the political bullying by the “Red Moles” of the ordinary, guilt-ridden, moderate undergraduates. It seemed to me that the best approach to the “troubles” was to take them up substantively on their merits as they arose. Let student protests be dealt with nor as irrational expressions of parricide (which doubtless they were for some students), but on their merits within the legal framework. I know this is nineteenth-century liberalism and not very fashionable, but though I know the arguments of Rawls on the monopoly of justice by those in power, I still believe in the rule of law and the process of legal change.

Not surprising, then, that I urged that if indeed the ROTC were at Harvard in violation of constituted procedures, then the procedures should be amended (for which there was no taste locally) or the ROTC should be phased out by legal means. It was an emotive issue. There was a strike, a mass meeting of ten thousand students in the Stadium with strike votes and the lot. Two members of the Faculty were invited to speak—Stanley Hoffman and myself, both of us political
moderates by temperament and stance—and we both urged moderation, a not very appealing line in that atmosphere! The chemist Paul Doty and I finally worked out a compromise acceptable to the Faculty and the Harvard Corporation, and I was given the sad job of negotiating the three Services out of the University—sad, for I found their serving officers honorable, sympathetic, and innocent.

The storm was over, but there had been enormous damage to the University. I recall entering occupied University Hall, having asked permission as Master of Currier House, to see what my people were demanding. Something irreversible had indeed happened there in the hallowed Faculty Room where I had been so awed at my maiden meeting by the thought of William James, George Santayana, and the presence of Whitehead and Roscoe Pound. Now, like a Hogarth print, it seethed with students in debate. It was something as irreversible, psychologically and symbolically, as I have ever experienced. It ended badly with a police "bust," and that further divided the University.

I'm often asked how I could leave Harvard after all those years and all those commitments. When the troubles were brewing, my wife and I agreed to take on the co-mastership of the new Currier House. Like many others, I foresaw troubles and thought that Harvard should try to react by reexamining itself as a university community. The bitterness of the "bust" spelled an end to that. The President, Pusey, finally resigned, and Harvard returned to a self-protective and very "managerial" orderliness under a new President. It was made plain that the Center for Cognitive Studies should now rejoin the Psychology Department, that the rule of departments was being reestablished and strengthened. I had also been very put off by a high-handed veto by the President of a cherished plan that Jerry Kagan and I had worked out jointly with the Children's Bureau, the National Institute of Child Health and Human Development, and the city of Cambridge, whereby Harvard would join in establishing a new "model" daycare center as a pilot project in national, local, and university cooperation. The mood was very depressed. George Miller left for Rockefeller, Morton White for the Institute, Kim

Romney for La Jolla, Alex Inkeles for Stanford—all close friends.

Move to Oxford

It was in this atmosphere, in 1971, that I received an invitation to take the Watts Professorship at Oxford. My weariness with the local scene was doubtless added to by the state of the nation: Nixon had just been swept into office by the greatest majority in history. Another element I considered was my feeling of "Europeanness" in intellectual formation. And the Oxford invitation also had some historical roots. In 1955, a "caucus" of Stuart Sutherland, Gilbert Ryle, and several others at Oxford had tried to get me to put in for the Chair that Humphrey was then vacating. But I had wanted to raise my two children in America, and Isaiah Berlin had, quite inadvertently, put me off by commenting about the prospects for psychology in Oxford—that the University was "like treacle: if you press hard against it, it is like stone; if you press gently, it giveth sweet progress." Now that my children were launched, Oxford had finally accepted (or seemed to have accepted) psychology, and the Watts Professorship had few administrative duties attached to it. My wife and I could afford the cut in salary. However, the biggest factor was Oxford itself. Alan Bullock, the Vice Chancellor, Isaiah Berlin, then President of Wolfson College of which I was to be a Fellow, and Larry Weiskrantz, who presides administratively over the Department of Experimental Psychology, were extraordinarily welcoming and cooperative. It was a lively scene. It looked like a good place to launch into a new phase of work in a bracing environment. I am enormously fond of Britain. I knew the country well (though not Oxford), had always enjoyed the open quality of discussion and the British capacity for friendship. They are reticent but loyal, and extraordinarily "personal"—capable of great kindness and, perhaps the other side of the same coin, also capable of stunning bitchiness. To have an Englishman for a friend or an enemy is an awesome
experience! Fair-mindedness and bloody-mindedness are their yin and yang.

