

**Jean Requin**  
**March 30, 1938 - June 21, 1996**

**A brief memoir**

by

Sylvan Kornblum

*This is the text of the opening talk of a two-day symposium that was held in honor of Jean Requin. Occasional references to this event, made during the talk, have been retained in this published version. Accomplishments like the ones that are summarized in these remarks do not come to a man working in isolation – and Jean Requin certainly did not work in isolation, nor was he an isolate, as you all well know. I have purposely refrained from listing his collaborators, simply because the list would have been too long, and the risk of omitting someone too great. However, collaborators he had – many of them – and many of these participated in this meeting.*

Jean Requin was born in 1938. He published his first article in 1962 at the age of 24, and died with his 113th article in press at the age of 58. He had a brief, but remarkably full and productive life, as will be made evident in the next two days.

Jean was equally at home in neurophysiology, epistemology, and cognitive experimental psychology. The ultimate goal of his 34 year career was to arrive at an integrated view of all three, and in this he was a true pioneer for he tackled this issue head on, way before it became fashionable to do so.

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His formative years were spent looking – and acquiring the wide expertise that he continued to develop, and to hone throughout his career. His degrees, which he obtained in Paris during a five year period, include degrees in literature, psychology, general and comparative psychophysiology, and neurophysiology. At the end of these five years he decided that he had spent enough time in the classroom, and that it was time to go and find things out for himself. He, therefore, accepted an offer from Jacques Paillard and set out for Marseille, where he remained until his death.

I listed literature as one of his degrees. This represented a serious effort on his part. He was not only very knowledgeable in literature, but in addition to his scientific articles, he also wrote fiction, and was hoping to do more of that after he retired. He often stood by sadly as he watched his well crafted Proustian prose mangled beyond recognition as his articles made the transition from French to English. Somehow Proust was not what the *Journal of Experimental Psychology* was looking for.

Very early in his career he staked his claim on Preparation as his research domain [The title of his first solo paper which was also the second published paper of his career (Requin, 1963) was: "Orienting of attention: one aspect of preparing for a motor response" – "L'orientation de l'attention: un aspect de l'attitude préparatoire à une réponse motrice], and made it his basis of operation – one from which everything else flowed, and to which he always returned. I know of no psychologist or neuroscientist today who has contributed more to our knowledge and understanding of preparation than Jean Requin.

The behavioral paradigm that he chose, and stuck with, was the RT task with all of its many variants. The basic task consists of three clearly identifiable events: a warning signal, followed by a response signal, followed by the subject's response. The time between the warning signal and the response signal is the warning interval, during which – in principle – preparation is taking place; the time between the response signal and the response is the reaction time, which is the principal dependent variable in this experimental paradigm. It is a universally accepted operating assumption of all investigators in this field that the better prepared a subject is, the faster and the more accurate will the response be. RT thus becomes the behavioral measure of the degree to which a subject is prepared to make a particular response. Given the events preceding a subject's response (the warning signal, the warning interval, and the re-

sponse signal) it is not difficult to imagine a whole range of experimental manipulations that systematically affect the subject's readiness, or preparedness to respond. For example, even in the case of the so-called simple RT task, where there is no uncertainty about the identity of either the stimulus or the response, varying either the temporal uncertainty, or the probability of the response signal is bound to affect the subject's preparedness. What makes the RT task interesting, of course, is that it permits us to make inferences about the subject's mental operations during the performance of the task. However, such inferences, argued Jean, would be on much more solid ground if they were also based on converging neurophysiological evidence.

Preparation was the cognitive wedge of Jean's research program. And he was fully aware of this very early on. A mere three years after his first publication he wrote:

"Preparatory states do not necessarily have phenomenological content. They are real only to the extent that they produce modifications in responses ... Therefore, in order to avoid circularity, and in order for preparation to become a topic of serious study, preparatory states must have observable effects beyond merely the changes that they produce in response parameters. Thus are we led to cast the problem in terms of physiological processes that precede either perceptual or motor acts, and thereby attest to the individual's engagement in goal directed behavior, prior to the overt reaching of that goal." (Requin, 1965, p. 101.)

