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Robert Rosen

Anticipatory Systems

Philosophical, Mathematical,
and Methodological Foundations

Second Edition

 Springer

Robert Rosen
(Deceased)

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Addendum 1: Autobiographical Reminiscences

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Preface to the Second Edition: The Nature of Life

What you are holding in your hands is a book about biology. About LIFE. It is a book that describes and elucidates, in scientific terms, the causal forces underlying the unique nature of all living organisms. It presents a set of rigorous and logical interlocking ideas which make clear what is generating the familiar behavior patterns we can observe in ourselves and all around us, everyday, and which characterize “life” as a systemic quality or feature of a system.

Life is Anticipatory

Living organisms have the equivalent of one “foot” in the past, the other in the future, and the whole system hovers, moment by moment, in the present – always on the move, through time. The truth is that *the future* represents as powerful a causal force on current behavior as the past does, for all living things. And information, which is often presumed to be a figment of the human mind or at least unique to the province of human thought and interaction, is actually an integral feature of life, itself – even at the most fundamental level: that of system organization.

These are all findings which are described and elaborated here and, in my opinion, this particular book represents the most important of my father’s scientific discoveries. The development of this aspect of his larger theoretical work on relational complexity (lately being called “Relational Science”) is able to explain what have, heretofore, remained the most baffling and inexplicable of organismic living behaviors. It also demonstrates how and why the clues have been missed by science for so long. The current, purely reactive, paradigm for science is able to do many things but it cannot be expected to help us adequately with problems and questions pertaining to living systems if living systems are not merely reactive. This becomes a critical issue because science represents the set of tools humanity uses for exploring and understanding ourselves, our universe, and our place in the web of life here on Earth. We need to be able to trust our tools to help us solve problems in the biosphere without generating side effects worse than the problems, themselves.

Every living organism comes into being with a system-based value for *health*. Emerging from that value for health is a guiding principle that is equally individual: *optimality*. The functional capabilities of life – metabolism and repair – are entirely guided by these two values. What is clear from the outset is that the values pre-exist the business of living. Perhaps *time* is not quite as linear as we have always presumed it to be. My father's view, in fact, was that, "Time is complex."

According to Robert Rosen, the means by which a living system is internally guided and controlled involves encoded information acting as an interactive set of models – of self, of environment, and of relations between the two... through time. These models have the capacity to *predict* next behavior (of self and/or of native environment) based on current behavior. The fact that these are model predictions, as opposed to prescience or prophesy, can be proven by studying the peculiar error behaviors that arise when the encoded information being used in the models no longer accurately represents the systems it was encoded from. For example, if the environment changes, quickly and radically, from the way it has been throughout the recent evolutionary past, the models will no longer be able to reliably predict next behavior of environment from current behavior.

This is what happens when an organism is moved from its native environment and transplanted to some new environment, as in the case of "annual" plants and flowers sold in my neighborhood in Western New York State. Or tropical plants and trees grown indoors in pots, here. Many of these plants are not really annuals, but perennials which can live for many years, sometimes decades and (in the case of trees) centuries. However, they come from environments that never have had a winter like ours, and therefore have no information about it, either. Native plants begin to enter dormancy in mid to late August, here, triggered by various environmental cues such as changing day/night length, but the "annuals" bloom merrily away in pots and in the garden right up until the first freeze kills them outright.

Another proof that these are models can be demonstrated by observing that they can be "fooled." The horticultural industry uses this situation to very good effect: Producing Easter lilies blooming in time for the Easter market, Poinsettias blooming in time for Christmas, and so on. All that is required to trigger initiation of the bloom cycle is to mimic the behavior of their native environment just prior to their natural bloom time. If we have figured out what the triggers or cues are, we can merely mimic those and achieve our ends. It can be quite specific: day or night length, soil or air temperature, temperature differential between day and night, a period of dryness of a specific length... each organism has its own triggers based on the encoded information in its models. All "instinctive" behavior of living organisms is based on the activity of such internal predictive models, generated from encoded information within their own systemic organization. To observe and learn about the annual migration of Monarch butterflies in North America gives us enough evidence to put us in awe of just how detailed the encoded information can be and how powerful is the guiding action of these internal models on the behavior patterns of all living things.

There are stark dangers embodied in this situation, however, which will become clear as you read this book. The dangers stem from the fact that many of the encoded models (and/or the information from which they are constructed) are either not able to be changed within a single organism's life time or else they change too slowly to be able to avoid disaster in a rapidly changing world. There is no way to know, from within a model, that the system it was encoded from has changed radically. The model will keep on making predictions using wrong information – and the organism will still be guided, partly or entirely, by those predictions. If the predictions are inappropriate, the behavior will similarly be inappropriate – perhaps to the point of mismatches that prove lethal to the organism. Because organism species within an ecosystem are so interlinked in their requirements and dependencies, the death of significant numbers of one species can initiate further rapid changes in the behavior of the local environment, which can ultimately cause rapidly escalating cascades of extinctions.

This is the Achilles heel, the innate vulnerability, of all anticipatory systems. With human-induced changes to the composition of Earth's atmosphere happening at an unprecedented pace over the past two hundred years, and the further unknown changes which are likely to be caused by them, we would do well to pay very careful attention to the warning that is inherent in these facts. Any model-based guidance system will only be as good as the encoded information it uses. If a model is constructed using inaccurate information, its predictions will be unreliable – and this is as true in science as it is in the guided behavior patterns of organisms.

Indeed, because human beings are living organisms, we are also anticipatory systems. The lessons we can learn and apply from this will impact everything. Something as ordinary and commonplace as *how we construct our food pyramid* should be based on what our bodies have encoded as “food”. My hope (my prediction) is that the ramifications of these ideas can expand the paradigm of science itself. The effects of doing so would benefit everything from medical science to psychology, social science, political science, economics – in fact: anything that involves human physiology, human thought and learning, and human interactions with each other or with the biosphere.

The nature of the human mind, in particular, has so far eluded most of the attempts we have made in science to understand it. We seek to comprehend both the nature of our own consciously aware mind and its origins – how it came into being. The final ground-breaking aspect of this book is that it realizes, within the fundamental theory being developed here, that the similarity between life and mind is simply that both are anticipatory systems. The peculiar, anticipatory nature of the mind, at the behavioral and physiological level, was described philosophically a couple thousand years ago (for example, in Buddhist teachings like the Satipathana Sutta) but it has never been explainable via science, until now. Anticipatory Systems Theory elucidates how it can be that both body and mind run on the same principles of model-based guidance and control.

In that light, it becomes clear that the only reason the human mind can anticipate is because life was already *that way*. Thus, the human mind is merely an evolutionary concentration of the same information-sifting, encoding, and model-

building capacities of living system organization. It is a model-building tool that is capable of error-checking and re-encoding faulty models – but in real time versus evolutionary time. The accuracy and speed of an organism’s ability to model and predict would naturally be something that selection pressures could act on. The boon to survival that human intelligence and imagination represents – allowing us to work around all manner of physical limitations in our environments and ourselves – certainly correlates with how overpopulated humanity has become.

However, the recognition in recent years of the need for human societies to be sustainable in order for us to maintain our own health and well-being, over time, brings with it an awareness that there is an acute and urgent need for scientific answers that we can rely on pertaining to biological questions, problems, and issues. When time is short, trial and error is hardly the most efficient or productive mode for sorting out our choices. Because our own welfare is inextricably bound up with the welfare of the biosphere, humanity will need to consider a much larger set of values for optimality than we have ever needed to use in the past. This book will be essential for helping humanity expand the scientific paradigm in such a way that it can finally be trusted to answer biological questions *accurately* and give us scientific models and model predictions that can *reliably help us in choosing the most optimal pathways towards a healthy and sustainable future*.

It is for all these reasons and more that I have worked to get this book republished in the form of an expanded Second Edition. An entire new area of science has already begun to spring up around this work, but without access to the theoretical underpinnings to guide its growth, I fear that it will be prone to develop improperly. I also wanted to include some of the new science, recruiting scientists I know personally who are developing it; allowing them to describe what they are doing and show the applicability of it. The true test of any theory is to put it into practice and see if it holds – see if it generates results and check that the results are beneficial. I think the evidence is conclusive. I leave it to the reader to decide whether I have my own models properly encoded, or not.

If it is true that knowledge is power, then this book is powerful, indeed. Use it wisely, and well.

Judith Rosen

[Note to Readers: Do not be intimidated by the mathematical notation in this book! In discussions with my father on this subject, he said that the mathematics represent *additional illustration of ideas already described in prose*. It was his form of “bullet-proofing” as well as whatever value could be made available to readers from absorbing the same ideas in a different way. I specifically asked him whether one needed to understand the math to fully comprehend the work and he said, “No.” Therefore, if advanced math is something you have not been trained for, concentrate on the prose and ignore the mathematical illustrations. You have Robert Rosen’s own assurances that you will not be missing any essential information by doing so.]