Harvard could not have been kinder, nor Oxford more cooperative, during my year of transition, a year that I spent on two enterprises. I wanted a year to read as widely as I could on the evolution of immaturity among primates, and during the year, I wrote "The Nature and Uses of Immaturity." I also wanted to prepare to sail the Atlantic in our old 32-foot water-line yawl, Wester Tll. The year done, my wife and I departed from Manchester, Massachusetts, on June 17, 1972, with a few friends. We broke the journey at St. John's, Newfoundland, and landed in Lymington on the south coast of England on July 14. It was a beautiful and trouble-free passage with some excitement from iceberg hazard in the dense fogs of the Grand Banks. We had birds with us all the way—puffins, terns, gannets, guillemots, dovekeys, razorbills, and even a displaced carrier pigeon who joined us in the Western Approaches, rested an hour, and departed for France. At the close of a very courteous inspection by Her Majesty's Customs and Excise, the boarding officer asked casually, "You don't by any chance have any drugs on board?" Yes, I said, I had six ampules of morphine for emergency injury. "What a pity, for it will mean filling out endless forms." He paused. "You're going to Oxford, aren't you? You must surely know somebody in the medical faculty there? Would you mind terribly taking those ampules with you and just giving them to him? It will save so much trouble." I'm told I am the first Oxford professor in history to sail himself across the Atlantic to occupy his chair. A rather specialized distinction!

What a contrast, a few weeks later, to fly to the International Congress in Tokyo. My new colleague, Peter Bryant, had told me that no visa was needed for Japan, and none is—if you are British. Fortunately, I was rescued by the incredibly efficient and welcoming Japanese organizers of the Congress.

Oxford has two worlds—the world of Colleges and the world of the University. The world of an Oxford College is intensely personal in a ritualized way, and highly convivial. It is a lens that focuses both the loyalty and the bloody-mindedness of the British character. There is no subject too small to matter if somebody in the College cares about it. The University is represented by one's Department, and I can only speak of Experimental Psychology. In Experimental Psychology, one's specialty matters deeply. Psychology is often regarded in the University as an "upstart" and an "easy option." In Britain, it is a field that receives little love and recognition from the Establishment. Only three British psychologists have ever been elected to the Royal Society, and none has ever been elected to the British Academy. There is a fair amount of self-hatred among British psychologists, that most often takes the form of dismissiveness toward specialties down-market in rigor from one's own. Some British psychologists, for example, are never so flattered as when mistaken for a physiologist or pharmacologist or geneticist. But perhaps this is increasingly endemic to psychology around the world.

Oxford psychology is curiously compartmentalized, and the emblem of its compartmentalization is the seating pattern at 11:00 coffee, in which each specialty group sits—the "information processors" here, the "brain and behavior" people there, the social psychologists in that corner, the developmentalists in another. It can easily be said that Oxford is greater than the sum of its Colleges. Of the Psychology Department it can be said, curiously, that the whole is less than the sum of the parts. For, individually, it is a Department of first-class people: Larry Weiskrantz, Jeffrey Gray, Donald Broadbent, Ann Trines, Peter Bryant, Michael Argyle, to mention only some of the better-known. The members are fiercely hard-working, both as teachers and as research workers, but turned in on specialties. It is difficult to get sustained conversation going on the shape of psychology as a whole.

My first six years at Oxford have been enormously rich for me. It did not start well. I cast around at first to find a fresh way of pursuing my interest in the earliest period of cognitive growth, in the years before the child began his career as a user of language. I had, so to speak, "brought a team" along with me—Aidan Macfarlane and Paul Harris had been with me at Harvard, visiting from Britain, and came back, and some students came as well. I very soon collected a group of research students and postdocs who could, in the Oxford manner, form their own little in-
group—and a very talented lot they were. Drew Marshall, who
had helped so much at Harvard in designing apparatus, came
along for the first few months to sort out the technical prob-
lems of the laboratory. Work started on perceptual develop-
ment, with apparatus designed by Stephen Salter, and the
group was soon turning out studies on the effective visual field
of very young infants, on mother–infant bonding, on the im-
 pact of early mother–infant separation occasioned by the in-
fant having to be put in intensive care. It looked as if one of
the outlets of the work on infancy was to be a collaboration
between our group, the pediatric group at the John Radcliffe
Hospital under Peter Tizard, and the Department of
Psychiatry—with Aidan Macfarlane, a physician–psychologist
serving as the bridging agent. The object of the research was
the exploration of how various forms of early care, par-ticular-
ly the forming of a close and early link between mother and
infant, affected the course of development. After spending
much time formulating a joint project, we were turned down
by the Medical Research Council. I think it was too “broad”
for official taste. It was my first “turndown”! We lost our team.