In retrospect, this one statement describes the essence of Jean's research program of the next 30 years. Coming as it did, in 1965, from a 27 year old, before the terms "cognitive" or "neuroscience" had been accepted in isolation, let alone together, it is a remarkable statement, but then Jean was a remarkable man.

I first met Jean in 1967 while we were both attending the second symposium on Attention and Performance. This particular symposium was held to commemorate the centennial of Donders' famous paper on the timing of mental processes. (A paper, by the way, written by a physiologist that appeared about 25 years after Helmholtz had startled the world by announcing that the speed of nerve conduction was finite and that he had measured it. So the ties between cognition, RT, and neurophysiology have been there from the very beginning. Jean was buying into a very solid tradition.)

Jean presented the results of a study where, by capitalizing on the fact that spinal reflexes were susceptible to centrally controlled modula-

tions, he was able to use these reflexes to investigate preparatory processes during a simple RT task (Requin, 1969). The warning signal was an auditory click, the response signal was a light, and the response was a foot extension. The warning interval lasted one second and at various points during that second, he elicited reflexes from both the responding and the non-responding foot.

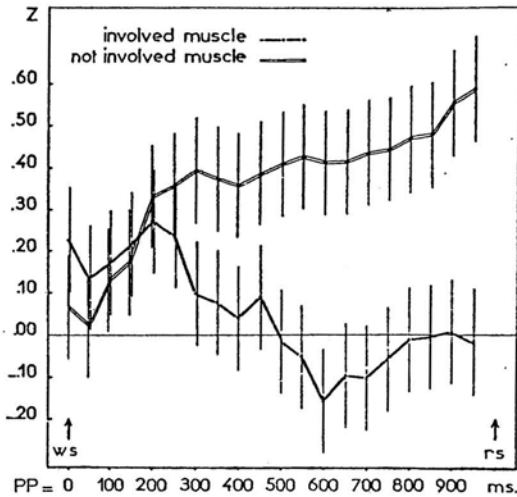


Figure 1. Evolution during a fixed period of 1 second (abscissa) of the amplitude of the T reflexes elicited in the muscle involved in the response and in a muscle not involved; the origin of ordinates (Z scores) corresponds to the mean amplitude of the reflexes triggered during the intertrial interval; ws is the warning signal; rs is the signal to respond to; 10 subjects; each point is based on 200 observations; limits of confidence of the means at the 0.05 probability level (from Requin, 1969).

The top curve shows the reflex amplitude for the non-responding foot, the bottom curve for the responding foot. Both increase at first, with the responding foot decreasing as the warning interval increases. Jean interpreted the early rise in both curves as the non-specific aspect of preparation, which he attributed to "attentive mobilization". The later parts of the curves he interpreted as the specific aspects of preparation. In particular, he saw the decrease in reflex amplitude for the responding limb as evidence of selective inhibition of the motoneuron pool for that

limb. Such inhibition, argued Jean, insulated the responding pathway from outside influences and made it more sensitive to the subsequent, and expected motor commands.

At the time these experiments were being conducted there was a group of investigators, particularly in England, that had begun looking into the problem of attention (e.g., Broadbent, 1958). For them, attention was primarily a perceptual filtering mechanism. This position later came to be called the "early selection" view of attention. The other view, the "late selection" view only appeared later, and very gradually. But it doesn't take much imagination to see that Jean's work on "preparation", even this early study, was among the first investigations on what eventually became the late selection view. And he more or less claimed as much when a few years later he asserted that "action is the main determinant of attention".

For the next fifteen years Jean continued to work on preparation, extending his neurophysiological measures to include, EMGs and EEGs, in both humans and animals, and single cell recordings in monkeys. He was also developing a theory of motor preparation, but more importantly, he saw the whole complex of psychological and neurophysiological data, theory, and techniques as complementary sides of the same coin and was beginning to see the need, as well as the way to integrate them into a coherent whole.