Prolegomena: What Speaks in Favor of an Inquiry into Anticipatory Processes?

Mihai Nadin

The book you hold in your hands is part of a larger intellectual endeavor. It can be read and understood as a stand-alone monograph. Yet to fully realize its predicaments and appropriately understand it, the reader would be well advised to become familiar with the entire trilogy: *Fundamentals of Measurement and Representation in Natural Systems* (FM, 1978); *Anticipatory Systems. Philosophical, Methodological and Mathematical Foundations* (AS, 1985); and *Life Itself. A Comprehensive Inquiry into the Nature, Origin, and Fabrication of Life* (LI, 1991). In their unity, they define Robert Rosen, their author, as an original and provocative scholar. Each of the three books is a self-contained monograph; but only understood in their unity, indeed, as a trilogy, do they provide the reader the opportunity to perceive their interconnectedness. To measure life is different from all other forms of measurement; to understand life is to acknowledge complexity. And once complexity is accounted for, anticipatory processes, corresponding to a comprehension of causality that transcends determinism, can be accounted for as a characteristic of the dynamics of the living.

Preliminaries

Mathematics and biology are subjects that rarely blend. This does not have to be so. Quite a few mathematicians and biologists have brought up their own frustration with each other's methods. Indeed, some biologists – at least those with a “traditional” background – would confirm that their passion for science found its reward in a domain of inquiry that was less mathematical than others. Computation, which after all is automated mathematics, is affecting a change, but not yet in the sense that biologists suddenly become passionate mathematicians. Robert Rosen's entire work – and in particular his contributions to the understanding of what defines the living – utilizes mathematics and biology. This is a difficult position: mathematicians are not necessarily conversant in matters outside their knowledge domain (which they consider universal); biologists prefer descriptions different

from those specific to mathematics. Given the difference between mathematical and biological knowledge, and given the fact that the respective practitioners are not conversant in each other's "language," Rosen, educated and fluent in both mathematics and biology, had a tough time in seeing his work accepted, and an even tougher time in having it properly understood.

These preliminary remarks are undoubtedly connected to the choice I made to write *Prolegomena* – not an Introduction – to the second edition of Rosen's *AS*. It was Immanuel Kant who, after his disappointment with a superficial and less than competent review of his *Kritik der reinen Vernunft* (1781; *Critique of Pure Reason*, as it is called in English), wrote *Prolegomena to Any Future Metaphysics That Will Be Able to Present Itself as a Science* (1783). Among the many questions he raised, "How can the intuition of the object occur before the experience of the object?" is of particular significance to the subject of anticipation. *Prolegomena* are to be understood as preliminaries for defining a subject. As a matter of fact, Kant, who himself indulged in mathematics, paid quite a bit of attention to *prolepsis*, which predates the word *antecipere* and the subject of *anticipation*. It represents the pre-conception, i.e., the preliminary understanding that eventually leads to our knowledge of the world (cf. Epicurus and the so-called *Stoa* – phases in Stoicism – whose ideas were conveyed indirectly through Roman texts). Although the concept of *prolepsis* is associated with anticipation, I will not venture to claim that anticipation as a knowledge domain originates 300 years BCE, or even from Kant. His *Prolegomena* are rather an inspiration for framing Rosen's *AS*. We know how the first edition was reviewed; we also know how it fared and, moreover, why a second edition is necessary. (Incidentally, Kant produced a second edition of his *Critique of Pure Reason* in 1787, i.e., six years after the first. The *Prolegomena* were very useful in clarifying some of the ideas and misconceptions of his book on reason.)

In the spirit in which Kant was mentioned, it is significant that Rosen's "trilogy" Measurement – Anticipation – Life (cf. also Louie 2008, p. 290) be juxtaposed to that of the famous idealist philosopher: *Pure Reason – Practical Reason – Judgment* (Kant 1781, 1788, 1790). The architecture of the arguments, leading to a coherent view, can only be properly recognized if we look at the whole. Admiring the entrance to a cathedral (or any other architectural creation) is never so convincing as the exploration of the entire edifice and the experience of the space and time it encapsulates.

In these preliminaries, I would also like to associate Kant's question regarding the role of intuition, as an anticipation, to some recent scientific reports. "Adaptive prediction of environmental changes by microorganisms" (Mitchell et al. 2009); or "Early-warning signals for critical transitions" (Scheffer et al. 2009) or "Stimulus Predictability Reduces Responses in Primary Visual Cortex" (Alink et al. 2010), to name recent works, are answers, even if only indirect, to his question. They qualify, however, as examples, not as foundational theories, of how far research in anticipation reaches out in addressing various aspects of life and living. The fact that even the most rudimentary forms of life (microorganisms) display anticipatory characteristics clearly aligns with Rosen's description in *AS* of the monocell and its phototropic behavior. Furthermore, perception processes, in particular those related

to seeing, allow for an optimum in brain functioning (“energy saving” feature). They correspond to our understanding of anticipation as an underlying process from which the evolutionary process benefits.

But before Kant, the future – which is essentially the focus of anticipation (regardless of how we define it) – was of interest to the most intriguing mathematics. Were mathematicians such as Blaise Pascal, Pierre de Fermat, or Jakob Bernoulli addicted to gambling (of one sort or another), or actually intrigued by the possibility of delivering predictive or even anticipatory descriptions? Winning the lottery could be rewarding; but so can the prediction of a successful course of action. Bernoulli’s *Ars conjectandi* (which [Collani 2006](#)) brought to my attention as a precursor of anticipatory modeling), and for that matter game theory, in particular the work of John Forbes Nash, Jr. years later (which afforded him a Nobel Prize) are yet other examples impossible to ignore as we define a context for understanding what anticipation is. The cultural context is very rich and obviously extends to art and literature. The examples mentioned are only indicative of the variety of perspectives from which we can examine the subject of anticipation. With the exception of Bernoulli’s book, whose object is conjecture in its broadest sense, in our days we encounter a rich variety of examples of particular predictive efforts. Humankind is pursuing increasingly risky endeavors: space exploration, genetic engineering, deep ocean oil exploration, financial instruments (e.g., derivatives). Therefore, the need to develop predictive procedures for evaluating the outcome increases.

Particular perspectives and specific areas of investigation (cognitive science, artificial intelligence, ALife, synthetic life, economics, aesthetics, among others) – which will be mentioned in the Prolegomena – are rather illustrative of the ubiquity of anticipatory processes. But they are not a substitute for a scientific foundation, which can only be multidisciplinary. Rosen’s *AS* conjures not only philosophy, mathematics, and methodology (identified as such in his book’s subtitle), but also developments in all those sciences that made possible the current focus on the living. Therefore it qualifies as a theoretic contribution upon which the community of researchers interested in anticipation can further build. For instance, no one can ignore that advances in understanding a variety of forms of creativity (scientific, technological, artistic, etc.) have allowed for a higher acceptance of the notions associated with anticipatory systems. However, such progress has not yet resulted in a better understanding of creativity as an expression of anticipation.

In March 2008, Springer Publishers contacted me regarding its intention to publish a second edition of *AS*. Since that time, the “state of the art” in anticipation research has continued to change on account of a rapidly increasing body of experimental evidence (cf. [Nadin 2010a](#), [Nadin 2011a](#), [2012a](#)), as well as research on the conceptual level. It became obvious to me that to write an Introduction in the classical form would not do justice to the book, to the author, to the subject. And it would not contribute to the type of research that led Rosen to write his book as part of the trilogy mentioned above. *Anticipatory Systems* is being republished in a new context. It can, and should be, read having *FM* and *LI* in mind. Albeit, the second edition is not only the occasion to correct errors (characteristic of publications in the pre-word processing age), but also to understand the impact of the notions it

advanced. The assignment implied a commitment I was not sure that I was willing to make, even after having argued in favor of a second edition of *AS*. My own research (2009a, 2010b, 2010c) takes the best of my time and effort. Many colleagues (whose opinion I asked for) guided my own decision-making and generously provided extremely useful references. In the meanwhile, and on the initiative of George Klir, General Editor of the Series, I prepared a special issue on Anticipation for the *International Journal of General Systems* (Nadin 2010a) – the Journal that previously published Rosen’s articles. This was followed by another thematic issue on anticipation (Nadin 2011b, Nadin 2012b). And when, finally, the Prolegomena became an inescapable project, I was able to build upon the effort made so far in understanding Rosen’s work, the context in which it was published, its impact. Distinguished colleagues who respect Rosen’s work helped. Given my own long-term dedication to the subject, it is clear that these Prolegomena (in the Kantian tradition) are meant to provide a context in which the work of many, together with my own, will be related to that of Robert Rosen.

