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# Portrait of a "Classical" Cognitive Scientist: What I Have Learned from Jacques Mehler

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It's my opinion that something is happening in, and to, cognitive science. We are being progressively dislodged from the neat, consistent, and exciting scheme in which we had settled for a long time. I shall call this scheme "classical" cognitive science. As I am about to tell, I mostly owe my introduction to, and settlement into, this beautiful classical universe to Jacques Mehler. The reasons why I think the scene is changing, possibly irreversibly, can be briefly summarized through a few examples, in the subdomains I happen to know best.

1. Syntactic theory is under radical reconstruction. The overall grand scheme that had reached its apex, around 1981–82, in the theory of government and binding (GB) is no longer tenable. Minimalism has supplanted it. The problem is that, in my experience at least (but I know I am not alone), minimalism is not directly teachable as such. We *have* to tell the GB story first, let it sink in, and *then* present minimalism as a reinterpretation of many of its central ideas. Does this unteachability of minimalism tell us anything at all? I do not know. But it might. Moreover, some old unsolved problems are now easily solved in the new frame, but other problems that had been solved satisfactorily by GB have turned out to be hard, or intractable, in the new model. Other styles of explanation, long forgotten, are again raising their heads, as competitors to, because suddenly awakened by, minimalism.

2. Lexical semantics is at a standstill. The deep, uniform, and explanatory links between lexical internal structures and syntactic structures, established in the pioneering works of Mark Baker, Dick Carter, Beth Levin, Malka Rappaport, Jane Grimshaw, and others, and culminating

in the remarkable paper by Hale and Keyser, is being questioned at its very roots by Jerry Fodor. It is he, not I, who has chosen as the subtitle of his 1998 book on concepts, *Where Cognitive Science Went Wrong*. In my terminology, Jerry is referring to classical cognitive science. The many brave rebuttals of his radical critique are not persuasive enough to reassert the well-foundedness of the classical approach. The solution—any solution—will have to accommodate many distinguos, and relativizations and partializations. I doubt that with some notable exceptions the field will ever be again as exciting and neat as it still was even five or six years ago.

3. Heuristics and biases, the field so firmly established by Tversky and Kahneman around 1975, is under attack. We are enjoined to go "beyond" heuristics and biases, and pay massive attention to evolutionary psychology. The classical experiments are being redesigned in myriads of variants, sometimes obtaining nonclassical results. Several authors suggest that the case for striking and systematic cognitive illusions has been overstated. I, for one, am unimpressed by these critiques, and vastly underwhelmed by the claims of evolutionary psychology. But it has become unthinkable to teach a course on judgment and decision-making these days without also covering these critiques and the new data and present cheater detectors and simple heuristics that make us smart. And let the audience decide.

4. Innatism is again under attack. The instant popularity with a lot of the high academe of Fiona Cowie's 1999 book *What's Within? Nativism Reconsidered*, and the rebuttals that are being circulated in preprint, bring us squarely back to the debate between Piaget and Chomsky. We are (as Fodor rightly remarks) pulled twenty years backward, as if nothing had happened. Some of us (surely in my case) are, alas, doubting that innatism in the domain of language and cognition will be considered acceptable by the academic world at large within our lifetime.

I'll come back to this postclassical transition of cognitive science at the end of these notes. I have to tell you first how I came onto the original scene. I have to tell you about my first encounter with Jacques.

## I Enter Jacques "Mahler"

In May 1968 (yes, the famous-infamous *mai soixante-huit*), when I was a research fellow at the Institute of Physical Chemistry in the University

of Rome, a colleague and friend of mine urged me to accompany her to a seminar at the Institute of Psychology. The speaker, Jacques Mahler (that's how I first misperceived his name, linking it to the composer), was a buddy of her and her husband from the old days in Buenos Aires. An interesting young fellow, she told me, who had first been a theoretical organic chemist at Oxford with the legendary Charles Coulson, then turned psychologist and linguist, having worked with Jean Piaget in Geneva, and with Noam Chomsky at MIT. These names were awesome enough to me even then, so I gladly accepted. Little could I know that this event, in the fullness of time, was bound to change my professional life, and my life as a whole.

