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Radical Constructivism:
A Way of Knowing and Learning

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Chapter 1

Growing up Constructivist: Languages and Thoughtful People

What is radical constructivism? It is an unconventional approach to the problems of knowledge and knowing. It starts from the assumption that knowledge, no matter how it be defined, is in the heads of persons, and that the thinking subject has no alternative but to construct what he or she knows on the basis of his or her own experience. What we make of experience constitutes the only world we consciously live in. It can be sorted into many kinds, such as things, self, others, and so on. But all kinds of experience are essentially subjective, and though I may find reasons to believe that my experience may not be unlike yours, I have no way of knowing that it is the same. The experience and interpretation of language are no exception.

Taken seriously, this is a profoundly shocking view. Some critics say that the emphasis on subjectivity is tantamount to solipsism (the view that nothing exists outside peoples' heads), because, they seem to think, it implies that individuals are free to construct whatever realities they like; others claim that the constructivist approach is absurd, because it disregards the role of society and social interaction in the development of an individual's knowledge. Both objections are unwarranted, and the later sections of this book will present formal arguments to demonstrate it.

I have mentioned the feature of subjectivity here at the outset, because I believe that the best way of providing an introduction to radical constructivism is to tell how I, as an individual subject, came to embrace it as a general orientation.

The beginning of this story, inevitably, has to do with my life and the roots of my dissatisfaction with traditional theories of knowledge. It will be a chronicle of gathering ideas from people I met and authors I read, none of whom, I suspect, would wholly agree with how I interpreted them and built up my model. Hence I want to preface my account with two explicit warnings.

The first is that everything expressed in this book is simply this author's view. It is an attempt to explain a way of thinking and makes no claim to describe an independent reality. That is why I prefer to call it an approach to or a theory of *knowing*. Though I have used them in the past, I now try to avoid the terms 'epistemology' or 'theory of knowledge' for constructivism, because they tend to imply the traditional scenario according to which novice subjects are born into a ready-made world, which they must try to discover and 'represent' to themselves. From the constructivist point of view, the subject cannot transcend the limits of individual experience. This condition, however, by no means eliminates the influence and the shaping effects of social interaction.

The second warning concerns my memories and the act of remembering generally. As the Italian philosopher Giambattista Vico (1744–1961) remarked, we cannot reconstruct the past exactly as it was, because we cannot avoid framing and understanding our recollections in terms of the concepts we have at present. Independently, two centuries later, Jean Piaget came to the same conclusion (1968). The story I am going to tell of my journey to constructivism, therefore, is the story as I see it now.

Which Language Tells It 'as It Is'?

Problems with the notion of reality cropped up early in my life because I grew up with more than one language. My parents were Austrians, and at home they normally spoke German. But up to the end of the first World War my father had been in the diplomatic service and he and my mother got very used to speaking English. When I was little, they would switch to English whenever they wanted to speak of things they thought unsuitable for a child — and there were more things of that kind then than now. For me, of course, this was a powerful incentive to get into that secret language as quickly as possible, and when I started repeating words and phrases I picked up from them, they could not resist correcting my imitations and helping me to learn English. As a result, I felt pretty much at home in both languages by the time I was about six years old.

When Czechoslovakia was created as an independent state after the first World War, my father, whose original home and property were in Prague, automatically became a Czech citizen (and so did my mother and I). He no longer could nor wanted to be an Austrian diplomat. He devoted himself to photography and settled in the South Tyrol, the part of Austria that had become Italy after 1918. There I occasionally played with Italian children and the elementary school I was eventually sent to, was half German, half Italian. Without trouble and almost without noticing it, I learned a third language. Because my mother was a great skier and mostly took me along into the mountains and when she competed in races, I effortlessly learned to ski and it became very much part of my early life.

At the age of ten, I was sent to Zuoz, a very international boarding school in Switzerland, where I got daily practice in all three languages. For the next eight years I was also taught French. Slowly I began to realize that learning a language in a school room was a different thing from learning a language in your every-day environment. The teacher explained what the

French words meant in the language we all knew (which happened to be German). He showed us how to pronounce them and explained the rules of grammar we needed to make sentences. Then we read French texts and learned to translate them. How we understood the words and the texts, therefore, was largely in terms of experiences we had had elsewhere. Thus the French we learned was grafted on the language we had grown up with. This was quite different from growing into a language by interacting with people who live in it day in and day out.

When we started to read Balzac and Maupassant and Anatole France, it dawned on us — perhaps because our teacher was a master at circumscribing what could not be translated — that to get into another language required something beyond merely learning a different vocabulary and a different grammar. It required another way of seeing, feeling, and ultimately another way of conceptualizing experience.

This was no more than a dim notion then, but it persisted because, after I had graduated from high school, it helped to make the multilingual world in which I lived a good deal more intelligible and congenial. Ingenuously and certainly without formulating it, I had stumbled on a way of thinking which, as I discovered some twenty years later, was the core of the well-known Sapir-Whorf hypothesis. Put in the simplest way, this hypothesis states that how people see and speak of their world is to a large extent determined by their mother tongue (Whorf, 1956). In retrospect, I think, it was my first-hand experience of this phenomenon that prompted my interest in epistemology. If language had something to do with the structure of my experience and therefore to some extent with the world that I considered to be real, I could not for long avoid asking the question, what the *real* reality behind my languages might be like and how one could know and describe it.

The Wrong Time in Vienna

From high school I went on to study mathematics, which I had always liked — perhaps because it seemed the only subject that did not depend on a natural language. I entered Zürich University, but after one semester my father told me that Swiss Francs were no longer available and if I wanted to continue my studies it would have to be in Vienna. I was not enthusiastic about this move, but I went there in the autumn of 1936. The Austrian Nazi movement, although officially forbidden, was making itself felt everywhere, including the corridors and lecture halls of the universities. It was a depressing atmosphere, and when, before the end of the second semester, I was offered the opportunity of a winter in Australia as ski instructor, I jumped at it.

As it turned out, this was to be the end of my academic education. But Vienna had introduced me to two authors that influenced me profoundly: Freud and Wittgenstein. Freud's work (especially his *Interpretation of Dreams*, 8th edition, 1930) suggested that one could try to devise a rational model of the workings of individual minds; and his method required that the analysis of what individuals had unconsciously implemented in their own minds always had to be brought forth by the individuals themselves. (Freud himself seems to have forgotten this principle in some later writings and many professional psychoanalysts disregarded it altogether.)