Allow me a presumption here: The fact that there is so much to consider in defining the context would have pleased him. I wish that Robert Rosen himself could have written the preface to this second edition of his book; but I will not, by any means, second-guess him.

The Path to the Publication of *AS*

The perspective of time and the evidence of increasing interest from the scientific community in understanding anticipatory processes speak in favor of describing the perspective from which anticipation was initially defined. Moreover, the ever-increasing variety of understandings associated with the concept invites clarification. In a nutshell, this is all I want to provide in these Prolegomena. Twenty-seven years ago, Robert Rosen’s book on anticipation first reached its readers. It was actually written in the first six months of 1979, but did not have an easy path on the way to publication. My own book, *Mind – Anticipation and Chaos* (1991) introducing the concept of anticipation was published six years later (it was also written well before publication).

Rosen’s scientific legacy is the result of scientific commitment that excluded compromise as a path to acceptance or recognition. It also excluded self-delusion: In his will, Rosen left his work (and library) to Judith, the daughter who stood by his side until his untimely death, and who does her very best to share what she learned from her father with those who are interested. The testament came with the following proviso: “You don’t have to do anything with it, Jude. In my opinion, either it continues based on its own merits or it dies a natural death. If it can’t stand on its own merits, then it doesn’t deserve to,” (Rosen 2008). For the reader inclined to see in the publication of the Second Edition of *AS*, a contradiction of Robert Rosen’s position, only this need be said: Many young (and not so young) researchers asked for it. Interest in someone’s writing is an acknowledgment of merit. It is worthwhile mentioning that the book continues to be frequently cited

(sometimes through secondary sources). Given the limited access to the original book, misinterpretations, prompted by less than adequate citations (errors in the first edition transmitted from one quoting author to another) sometimes led to confusion. One of those who helped me in preparing this text (Olaf Wolkenhauer, Systems Biology, Rostock University) recently wrote to me: “We are getting tired of guessing what Rosen meant.” At least as far as *AS* is concerned, no guessing should replace consulting the work itself.

How *AS* came to be published deserves a short note because science and integrity, associated with originality, cannot be conceived as independent of each other. Based on some accounts, Rosen’s manuscript was reviewed by an old nemesis of Rashevsky, and publication was quashed. Six years later, it eventually became the first volume in the *International Series on Systems Science and Engineering*, published by Pergamon Press, under the heading of the International Federation for Systems Research. The Federation itself (founded in 1980) was no less controversial. In the spirit of the activity of those who set the foundation for a system’s approach (Wiener 1948, Ashby 1956, Bertalanffy 1968), it challenged excessive specialization, advancing a holistic view of the world. George Klir, its first president, practiced a systems approach focused on knowledge structures. He met Robert Rosen in 1970, after visiting von Bertalanffy at SUNY–Buffalo (where von Bertalanffy, professor in the School of Social Sciences introduced him to Rosen). They remained in touch, and between 1971 and 1972 they explored the feasibility of a new journal. In 1974, the first issue of the *International Journal of General Systems* was published. In acknowledging Rosen (“He was in some sense a co-founder of the Journal”), Klir makes note of the fact that Rosen was an active Member of the Board and published constantly from the first issue on. In 1975, an opening at the Department of Systems Science at SUNY–Binghamton prompted Klir to recommend to his Dean, Walter Lowen, that the University recruits Robert Rosen. (Rosen described a visit by Klir and Lowen to Buffalo in 1974.) When asked, Rosen, a scientist of high integrity, would not consider moving without his closest collaborators (Howard Pattee and Narendra Goel) at the Center for Theoretical Biology at SUNY–Buffalo. In the end, as the Center was abolished in 1975, Rosen accepted the generous offer from Dalhousie University (Killam Professor, “like a five-year sabbatical”) in Nova Scotia, Canada, while Pattee and Goel moved to Binghamton; they all remained in touch. In 1978, Klir published Rosen’s *FM* in the General Systems Research Series (also a first volume in the series) of Elsevier. Trusting Klir, Rosen finally submitted *AS* to him. Klir realized that it was a difficult text, by no means typical of publications on biology, systems theory, mathematics, or philosophy – disciplines that were integrated in this text – of that time, or of any time, for that matter. His interest in Robert Rosen’s work was the result of numerous interactions in which scholarship and character proved flawless. One of the reasons I accepted to write these Prolegomena is because George Klir deserves our respect for his dedication to scientific integrity. He realized the need for this second edition and helped in making it happen.

As far as my inquiry regarding the beginnings of Rosen’s interest in the subject of anticipation shows, the early 1970s were pretty much the time of his attempts to specifically address the subject. But let’s be more precise, not for the sake of

archival passion, rather in order to place the subject in the larger framework of his research. Since 1957, when Rosen joined Rashevsky's Committee on Mathematical Biology at the University of Chicago, he was prepared to make relational biology the reference for his own theoretic work. Rosen's discovery of the "(M,R)-Systems" was the starting point. In order to define the living in its concrete embodiment as organism, he advanced a class of relational models called "(M,R)-Systems". M stands for metabolism; R for repair; and the system defines relational cell models that describe organisms. In this view, the object of inquiry of biology is the class of material realizations of a particular relational structure expressed in the "(M,R)-Systems". From here on, a large body of publications testifies to the intellectual effort of defining what life itself is. A material system is an organism if and only if it is closed to efficient causation (Rosen 1991). It is worth noting that the definition is focused on causality (in the Aristotelian tradition of material, formal, efficient, and final causes); also, that life is embodied in organisms. The matter of organisms is important; but what defines life is *what* organisms actually do – moreover, *why* they do it. In his *LI*, Rosen dealt with the "necessary condition, not sufficient one, for a material system to be an organism." Complexity, which is characteristic of life, and which in the final analysis explains anticipation, "is the habitat of life . . . not life itself." Let us take note that complexity is defined ontologically – as pertaining to existence – not epistemologically – as pertaining to our knowledge of what exists. Rosen ascertained that something else is needed to characterize what is alive from what is complex (Rosen 2000). The simplest "(M,R)-system" is one of replication, repair and metabolism entailing one another. It is, of course, a formal representation of the living (organism, cell).

Between *AS* and his next book, Rosen's original contributions result from a process of discovery impressive in its breadth. His subject was neither more nor less than what life is, after all. The manuscript of *AS* submitted to Klir was conceived over a long time, with many of its hypotheses subjected to discussions in various colloquia, seminars, and conferences. The original manuscript (typed) – preserved by his most talented student (Rosen's own characterization of Aloisius Louie) – went through a judicious editing process. In science, new perspectives rarely break through on account of their soundness. And even less on account of their novelty. Like anyone else in society, scientists are captive to their respective views and quite often unwilling to allow opposing views to enter the arena of public debate. A rather nasty review of one of Rosen's books, *Dynamical System Theory in Biology*, Vol. I, 1970 (Cohen 1971), made Rosen give up publication of the second volume (the manuscript still exists), and even decide to never again write or publish textbooks. The review states, "Physics envy is the curse of biology," (p. 675). Rosen was not in the envy business. Respectful of physics, and working closely with physicists, he rather questioned a physics-based foundation of biology and tried to advance an alternative foundation. Let it be spelled out: In this respect, his approach and my own have much in common.

This brings up the second reason for accepting Springer's invitation: Robert Rosen himself. No, I was not one of his students or colleagues. And I have to confess that when I first heard his name – brought to my attention in the spring

of 1992 by a former graduate student of mine, Jeffrey Nickerson (currently Director of the Center for Decision Technologies, Howe School of Technology Management, Stevens Institute of Technology, Castle Point-on-Hudson, NJ) – I could not relate it to anything of interest to me at that time. My book, *Mind – Anticipation and Chaos*, the result of inquiries into cognitive processes and semiotics, gave me the illusion of being the first to project the notion of anticipation into scientific dialog. In the 1970s, my interest in the scientific foundation of aesthetics (information aesthetics, in particular) prompted the realization that artistic endeavors, and by extension creativity, imply anticipatory processes (cf. Nadin 1972). But, as I would discover, that was also the time when Rosen started his own inquiry into the relevance of predictive models in society. I was wrong in my illusion of being the first, but not unhappy that an intellectual of his worth considered anticipation a subject of scientific interest. The relative simultaneity of our independent interest in the subject is probably not accidental. In the second part of these Prolegomena, I shall return to the “broader picture,” i.e., to all those persons who at that particular time (after a world war with millions of victims) were asking questions pertinent to a future that seemed unstable (the cold war, nuclear weapons, economic and political instability). It is by no means incidental that such matters preoccupied researchers in the “free world” (as the West defined itself) as well as in the self-described “communist paradise” (the Eastern Block and the Soviet Union). That was a time of many events in which the future begged for some acknowledgment. My library research (at Brown University, Ohio State University, and even at OCLC in Columbus, Ohio) on the key words *anticipation*, *anticipatory systems*, *anticipatory processes* did not identify anticipation as a subject. Nickerson’s own inquisitive energy helped me discover Rosen’s book, which I wish I had read before writing mine. The result of this late discovery was my letter of March 24, 1995 to Rosen. He answered, “I wasn’t aware of your book either. It looks beautiful. I think books should be works of art.” He went on to say:

I’ve obliquely been continuing my work on anticipation, which as you know is a very large subject; in fact, a whole new way of looking at the world. It played a major role in developing a concept I called *complexity* [italics his].... I have another book (*Life Itself*, Columbia University Press, 1991) in which this aspect is developed.