Jacques impressed me at the time as looking more like the typical Belgian businessman than the typical scientist. He was clean-shaven and impeccably dressed in a dark three-piece suit, and spoke Italian with amazing fluency and naturalness, using an often invented, though always quite plausible, vocabulary. Over the years, many of Jacques's creative Italian expressions have stuck in our family vocabulary.[1] In hindsight, the persistence in Jacques's Italian of the setting of his phonological and syntactic parameters from Spanish is a clear instance of the resiliency of the linguistic setup of our mother tongue (this phenomenon is well analyzed by Nuria Sebastián-Gallés and Laura Bosch in chapter 21 of this book).

The topic of his talk was, somewhat strangely in hindsight, the tracking of ocular movements in reading. The central message that I retained was that a remarkable variety of underlying syntactic structures of great subtlety and richness could be revealed (according to Jacques, at the time) by tracking ocular movements. To the best of my knowledge, Jacques has not further pursued this line of inquiry, nor has it turned out to be a very fruitful method in anyone else's hands for many years (though there are interesting new results now) but that first take-home lesson from Jacques in classical cognitive science was crucial nonetheless.

*Lesson 1: There are many complex underlying structures in linguistic expressions, and they can be evidenced by monitoring a subject's real-time processing.*

## II Getting to Know Mehler

We had a nice long chat with Jacques after his talk, outside the Institute of Psychology. I was left dumbstruck by his report of the vast disruption of ordinary Parisian life caused by the revolt of the students in that fateful May '68. The Italian press had obviously underplayed, in those initial days, the momentous impact of the revolt. Jacques told us about banks and shops being closed, about army and police patrolling the streets, about many violent clashes. The magnitude of the first spark of the events that were bound to change our existence as teachers and as citizens for many years to come suddenly sank in, thanks to Jacques.

I instantly liked him very much. He was not only obviously very bright but also fun to be with. He had a pleasant no-nonsense straight-to-the-essentials communicative style.

In April '69, having won a plush scholarship from the European Molecular Biology Organization, I moved to Paris-Orsay, to work with the Nobelist-to-be Pierre-Gilles de Gennes. I was living in Paris, and Jacques was among the acquaintances with whom I planned to try to establish better contacts. Jacques kindly invited me to dinner, on a gorgeous June evening, and introduced me to his family. He was then living near the Avenue Foche, opposite the famous Palais Rose, whose pending renovation was then fully under way. Jacques took me to visit the palace, in an after-dinner stroll. He had changed a lot physically. He wore a beard, and was dressing more like an artist than like a banker. Ten minutes into the evening I already felt as if we had always been friends.[2]

## III Intimations of Cognition

We quickly became good friends, and I indulged myself in attending, now and then, under his suggestions, some lectures and seminars in his laboratory.

I was particularly impressed by a talk by Sydney Strauss, from Israel. He had clear evidence that Piaget's famous horizontal stages were not horizontal, after all. Children who clearly mastered conservation in problems dealing with volume and weight failed to apply conservation to

problems dealing with temperature, concentration, or speed. The kids' exclusive focus on one dimension only of the situation, regardless of other relevant factors, mimicked exactly the results of Piaget's famous experiments on the conservation of weight and of volume, except that these kids demonstrably mastered conservation for weight and for volume. The number of sugar lumps dissolved in a water container, regardless of the size of the container, decided whether the resulting liquid was thought to be sweet, very sweet, or very, very sweet. After pouring the contents of one container judged to be "very sweet" into another, also judged to be "very sweet" the resulting liquid was judged to be "very, very sweet." The parallel progression of two toy cars along the linear projection of two trails decided how fast they were thought to travel, regardless of the fact that one trail was straight, while the other was curvilinear. Even when the second was then stretched into a longer line than the first before their eyes, these kids insisted that the two cars had traveled equally fast. This intuition persisted even when both cars moved in a straight line on trails of visibly different length, but starting together and reaching the end of their respective trails together.

Moreover, Sydney showed us an impressive instance of a cognitive illusion for grown-ups. (In the last several years I have used this demonstration in dozens of classes and lectures.)