Jean's work was rapidly gaining recognition both in France and on the international scene. One indication of Jean's rise in the ranks of French science came in 1984, when the CNRS awarded him the silver medal – the very first ever given for work in psychology. Later, in 1993 the CNRS decided again to award a silver medal to a psychologist. And, not surprisingly, the recipient was a member of Jean's laboratory – Jacques Vauclair. As modest as Jean was, he took enormous pride in both medals – as well he should have.

In 1980, David Rosenbaum published one of the most important, at that time, behavioral paradigms in the study of motor control (Rosenbaum, 1980). Rosenbaum showed that in a task where the response consisted of making either a long or a short movement, either forward or backward, with either the left or the right hand, if subjects were given information about which arm they had to move, and/or which direction, or the distance that they had to move it, they could take advantage of this advance information by preparing these various aspects of the movement, separately. This particular experimental procedure was the

late selection version of the priming paradigm that Posner had introduced earlier (Posner & Snyder, 1975), and it established the idea that different aspects of a multidimensional movement could be prepared separately, and ahead of time. It was as if preparing to make a movement consisted of setting parameters in the execution program for that movement. This, of course, was a different kind of preparation than what Jean had been working on up to that point. He immediately saw its importance and launched the series of – still ongoing – studies on motor preparation using single cell recordings with monkeys.

If one had to summarize the findings of the many years of work that this effort represented it would be that different neurons in the motor cortex change their levels of activity depending on which parameters of the movement is given to the subject ahead of time. This was an important result which, together with other neurophysiological evidence, led Jean to propose a three-step model of motor preparation (see Figure 2).

He first presented this model when he came to Ann Arbor in 1984. The model drew on behavioral, cognitive, and neurophysiological data and theory and – necessarily – addressed the issue of the integration between the three. The meta-theoretical framework for the model was stage theory. That is, the notion that one can partition both cognitive function and brain structures into separate, identifiable entities, and study the effects of different experimental variables on these entities. One of the fundamental assumptions of stage theory, any stage theory, is the strict seriality of its stages. Feed-forward processes, which had recently emerged as powerful explanatory principles, could, therefore, not be accommodated by the classic notion of stages. The preparatory processes, in the center of the diagram, were, therefore, not part of these stages – even though they affected and modulated them.

On the left of Figure 2 are the three, serially organized brain structures that Allen and Tsukahara (1974) had previously proposed were at the basis of all voluntary motor action. On the right, are the three functional stages that psychologists had proposed as a model of motor organization. In his paper, Jean explored the possibility of an isomorphism between these two structures.

If such an isomorphism existed, argued Jean, it would mean that the three processing stages postulated by the psychologists could be implemented by the three structural systems identified by the neurophysiologists, and that each discrete processing operation in this motor organization was manipulable by different preparatory variables that

would show up as effects in both behavior, and in neurophysiological indices.