Wittgenstein's *Tractatus* (2nd printing, 1933) captivated me above all because of the elegant neatness of his exposition. It seemed convincing, even if I did not altogether understand it. During the years that followed I reread the book several times, and one day woke from the spell, when I came to proposition 2.223:

> In order to discover whether the picture is true or false we must compare it with reality. (Wittgenstein, 1933)

It suddenly struck me that this comparison was not possible. In order to make it, one needed to have direct access to a reality that lay beyond one's experience and remained untouched by one's 'pictures' and their linguistic formulations. I felt that there were things one could say and believe to be true in one language, and yet one could not translate them into another. There seemed to be no way of showing their truth outside the context of experience in the particular language. I put away the *Tractatus*, and began to look elsewhere. (Many years later, when I read it once more, I realized that there was much in it that I had not understood.)

Growing Roots in Dublin

A few months before Hitler started the war, Isabel, my wife, and I were in Dublin. She was British, and when my Czech passport expired, I was able to obtain a 'stateless' alien's permit. This allowed us to stay. It did not make me eligible for a regular job, but I could do freelance work or work on a farm. With a friend I had made in Dublin we invested what money we had in a small farm in County Wicklow. He had been trained as a farmer and I was fit enough to do field work. Walking behind horses with a plough or a harrow was wonderful work for me: most of the day it leaves you free to think.

After Wittgenstein, I had read Joad's *Guide to Philosophy* (1936) and some Bertrand Russell, and there was lots to think about. And then I had the good fortune to make friends with Gordon Glenavy and Ned Sheehy, two amateur philosophers who had studied Berkeley and interpreted him in a way that made very good sense to me.

Berkeley's famous dictum *esse est percipi* has usually been taken as an ontological statement, i.e., a statement about the nature of reality. According to this view, he was saying that being perceived generates the existence of things. If this had been his intention, the many quips made by his critics would be quite justified and one could conclude that it was indeed absurd to hold that a tree in the forest falls and makes a noise only if someone sees and hears it fall. But there are reasons to believe that he did not intend to say this. First of all, he wrote the Latin phrase at the very beginning (paragraph 3 of 156) of a treatise to which he gave the title: *Of the Principles of Human Knowledge* (1710). He was not a sloppy author who picked his titles thoughtlessly. If he chose this one, he intended to write about human knowledge, not about ontology. Second, he explicitly laid out what he meant by the Latin slogan:

> The table I write on I say exists, that is, I see and feel it; and if I were out of my study I should say it existed — meaning thereby that if I was in my study I might perceive it, or that some other spirit actually does perceive it. (Berkeley, 1710)

And Berkeley adds a general explanation:

> There was an odour, that is, it was smelt; there was a sound, that is, it was heard; a colour or figure, and it was perceived by sight or touch. This is all that I can understand by these and the like expressions. For as to what is said of the absolute existence of unthinking things without any relation to their being perceived, that seems perfectly unintelligible. (ibid., Part I, par.3)

He is, in fact, defining the way he, Berkeley, wants to use the words *esse* (to be), 'to exist', and 'existence', when he is concerned with human knowledge. He also asserts that, for him, the term 'existence' has no intelligible meaning beyond the domain of experience.

His ontology is a different matter. He was a believing Christian (so much so that he became a bishop) and he therefore based his ontology on revelation, not on rational knowledge. To make it jibe with his theory of knowing, he added a mystical detail: because God perceives all things all the time, their permanence is assured. But this permanence belongs to the domain of metaphysics, not to the study of rational human knowledge. (There is a great deal more I gathered from Berkeley throughout the years and his name will crop up in later sections of this book.)

In 1939 *Finnegans Wake* was published and, although Joyce had lived in self-imposed exile for about two decades, the event was celebrated like no other in Dublin's intellectual circles. An informal group was formed of people who knew other languages, to try and unravel some of the countless puns that make up Joyce's extraordinary text. The group lasted through two meetings during which we covered the first three pages, but then our enthusiasm as well as the supply of Irish Whiskey dried up. In the opening lines of *Finnegans Wake*, however, there is the first of many oblique references to Vico, a name I had never heard before. I was told that Giambattista Vico was an eighteenth-century philosopher in Naples. If Joyce had chosen him as one of the threads in his work, I thought, he must be worth reading. I discovered that the Public Library in Dublin had an early Italian edition of Vico's *Principi di scienza nuova* (1744), and the next few times I could steal away from the farm, I went there to read it.

Vico's notions that we can rationally know only what we ourselves have made, and that the knowledge of poets and myth-makers is of a different kind, fitted well between some of the disconnected ideas in my head. Only very much later did I come to read Vico's treatise on epistemology (1710), which, as far as I know, is a first explicit formulation of constructivism (see Chapter 2).

Interdisciplinary Education

Shortly after the war ended, my farming friend fell in love with a British visitor and wanted to follow her to England. Isabel and I had loved Dublin and the life on the land, but the desire to return to a drier, sunnier climate had grown as we developed the first symptoms of rheumatism. We sold the farm, and after I had acquired Irish citizenship, I once again had a valid passport and was free to travel. We managed to start up my old car that had been mouldering in a shed for six years, packed our books and our two-year old daughter, and left for Paris, Switzerland, and eventually Merano, my former home in Northern Italy. Though we had planned to return to Ireland, we postponed it when, by a fortunate accident, I met Silvio Ceccato. This meeting, more than any other event, determined the future course of my life.

Ceccato had studied music, composed an opera that had been performed, and become intensely interested in the literature on aesthetics. He found no illuminating answers and went on to spend some twenty years reading philosophy. When we met, he had concluded that there was something basically wrong with the traditional approach and that a different way could be found. He had a fairly clear idea what this way would be, and he assembled a group of interested friends to work it out. He called the group 'The Italian Operationist School' and they were then in the process of developing a new theory of semantics. The group comprised a logician, a linguist, a psychologist, a physicist, an engineer, and one of the first computer buffs in Italy.[1] None of them was fluent in anything but Italian, and when Ceccato discovered that I spoke four languages and had congenial interests, he asked me to join the group. Since he had explained that they were trying to reduce all linguistic meaning, not to other words, but to 'mental operations', I was immediately hooked.