A shoelace is tied into a loop, and the loop is then held between the index fingers and thumbs to form a square. By slightly approaching each index finger to the opposing thumb by an equal amount for each hand, the square slowly turns into a rectangle. The audience is invited to judge the ratios of the *areas* of these two figures. Having absorbed, as a physicist, a whole course on differential geometry, with plenty of cases of surface maximization, I judged that "obviously" the rectangle had a smaller area than the square. After all, since the perimeter is conserved, the area cannot be conserved too. Sydney and Jacques, much to my surprise, claimed that the majority of adult subjects, even well-educated ones, wrongly judge the two areas to be identical. And they persist in their intuition of conservation even for the areas of the more and more elongated rectangles that one slowly obtains by progressively approaching the opposing index fingers and thumbs. Until you have no area at all! Now what?

They were right. Indeed, the vast majority of subjects, of all ages, including highly educated persons, judge the area to remain constant. Some pause at the end, when no area is left at all, and are ready to retract their previous judgment. But the majority, though somewhat puzzled by the final disappearance of the area, persist in their judgment that the area is being conserved.

The cumulative lesson was clear and powerful: we have problems with judgments of conservation through all our life. There are no horizontal stages à la Piaget.

I was greatly impressed. I knew enough about psychology to be aware that Piaget was considered absolute avant-garde (in Italy, this was indeed the common opinion, then even more than now). I was being introduced to a whole post-Piagetian line of inquiry, well beyond the alleged avant-garde. Jacques patiently explained to me the notion of "cognitive strategies" (flexible strategies, not uniform horizontal stages), referring me to the works of Jerome Bruner.

*Lesson 2: There are several important conceptual changes in the child's cognitive development, but none of them is "horizontal," and they do not all happen at the same age.*

## IV Smart, Then Less So, Then More So: Enter the U-shaped Curve

Jacques had some stunning data of his own on very young children (he systematically used, and still often uses, the colloquial Italian word *ragazzini* more aptly applied to kids above six or seven years of age). But these children were much younger (three to four), and allegedly well below Piaget's conservation stage. They were presented by Jacques with two rows of candies on a flat table. It was made clear to them that they were allowed to choose and keep the candies of one, only one, of the two rows. The number of candies was exactly the same for both rows, but one row was more elongated (the candies were more broadly spaced out than in the other). The very young chose one or the other indifferently. The older ones (four to six) indeed preferred the elongated row (as observed by Piaget, thinking it contained more candies). And then the "conserving" older children (seven and above), again chose indifferently, well aware

that the number of candies was conserved by the spatial transformation. The crucial datum was the indifferent choice of the very young children, a fact never tested by Piaget, and that flew in the face of his celebrated stages. Another important lesson from Jacques.

*Lesson 3: The most typical learning curve of humans in various cognitive domains is U-shaped.*

Combined with the previous one, this tells us that there are many dips and drops in cognitive development, and that the troughs are not co-occurring in time. An updating of this lesson today ought to change the quantifier in the sentence: One of the most typical learning curves is U-shaped. The issue of continuity in development is still a central topic today (see Baillargeon, chapter 19; Carey, chapter 17; Gelman and Cordes, chapter 16; and Spelke and Hespos, chapter 18).

## V Neonate Cognition

Another talk by Jacques impressed me greatly. With Peter Eimas, he had studied speech perception in the newborn, in the few-weeks-old, and in the few-months-old, revealing clear cases of very, very early categorial perception. A sophisticated piece of machinery (the Vocoder) could progressively vary, by a precise and programmable number of milliseconds, the time lapse between two acoustic formants of a syllable (*ba*, *da*, *ga*). In the adult, the effect was very sharp: Up to a steep threshold, the syllable was heard as being always the same (say, *ba*), then, abruptly, for any time delay above the threshold, the syllable was distinctly heard as being different (*da*), and was then perceived as the same syllable up to the next threshold (turning abruptly into *ga*), and so on. With tiny little babies, strikingly enough, the result was essentially the same. The method that Jacques and Peter used was non-nutritive sucking. The perception of sameness in a series of stimuli leads to decreasing rates of sucking (betraying increasing boredom), while the perception of novelty leads to more intense sucking (betraying increased attention and interest). The data offered very neat and very persuasive evidence of strong innate components in our linguistic capacities. After having heard so much about the ubiquitous "projection" of structures from the mind onto the external

world, there I was, finally seeing a real paradigm case. Of great interest was also the fact that if these speech recordings were played backward, neither the adult nor the baby could hear any difference at all. They all just sounded like indistinct chirps.