My own interests turned increasingly to the study of early
communication behavior in children prior to the onset of full
lexico-grammatical speech. It was a surprising departure for
me. I had never really worked directly on language acquisition
before—though I had been close to Roger Brown, David
McNeill, and George Miller. I think it was probably Patty
Greenfield’s work on the structure of holophrastic speech that
got me directly involved. My previous reluctance to get in-
volved in language work stemmed, I think, from the feeling
that formal syntactic analysis was only indirectly related to the
psychological processes that interested me. Syntax was a
generative system whereby one could go powerfully “beyond
the information given.” In this sense, it was like mathematics.

Roger Brown’s book had come out in 1973. I had read it
with great admiration, along with the new Greenfield–Smith
manuscript. But one particular paper led me to the path I now
travel: Joanna Ryan’s on the social context of language in the
Martin Richards volume on socialization. It is a beautiful pa-
per, and it guided me into a literature “endemically”
Oxford—speech–act theory and, more generally, modern
pragmatics. I read Austin’s How to Do Things with Words, John
Searle’s Speech Acts, Grice’s chapter on the “cooperative princi-
ples” from his William James Lectures. Then on to Harrison,
Strawson, and Lyons on deixis. We began a longitudinal study
of six infant–mother pairs. Michael Scaife began his work on
the child’s sensitivity to pointing as an ostensive precursor to
later forms of reference; Andrew Meltzoff, who had come
with me from Harvard, was beginning his work on earlyfacial
imitation during the opening weeks of life.

I had imported the weekly “coffee-and-doughnut seminar”
that had been a feature of our research group at the Harvard
Center. It was extremely lively. But there were differences
between Oxford and Harvard. The Oxford tradition of cut-
and-thrust can, at its worst, be appalling. It leads to point-
scoring and to the shooting-down of new and interesting
hypotheses by the immediate marshaling of the limiting or in-
validating case. For those deft in debate, it is an exciting form
of flirtation. But scientific productivity is not particularly cor-
related with deftness in debate. My research group was a mix
of people from quite divergent backgrounds—English, Scots,
American, Spanish, Swiss—and our visitors ranged from East-
ern European linguistic philosophers to Australian enthusiasts
reporting on infant perception. In time, we formed an ex-
traordinarily lively group. Of all my students, I think the ones
who have the least skill in collaborating are the English. They
seem least able to resist savaging an exposed flank. On the
other hand, they are perhaps the ones most capable of lone
work and a high degree of self-direction.

I began straining toward a general theory of how young
children acquired the uses of language. Communication in-
volved conventional procedures for fulfilling various extraling-
guistic functions—calling attention to objects, demanding or
requesting, forming social relationships, and so on. These
communication procedures emerged and became conven-
tionalized in well-practiced action formats cooperatively in-
volving mother and infant. The procedures are, at first, highly
controlled by the mother in a way to join her and her child in
a net of contingent interactions, the mother operating by re-
liance upon an updatable theory of the child’s performance.
The first results of this work began to appear in early 1975 in
a paper on “From Communication to Language” and in more specific technical papers since. To say more on this matter would carry us into the future—a future now taking shape, I hope, in the form of a book. It is to be called *Acquiring the Uses of Language*.

The more deeply I have gone into the psychology of language, the more impressed I have become with the absence in psychology of certain forms of psychological analysis that are needed in the study of language acquisition and language use generally. One such is the role of intention and the perception of intention in others. Language use is premised, in a massive way, upon presuppositions about *intentions* and the *reasons* why people do or say things. Yet psychology, or at least positivistic “causal” psychology, ignores the role of intention and assigns no interpretation to reasons in the regulation of behavior. Such matters are most often treated as epiphenomena. How then can psychology connect with linguistics and sociology and anthropology and jurisprudence, where the establishment of conventions is premised on intentions and reasons and even “responsibility”? In 1976, I was invited to give one of the Herbert Spencer Lectures at Oxford, and I used the occasion to voice these misgivings, urging that psychology should not cut itself off from the sciences of culture. I’m afraid that most of my Oxford colleagues in Experimental Psychology felt I had let their side down! When my lecture appeared in *The Times Literary Supplement*, there was a double-barreled barrage—fan mail from one part of the psychological community and brickbats from the other. Skinner and I—i had attacked the ant intellectuality of behavior theory as an instance of our illness in psychology—had a sharp and overheated and prolonged exchange in the correspondence columns of *The Times Literary Supplement*, and in the spring, there followed a Department seminar. If nothing was settled, at least the debates caused some buried presuppositions in psychology to come out into the open, where they belong.