The idea to define concepts in terms of operations stemmed from Percy Bridgman, the American Nobel-prize physicist, who had developed it in the context of analysing key-concepts in Einstein's theory of relativity (Bridgman, 1927). Unfortunately, Bridgman's 'Operationalism' was appropriated by the behaviourist movement in psychology and criticized by philosophers on the basis of excerpts that focused on the physical operations of measurement. What Bridgman had said about the mental construction of concepts was generally disregarded. For me, the thesis that words stand for concepts and that definitions should specify the operations one has to carry out to build up these concepts, fitted nicely with Vico's principle of the construction of knowledge.

During the following years, my apprenticeship in Ceccato's group, which met informally two or three times a year for a few days of intensive discussion, taught me to question all conventional ideas and the tacit assumptions in the traditional theories of knowledge. In 1949 Ceccato founded *Methodos*, an international journal on language analysis and logic, and I was asked to translate Italian and German contributions into English.[2] The pay was miserable, but it was a unique opportunity and I was able to earn most of my living as a journalist.

When Ceccato gave me his article for the first issue of *Methodos* to translate into English, I had no idea how difficult this would be. He had written a parody that presented the history of epistemology as a game, not unlike poker, in which the great philosophers of the western world were the players. The goal was to establish a fundamental value, but it was forbidden to agree on it at the outset. Each player, therefore, had to introduce his own choice surreptitiously and, if he was skilful, make it seem necessary and self-evident at the end (Ceccato, 1949).

Today, I cannot read my translation without embarrassment: there were allusions I did not understand and much of the irony passed me by. In time, however, the continuous contact with the journal widened my philosophical horizon and translating was the best possible training in the use of words.

A Close Look at Meanings

In 1955 Colin Cherry invited Ceccato to the third London Symposium on Information Theory and encouraged him to apply the results of his operational analyses to machine translation, a field of research that had recently sprung up. At the time the United States military commands were said to employ a large army of translators to keep up with scientific, technical and other information that was published in Russia, and they hoped machines would help to cut the waiting time.

By then Ceccato was lecturer in philosophy at the University of Milan. It was said that he received the appointment because some of his publications provided arguments against communism. However, when someone realized that the same arguments could be used against any dogma, including that of the Church, students were no longer advised to go to his lectures. The lectureship, however, enabled Ceccato to create the first Centre for Cybernetics within the framework of the University, and a proposal for research on machine translation could be submitted to the US Air Force (Ceccato, 1960). The proposal was accepted and, for the first time, Ceccato was able to pay people to work with him. In 1959 I became a full-time research assistant at the centre.

My first major task was to provide an analysis of the concepts that English and other languages, including Russian, express by means of prepositions (I had two native speakers of Russian to work with). It was a bottomless subject and it occupied me long after the Milan project had come to an end (Glasersfeld, 1965). To begin to see the complexity of the problem, one need only ask, for instance, what conceptual relation, say, the word 'by' indicates in the following expressions:

He opened the box *by* brute force;
She spent an hour *by* the river;
This time we came *by* the fields;
I tried to read her letter *by* moonlight;
Have this ready *by* Friday!
My doctor swears *by* vitamin C.

And there are other relations that had to be distinguished in a detailed analysis, because in each of the languages that concerned us one needed a variety of expressions to translate the English 'by'. Since this is a question of conceptual relations, it demonstrates that different languages determine different conceptualizations.

Working in this area (in which there are countless examples of conceptual discrepancies between nouns, verbs, and adjectives that are given as equivalent in bilingual dictionaries) confirmed my deep feeling that each language entails a conceptually different world. Translation, in the sense of rendering in the second language the identical conceptual structure that was expressed in the first, was impossible, and our conceptual analyses demonstrated why.

Of course, there is a great deal of practical overlap because the differences are often very subtle and seem irrelevant in ordinary experiential situations. What we call communication works well enough whether an English girl says 'I like that boy', or an Italian '*questo ragazzo mi piace*' — it does not seem to matter that the one expression assigns the active role to the girl, the other to the boy. But it does show that the speakers' worlds are conceptualized differently.

The American Connection

After Ceccato's project had come to an end, another US Air Force research office became interested in the type of conceptual analysis we had been doing and decided to finance a more modest effort which I directed at the Milan Institute for Documentation. They desperately needed translators and shared the hope that computers would soon be able to help with that job. Our project monitor was Rowena Swanson, and it was she who first brought me into contact with Warren McCulloch, Heinz von Foerster, and Gordon Pask, the leaders in the new field of cybernetics. Though Dr Swanson herself was not a scientist by training, she had the most remarkable understanding of the process of scientific research and the value of interdisciplinary connections. Her office became something of a clearing house for novel ideas and her policy of bringing together the people whose work she sponsored provided invaluable stimulation to everyone concerned.

In the following two years Piero Pisani, Jehane Barton, and I worked out a novel approach to the analysis of the meaning of sentences by computer (Glasersfeld and Barton Burns, 1962). Because the large machines in those days were few and rarely accessible, we represented our system on some 120 square feet of plywood on the wall of our office and simulated the computer's basic operations (reading, comparing, and writing symbols) by moving an army of drawing pins by hand. It was an incredibly slow procedure but had the advantage of making immediately visible any inconsistencies or bugs in the programme we were designing.

Then, in 1965, the Washington office told us that there would be no more money for our kind of research in Italy, but they would continue to finance us if we came to the United States. It was a difficult choice. None of my colleagues had been to America, nor had they ever considered leaving Europe. But we did want to go on with the project, and in the end we decided to make the jump. We arrived in Athens, Georgia, towards the end of 1966.

In the spring of 1969, Isabel, with whom I had shared life for more than three decades, died of an embolism, suddenly, without the slightest warning. It took me several days to grasp that the world I had taken for granted was gone. I began to realize that the notion that we construct our reality is not just an intellectual gambit. The reality we had built and sustained together was falling apart without her. The only thing that kept me going during the year that followed was work. And then Charlotte came along, agreed to marry me, and bit by bit we set out to build a new world.