*Lesson 4: Objective physical differences in sensory stimuli may not map at all onto cognitively processable differences. And relevant cognitive differences may not map neatly onto physical differences.*

The mind literally, not metaphorically, projects structure onto the external world. It cannot suck it from the world, it cannot literally learn this structure. Another capital lesson, originally due to Chomsky, but that percolated to me through Jacques, was the so-called poverty of the stimulus argument.

*Lesson 5: Rich cognitive capacities that are in place very, very early, and that cannot possibly have been shaped by experience, cannot be explained through learning. They are internal to the mind.*

## VI Modularity (in Hindsight)

Since categorial perception is not pervasive in the realm of sounds, but is clearly present in speech, this means that our mind is naturally endowed with specialized processing units, governed by highly refined and abstract operations. In the course of a conversation Jacques also mentioned another specialized cross-modal effect, unique to speech perception (in hindsight, I think it was the McGurk effect, though the name did not stick in my memory then). It does not apply to bouncing balls, the rustling of leaves, or the screeching of tires. It is proprietary to speech perception.

Around that time, two more lessons from Jacques crystallized in my mind. I'll phrase them as closely as I can in words I might have used at the time:

*Lesson 6: The mind has subdivisions into specific domains. They operate in ways that are somewhat similar to reflexes, except that they can be vastly more complex than mere reflexes.*

The case of sign languages, to which Jacques introduced me, also introducing me, much later on, to Ursula Bellugi-Klima in the flesh, made the next lesson quite vivid.

*Lesson 7: These cognitive domains crisscross the traditional subdivision into the five senses. Language is one such domain and much of its specialized nature is preserved regardless of whether it is presented in the acoustic or in the visual mode.*

These crucial insights were not a bunch of disconnected and episodic ideas. They coalesced neatly into an articulated and rich domain of inquiry. A domain I was beginning to like more and more. Jacques had, in the space of about two years, progressively introduced me to classical cognitive science. Then something happened that steered my way into it for good.

## VII The Two Jacques

In the meantime, with de Gennes's blessing, I had moved to the Institut Pasteur and was working under Jacques Monod's supervision. In one of the precious and blessingly frequent conversations I had with Monod, I mentioned these data and these techniques. A staunch innatist himself, Monod encouraged me to invite Jacques to give a talk at the Pasteur. I objected that it was a topic lying very far from the daily concerns of the molecular geneticists. "Tant mieux, ça leur fera du bien" [So much the better, it will do them good] was Monod's instant reply. He added that he could not imagine, in perspective, a more fascinating field of inquiry *for a biologist* than the exploration of the limits of the genetic contribution to culture, the boundaries of the *enveloppe génétique* in shaping the human mind. This expression, now made famous by Jean-Pierre Changeux, who was at the time a close collaborator of Monod after having been a much beloved and esteemed pupil, was decidedly congenial to biologists of that caliber. So I invited Jacques.

He presented the data on categorial perception and reaped a great success.

Some years later, at a restricted planning meeting, Monod said emphatically (his precise words are engraved in my memory): "Ce que fait Mehler *c'est* de la science. Et, croyez-moi, j'en sais quelque-chose" [What Mehler does *is* science. And, believe me, I know something about science].

Monod eventually encouraged me to venture into this new and promising field, which we had tentatively labeled "bioanthropology." Jacques's

seminar at Pasteur, in hindsight, was a milestone. It gave me the much-needed approval of Monod to enter into the biology of the mind, and, via the steady connection between Jacques and Chomsky and Luria, and the ages-deep connection between Monod and Luria, it intensified in Monod himself an active interest in this field.

A centerpiece of this connection was, of course, Changeux. I would like to hear how Jean-Pierre reconstructs his own memories, but I think that this first encounter with Mehler was quite important for both. He and Jacques were naturally destined to combine forces and interests, and ultimately share brilliant young collaborators, as they have. I, for one, am happy to think that I have had a non-negligible role in starting and, at least initially, feeding the collaboration between them. That seminar at Pasteur was the first step.