We exchanged copies of our books; and I was hoping that we would meet. It was not to be. During my sabbatical at Stanford University (Spring, 1999), Dr. Daniel Dubois, of the now firmly established Center for Hyperincursion and Anticipation in Ordered Systems (CHAOS), at the University of Liège, Belgium, invited me to prepare a presentation for the Conference on Computing Anticipatory Systems (CASYS, 1999). I dedicated my text to Robert Rosen’s memory. (Later, during a lovely private conversation, Dubois mentioned that he was not aware of Rosen’s premature death, although he knew that Rosen had been severely ill.) Yes, I still feel a sense of loss. I owe it to his memory to write these Prolegomena, even though (as stated above) I wish that he himself could have prefaced this second edition.

It is probably less important to the science to which we are dedicated than to maintaining a sense of academic interconnectedness that such considerations

be shared. The reason I brought them up is straightforward: Many in academia respected Robert Rosen, but the extent to which academia accepted his work by no means reflected an understanding of its originality. It is worth mentioning that Mickulecky – who identifies himself as a colleague – called him “the Newton of biology,” (Mickulecky 2007), but it is also inconsequential. So is the title “Biology’s Einstein” (Staiger 1990). Hyperbole does not carry value judgments, rather emotional content. As we shall see, only two authors reviewed *AS*: one was a graduate student, stimulated to do so by George Klir; the other was a Hungarian scientist interested in control theory. A prior book (*Theoretical Biology and Complexity*, 1985b), edited by Rosen, in which Rosen’s own text brought up anticipation, was also barely reviewed, and mostly misunderstood. Even now, when anticipation is no longer a concept prohibited in scientific discourse, hundreds of articles are published with nominal reference to *AS*, and only very rarely with the understanding of Rosen’s comprehensive epistemology. This should not be construed as an expression of anti-Rosen sentiment. Both hyperbole and marginal acknowledgment define yet another reason to give researchers the foundational work that gives meaning to their inquiry and helps in advancing research in the field. The very active discussion on the Rosen mailing list (rosen@home.ease.lsoft.com) on aspects of Rosen’s writings – Rosen would at most have glanced at such mailing lists – will probably benefit from the publication of this book.

In the Perspective of Relational Biology

The purpose of these Prolegomena is by no means to explain *AS*. It speaks for itself, in a crisp but not facile language. After so many years since its publication, it continues to stimulate contributions – theoretic and experimental – of significance. But the context has changed. The life sciences pretty much take the lead today; no need to envy physics. (In a recent book, Louie smuggled in a polemic note: “Biology-envy is the curse of computing science;” cf. 2009, p. 261.) The pendulum has swung from the obsession with physics and chemistry – no less respectable and reputable in our time – to infatuation with genes, cells (stem cells more than any other), protein folding, and synthetic life. What has not changed is the need to understand the fundamental characteristics of the living. And this is what distinguishes Rosen from most researchers in this domain. These Prolegomena to the second edition of *AS* actually report on the state of the art in anticipation research. In other words, they present the indisputable impact that *AS* has had, regardless of whether those who speak “prose” (to bring up Molière’s character, Monsieur Jourdain) know that they do it – i.e., are aware of Rosen’s work – or do not – i.e., have never heard of him, or discard his work as speculative. For someone outside academic dialog, this could appear as an attempt to see how ideas percolate. The psychology of the process (the obsession with “who was first,” “who owns what,” or “whose understanding,” etc.) is a subject for others, especially because this never affords better understanding of the subject.

While he was working on *AS*, which was *in status nascendi* for almost 15 years, Rosen pursued a very ambitious research agenda. His doctoral thesis, which advanced the “(M,R)-Systems” opened a new perspective within which anticipation is only one aspect. This needs to be brought up since in his autobiographical notes, Rosen made a specific reference: “... the “(M,R)-Systems” have an inherent anticipatory aspect, built into their organization.” Still, as we read his *Autobiographical Reminiscences* (2006a), it becomes clear that in implicit terms, the entire focus on relational biology, in line with Rashevsky’s view, is conducive to a line of inquiry that ultimately questions the centuries-old reductionist-deterministic foundations of biology. This is the crux of the matter. Expressed otherwise, the seed of inquiry leading to anticipation is housed in the new perspective from which the “(M,R)-Systems” are derived as a dynamic description of the living cell, obviously contrasted to the atomic model inspired by physics, which is reactive in nature. The biologist, the mathematician, and the philosopher fuse into a new type of scientist. He is no longer willing to further build on the Cartesian foundation, but rather taking up the challenge of submitting an alternative fundamental understanding. Rosen himself brought up the work of Schrödinger (1944), Wiener (1948), and Shannon (1948), as well as game theory, and especially Systems Theory, in particular Bertalanffy and Ashby. Let me quote: “To me, though, and in the light of my own imperative, all those things were potential colors for my palette, but not the palette itself” (Rosen 2006b).

On previous occasions, I pointed out that a name is missing here, and not through some malicious intention: Walter M. Elsasser. Rosen was aware (and respectful) of Elsasser’s work and occasionally referenced his work. The reason we must evoke Elsasser is to provide a broader view of the work in which Rosen was engaged. Educated as a physicist, Elsasser made it the major focus of his work (after he arrived in the USA) to challenge the reductionist understanding of the living. His book, *The Physical Foundation of Biology* (1958, also by Pergamon Press) followed by *Atom and Organism* (1966) and *The Chief Abstraction of Biology* (1975) was a daring attempt to look at what makes life what we know it to be. I would like to point out that *Reflections on a Theory of Organisms* (1987) was, by no coincidence, almost simultaneous with Rosen’s *AS*. (His Measurement book and Elsasser’s book on abstractions of biology appeared in 1978 and 1975, respectively, from the same publisher.) I already confessed that I was unaware of Rosen when my book, *Mind – Anticipation and Chaos*, appeared. I was unaware of Elsasser, as well. During my sabbatical at the University of California–Berkeley (Electrical Engineering and Computer Science), his work was brought to my attention by Dr. Harry Rubin, a distinguished molecular biologist, who wrote the Introduction to the 1998 edition. Elsasser opposed a holistic view to the reductionist model. He proceeds from within the physics to which he remained loyal (given his formative years, cf. *Memoirs of a Physicist in the Atomic Age*, 1977). Under the guidance of, or in interaction with, Werner Heisenberg, Wolfgang Pauli, Albert Einstein, John von Neumann, Hans Bethe, Max Born, Arnold Sommerfeld, and, not to be omitted, Erwin Schrödinger – whose questions “What Is Life?” he adopted, Elsasser tried to provide a foundation for biology as strict as that of physics. Their interaction stimulated the reformulation of fundamental biological questions.

At this juncture, I felt the need to establish the context within which Rosen's contributions were made, in particular those leading to defining anticipatory processes. To this context belongs the activity of the Center for the Study of Democratic Institutions – the brainchild of Robert M. Hutchins, who for many years served as President of the University of Chicago. As seductive as it is, the history of the Center would take us off course from our present aim. It suffices to mention two things. First: Rosen explicitly acknowledged the impact of the Center as his book came together: "The original germinal ideas of which this volume is an outgrowth were developed in 1972, when the author was in residence at the Center for the Study of Democratic Institutions" (1985a). Second: as a member of the Center, Rosen contributed discussion papers over many years. These texts are highly significant to our better understanding of the implications of his research on anticipation. While the fundamental questions led to a new perspective, none of the hypotheses advanced remain exercises in formal biology. Rosen was a very engaged individual; in his own way, he was an activist. He lived his time; he wanted to understand change; he obliged explanations, and even methods of improvement. It was unfair of many of the commentators of his work to see in him a rather esoteric researcher, disconnected from reality, only because his arguments were often accompanied by or articulated in the extremely abstract language of mathematics, in particular, category theory (which he adopted for his uses in biology almost as soon as it appeared). He was a beneficiary of the two "fathers" of category theory: Eilenberg at Columbia University, and MacLane at the University of Chicago (1945, 1950). Subjects such as planning, management, political change, and stable and reliable institutions informed presentations he made at the Center during the 1971–72 academic year, when he was a Visiting Fellow. In an article published seven years later (1979), Rosen made this explicit:

I have come to believe that an understanding of anticipatory systems is crucial not only for biology, but also for any sphere in which decision making based on planning is involved. These are systems which contain predictive models of themselves and their environment, and employ these models to control their present activities (p. 11).