And then there was another unexpected break with the past. At the end of 1969, Mr Nixon wiped out the Air Force Office that had been sponsoring us as well as some twenty other research projects in computer science and communication. It was then that, like the Good Fairy, the University of Georgia, where we had a contract with the Computer Centre, stepped in and adopted all of us. Brian Dutton, who had replaced Jehane Barton and whose Ph.D was actually in medieval Spanish poetry, slipped into the Romance Languages Department; Piero Pisani was snapped up by the Computer Center; and I was taken in by the Department of Psychology. So began a never contemplated life in academia.

Introduction to Psychology

Two members of the Department, who differed from their largely behaviourist colleagues, had some sympathy for my ideas about language and most generously helped me by letting me sit in on their courses. One was Bob Pollack, who just then (1969) had edited a book of Alfred Binet's research papers that showed the French author as a psychologist of far greater depth than the 'Binet Scale of Intelligence' might lead one to believe; the other was Charles Smock, a developmentalist who had been trained for some time in Geneva.

To keep up with Pollack's course, I had to read a lot on perception, because this was his specialty. As I had never thought about the mechanics of seeing, I learned a lot about the models current in psychology. On the one hand, I found them fascinating because of the ingenious experiments that provided the data with which the models could be 'confirmed'. On the other, I was amazed at the general lack of epistemological considerations. What the eye sees — light, colour, and shape — was usually taken for granted as a physical given, and the research focused on the sensory mechanisms that could convey a presumed reality to the brain. No one seemed to doubt the assumption that Wittgenstein had expressed so succinctly in his proposition 2.223 (see above). The aim of the experiments was always to discover how the eye manages to see what is there, as though to perceive were simply to *receive* something that exists ready-made. The naive metaphor of the photographic camera seemed to dominate the field, in spite of the fact that the scene in front of a camera, as well as the picture that comes out of it, are obviously a product of the very perceptual processes they were studying.

I did, however, come across one spectacular exception: the perceptual oddity experts occasionally referred to as the 'Cocktail Party Effect'. This is a phenomenon we all are familiar with, without having studied psychology. It can happen anywhere. You have been buttonholed by someone who is telling a boring story. Suddenly you become aware of a much more interesting conversation that is going on behind you. You don't want to offend the bore, so you follow what he says, but just enough to be able to make an encouraging noise whenever he pauses to catch his breath. The main part of your attention is on what is being said behind you. This means that you are able to switch your attention at will to different points in your auditory field. It is not a question of one stimulus being more 'salient' than another, because the speech of your boring companion is louder and clearer than the dim conversation of the people you don't see. It is obviously a question of your subjective interest.

I was fascinated by this and discovered that famous experimental psychologists, such as Donald Hebb, Karl Lashley, Wolfgang Köhler, and the Russians Zinchenko and Vergiles, had independently noticed and experimentally demonstrated the same phenomenon in the visual field. It struck me as a truly revolutionary fact, yet none of the psychology textbooks that I came to see during the following years mentioned it. I shall return to the mobility of attention later (see Chapter 9), but here is how Köhler (1951) described one of his results:

> When two objects are given simultaneously in different places while the eyes do not move, we can compare these objects, and say whether they have the same shape. (Köhler, 1951, p.96)

In other words, we can see objects that are in different parts of our visual field and, *without moving our eyes*, compare one with the other. Our attention obviously has the power to move within the visual field just as it can move among and select from speech sounds that arrive at the same time. Indeed, in the visual field we do this quite often, for instance when our eyes are fixed on the computer screen and at the same time we notice that someone we know has walked past the window beside our desk.

For me the realization of this capability was an enormous encouragement to pursue the search for the active element in the perceiver and, ultimately, the builder of knowledge.

Collaboration with a Chimpanzee

That the University of Georgia adopted me as a psychologist was due to the fact that my interest and work in computational linguistics happened to fit into an empty slot in that department. Once more a lucky coincidence with this move led me into a venture that was as fascinating as it was unexpected. Ray Carpenter, one of the leading primatologists in the United States had joint appointments at the University of Georgia and the Yerkes Primate Research Center in Atlanta. Just at that time, the first reports on the chimpanzee Washoe, who was learning sign language, had been published by the Gardners at the University of Nevada. The discussion about whether or not a chimpanzee could acquire a language became heated and spread far beyond the specialized journals. The Yerkes Institute wanted to join the fray and planned to set up experiments that could provide more rigorous tests than the subjective evaluation of fleeting exchanges of hand signs. Ray Carpenter promoted the idea of creating a communication system consisting of keyboards and a computer that could record all interactions. When he heard of our work in computerized language analysis, he asked me whether I would be interested in designing the language and computer components of the system planned at the Yerkes Center. I talked it over with my computer colleague Piero Pisani, and we decided to go ahead.

I designed the 'Yerkish' language, using geometric designs as symbols for words (concepts) and a simplified but very strict grammar to govern their formation into sentences. By sequentially pressing keys of the key boards, code signals standing for words were sent to the computer, which contained the vocabulary, the grammar, our system for checking the correctness of sentences, and the rules for responding to some two dozen requests the chimpanzee Lana was to formulate in Yerkish word symbols. Pisani achieved the miracle of fitting all this into the minute memory of a PDP computer (Glasersfeld, 1977; Pisani, 1977).[3]

For six years we worked with the primatologists and the technicians of the Yerkes Center and the accomplishments of the chimpanzee Lana caught the attention of the press and TV. It was indeed absorbing and enjoyable work. But then there came a point when we resigned from the project, because irreconcilable differences had arisen regarding the direction of the research which remained firmly embedded in the behaviourist tradition. Nevertheless, I have no doubt that I owe whatever reputation I have gained in the field of psychology to the remarkable talents of Lana.

My background in conceptual analysis, however, bore another fruit. Michael Tomasello, one of the students whose master's thesis and dissertation I had the pleasure of directing, undertook the gigantic task of recording and analysing, together with his wife, all the first linguistic manifestations of their daughter during the second year of her life. To my knowledge, it constitutes the only complete database of early language acquisition, and it has proven a gold mine for the development and testing of theories in that area. It was an invaluable opportunity to see just how useful Ceccato's approach to the construction of concepts that we had further developed and expanded in our computer procedures would be in the analysis of children's conceptual development. Tomasello's recent book on a central topic of language acquisition, *First Verbs: A case study of early grammatical development* (1992), may well turn out to be a landmark.