## VIII The Royaumont Center

Then came the Centre Royaumont pour Une Science de l'Homme, of which I was appointed director, and Monod chairman of the board. I'll leave that complex story for another occasion, but I cannot refrain from reminiscing, with undisguised pride, that the young core of the center I had the privilege of assembling was formed by Jacques Mehler, Jean-Pierre Changeux, Dan Sperber, François Dell, and (later) Antoine Danchin. This short list speaks for itself. And it is no illusion that we were actually doing cognitive science before the label was fully consolidated.

This brings me to two curious episodes, both of which involve Jacques.

The first happened at Endicott House, a sumptuous residence outside Boston, given to MIT by a rich family of insurers. The Royaumont center, in close collaboration with Chomsky and Luria, organized a weekend-long informal workshop at Endicott House, to explore this famous biological "envelope" of the mind. If attendance by the senior component was impressive there and then (Monod, Chomsky, Luria, Zella Hurwitz, Hans Lucas Teuber, Vassily Leontieff, Daniel Bell, Michael Scriven, Daniel Lehrman, Edgar Morin), attendance by the young was no less impressive in hindsight (Jacques, Susan Carey, Ned Block, Peter and Jill De Villiers, Eric Wanner, Paula Meniuk).

At one of the sessions, Jacques had just started presenting data from psycholinguistics, including the celebrated archetypical garden-path sentence "The horse raced past the barn fell." A question was asked and before Jacques could reply, Noam sprang up, went to the blackboard, almost snatched the chalk from Jacques's hand and answered the question at length. The time allotted to the presentation by Jacques was almost entirely usurped by Noam and by the intense discussion that ensued. Jacques sank into a mood of frustrated resignation, and he later privately shared his sour grapes thoughts with me. He said: "Once a student, you are considered a student until you die." Contrary to his impression, though, the episode was quite favorably received by us all. It had shown, more than any words could, how fond of Jacques Noam was (and still is), how freely interchangeable with his own Noam then considered Jacques's data and interpretations, and how close their collaboration was.

## IX Piaget's Ban of Jacques

The second episode involves Piaget during the preparation of the Royaumont debate with Chomsky. To cut a long story short, Piaget had manifested to Scott Atran his desire to meet with Chomsky at length. He wanted just the two of them, with an interpreter, possibly Guy Cellérier. I had slowly managed to persuade Piaget to turn this ultraexclusive binary exchange into a wider event, at Royaumont. One day I sent to Piaget a tentative list, for his approval, announcing my visit to him in Geneva a few days later. In his office in Geneva, Piaget was adamant in forbidding any participation by Jacques in the meeting. "Mehler n'a jamais compris ce que je dis" [Mehler has never understood what I say]. I found this claim, to say the least, unsubstantiated, but I knew well by then that their respective views on cognitive development were in stark contrast one to the other. Little did I know at that moment that Piaget was prone to assume that whoever disagreed with him could only do so because he or she could not understand what he was saying. Piaget's unshakable confidence in his being right made him assume that understanding him and believing what he said could not be two different things. This was to emerge loud and clear in the debate, and is now testified in print. Until

his death, Piaget was persuaded that not even Chomsky understood what he said. But I am losing track of my story.

Piaget threatened not to come if Jacques was invited to the Royaumont debate. I tried to counter, but had to desist. I hurried to see Jacques upon my return to Paris. Jacques was equally adamant in enjoining me to stand firm. His exclusion was intolerable.

A very hot potato had landed in my lap.

I decided to play subtly and, if necessary, deviously. Some months later Scott Atran managed to assuage Piaget a bit about Mehler, transforming the veto into mere displeasure. Jacques, in the end, graciously accepted to make no presentation at Royaumont, but was officially present anyway, and then was given, as promised, ample possibility to write comments for the final proceedings.

It is not that I want to repaint, at any price, every such anecdote as positive, but I was impressed then, and maintain this impression in hindsight, by the fact that Piaget was ready to forgo a much sought-after, once-in-a-lifetime opportunity to meet with Chomsky, simply because Jacques was present. No matter how unpleasant, this was also a quite clear calibration of the scientific status that Jacques had already attained in Piaget's eyes back in the mid-seventies.[3]

## X Cognition Trieste Style

I am coming now to an interesting later venture, my participation in which I also owe to Jacques: the Scuola Internazionale Superiore di Studi Avanzati (SISSA). That's where he is now, a full professor, having restarted, with unabated enthusiasm and drive, a new laboratory. The beginning of this venture dates twelve years back. Another old Argentinian-Italian buddy and relative of Jacques, the distingushed physicist Daniele Amati, director of the International School of Advanced Studies in Trieste, a graduate school in physics, mathematics, and biophysics, wanted to explore new avenues to extend and diversify the school. Cognitive science seemed a distinct possibility, so he naturally turned to Jacques.