Two other ventures were launched in these past six years in Oxford. They are rather of a piece. One is a publishing venture, *The Developing Child*. There is such a gap between current, scientific knowledge about human development and its application in daily life, either in affecting policies (as in education or child care) or, indeed, in guiding or alerting an individual parent. The arranging of the course of human development, whether private or public, is in some crucial respect a “policy decision” based on a hoped-for outcome. I should have the benefit of the latest and most robust knowledge we have about human development. Several of us hit on the idea of a series of books, written for the intelligent layman and the nonpsychological professional, summing up the state of the art in various domains of developmental psychology. Michael Cole, Barbara Lloyd, and I, with the Harvard University Press in America and Open Books/Fontana in Britain were able to convince some of the top scholars in the world to write such books. More than a half dozen have appeared. More will follow.

The other venture is, in George Miller’s generous sense, about “giving psychology away,” in this case developmental psychology. It did not take me long after my arrival in Britain to get back on my old battleground—the adequate provision of care for young children. Something deep is happening in the economically developed world. In 1974, in Sweden (to take the most advanced case), slightly more than half the mothers of children of five years or less were in the labor force. (Britain lags behind, but is catching up fast.) Who looks after their children, and how? Another facet of the same problem: In London, the incidence of clinical depression among mothers at home with children under five is running at about 25 percent, treated mostly by tranquilizers and mild antidepressants. There are plenty of studies to indicate what the pressures are on mothers—economic and psychological—and what kinds of factors put young children at risk. Indeed, there are now quite a few good studies about how part-time and full-time care outside the home can be arranged and organized so as to help the child. But what is extraordinary in Britain, as in America, is how little impact such research has on the practice of child care. Practitioners do not know about the work, or they pooh-pooh it as not related to their situations, or (if it runs counter to their child-care “ideology”) they dismiss it.

When the British Social Science Research Council asked me whether I would like to field some research on the state of child care in Britain, I chose this for my topic: How does one
get research on human development into practice? We are studying Oxfordshire as a microcosm and are mounting cooperative local studies with key practitioners serving as collaborators. To narrow our range of concerns, we have centered on the development of sustained attention or “concentration” in preschool children. How can one get practitioners to improve their practices with this goal in view? The research literature points to a trinity of factors that seem to increase concentration:

1. Opportunity of playing and interacting with adults.
2. Challenging materials with intrinsic structure.
3. Play and games with considerably elaborated rule structures.

But this trinity, alas, runs counter to middle-class, liberal, nursery dogma that urges the adult to keep off the child’s back, to provide materials (water, sand, clay) that permit expressiveness, and to see that the children’s play is spontaneous and minimally constrained. It is not going to be easy to “give psychology away”! However, the study is in process and we shall see.

Some Sort of Summing Up

My theoretical interests have been strongly functionalist. Hypotheses in perception, strategies of thinking, functions of attitudes, uses of language—all of these mark me as following the tradition of James, Dewey, McDougall, Vygotsky, and Tolman. And, surely, my developmental studies express the same interests. I am also a mentalist and have always felt that the banning of “mental” concepts from psychology was a fake seeking after the gods of nineteenth-century physical sciences. Professor Boring, whose historical world tended to be structured in neat, bipolar category pairs, would surely have put me in the tradition of Act Psychology with Ach, Brentano, and the South Germans, rather than among those interested in the contents of mind, with Wundt and Fechner. I am interested in structure and in the explication of structural principles, but always as an expression of some function and in the light of criteria derived from an assessment of function.

I realized fully one evening in the company of the anthropologist Alfred Kroeber that, like him, I believed in the “supraorganic” status of culture, definable independently of individuals who “represented” it in various roles—what Karl Popper today calls “World Three.” I see culture as a product of evolution, which, once emerged, changes the biological status of man irreversibly and makes the application of Darwinian principles false and misleading. Biological factors constrain what culture can be, but culture has laws of its own. In this respect, I have always found myself sympathetic to such diverse “culturalisms” as Vygotsky’s Marxism and De Saussure’s structuralism.