Discovering Piaget

To the late Charles Smock, whom I remember with much affection, I owe my introduction to the work of Jean Piaget. It seems ironic that I should have had to come to America and to lose my research job in order to be introduced to the author who was to influence my later thinking more than any other. One evening, in one of the many long talks I had with Charles about language and epistemology, he said: 'It's funny, quite a few of the things you say I have heard from Piaget.' So I began reading Piaget — and since Charles had a large collection of texts he had acquired in Geneva, I read Piaget in French.

In the years that followed, when I came to teach courses on Piaget to students who could read only English, I realized how difficult, if not impossible, it is to understand the Piagetian orientation from translations. With very few exceptions (e.g., Wolfe Mays or Eleanor Duckworth) the translators seem to have a naive (i.e., naive realist) theory of knowledge and unconsciously bend what they read in Piaget's original texts to fit their own view of the world. As they cannot do this with everything, their translations often convey ideas that are incompatible with Piaget's theory or are downright incomprehensible.[4]

One example that might be of interest to mathematics educators is the translation of Piaget's book *The Child's Conception of Number* (1952a). There is first of all the inexplicable omission, on the cover and elsewhere, of the second author's name, Professor Alina Szeminska (Warsaw University), of whom Piaget says in his foreword that it was *her* talent that made possible the development of the particular methods of analysis used in the work. Then there is the recurrent mistranslation of individual terms, e.g., 'graduation' instead of gradation, 'equating' instead of equalizing. Most serious, given the topic of number, is the translators' indiscriminate use of the word 'set' for the French expressions for collection, quantity, row, sequence, series, and others. Small wonder that English-speaking mathematicians who read the translation thought: Who is this clown who writes about number when he doesn't even know what a set is!

The reader's understanding is further sabotaged by the translators' frequent omission of explanatory phrases and sometimes whole paragraphs. The unacceptable translation of this and other volumes provided part of my motivation for trying to present Piaget's thought to English students in a less distorted fashion. The correction of mistranslations, however, was not my primary goal. Having to teach Piaget from English textbooks, my main objective was to correct some of the basic misunderstandings concerning the nature of the constructivism that forms the backbone of his 'genetic epistemology'.

From Mental Operations to the Construction of Reality

Piaget was not the first to suggest that we construct our concepts and our picture of the world we live in, but no thinker before him had taken a developmental approach. To someone who is driven to ask about the source and validity of knowledge by circumstances of experience (in my case, the plurality of languages), it seems obvious that the best and perhaps only way to find out how knowledge is built up, would be to investigate how children do it. For traditional philosophers, of course, this would be committing an unforgivable sin, because to justify knowledge through its development rather than by a timeless logic, is what they call a 'genetic fallacy'. But Piaget was not a traditional philosopher.

In his *'La Construction du Réel chezl 'enfant* (Construction of Reality in the Child) (1937/1954) he presented a model of how a basic scaffolding — the conceptual structure of objects, space, time, and causality — can be built up. It serves as the framework within which a coherent experiential reality can be constructed. But this construction is not free, it is inevitably constrained and limited by the concepts that constitute the scaffolding. This is one of many points of overlap with the *A Theory of Personality: Psychology of Personal Constructs* of George Kelly (1963), who expressed the idea in the most general way:

> To the living creature, then, the universe is real, but it is not inexorable unless he chooses to construe it that way. (Kelly, 1963, p.8)

Piaget himself explained what he intended by his genetic epistemology:

> So, in sum, genetic epistemology deals with both the formation and the meaning of knowledge. We can formulate our problem in the following terms: by what means does the human mind go from a state of less sufficient knowledge to a state of higher knowledge? The decision of what is lower or less adequate knowledge, and what is higher knowledge, has of course formal and normative aspects. It is not up to psychologists to determine whether or not a certain state of knowledge is superior to another state. That decision is one for logicians or for specialists within a given realm of science. (Piaget, 1970a, pp.11–12)[5]

Having been prepared by Vico, Berkeley, Wittgenstein, and Ceccato, I read the quoted passage as a natural extension of a statement Piaget has repeated in many places, namely that knowledge is *not* a picture of the real world.

When he confronts 'less sufficient' or 'less adequate' with 'higher' knowledge, he is in fact saying that the *meaning* or value of knowledge lies in its function; and when he says that the adequacy of knowledge must be evaluated by logicians or scientists, he is simply explaining that it must be tested for logical consistency (non-contradictoriness) and for experiential validity (e.g., in experiments).

People who are tethered to traditional epistemology seem to be able to read this passage without being shaken in their belief that the better knowledge gets, the better it must represent an ontological reality. Hence, in their writings about Piaget, be they followers or critics, they disregard that, having started as a biologist, he saw cognition as an instrument of adaptation, as a tool for fitting ourselves into the world of our experience.

Because 'adaptation' and 'adapted' are frequently misunderstood (see Chapter 2) and 'adequate' tends to be interpreted as utilitarian, I adopted the biologists' term 'viability'. Actions, concepts, and conceptual operations are viable if they fit the purposive or descriptive contexts in which we use them. Thus, in the constructivist way of thinking, the concept of viability in the domain of experience, takes the place of the traditional philosopher's concept of Truth, that was to indicate a 'correct' representation of reality. This substitution, of course, does not affect the everyday concept of truth, which entails the faithful repetition or description of a prior *experience*.

For believers in representation, the radical change of the concept of knowledge and its relation to reality, is a tremendous shock. They immediately assume that giving up the representational view is tantamount to denying reality, which would indeed be a foolish thing to do. The world of our experience, after all, is hardly ever quite as we would like it to be. But this does not preclude that we ourselves have constructed our knowledge of it.

Radical constructivism, as I said at the beginning, is a way of thinking about knowledge and the act of knowing.

Because of its breach with the philosophical tradition it was (and still is in many quarters) quite unpopular. When I first submitted papers to journals, I received innumerable rejection slips from editors, one of whom stated his objection with endearing clarity: 'Your paper would be unsuitable for our readers.'