Soon, a cognitive unit, TECS, the Trieste Encounters in Cognitive Science, was activated on the Adriatic coast. Jacques was kind enough to involve me in this enterprise, which offered me, on top of the beauty of

the place, and the joy of being intermittently among distinguished colleagues and good friends, the immense pleasure of reestablishing a contact with my initial origins (as a physicist). The several periods I have spent at SISSA have been absolute bliss, and the workshops organized by Jacques there a further occasion to learn about cognitive science at its best. I'll select two of them, strictly related, for reasons that will soon be evident.

**First Workshop**

Languages differ in the strength, neatness, and pervasiveness with which words are parsed into syllables. Syllables have just one fixed structure in some languages (e.g., consonant-vowel, CV), a more flexible, but still stringent, small set of structures in others. And then . . . , then there is English. Jacques and Anne Cutler had set themselves to the hard task of exploring these differences systematically, tracking their possible consequences on language acquisition.

Trieste became the hub of this multilaboratory and multidisciplinary enterprise. Jacques and Anne organized an initial weeklong state-of-the-art planning seminar, which was repeated five years later, to wrap up and evaluate the results. The difference in content and climate between these two workshops, with a five-year gap, made quite palpable to me the transition to which I have alluded at the beginning of these notes, and on which I intend to close.

These are my recollections of the first workshop. Jacques had shown, among many other interesting things, that babies are sensitive to the sheer number of syllables. A repetition of bisyllabic words, or possible words, that only have in common their bisyllabicity, engenders boredom. If a three-syllable word appears in the stimulus, the baby is suddenly interested. It also works in the other direction. Being able to count syllables is crucial to being able to place accents correctly, since in many cases the tonic accent falls on the nth syllable from the beginning, or from the end, of a word. And where the accent reliably falls is a powerful clue to segmenting the stream of speech into words, especially when you are a baby, and do not know the words yet.

French, Italian, and Spanish are rather strongly syllabified languages. Adult speakers find it easy to identify a certain syllable, but are dismal

at identifying the same string of phonemes, when they sometimes form a syllable, and sometimes don't. *Bal* is a syllable in "balcon," but not in "balise" and "balance." It's hard to track, and quickly detect, the sameness of the *b-a-l* sequence across these two kinds of words. The syllable constitutes a prominent, natural, and pervasive unit to the speakers of Romance languages, but not at all, or much less so, to speakers of English.

What happens when the speaker of one language parses words and sentences in the other language? Does he or she project the syllabification strategy and the accent patterns of his or her mother tongue? Initial results gave a clear positive answer. Romance speakers syllabify each and every speech sound under the sun, notably including English. English speakers, on the converse, are in trouble when having to parse a Romance language into syllables.

This has, as we just saw, interesting consequences for the identification of word boundaries in language acquisition. Anne Cutler presented indubitable evidence that the segmentation of the stream of speech into words is not physically based. The momentary gaps in the acoustic stream do not correspond *at all* to word boundaries (another fine confirmation of lesson 4 above). The child has to project these boundaries onto the stream. How does he or she do that?

Syllabification and the projection of simple, basic accent patterns are excellent devices. The hypothesis, to be later exhaustively explored by Maria-Teresa Guasti and Marina Nespor, in close collaboration with Jacques, is that the French child parses the stream tatatatatatatata as tatá/ tatá/ tatá/, while the English child parses it as tàta/ tàta/ tàta/ (one language is said to be iambic, the other trochaic, a fundamental distinction that applies to many other languages as well).

The stream prettybaby is naturally parsed in English as prétty bàby, while the stream jolibebe is equally naturally parsed in French as jolí bebé. A child that adopts this simple strategy is right most of the time. The child will get most of her "possible words" in the lexicon correctly.

In a nutshell: differential speech processing across languages fell neatly into a pattern of limited, sharp, mostly binary, differences to be set by the child once and for all, at a few specific "decision-nodes." It reminded us pleasantly of the "principles-and-parameters" scheme in syntax.