For all my theoretical interests, I am (as the record must surely show) attracted to action. That, too, must express my functionalist bias. I have never been, for more than a few months at a time, without a cause of some practical sort—from devising science and mathematics curricula for children in developing African nations, to trying to increase the use of human potential in a giant technical corporation (General Electric), and even to giving advice on how to turn the rich coffers of Time-Life Inc. to the publishing of good science and humanities, etc., etc. I have also done my turn, as so many have, on panels in Washington, in the Presidency of APA and several of its Divisions. Certainly, my involvement must derive in large measure from an interest in power and influence, which I take to be ordinary enough. I have never been much involved in electoral politics, except for the time when I helped John Kennedy run a poll of our 11th Congressional District of Massachusetts his first time out and even that was more out of friendship than political interest. Though the exercise of power as a phenomenon has always interested me, I have little taste for doing much more than advising about it. I have never wanted to enter politics or “go into government.”

Neither have I ever hankered after a “pure” research job. I enjoy teaching, enjoy narcissistically the “hooking” of others on problems that move me. I have had a dazzling array of graduate students, and many of them do all manner of distin-
guished work and have come through the experience intact enough to hold distinguished chairs from St. Andrews through Princeton to Macquarie. It is not very difficult to have such students if you teach at places like Harvard and Oxford with an occasional summer session at Berkeley! I even like teaching undergraduates and must have lectured to thousands of them over the years. It has its function, lecturing, but it is not a very deep one pedagogically. I have worked extremely hard at preparing lectures all these years. Was it the best way to expend pedagogical energies? Like others of my kind, I have delighted in having postdoctoral fellows: You get all the benefits of collaboration after others have done the lowly work of preparing a collaborator for you! I have hugely benefited from their presence, as a perusal of my bibliography will show.

I have greatly enjoyed collaboration. My collaborators have been my major tutors: Leo Postman, Robert White, George Miller, David Krench, George Klein, Renato Tagiuri, Jackie Goodnow, Patricia Greenfield, David Olson, Norman Mackworth. I have also (or mostly) enjoyed supervising doctoral theses. I cannot keep sufficiently dispassionate to avoid battles of will from time to time, which pain me (and pain my suffering student far worse). I am rather "social" in most aspects of my psychological work—although I grow less so with age, I suspect.

One part of my intellectual life has, however, always been highly introverted and solo. I published a book of essays in the early 1960s entitled Essays for the Left Hand. When I am "playing" near the source, in literature and the arts, trying to turn intuition into something more communicative and ponderable, I become very much the loner. My left hand goes through unpredictable cycles, probably responding to what it can pick up in the "rag-and-bone shop of the heart."

Do I feel I've been "successful"? Yes, in some conventional way. I've earned the usual honors and medals and have a boxful of honorary degrees. But in some curious, rather distressing way, the extrinsic honors seem never to connect with the parts of my mind with which I think about problems, do research, teach, or get involved in public matters. The ceremonies of award always seem to be for a character who is not a member of my cast. So the question needs to be asked in some other form. I do not feel as if my work has brought off any profound revolution either in my own thinking or in the world of scholarship or human affairs. In certain crucial respects, I feel I have failed. I had hoped that psychology would remain a unity, would not become fragmented into rather incommunicative subspecialties. It has. I had hoped it would find a way of bridging the gap between the sciences and the humanities. It has not. I had hoped it could guide social or public policy to a better formulation of democracy. Perhaps that was my misreading of the nature of politics.

In one respect, I have been extraordinarily well provided for—in my friends, inside psychology and out:

Think where man's glory most begins and ends
And say my glory was I had such friends.

But having said that much, I must add a perspective toward the future. I think that the work on which I and my friends are now engaged, on the acquisition of language and on the manner in which language serves communicative functions, I think this work is beginning to take hold. It is concerned with many of the issues about which I expressed concern a moment ago—with intention, with culture, with the relation of structure and function. Perhaps a link with linguistics will even provide an arterial connection with the humanities. Psychology is divided and cut into pieces, but there are some fragments around that may regenerate a new and more interesting whole. In any case, the prospect is lively enough to keep me at the eighty-hour week with relish!

1977
Selected Publications by Jerome S. Bruner


[This volume of representative articles also contains a complete bibliography up to 1973.]
From communication to language. Cognition. 1975, 3, 155–267. (a)
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