Having come from Europe and without background in psychology, it took me some time to discover what the problem was. In 1967 and for the decade that followed, the intellectual climate that pervaded departments of psychology and linguistics in the United States was largely dominated by behaviourism. As late as 1977, Skinner reiterated: 'The variables of which human behaviour is a function lie in the environment' (Skinner, 1977, p.1). If one believed in that kind of determinism, there could be no room for theories of mental construction. However, the belief in environmental determinism would make sense only if one had access to an objective environment, so that one could show that a particular piece of that environment causes a particular behaviour. But what a scientist — or indeed any reflective person — categorizes as his or her environment and then causally relates to observed behaviour, is always a part of that observer's domain of experience and not an independent external world.

A Decisive Friendship

The constructivist way of thinking on which I had been launched by Ceccato and Piaget obviously had no chance of making inroads upon the established dogma of the disciplines to which, I felt, it might have something to say. If Piaget himself had not succeeded in being taken seriously as a philosopher, it was clear that an obscure outsider could get nowhere. In linguistics, the work of Noam Chomsky had brilliantly turned the tables on the behaviourists. In doing so, however, he had posited the fundamental elements of language as innate, and this assumption closed the door to the constructivist approach. Psychology still bracketed the mind and proudly declared itself 'the science of behaviour'. In their textbooks, students were warned against the futility of philosophizing. Empiricism (which was usually understood as realism) was the password — and the one thing I did not have was empirical data to show the usefulness of the constructivist approach.

It was a great stroke of fortune that Charles Smock brought me together with Leslie Steffe. Once more a meeting profoundly influenced my life and work. Steffe ran a Piagetian research project in the Department of Mathematics Education at the University of Georgia. When we started talking about cognitive development and conceptual analysis, we at once discovered a vast area of agreement. For me, this was not only an encouragement but it came to form the basis of a collaboration that has been a truly wonderful experience ever since.

Whenever I think of this collaboration, I remember the passage in which the Nobel laureate Sir Peter Medawar demolished the popular image of the scientist 'as a regular, straightforward, plain-thinking man of facts and calculations'. Rather, he wrote:

> A scientist commands a dozen stratagems of inquiry in his approximation to the truth, and of course he has his way of going about things and more or less of the quality often described as 'professionalism' — an address that includes an ability to get on with things, abetted by a sanguine expectation of success and that ability to *imagine* what the truth might be which Shelley believed to be cognate with a poet's imagination. (Medawar, 1984, pp.17–18)

Substitute 'viable explanation' for the word 'truth', and you have what to me is a perfect portrait of Les Steffe. In the course of the innumerable fierce arguments we had throughout the years, we gradually expanded and refined each other's thinking, struggling at times for days to formulate our ideas so that they might become acceptable to both of us. There was also John Richards, a trained philosopher, who for long stretches participated in the battle; and Paul Cobb and Patrick Thompson, both graduate students of Steffe's, lived through the gruelling months of discussion that helped to forge a plausible model of what children might be doing on their way towards a concept of number and the basic operations of arithmetic (Steffe *et al.*, Steffe, Richards and Glasersfeld, 1978; Steffe, Thompson and Richards, 1982; Steffe, Glasersfeld, Richards, and Cobb, 1983).

Though I knew nothing about research in education and remembered little from the few semesters I had studied mathematics, there had always been in the back of my mind the idea that conceptual analysis would sooner or later have to deal with mathematical concepts. For a constructivist it was obviously impossible to think of numbers and geometrical forms as God-given. Nor could one accept the Platonic view of pure forms that float about as crystals in some mystical realm beyond experience. One would have to investigate their genesis as abstract entities in an experiential domain.

Mathematicians, from Euclid down to our time, are extremely noncommittal about the make-up of their basic concepts. Numbers are the raw material of their abstract edifices, and much the way bricklayers do with bricks, they take them for granted.[6] Only they themselves could throw some light on how they arrived at their elementary concepts, but given their competence, the question obviously seemed trivial to them.

The philosophers I read, though some of them said quite clearly that number is 'a thing of the mind' (see Chapter 9), were no help either, because they did not explain how this mental entity could be produced. The one exception I found was Edmund Husserl, the founder of phenomenology, who proposed that the operation that forms discrete unitary objects in our perceptual field is essentially the same that underlies the concept of 'one' and, on a subsequent level of abstraction, enables us to comprise any collection of such ones in a discrete unitary entity that we call 'number' (Husserl, 1887, pp.157–68). This idea was certainly helpful and it fitted well into Piaget's theory of reflective abstraction. What was needed to substantiate it, was a domain of experience where such abstractions were likely to be made. Observing children was the answer.

Teaching Experiments

When I started working with Leslie Steffe, he was already far into the development of a method he called 'teaching experiment'. It was a hybrid of Piaget's 'clinical method' of interviewing children and educational research. Its goal was to establish a viable model of their constructive activities in the context of arithmetic. Steffe's approach differed in that its purpose was to create situations that would allow the investigator to observe children at work and make inferences as to how they build up specific mathematical concepts. If these concepts were abstractions from reflection rather than from sensorimotor experiences, it was clear from the outset that whatever inferences could be made about them would contain an element of conjecture. In time, however, the resulting hypothetical model achieves a high degree of plausibility and predictive usefulness.

> As with all general theoretical constructs, it is difficult to apply them to specific situations, when the cognizing subject is not ourselves but a 'subject' we are observing. In practice there may be observable behavioral indications, on the basis of which levels of abstraction can be determined, but making that determination is not simple. One might say that assuming something as 'given' or not is exclusively the subject's business. Hence, at best an observer can make educated guesses, taking into account — as does any experienced diagnostician — several indications collected over an extended period of observation. (Steffe and Glasersfeld, 1988, pp.18–19)

In the teaching experiments, there is no teaching in the conventional sense, and no curriculum. Yet many of the situations presented to the children contained arithmetical problems they might have encountered in school. What mattered, however, was not the solution but the children's untutored individual approach or, as Steffe says, the children's mathematics. The experiment proceeds, not along a fixed, preconceived plan, but the investigator has to invent it step by step according to what the child says or does. It is videotaped, and the real work takes place when the members of the team review the tapes and discuss them until they can agree on an interpretation.