Hutchins conceived the Center as an intellectual community united in the Dialog:

Its members talk about what ought to be done. They come to the conference table as citizens, and their talk is about the common good [...] The Center tries to think about the things it believes its fellow citizens ought to be thinking about (as cited by Rosen 1985a, p. 3).

Among those working at the Center were political scientists, journalists, economists, historians, even philosophers, but not natural scientists. Still, Hutchins' vision and position of principle gave Rosen's presence meaning, as he himself noted, quoting from this visionary intellectual: "Science is not the collection of facts or the accumulation of data. A discipline does not become scientific merely because its professors have acquired a great deal of information." And yet another:

The gadgeteers and data collectors, masquerading as scientists, have threatened to become the supreme chieftains of the scholarly world. As the Renaissance could accuse the Middle Ages of being rich in principles and poor in facts, we are now entitled to inquire whether we are not rich in acts and poor in principles (p. 12).

This was not different from Elsasser's arguments, and was so close to Rosen's own thinking that Rosen realized that working on a theory of biological systems allowed him to formulate the characteristics of biology as an "autonomous science," for which he would then suggest means to formalize. He made a major observation:

The physical structures of organisms play only a minor and secondary role [...] The only requirement which physical structure must fulfill is that it allow the characteristic behaviors themselves to be manifested. Indeed, if this were not so, it would be impossible to understand how a class of systems as utterly diverse in physical structure as that which comprises biological organisms could be recognized as a unity at all.

The Social and the Biological

Rashevsky's relational biology, which Rosen helped develop, stands in contrast to the then dominant analytical approach. Rosen's approach was to focus on functional aspects, on understanding behaviors. John Wilkinson, a Senior Fellow at the Center, extended the invitation to Rosen hoping that his own focus on structure would benefit from interaction with a person focused on function. The parallels between biological processes and social structures led to one of those questions that only Rosen would formulate: "What would it mean if common models of organization could be demonstrated between social and biological structures?" (p. 13). It was very enticing for him to see a variety of disciplines finally cooperating, as it was a challenge to characterize the dynamics of life without having to account for underlying causal structures. In societal situations, the aggregate behavior, involving a multitude of processes, appear quite differently to an observer than to those involved. No less enticing were the considerations regarding the use of social experience as a means for deriving biological insights, and reciprocally, the possibility to develop insights into properties of social systems by building upon biological experiences. Rosen confessed:

In short, the Center seemed to provide me with both the opportunity and the means to explore this virgin territory between biology and society, and to determine whether it was barren or fertile. I thus almost in spite of myself found that I was fulfilling an exhortation of Rashevsky, who had told me years earlier that I could not be a true mathematical biologist until I had concerned myself (as he had) with problems of social organization (p. 14).

From this attempt to establish homologies between social and biological organization, Rosen expanded to predictive models, and realized that the stimulus-reaction explanatory concept could not account for situations in which subjects predict consequences of their own actions, moreover, for situations in which a course of action is changed not as a result of stimuli, but in accordance with a subject's predictive model. The switch from descriptions limited to reactive behavior to the much richer descriptions of what he termed anticipatory behavior resulted from a different understanding of the living. That the agency through which predictions are made turns out to be a model corresponds to the fundamental contributions Rosen

made in defining the “(M,R)-Systems”. It was noted by Kercel (2002, 2007), among others, that Rosen’s epistemology defines properties of logical and mathematical structures. *Impredicativity* is such a property – every functional aspect of the model is contained within another functional component. As we shall see shortly, this is the case of the system and its model unfolding in faster than real time, i.e., definitory of the model, not of reality as such. This means that once we acknowledge the complexity of natural systems, we need the appropriate concepts to describe them, under the assumption that a natural system whose entailment structure is congruent with an impredicative model. But as pervasive as anticipatory behavior was, it was not yet operational in the sense of being easy to translate into a coherent theory, and even less in applications to problems of forecasting and policymaking that were the focus of the Center. The “fortuitous chain of circumstances” described in Rosen’s paper explain why his involvement with the Center can be characterized as yet another element of the context that inspired him, as well as others (members of the Center or not), in questioning the entire analytical foundation of reductionism and determinism.

Readers of *AS* already know that major consideration is given to causality, in particular to phenomena that involve purpose or, alternatively, a goal. It is not the intention of these Prolegomena to offer a *compte rendu* of the book, or of Rosen’s view of anticipation. As already stated, the hope is to provide here a frame of reference. Therefore, from among all those whom Rosen named in his autobiographical notes (and in many other writings), we need to highlight Aristotle, whose work on a typology of causes remains a constant reference. *Causa finalis*, eliminated from scientific vocabulary with the same epistemological fury as vitalism was, found a new champion in Rosen. His strict understanding of dynamics implies finality.

One final note about the onset of Rosen’s work on anticipatory behavior: “Planning, Management, Policies and Strategies: Four Fuzzy Concepts” (1972) was the first of a number of working papers that define his research agenda at the Center. The first lines of this paper could have been written in our days:

It is fair to say that the mood of those concerned with the problems of contemporary society is apocalyptic. It is widely felt that our social structure is in the midst of crises, certainly serious, and perhaps ultimate. [...] The alternative to anarchy is management; and management implies in turn the systematic implementation of specific plans, programs, policies and strategies (p. 1).

Rosen brought up the conceptual requirements for a methodology (“a ‘plan for planning’”) that would allow avoidance of “an infinite and futile anarchic regress.” Given the audience at the Center, i.e., none with a background in mathematics, biology, systems theory, or the like, Rosen built his arguments in favor of defining anticipatory behavior in an almost pedantic manner. But in essence, it was within this context that the major ideas of his future book on anticipatory systems were articulated. The intellectual profile of his listenership and the broad goals of the Center, which Rosen explicitly adhered to, had an impact on formulations, examples, and the general tone. Aware of the fact that “how planning could go wrong” was on the minds of his Fellows at the Center, he explicitly addressed

the question, taking note of the fact that a system's integrated perspective is not bulletproof, just as "the defect of any part of a sensory mechanism in an organism leads to a particular array of symptoms," (1972, p. 3 and 1974, p. 250). At the center of his conception is the "Principle of Function Change": the same structure is capable of simultaneously manifesting a variety of functions. Rosen remained fully dedicated to his research in the foundations of biology and, in a broader sense, to the broader task of reconsidering the reactive paradigm. He was aware of the need to focus on what a model is, and to further define the relation between a biological entity and its model; that is, the relation between something represented and its representation. It is within this broader realm that Rosen realized the urgency of understanding how an open system (the natural system) and its model – always less open – understood in their relative unity, eventually make predictions possible.

Natural and Formal Complexity

Anticipatory Systems followed another foundational text: "Organisms as Causal Systems which are Not Mechanisms: An Essay into the Nature of Complexity" (1985b). By no means to be ignored, the other two contributions – "The Dynamics of Energetics of Complex Real Systems" (by I.W. Richardson) and "Categorical System Theory" (by A.H. Louie) – make it clear that Rosen's research reached another level; those who worked with him were encouraged to examine the various implications of higher complexity definitory of the living. The distinction between simple systems (or mechanisms) and organisms came clearly into focus. Rosen denied, in very clear formulations, that biology is nothing more than a particular case for physics (p. 166), and argued in favor of a mathematical language appropriate to the task, which is, in his view, category theory. Concluding remark: "Complex systems, unlike simple ones, admit a category of final causation, or anticipation, in a perfectly rigorous and nonmystical way," (p. 166). It is in this very well organized essay, of a clarity not frequently matched in his very rich list of articles and books, that Rosen defined a fundamentally new perspective. Schrödinger's question "What is Life?" – which became the focus of his work – led to his description of "relational biology," a concept originating, as already mentioned, with [Rashevsky \(1954\)](#), and which led to the realization that only after abstracting "away the physics and the chemistry," ([Rosen 1985b](#), p. 172) can we reach the organizational features common to all living systems. Rashevsky used graphs, whose "nodes were biological functions" and whose directed edges were "relations of temporal or logical procedure," (p. 172). But, as Rosen noticed, his mentor was ahead of his time:

... the time was quite wrong for his new relational ideas to find any acceptance anywhere. In biology, the 'golden age' of molecular biology was just beginning; experimentalists had no time or use for anything of this kind. Those who considered themselves theorists either were preoccupied with the reductionist modeling that Rashevsky had earlier taught them or were bemused by seductive ideas of 'information theory,' games theory, cybernetics, and the like, regarded Rashevsky and his ideas as generally archaic because he did not take direct cognizance of their enthusiasms (p. 173).