All this, and much more that I cannot summarize here, was very clear and very clean in the first workshop. That workshop was, again in my terminology, the epitome of "classical" cognitive science. Then came the second workshop, five years later.

### The Second Workshop, and Beyond

Nothing was quite the same any more. An awesome quantity of data had been reaped, and many complications had arisen. Not that the neat former hypotheses were wrong. But they were not quite right either. The picture was more complex, more nuanced, with several remarkable specificities in different languages.

In a nutshell, we were already in a postclassical cognitive science.

This impression was confirmed by another workshop at SISSA, some months later, organized by Luigi Rizzi and Maria-Teresa Guasti, on the acquisition of syntax. Many interesting new data were presented, and this workshop also gave me the impression that the principles-and-parameters model, though basically still alive, needs a lot of refinements and qualifications to be viable. It hammered the point that we had entered a different phase in the development of cognitive science.

## XI On Classical, and Not-So-Classical Cognitive Science

In the spring of 1990, while organizing at MIT the annual meeting of the Cognitive Science Society, I was a chairing a small committee formed by Stephen Kosslyn, Steven Pinker, and Kenneth Wexler, all "classical" cognitive scientists to the marrow. We had before us some 300 submitted papers to be dispatched to the referees. In order to do it responsibly, we had to grasp what each paper was about. After having jointly processed the first hundred or so papers, Steve Kosslyn, rather puzzled, and somewhat exasperated, made a quite telling remark: "Gee, is *this* cognitive science?!"

The dominant topics, in fact, were flat-footed applications to a variety of very practical problems, applications of connectionism, and downright artificial intelligence, with many instances of standard problem-solving of the Pittsburgh variety. We shook our heads, in resignation, and then continued unabated.

I have been rethinking about this remark many times. As noticed by Kosslyn at that meeting, it was (and still is) another brand of cognitive science than the one I had initially learned from Jacques. A different enterprise than the one which I decided, some 25 years ago, to participate in.

I think I owe the reader a few more lines of clarification about this transition from what I have called "classical" cognitive science to a new variety, which I will call "nonclassical." A germane standard characterization of the former, in Chomsky's own term, is "rationalist cognitive science," a label that I like a lot, but that presents a delicate problem of appropriation. Cognitivists of other persuasions do not accept gladly to be dubbed, by implication, as nonrational. Perhaps calling it classical avoids this rebounding effect.

In a nutshell, and capitalizing on the lessons that I received from Jacques, I see classical cognitive science as being profoundly marked by the following overarching explanatory strategies: (1) unrestricted nativism (no capacity or content is deemed too complex or too specific to be imputed to the innate capacities of the brain/mind, if such attribution solves otherwise insoluble problems). As a consequence, we have (2), learning is essentially a triggering phenomenon (the idealization of single-trial learning *is* an idealization, but one close enough to reality). As a further consequence, it should be noted that, therefore, no historical statistics of the unfolding of the stimuli over time *can* be relevant to (classical) cognitive science. Connectionism and empiricism have reinstated the centrality of statistical analyses of the inputs, setting themselves apart (quite proudly, one can add) from classical cognitive science. The fact that Jacques, and other "classical" cognitive scientists for whom I have great respect, are presently tackling precisely such statistical analyses in the domain of phonology and syntax shows that something is changing in the picture.

The next classical core tenet, about which I can be dispensed from saying much here, is (3) massive modularity. The rich harvest of "strange" and very specific cognitive deficits witnessed by cognitive neuroscientists over many decades lends increased support to modularity. Yet, the growing resistance with which the inescapable modular conclusions of this vast literature are being met also shows, I think, a change of

inclinations and explanatory standards in present-day cognitive science. From SLI (specific language impairment) to prosopagnosia, from domain-specific semantic deficits to Williams syndrome, the standard (and, in my opinion, still correct) modular interpretations are being challenged by insiders and outsiders as well. "Much more goes berserk in those cases" is the recurring critical punch line. Cognitive deficits produced by restricted brain lesions are alleged to be, after all, not so specific and circumscribed. The case is pending, and modularists now face a fight.