It is easy to understand that reviewers of research proposals tend to be turned off when they are told that the methodology of the proposed research is the teaching experiment, but what the experiments will consist of cannot be foreseen, because it depends on the reactions of the subjects. We presume it was the quality of the publications emanating from Steffe's group (e.g., Steffe, Glasersfeld, Richards and Cobb, 1983; Steffe and Cobb, 1988) that has assured continued funding.

In the early 1970s, Piaget once more became fashionable in the United States, and this time the focus was on his constructivism rather than on the 'stage theory' that had previously been emphasized. As a result, a great many authors began to profess a constructivist orientation, though they seemed unaware of the principles of Piaget's epistemological position. Especially researchers in mathematics education assimilated the notion that children gradually build up their cognitive structures (hardly a novel idea), but they disregarded the fact that Piaget had changed the concept of knowledge. Consequently, when I was teaching genetic epistemology, I wanted to distinguish my approach from what students might be reading elsewhere about versions of constructivism that seemed trivial. I called the model I had been working on 'radical' and laid out its two basic principles:

- knowledge is not passively received but built up by the cognizing subject;
- the function of cognition is adaptive and serves the organization of the experiential world, not the discovery of ontological reality.[7]

The Spreading of Constructivist Ideas

In January 1978, Heinz von Foerster and Francisco Varela organized a conference in San Francisco that bore the title 'The construction of realities'. It was a closed symposium that brought together some thirty authors and scientists from a variety of disciplines who had in some way documented their belief that knowledge could not be found or discovered ready-made but had to be constructed.

It was a remarkable experience to learn that there were accomplished and widely respected thinkers in biology, sociology, political science, logic, linguistics, anthropology, and psychotherapy, who had in individually quite different ways come to the conclusion that the traditional epistemology could no longer be maintained. But, as so often in meetings of highly original minds, most of the time was spent on arguing about relatively small individual discrepancies, and very little on trying to formulate basic constructivist principles on which, it seemed, most if not all could have agreed.

For me, nevertheless, it was a most encouraging event. It was my only meeting with Gregory Bateson, and to listen to his comments and his wonderfully gentle way of pointing out a contradiction in a speaker's presentation was as important a lesson as the many insights he had provided in his writings.

It was also at this symposium that I met Paul Watzlawick, whose charming little book *How Real is Real?* (1977) I had shortly before used in a course of mine. Apart from being of Austrian origin, we had another thing in common: we had both lived and were still living in several languages. In many ways this liberation from a single mother tongue facilitates an immediate understanding of certain aspects of constructivism that take hard work and reasoning in all whose world view is constrained by a single language. Paul Watzlawick then invited me to write the introductory essay in his *The invented reality* (German edition, 1981; English, 1984), a book that has done more than any other to spread constructivist ideas.

Apart from my immeasurable debt to Leslie Steffe, I owe a great deal to the graduate students who worked with him and to those who took my courses in the Psychology Department. They were a motley lot, because many of them did not come from my department but from philosophy, linguistics, and mathematics education. Compared to one's peers — colleagues on the faculty and researchers who listen to presentations at professional meetings — graduate students are less inhibited. They tend to ask questions when what is said does not make sense to them. Thus they often put their finger on unconscious, unwarranted jumps in the development of ideas and inconsistencies in the presentation. No doubt this is of help to researchers in all disciplines. In the area of constructivism it is downright invaluable for a quite specific reason. I have often said that adopting the constructivist orientation requires the modification of almost all one has thought before. This is laborious and difficult to carry through. We are usually quite unaware of many habitual patterns of thinking in our minds. And there is another obstacle: the language in which our thoughts have to be formulated, be it English, Italian, or any other natural language, has been shaped by the naive realism inherent in the business of practical living and by a few prophets who were convinced to have access to an absolute reality.

For the very reason that radical constructivism entails a *radical* rebuilding of the concepts of knowledge, truth, communication, and understanding, it cannot be assimilated to any traditional epistemology. Above all, it seems enormously difficult to appreciate that it is not an orientation that claims to reveal an ultimate picture of the world. It claims to be no more than a coherent way of thinking that helps to deal with the fundamentally inexplicable world of our experience and, most important perhaps, places the responsibility for actions and thoughts where it belongs: on the individual thinker.

Retirement and a New Beginning

At the 1975 meeting of the Jean Piaget Society in Philadelphia, I for the first time presented the radical interpretation of genetic epistemology to a larger public. As it was a plenary session, there was not much discussion. But it had two important consequences for me. Hermine Sinclair, a long-time collaborator of Piaget, vigorously encouraged me to go on with my work, and it is to her that I owe my first invitation to Geneva. The presentation also led to a long talk with Jack Lochhead who was in the process of starting a Piagetian research group on cognition in the Physics Department of the University of Massachusetts. In the years that followed he invited me several times to give workshops on conceptual analysis, because he and his colleagues were trying to develop a more effective way of teaching physics and the mathematics it requires. Like other educational researchers, they had noticed that many students, were quite able to learn the necessary formulas and apply them to the limited range of textbook and test situations, but when faced with novel problems, they fell short and showed that they were far from having understood the relevant concepts and conceptual relations that constitute the actual framework of physics.

When I retired from the University of Georgia in 1987, Jack Lochhead called and said, why don't you come and work with us? By then his group had been established as an independent institute of the University at Amherst. The research in physics education, the modest title 'Scientific Reasoning Research Institute', the promise of snow and skiing in Massachusetts, and the fact that Charlotte's children would be much closer there, proved irresistible.

Work in the Institute soon made clear to me that teaching physics was not quite the same as teaching arithmetic in the elementary grades. Although the fundamental concepts in both areas are abstract constructs, their use is markedly different. In mathematics, concepts can be combined and related in all the ways the mathematician deems legitimate within the rules he has accepted; and new abstractions from such compounds may yield new levels of operating. If some of the resulting abstract structures are found to be applicable to worldly problems, this may be gratifying to their inventor but it is irrelevant to the pursuit of mathematics. In physics, however, the process of abstraction is doubly constrained. Not only must it comply with logic and be conceptually coherent, but its results must also withstand experimental tests, that is to say, they must fit experiential situations. In short, mathematics is self-sufficient and its goals lie within its own domain. Physics, in contrast, has an instrumental component, in that it has to provide theoretical models that help to organize our experiential world.