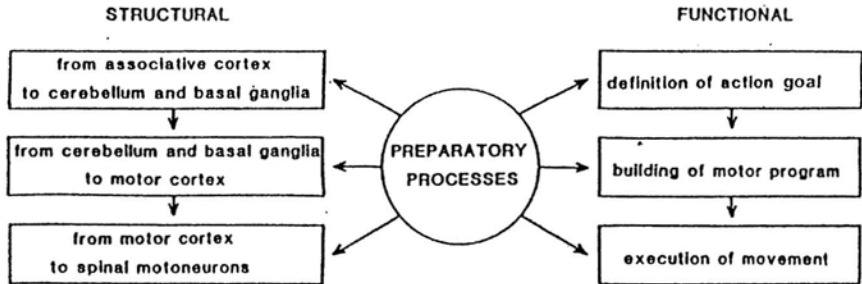


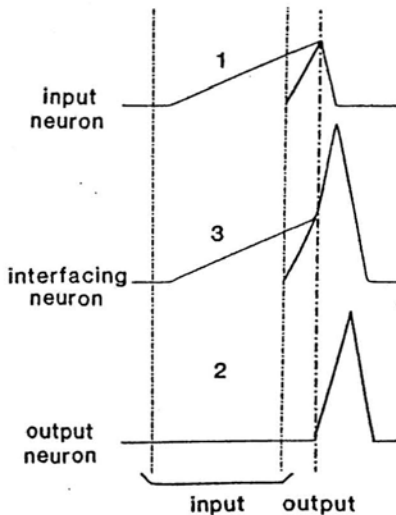
Figure 2. Schematization of a three-stage model suggesting a direct correspondence between structural (in terms of neuronal pathways) and functional aspects of motor organization, each stage being a possible target for preparatory processes (from Requin, Lecas, & Bonnet, 1984).

The evidence, both from his own lab and from the literature, was very encouraging. Stimulus probability seemed to affect the first set of stages; advance information regarding direction seemed to affect the second set of stages; and advance information about distance, or force, seemed to affect the second and third stages.

Note that this is precisely the kind of reasoning and theory construction that would very soon give rise to the structure/function debate in cognitive neuroscience. And this was a problem which, for very good reason, would take up more and more of Jean's time. In fact, it was this set of issues that finally led to the radical proposal that he made shortly before he died.

The next time he looked at this problem was at a much more detailed, finer grained level. The isomorphism that he had proposed rested on the assumption that, both structurally and functionally these stages were serially organized, functionally specialized, and homogeneous. All aspects of this assumption were now coming under increasing criticism and challenge both in psychology and in neurophysiology. For example, Eriksen and Schultz (1979), and McClelland and Rumelhart (1986) in psychology, and Szentagothai (1978), and Mountcastle (1979) in neurophysiology were all proposing radical revisions of the then current views in their respective fields.

As is true in all sciences, as long as one sticks to prototypical cases the problems are relatively few. In this instance, as long as one could speak of sensory or motor neurons, and sensory or motor areas everything was fine (Of course how such different cells or areas speak to one another is another problem). However, the closer one looked, and regardless of what criteria one used, very few neurons or areas were prototypical – there were serious boundary problems. Most cells were either more motor than sensory, or the other way around, and most areas had mixtures of so-called motor and sensory cells. The refusal of the brain's functional anatomy to be rigidly compartmentalized made it hard for theoreticians like Jean. So he went back over some of his old data, ran a series of new experiments and proposed a new way of classifying neurons, and by implication, brain areas. This is schematized on the next two figures.



*Figure 3. Schematic diagram showing the hypothetical arrangement of "input", "interfacing", and "output" neurons and their possible pattern of neuronal discharge in a reaction time task with prior information. The discharge of "interfacing" neurons shows features of both "input" and "output" neurons, with an increase in activity during the preparatory period and also with the onset of movement. In the absence of prior information, this same pattern of discharge (thick line) is observed after the response signal (from Requin, Riehle, & Seal, 1988).*



On Figure 3 we see an activity diagram for the three types of cells that Jean was now proposing. The first vertical dotted line represents the warning signal, the point in time at which advance information is presented on the basis of which the subject could, in principle, prepare the response. The second dotted line represents the response signal. The third dotted line represents the onset of the response. The curves represent the idealized time course, and levels of activation for each of the three types of cells that Jean was proposing. So, for an input neuron, labeled 1 at the top of the figure, we see that the level of activation starts rising as soon as the advance information is presented, continues to rise past the point at which the response signal occurs, and then drops as soon as the response is made. This, Jean called a "truly preparatory" unit. For an output neuron, labeled 2 at the bottom of the figure, there is absolutely no activity until the response is made. This he called a "truly motor" unit. And for the interface neuron, labeled 3 in the middle of the figure, we see some of the properties from each of the first two cells.

Jean proposed that cortical organization was generally based on these three types of cells. And some of his own data supported this conjecture. In particular he had noticed that these three types of cells peaked at different, and successive times. This suggested not only that these cells were arranged in sequential order, but as it turned out, it was the correct sequential order.