A great deal of effort was spent on defining the “(M,R)-Systems”, in particular on replication mechanisms inherent in the organizational features represented. However, the centerpiece, and appropriately so, is the modeling relation between a natural system and a formal one. Any reader of *AS* would be well advised to read Rosen’s essay (even though its main line of argument reverberates in the book). It is here that the intrinsic limitations of the Newtonian paradigm are spelled out in detail. And it is here, as well, that the major subject of causality, including the teleological, is addressed up front (cf. p. 192). Moreover, it is here that the “mathematical image of a complex system” comes into focus, and becomes subject to mathematical category theory (although the author did not specifically apply it here).

From the very rich text, I would like to refer to Rosen’s considerations on information, specifically, on an alternate approach that relates to his preoccupation with measurement. He defined information as “anything that is or can be the answer to a question” (cf. p. 197). This prompts the observation that formal logic (“including mathematics,” as he put it) does not account for the interrogative. Therefore, information cannot be formally characterized. Rosen used the formalism of implications (If A, then B) in order to eventually formulate a variational form (If δA , then δB) that brought up measurement: “If (initial conditions), then (meter reading)?” and an associated formulation on variations: If (I make certain assumptions), then (what follows?). This is, in his words, “analogous to prediction” (p. 199). The conclusion is powerful: “When formal systems (i.e., logic and mathematics) are used to construct images of what is going on in the world, then interrogations and implications become associated with ideas of causality (p. 199). The reader is encouraged to realize that this is exactly why the Newtonian paradigm cannot accept Aristotle’s *causa finalis*: a (formally) logical system that does not have what it takes to represent interrogation, cannot account for information that always involves a telic aspect: the “What for?” of information. The conclusion is simple (and elegantly formulated):

Like early man, who could see the earth every evening just by watching the sky but could not understand what he was seeing, we have been unable to understand what every organism is telling us. It cannot be stressed strongly enough that the transition from simplicity to complexity is not merely a technical matter to be handled within the Newtonian paradigm; complexity is not just complication but a whole new theoretical world, with a whole new physics associated with it (p. 202).

There is an old question that informs these Prolegomena: If a tree falls in the woods and no one is around to hear it, does it make a sound? In semiotics, one of the research fields from within which my own notion of anticipation took shape, nothing is a sign unless interpreted as a sign. The noise caused by the falling tree is a physical phenomenon corresponding to friction. It propagates at a distance that corresponds to the energy involved (the falling of a huge tree can be heard at a farther distance than the falling of a bush). The energy dissipated in the process can be measured exactly. In trying to define natural law, Laplace (1820, as quoted in [Rosen 1985a](#), p. 9) convincingly described the kind of inferences possible in the reductionist world: An intelligence knowing, at a given instant in time, all forces

acting in nature, as well as the momentary position of all things of which the universe consists, would be able to comprehend the motions of the largest bodies of the world, as well as the lightest atoms in one single formula. To him, nothing would be uncertain, both past and future would be present in this eyes. In the years in which Rosen challenged a description of the world that simply does not account for the richness of life, I myself referred not only to Laplace, but to *Ecclesiastes*: “There is no new thing under the sun” (Nadin 1987). Everything is given, and with it, the laws describing it. We need only a good machine to reconstitute the past from the energy that preserved the noise of the falling tree, as it preserves all the thoughts ever expressed by those speaking to each other. The oscillations of air molecules could help us hear what Aristotle said, and even Socrates, whose words, we assume, Plato wrote down (or made up). My text on the mind, coming together within the timeframe when anticipation was becoming a necessary construct for understanding how minds interact, challenges the acceptance of reductionism while actually having as its object a novel, Umberto Eco’s *The Name of the Rose*. The detective story was probably written from end to beginning, or so it seems. It has a clear final cause, and it offered the author, a distinguished historian of the Middle Ages, the occasion to pose questions relevant to how representations are elaborated. Is there something there – a person, a landscape, or a process – that we simply describe, draw, take a picture of? Or do we actually notice that what is alive induces changes in the observing subject that eventually result in a representation? Even in the universe of physics, the static notion of representation was debunked as quantum mechanics postulated that to measure is to disturb. Rosen (1978, and myself, Nadin 1959) in other ways, said: “to measure is to BE disturbed.” That is, the dynamics of the measured affects the dynamics of the measuring device. Every interpretation is the result of interactions.

Together with the rather impressive number of Center presentations (published in the *International Journal of General Systems*; see references for details) that preceded the book, *AS* invites consideration of its echo in the scientific community. Please remember the question, “If a tree falls in the woods and no one hears the noise, does the event register as directly consequential?” It also invites consideration of Rosen’s essay, “Organisms as causal systems which are not mechanisms” (1985b) in the *Theoretical Biology and Complexity* volume. Let’s be up front: *AS* prompted two reviews: one by Minch (1986), at that time a graduate student at Binghamton, and one by Vámos (1987) of the Technical University of Budapest (Hungary). The Essay volume attracted René Thom, the distinguished mathematician (Catastrophe Theory is associated with his name), Lee Segel (Weizmann Institute of Science, Israel), Lev Ginzburg (SUNY–Stony Brook), and P.T. Saunders (King’s College) to review it. In the perspective of time, this is rather little given the significance of the work. But it is also telling in respect to the difficult cognitive challenge that the work posed, and still poses. Minch, now a respected researcher in his own right, might not have fully realized the impact of the radical ideas that Rosen advanced, but everything in the review is evidence of solid judgment and the desire to understand. He stated: “The essential difference between reactive and anticipatory systems is that reactive control depends on correction of an existing deviation, while

anticipatory control depends on preventions of a predicted deviation” (cf. p. 405). For anyone trying today to convey to the scientific establishment why the study of anticipation is relevant, this sentence from Minch’s review says it all. Minch thoroughly referred to the *modeling relation* – “between a natural system and a formal system” – and to their linkage. He was able to realize the importance of a new understanding of time (“In particular, he shows how we can view models and systems as parameterized by different times” p. 406). The review deserves to be quoted in more detail than appropriate in Prolegomena. The book as a whole, Minch states, is both radical and profound.

It is radical because it not only develops and propounds a paradigm, which is very different from the traditional, but also finds inadequacies in the epistemological roots of science, and overcomes these inadequacies. It is profound because of the depth of the discussion and the extent of its implications (p. 408).

Vámos could not find anything new. In the Essay reviews, Thom admired “an extremely interesting piece of epistemological thinking,” as well as the discussion on causality, “The rediscovery of Aristotelian causality theory, after centuries of blind positivist rejection, has to be hailed as one of the major events in modern philosophy of science.” Neither Segel (“I oppose his urgings to go beyond the evolving state description that was so successful in particle physics,” 1987), nor Ginzburg (1986) realized the significance of Rosen’s model. Saunders, “fascinated by the third chapter (i.e., Rosen’s text) is also taken by the novelty of the approach to causality and the non-Newtonian dynamic system.

Various Understandings of Anticipation

At this moment, the reader of the Prolegomena dedicated to what supports the science that Rosen attempted to initiate might call into question the wisdom of printing a second edition of a book less than enthusiastically received since its beginning – but out of print, nevertheless. I hasten to add that my own survey of anticipation-pertinent scientific publications has resulted in a very interesting observation: Very few mainstream researchers quote Rosen directly; secondary sources, in articles inspired by Rosen’s work, are usually quoted. Rosen is present, i.e., his ideas are either continuously reinvented – I can imagine him smiling about this – or, better yet, there is a definite Rosen presence even in research that is ultimately divergent from his understanding of anticipation. Here I refer explicitly to various attempts to get machines to anticipate one way or another – a subject to which I must return since there is so much and often very good work to survey. The destiny of Rosen’s writings, not unlike his actual destiny, might be a good subject for a novel or a movie. After all, John Simon’s apt description (*New York Magazine*, 1990), inspired by Richard Nelson’s play *Some Americans Abroad*, was an eye-opener to many who still maintain an idealized view of academia, or are convinced that science is a *sui generis* immaculate conception enterprise in which the best always wins.

The intensity of hatred, infighting, and back-stabbing increases with the marginality of a profession; hence, for rivalry, animus, and vindictiveness, there is no business like education. Anyone who has put in time in academia has witnessed intrigue to make a Renaissance Italian dukedom or a Reformation German principality look like an idyll by Theocritus.

Rosen faced bureaucratic imbecility a bit more than his peers did, and became the victim of that infighting and back-stabbing. He distanced himself as much as possible from this behavior. But after all is said and done, and while the difficulties he faced affected him, this is not what defines Rosen as a scholar. It would be unfair to his legacy to put more weight on the unfairness he faced than on the original thinking that defines his contribution. No scientist of integrity will lightly challenge the fundamental epistemological assumptions informing the dominant understanding of life within and outside the scientific community. Generation after generation, we were all educated, and continue to be educated, in the Cartesian understanding. A highly successful body of knowledge testifies to the revolutionary power of this explanatory model of the world. Still, before Rosen, and after Rosen, positions were articulated in which Cartesian doubt – *Dubito ergo sum* – is expressed, moreover in which alternative explanations are advanced (cf. Wigner 1961, 1982). On several occasions, I presented such views (Nadin 2000, 2003, 2009b, for example), informed by Rosen’s work and by other attempts to free science from a limiting view of how knowledge is acquired.