Support from Physics and Philosophy of Science

Although the problems that made up the daily work at the Institute were usually concerned with high-school physics and did not involve relativity or quantum theory, I had the opportunity to sit in on colloquia and meetings where such topics were discussed. Much of what I heard there seemed to confirm that it had not been misinterpretation, when years before I had picked a quotation here and there from the writings of the great physicists in our century. They all have at some point made statements that indicate they were aware of inventing or constructing theory rather than having it forced upon them by a collection of data. Werner Heisenberg, for instance, wrote:

> In the natural sciences, then, the object of research is no longer nature as such, but a nature confronted by human questions, and in this sense, here too, man encounters himself. (Heisenberg, 1955, p.18)

For the constructivist, such statements are a welcome corroboration, even if the same physicists, in their day-to-day work adopted a far more realist stance.[8] It is not at all surprising that a problem-solver takes *for real* the problematic experiential situation with which he is struggling. His task is technical, it lies within a specific, circumscribed area of experience, and its solution would not be advanced by epistemological considerations.[9] Only when he has solved it, may he adopt a philosophical attitude and conclude that his solution is an instrument for the organization and 'explanation' of experience rather than a representation of reality.

From the outset, it was clear that, in developing the constructivist approach to knowledge, the philosophy of science was an area that could not be skirted. Among my eclectic readings, one author in particular provided an irresistible challenge.

Karl Popper (1968) gave an excellent description of the instrumentalist view of science and then attempted to show that it was logically flawed. An attentive reader, however, will notice that his refutation is ultimately based on nothing but his metaphysical belief that scientific theories *can* approximate a rightness ('truth') that lies beyond the level of instrumental viability in given situations. The cornerstone of this belief are his notions of falsifiability and crucial tests. He claims that the fact that 'Newton's theory was falsified by crucial experiments which failed to falsify Einstein's' means more than that it broke down under certain experiential circumstances. Instrumentalism, he says, has nothing equivalent to such tests.

> An instrument may break down, to be sure, or it may become outmoded. But it hardly makes sense to say that we submit an instrument to the severest tests we can design in order to reject it if it does not stand up to them: every air frame, for example, can be 'tested to destruction', but this severe test is undertaken not in order to reject every frame when it is destroyed but to obtain information about the frame (i.e. to test a theory about it), so that it may be used *within the limits of its applicability* (or safety).
>
> For instrumental purposes of practical application a theory may continue to be used *even after its refutation*, within the limits of its applicability: an astronomer who believes that Newton's theory has

> turned out to be false will not hesitate to apply its formalism within the limits of its applicability . . .
>
> Instruments, even theories *in so far as they are instruments*, cannot be refuted. The instrumentalist interpretation will therefore be unable to account for real tests, which are attempted refutations, and will not get beyond the assertion that *different theories have different ranges of application*. But then it cannot possibly account for scientific progress. (Popper, 1968, pp.112–13)

On the strength of this, Popper concludes that instrumentalism is an 'obscurantist philosophy' (p.113).

For me, this passage was truly illuminating. Clearly, Popper had fully understood the thrust of instrumentalism. His example of the astronomer was an accurate prediction of how the scientists and engineers of NASA went about their business when they sent a man to the moon: they calculated everything according to Newtonian formulas, because this was far simpler and less time-consuming than using Einstein's, although of course they all knew that Newton's theory of the planetary system was no longer considered to be true.

Yet, because Popper is wedded to the notion of scientific progress, he is compelled to use the term 'refutation' in two different senses. On the one hand, it is the generation of circumstances in which a theory, used as an instrument, does not procure the desired result (or, in my way of speaking, is not viable). On the other hand, it is a crucial experiment that shows the theory to be *false* (where 'false' is interpreted as the opposite of 'true').

This is where a metaphysical belief enters the game. Indeed, Popper himself seems to have been aware that he was fudging. At the end of the chapter that is to justify his belief in the 'growth of knowledge', he writes, albeit in a footnote:

> I admit that there may be a whiff of verificationism here; but this seems to me a case where we have to put up with it, *if we do not want* a whiff of some form of instrumentalism that takes theories to be mere instruments of exploration. (ibid., p.248, my emphasis)

That is the difference. Radical constructivism is uninhibitedly instrumentalist. It replaces the notion of 'truth' (as true representation of an independent reality) with the notion of 'viability' within the subjects' experiential world. Consequently it refuses all metaphysical commitments and claims to be no more than one possible model of thinking about the only world we can come to know, the world we construct as living subjects. Because this is a difficult and shocking change of attitude when one first comes to it, I want to reiterate once more that it would be misguided to ask whether radical constructivism is true or false, for it is intended, not as a metaphysical conjecture, but as a conceptual tool whose value can be gauged only by using it.

Notes

1 They were Giuseppe Vacccarino (logician), Ferruccio Rossi-Landi (linguist), Enzo Morpurgo (psychologist), Vittorio Somenzi (physicist), Enrico Maretti (engineer), Enrico Albani (computer scientist). Their subsequent publications that are relevant to my topic are listed among the references.

2 The journal lived for fifteen years and has recently been revived under the name *Methodologia* by Ceccato's student, Felice Accame.

3 It may seem incredible today, but all the computer memory that was available for the language system was 4 kilobytes.

4 I myself, indeed, have sinned in this respect, because for quite some time I translated Piaget's French *intelligence* as 'intelligence', forgetting that in many contexts it has to be read as 'mind' because that noun is not available in French.

5 Unless otherwise indicated, the translations of quotations from French, Italian, and German texts are mine.

6 The outstanding exception, of course, is the 'intuitionist' mathematician, L.E.J. Brouwer, but I did not become aware of his relevant paper (Brouwer, 1949) until after I had published my 'attentional model' (Glasersfeld, 1981a).

7 Although I had used this definition in lectures and talks, it did not appear in print until 1989, in my piece on constructivism in the *International Encyclopaedia of Education* (1989a), Supplement 1, p.162.

8 I have used similar quotations from Helmholtz, Mach, Einstein, and Bridgman in my papers, and others can be found in the philosophical writings of Bohr, Dirac, Born, and Schrödinger.

9 Note that I am using 'technical' to refer to the technique or method of science, not to machines and technology.