Keep in mind that this sort of investigation and theorizing strikes at the very heart of the structure/function issue – an issue that comes up at all levels of resolution from the micro to the macro level. Jean's next move was to try to bridge the cellular and structural levels by putting these three types of cells together into functional units that he called neural modules. These modules, he argued, were the basic constituent units for all cortical areas, as Szenta'gothai (1978) and Mountcastle (1979) had hypothesized earlier: all modules had the same basic properties and performed the same basic operations within a particular area.

On Figure 4 we see the internal structure and connectivity of the three cortical areas that Jean had previously postulated were involved in motor planning. Later he would argue that these modules were linked into networks which, depending on their connectivities, could generate various other properties. At this stage, however, we can ask: If these modules are all composed of the same types of cells, and they perform the same functions, how do we get functional differentiation?

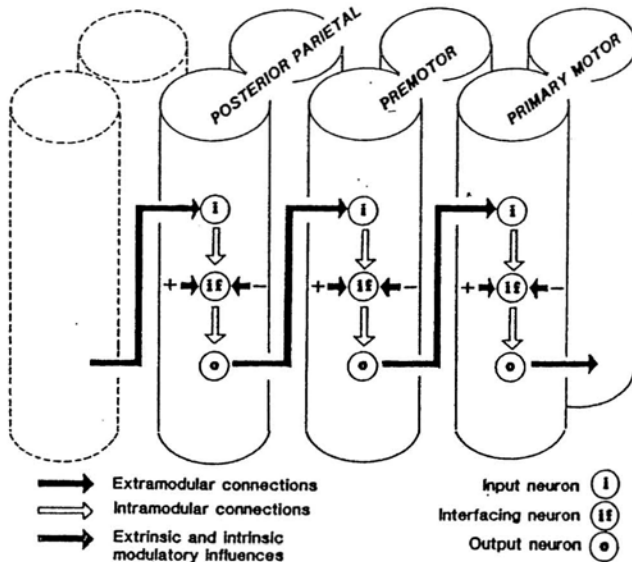


Figure 4. Schematic diagram of the hypothetical anato-functional organization of cortical structures involved in motor planning and execution. Each module is made up of three types of neurons of which the "interfacing" neurons are subject to modulatory influences which facilitate or inhibit the passage of the nervous influx (from Requin, Riehle, & Seal, 1988).

The answer, which really outlines a whole new research program, goes something like this.

Given the functional heterogeneity of neural structures at the macro-anatomical level, and the continuum of function at the cellular level, functional differences between modules could, in principle, be implemented by varying the proportions of sensory, motor, and interface neurons within modules, and functional difference between and within areas could be implemented by varying the proportions of different types of modules within a network.

Thus, for example, as we go from parietal to primary motor cortex, the proportion of input and interfacing cells would decrease, while the proportion of output cells would increase. In fact, Jean was able to verify such differences in proportionality in his own data.

A very important consequence of this analysis is that it transforms the structure/function issue from a qualitative to a quantitative question. And this became the basis of what Jean was beginning to think of as the new paradigm – in the Kuhn sense of the term – in cognitive neuroscience.

He sounded the trumpet of what he called the paradigmatic crisis in cognitive neuroscience in Ann Arbor, in 1990. The occasion was the 25th anniversary of the Attention and Performance Symposia. The three symptoms that he saw as indicative of the crisis were:

1. The strain of trying to understand experimental facts in the context of cognitive boxologies on the one hand, and neurophysiological patchwork models on the other.

2. The strain of trying to hold on to old familiar models by making increasingly complex modifications and additions to them – a bit like the epicycle phase in astronomy, and

3. The proliferation of new and competing theories, none of which, thus far, had been sufficiently compelling to replace the old.

"Most likely," said Jean, "the paradigmatic revolution will emerge from an unexpected synthesis of some of these influential ideas with the old" (Requin et al., 1993, p. 765).