Finally, the signature of classical cognitive science also was (4) drastic restrictions on the number of points of possible variation, and drastic restrictions on the acceptable values at each point (the principles-and-parameters paradigm). The realm of language, phonology notably included, was the prototype case. Partly because of the raving success of optimality theory in phonology (also a parametric theory, but with unprecedented degrees of optionality built in), partly because of the difficulty in exactly spotting the points of parametric variation in syntax, the picture has been blurred somewhat. Combining some vestige of a parametric theory with statistical regularities in the input seems to be the present challenge. My expectation is that the final solution will be decidedly nonclassical.

## XII Conclusion: The Demise of the Old Guard, and of Classical Cognitive Science

I cannot consider it a coincidence that Jacques, at that moment, disbanded the TECS committee and replaced it wholesale with much younger (and terrifically bright) cognitive scientists. He did the same also with the editorial board of *Cognition*. Out goes the old guard, and in come the young Turks. Adding the young and bright is splendid, and most welcome to us all. But why have them replace us wholesale? Could we not coexist with them? Are we really *that* old? Jacques's deeper agenda is somewhat inscrutable these days, to some of us but I think I know why he did this, and he may be right once again.

It's the business of scientists to determine, as best we can, what there is. It would be silly to force the data to be what we would like them to be. If our theories and schemata and styles of reasoning do not match

reality, we should revise the former, not manipulate the latter. Are we, at least, entitled to feel vaguely uncomfortable? Possibly. But that should not retain us from moving ahead. But, are we *capable* of moving ahead? Jacques must have grown doubtful about this. Maybe this is why he is "retiring" us a little prematurely. And why he decided to restart from scratch, with a clean slate and with much younger collaborators.

Jacques is to me, for all the reasons I have sketched above, the prototype of the "classical" cognitive scientist. His whole career, and the first 25 years of *Cognition*, testify to this. Therefore it's a credit to his intelligence and vitality and imagination that he intends to move ahead, cost it what it may. He plans to proceed to a nonclassical phase with the young ones, unencumbered by our common past. This is the latest, and possibly the hardest, lesson I'll have to learn from Jacques.

## Notes

1. We are fond of imitating his way of saying *belissimo* (one *l* only), *rrabia* (two strong *r*s, and one *b* only), and *cafoone* (with a long *o*). A lemon of a car is to us *un polmone* (an expression then current in Roman Italian to execrate a miserably underpowered and sluggish car). Except that it had an *o*—*polmone*, meaning "lung." But Jacques's *u* variant "pulmone" added force to the contempt, and sounded better than the original.

2. I was then riding a Triumph motorcycle, which I had parked on the pavement under the Mehlers' windows. The kids liked it very much from afar, and asked to have a closer look. So did Jacques. I think I infected him there and then with a passion for motorcycles that was destined to stick with him, and that would, years later, produce an unforgettable ride with Jacques and an Italian friend, all the way from Paris down to the Cote d'Azur, through wonderful secondary roads that Jacques was privy to (the Plateau de Mille Vaches and the *arrière pays* of Provence). In turn, he infected me with an interest (then not yet a passion) for the study of the mind that was destined to stick even more.

3. When the Royaumont center was forcibly disbanded in 1980, well after Monod's untimely death, Jacques wrote a beautiful letter to me, expressing solidarity (gladly accepted) and gratitude (a little harder for me to accept, since I felt that I was the one who owed gratitude to him).

In this letter, he said that I had played a central role in making him feel more at home in Paris, and in introducing him to eminent and interesting colleagues. I could not believe my eyes. I, the recently arrived and quite precarious visitor, had pictured *him* as firmly and stably entrenched in his splendid apartment near the Avenue Foche, and in the Parisian academic community. Moreover, it had

been Jacques who had kindly introduced me, over the years, to the giants of cognitive science, and familiarized me with their work.

During all those years, most prominent in my memory, among many others, are my encounters at Jacques's house or in his laboratory with Jerry Fodor, Lila Gleitman, Tom Bever, Merrill Garrett, Richard Kayne, Ursula Bellugi, Albert Galaburda, and Peter Jucszyk). It defied my imagination how he could really think (but apparently he did) that it was *I* who had facilitated *his* entrenchment in the Parisian academe, and into the bigger world of science at large. Sweet of Jacques to think so, but it was plainly not the case. All the more reason for me to be grateful.