Rosen’s realization of the limits of the reaction paradigm is part of his broad conception of the living. Our ability to gain knowledge about it is affected by the Cartesian perspective. To transcend this view, scientists ought to “discard knowledge” (as Niels Bohr put it), and to see the world anew. In a letter (1993, cf. *Praefatio*, Louie 2009), Rosen alludes to how the “official” position of science often leads to opportunistic positions: “The actual situation reminds me of when I used to travel in Eastern Europe in the old days, when everyone was officially a Dialectical Materialist, but unofficially, behind closed doors, nobody was a Dialectical Materialist.” Due to my own life story, I happen to know what this means. My own understanding of anticipation was informed by semiotics – which Rosen and some of his colleagues considered worthy of their attention (they referred mainly to the symbol). At the time (1980–1985), I was teaching at the Rhode Island School of Design and advised some very creative students. I also served as adjunct professor at Brown University, working with the Semiotics Group and students in Computer Science. Cognitive science, in particular Libet’s work on readiness potential (Libet et al. 1983), also informed my research. As Eminent Scholar in Art and Design Technology (Ohio State University, 1985–1989), I was confronted by what Rosen would have qualified as the “mechanistic view of creativity,” characteristic of the early days of fascination with computers and their applications in modeling, simulation, and animation. This was the place where “flying logos” originated, where computer graphics – machine-generated imagery and computer-supported animation – made national headlines. The effort received the usual funding (National Science Foundation, Department of Defense, and the like), prompted less by scientific significance and more by media attention. Ever

since my formative years in Romania, first at the Polytechnic Institute and then as a graduate and doctoral student at the University of Bucharest, the question that shaped my intellectual profile concerned creativity, in particular: Can machines be creative? Better yet: Is creativity an expression of deterministic processes?

Today I know that this is a typical Elsasser problem, as it is also a Rosen problem. And it lies at the confluence of the physical (i.e., matter), as a substratum of all there is, and the dynamics of the living. As I wrote my first computer graphics programs (late 1960s), I realized that they encapsulated knowledge of visual expression and representation, and allowed for high performance of repetitive tasks. But the essence of any creative act is that it results in something that has never existed, not in the mindless reproduction of what is already available.

At Ohio State, I could have easily fallen prey to the comfort of a prestigious tenured position. Playing the usual funding games – if you correctly assess the direction from which the “funding winds” blow and don’t challenge established science, you get to “kiss the girl” – and making the conference rounds was part of it. But I did not want that. My concern with the multitude aspects of creativity brought me close to the then still incipient interest in the role of the brain, and into the mind-brain discussion. Intellectually, this was the hottest subject, probably because it took place at the confluence of disciplines (and had a clear European flair, as with Eccles).

I left Ohio State – probably as Rosen left Chicago or Buffalo – but not before accepting the invitation of the Graduate School to give a lecture. My take on anticipation, which was the focus of this lecture, was based on Libet’s measurements. I was not interested in free will, as I did not focus on how synapses actually take place. The scientific question I tried to answer was, “How do minds anticipate?” that is, how brain activity is triggered before an action, not in reaction to something else. “How does the brain know in advance that I will move my arm or scratch my head, or avoid a collision?” In addressing the question, I used the mathematical model of dynamic systems, and I advanced some hypotheses: The mind controls the brain; actually interactions of minds make anticipation possible. Anticipation can be described as an attractor within a space of many possible configurations. The lecture devoted some consideration to senescence (in particular, dementia), and even more to creativity. This lecture, which somehow paralleled Rosen’s lectures at the Center in Santa Barbara, eventually became a manuscript in search of a publisher. (I wish I had known George Klir at that time.) The text also defined my priorities as a researcher. Indeed, I did not want to write programs to be used for more flying logos and commercial art garbage. Repetitive tasks were not for me, just as the automation of bad taste, along with the production of mediocrity for satisfying such bad taste, never captured my interest. To give up an endowed chair sounds almost heroic when one thinks about the uncertainties in academia. But it saved my life as a scientist.

At this time, I was back in New York City and had the opportunity to see a fascinating show at the New York Academy of Sciences. Tod Siler, whose artwork was being exhibited, was a Ph.D. candidate at the Massachusetts Institute of Technology, combining art and brain science. His show seemed to reverberate my own thoughts. It seemed to me that if I could find a publisher for my book, his

images would be congenial to my ideas. Fortuitous events brought me to Belser Presse (Stuttgart/Zurich), which produced a series dedicated to scientific texts of broader relevance than the usual monographs. The series “Milestones in Thought and Discovery” published Leibniz’s *Two Letters on the Binary Number System and Chinese Philosophy* (1968); Heisenberg’s *The Laws of Nature and the Structure of Matter* (1967); and the testimony of Furrer (1988), the first German astronaut. These titles were selected by an extremely competent scientific advisory board. The members took a long time deliberating whether my book and Siler’s images should be published, but in the end an exquisite limited edition was printed. It became the preferred gift among an elite, but not necessarily the subject of scientific debate, as was my hope.

And as my new academic endeavors would have it – teaching in Germany for ten years – my focus on anticipation – in the meanwhile informed by Rosen’s work and influenced by my contact with him – led to more theoretic work. It also led to attempts to test hypotheses in various fields of anticipation expression: communication, design, architecture, human-computer interaction, the various arts. In 2002, the antÉ – Institute for Research in Anticipatory Systems was incorporated, and one of its first projects was a hybrid publication: book (*Anticipation – The end is where we start from*, Nadin 2003), Website (a knowledge base for the community of researchers interested in this area), and a DVD (presenting examples of anticipation ranging from chess to a simple protein folding game). This was the first time that some of Rosen’s work was made available in digital format.

With my appointment as Ashbel Smith Professor at the University of Texas at Dallas, the Institute found a new host, and new fields of inquiry. I would not have accepted the endowed chair in Dallas if the terms of my employment had not specifically spelled out a framework of activity corresponding to my total dedication to research in anticipation. Interestingly enough, Rosen himself worked for a year in Dallas (actually in Arlington, part of the greater Dallas–Fort Worth Metroplex) – he did not like it – before his stint in Kyoto. More to the point, his writing on senescence, and my work in addressing the loss of anticipation in the aging somehow fused in the Project *Seneludens* (Nadin 2004). Through brain plasticity (stimulated by involvement in games with a cognitive and physical component), anticipatory characteristics, vital to maintaining balance and a variety of actions, can be maintained.

The Institute also organized three international symposia: Vico’s *Scienza Nuova*, 2005, *Anticipation and Risk Assessment*, 2006 (see Nadin 2009a), and *Time and the Experience of the Virtual*, 2008. A special issue of the new *Journal of Risk and Decision Analysis* is dedicated to Risk and Anticipation. The University gave serious consideration to acquiring Rosen’s library; my own desire to organize an annual Robert Rosen Memorial Lecture will one day be realized.

These details should be read as testimony to a very dedicated interest in a subject that will always have a reference in Rosen’s *AS*. Maybe the best way to conclude these remarks is to set side-by-side one of Rosen’s definitions and one of mine:

Rosen	Nadin
An anticipatory system is a system whose current state is determined by a (predicted) future state.	An anticipatory system is a system whose current state is determined not only by a past state, but also by possible future states.

As subtle as the difference is, it only goes to show that could Rosen be around, he would continue to work on foundations as well as on applications.

Distinguishing between prediction and anticipation is the subject that could be of further help in defining anticipatory processes. Prediction and anticipation are not interchangeable. Predictions are expressions of probabilities, i.e., description based on statistical data and on generalizations (that we call scientific laws). While not unrelated to probabilities, anticipations involve possibilities. Zadeh's genius in defining possibility is expressed in the accepted dicta: Nothing is probable unless it is possible. Not everything possible is probable. The model of itself, which unfolds in faster than real time, in Rosen's definition (1985a) is driven by both probability realizations and possibility projections. It is in respect to this fundamental distinction that I submitted the thesis according to which the complementary nature of the living – physical substratum and specific irreducible dynamics – is expressed in the complementary nature of anticipatory processes (Nadin 2003, 2009c).