Thus, what in 1962 had begun as a logical imperative (obtaining converging neurophysiological and behavioral evidence of preparatory processes), became in 1995 an attempt to launch a new paradigm based on the total integration of the concepts, methods, data, and models in the neuro- and the cognitive sciences. This new mix, said Jean, implied a radical redefinition of the central problems of cognitive neuroscience and of the framework within which they were to be addressed. In sketching the outlines of this new paradigm, Jean explicitly warned against succumbing either to neurophysiologically based forms of naive reductionism or to cognitively hatched forms of wholistic tyranny (those are his terms): "It is precisely the mutual recognition of the complementarity of the cognitive and neural approaches that will guarantee the richness, and fertility of their integration."

This last statement is taken from a document that established the Center for Research in Cognitive Neuroscience, which he obviously envisaged as the crucible from which this new paradigm would emerge. And as you hear the papers, and see the posters during the next two days, you will see that Jean was really on the threshold of a new phase in his scientific career.

In addition to his personal involvement in the research that I have just finished sketching for you, Jean spent an enormous amount of time and energy establishing and promoting cognitive neurosciences in France. These efforts paralleled, and were a direct expression of the research path that he was pursuing in the lab.

Thus, in 1976, he was in charge of the department of Experimental Psychobiology in the Institute of Neurophysiology. In 1986 this had become the Cognitive Neurosciences Unit. Six years later, in 1992, it became the Laboratory of Cognitive Neurosciences, and in 1996 it was the Center for Research in Cognitive Neurosciences.

Each change in name was accompanied by an increase in the size and scope of the enterprise. In spite of this growth, however, his labs, and now the center that he founded, all retained a remarkable internal consistency. Jean managed this growth the way a portfolio manager handles his investments. He had a buy-and-hold philosophy: whatever stock Jean bought, he bought for the long term. He rarely did any pruning. He simply kept adding, and integrating the additions into what was already there – and he made this work. The result is that there exists in Marseille today one of the world's leading centers for research in cognitive neurosciences, where less than one year after Jean's death, a certain serenity seems to reign, thanks in no small part to the enormous affection, loyalty, and respect that the people have for Jean, and the stabilizing hand of Catherine Thinus-Blanc. Work is proceeding apace.

Shortly before he died, Jean successfully spearheaded a campaign to fund, and to locate a brain imaging facility in the Marseille area, and formed a regional federation of five laboratories and research institutes on brain sciences. This federation is thriving today.

What manner of man was Jean Requin?

Whatever the size of his lab, he was always involved in every aspect of it from its conception, founding, and funding at one end, to the details of setting up a new recruit's labs and offices, at the other. His style of management was unique – he simply roamed the halls, stuck his head into this office or that; would stay for a minute here, or disappear for hours there. In between, he was in his office – writing. Sometimes the writing would come pages at a time – sometimes a sentence at a time. And between pages or sentences, it was halltime – Jean was not one to sit and stare at a blank sheet of paper for very long – and by the time he got back to his office, he would pick up his pen and there would be another page, or another sentence.

Jean's thinking was neither "top down" nor "bottom up" – it was problem driven. Whatever the problem, he tried to find an epistemologically valid way of approaching it with simplicity being a touchstone for the solution. If it wasn't simple, it probably wasn't right.

He was forever skeptical – but always encouraging.

He worked hard, and played hard.

He was a very good sailor, an excellent cook, and an enthusiastic musician.

He was passionate and rational, demanding and supportive, modest as well as proud of what he had accomplished thus far.

He had a wonderful sense of humor, abhorred authority, including his own which was considerable, had a profound dislike of any kind of formality – he may have owned as many as two ties – and would probably be embarrassed by this two day symposium with him as the center piece.

He was a superb scientist, a generous colleague, and a wonderful friend.

According to Spinoza, immortality is the degree to which others remember what we did and who we were. If what's happening here during the next two days is any indication, Jean Requin will be around for a long time.

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