A Broader Context – Awareness of Anticipation

The perspective of time and the evidence of increasing interest from the scientific community in understanding anticipatory processes speak in favor of describing the premises for the initial definition of anticipation. The work (1929) of Alfred North Whitehead (1861–1947) advanced the idea that every process involves the past and the anticipation of future possibilities. This thought is part of a larger philosophic tradition sketched out in the attempt to indentify early considerations on the subject. Indeed, let us be aware of the variety of understandings associated with the concept, because otherwise there is a real risk of trivializing anticipation before we know what it is. Burgers (1975) was inspired by Whitehead. Although he came from physics, Burgers brought up choice and the preservation of freedom as coextensive with anticipation. Bennett, an anthropologist, saw anticipation as “the basis for adaptation” (1976a, p. 847). In his book (1976b), the same broad considerations made the object of an entire chapter (VII), in which Whitehead's notion of anticipation, extended to the entire realm of reality, is limited to living systems. Both Burgers and Bennett are part of the larger context in which anticipation slowly became part of the vocabulary of science and philosophy at the end of the last century.

Another area of investigation that leads to explicit considerations of anticipation is psychology. Not unexpectedly, “work and engineering psychology” (Hacker

As we shall see, where Feynman's model and considerations on anticipation and computing, related to the work of Rosen and Nadin, diverge is not difficult to define. For him, as for all those – from Aristotle to Newton (*Philosophiæ Naturalis Principia Mathematica*) to Einstein – who made physics the fundamental science it is, there is an all-encompassing Nature, and physics is the science of Nature. In other words, physics would suffice in explaining anticipatory processes or in computationally simulating them.

Svoboda (1960, cf. Klir 2002b) published a “model of the instinct of self-preservation” in which the subject is a computer itself. Its own functioning models self-preservation under external disturbances. A probabilistic description based on inferences from past experiences quantifies its predictive capability. Pelikan (1964) further elaborated Svoboda's original idea. Probably, as we advance in our understanding of anticipation, there will be more contributions that, in retrospect, will deserve our attention. For example, in 1950 (cf. Gabel and Walker 2006) Buckminster Fuller outlined a class in anticipatory design (taught at MIT in 1956). In this class, “Eight Strategies for Comprehensive Anticipatory Design Science” were spelled out. Fuller took a broad view of what it means to introduce new artifacts into reality. There is a lot to consider in terms of how they change the given environment and the behavior of individuals and communities. Teleology, i.e., the goal-driven aspect of design, is to be understood in relation to what he called “precession”: the sequence of steps that lead from the assumed goal (subject to continuous reevaluation) to the end result (itself subject to further improvement).

The American economist Willford Isbell King (at one time Chairman of the Committee for Constitutional Government) published *The Causes of Economic Fluctuations: Possibilities of Anticipation and Control* (1938). The circumstances (in particular the Great Depression) explain the subject and hope. The same title could be used in our days. Fluctuations continue to haunt us, and predictive models developed so far are not very helpful when it comes to avoid dire consequences. At about the same time (1937, actually), George Shackle, under the supervision of Friedrich von Hayek, finished his dissertation, which led to his first book (1938). Expectation, as a particular form of anticipation is connected to his future contributions to defining uncertainty. Let us take note that in examining the time vector from the beginning of an action (threshold) and the time vector from the end of the action in reverse, Shackle noticed that we never have enough knowledge in order to understand the consequences of our actions. Of interest to us today is Shackle's understanding of possibility, and the contradistinction to probability. A short quote is indicative of the anticipation implications of his writing: “It is the degree of surprise to which we expose ourselves when we examine an imagined happening as to its possibility...” (cf. Klir 2002a for an in-depth analysis). As far as I was able to establish, Shackle did not use the word *anticipation*, but he referred to imagination as guiding choices (1979). His conceptual contribution in understanding imagination as related to the space of possibilities will surely lead to more elaborations of interest to research in anticipation.

Possibility and its relation to probability that was of interest to Shackle (cf. 1961) will have to wait for a more comprehensive approach until Lotfi Zadeh (1978), and

subsequently many distinguished followers, gave it a foundation. Zadeh himself arrived at possibility via fuzzy sets. In June (2009a), Zadeh, continuing his tireless investigation of the realm of knowledge he opened when introducing fuzzy sets, made note of the fact that judgment, perception, and emotions play a prominent role in what we call economic, legal, and political systems. Many years ago, Zadeh (1979/1996) invoked the views of Shackle, among others, as an argument in introducing information granularity. This time, acknowledging complexity – which, as we shall see, is the threshold above which anticipatory behavior becomes possible – Zadeh took a look at a world represented not with the sharp pen of illusory precision, but with the spray can (“spray pen geometry”). Where others look for precision, Zadeh, in the spirit in which Shackle articulated his possibilistic views, wants to capture processes unfolding under uncertainty. We realize, at least intuitively, that anticipations (like imagination) are always of a fuzzy nature, and it seems to me that Zadeh’s new work, intensely discussed on the BISC mailing list, will make the scientific community even more aware of this condition.

It is significant that economics prompts the type of questions that unite the early considerations of King (1938) and Shackle (1938) with Klir’s considerations (2002a) and Zadeh’s (Zadeh et al. 2009b) very recent attempts to extend fuzzy logic. Questions pertinent to economics (and associated fields of inquiry) will undoubtedly further stimulate anticipation research. We want to know what the possibilities for success are, or at least what it takes, under circumstances of uncertainty, to avoid irreversible damage to our well-being, and often to society’s.

Where Do We Go From Here?

The knowledge base reflecting the multitude of perspectives from which individual authors proceeded as they pursued the subject of anticipation continues to grow. This is no longer a preliminary stage, although it would be illusory to assume that a well-defined description has been established. Despite the urgency of providing an anticipatory perspective to the fast dynamics of our time, there are no university classes dedicated to it, and no research initiatives specifically informed by this perspective. Everyone hopes for good predictions; money is spent on funding predictive methods, as though prediction could substitute for anticipation. Still, the subject quite often percolates among the many research themes associated in some ways with cognitive science, computer science, artificial intelligence, and even ALife.

With Rosen’s concept of anticipation as a reference, the following consideration will provide pointers to relevant research. A distinction will be made between studies pursuing Rosen’s theoretic outline, and studies defining the field in ways other than his own; or better yet, what he called pseudo-anticipation. No author could claim credit for a full account. We can more easily find what we look for, but at times to formulate the question is more challenging than to advance a hypothesis as an answer. Example: Ishida and Sawada (2004) report on a very simple experiment

of human hand movement in anticipation of external stimulus. Unfortunately, while actually reporting on anticipation, the authors never name the concept as such. (It is from this experience that I discovered how many Japanese scientists would be happy to have access to a new edition of AS.) In other cases, anticipation, the word, is present, but the results presented have actually nothing to do with it. I prefer not to single out an example because, after all, there is nothing to object to in what is presented, but rather to the use of a concept that has had a precise meaning ever since Rosen's AS and other contributions (mentioned in the previous section of the Prolegomena). The fact that the scientific community at large has not embraced the view reflected in Rosen's particular interpretation, or in definitions congruent with his, means only that more has to be done to disseminate the work, in conjunction with its understanding. Einstein's assessment – No problem can be solved from the same consciousness that created it – is relevant not only for those willing to step out from their epistemological cocoon, but also for those who literally cannot find useful answers within the epistemology they practice.

Classical research in psychology – in particular, on receptive-effector anticipation (Bartlett 1951) – prepared the way for perceptual control theory (PCT) initiated by William T. Powers (1973, 1989, 1992) around the notion of organisms as controllers. All this is obviously different from Hacker's work in what used to be the German Democratic Republic. Kelly's (1955) constructivist position is based on validation in terms of predictive utility. Coherence is gained as individuals improve their capacity to anticipate events. Since the premise is that knowledge is constructed, validated anticipations enhance cognitive confidence and make further constructs possible. In Kelly's terms (also in Mancuso and Adams-Weber 1982), anticipation originates in the mind and is geared towards establishing a correspondence between future experiences and predictions related to them. The fundamental postulate of this theory is that our representations lead to anticipations, i.e., alternative courses of action. Since states of mind somehow represent states of the world, anticipation adequacy remains a matter of validation through experience.

Anticipation of moving stimuli (cf. Berry et al. since 1999) is recorded in the form of spike trains of many ganglion cells in the retina. Known retinal processing details, such as the contrast-gain control process, suggest that there are limits to what kind of stimuli can be anticipated. Researchers report that variations of speed, for instance, are important; variations of direction are less significant. That vision remains an area of choice in identifying anticipation is no surprise. An entire conference (University of Dundee, 2003) was dedicated to Eye Movements – considered “a window on mind and brain” – while the European project MindRaces (2009): *from reactive to anticipatory cognitive embodied systems* encouraged studies in this field, given its applied nature (Pezzulo 2007a, 2007b). Balkenius and Johansson (2007) contributed to the project the research of anticipatory models in gaze control, integrating reactive, event-driven and continuous-model-based location of target. Obviously, learning in their view is rather different from Rosen's notion, which predates AI's focus on learning in our days. It is encouraging to notice that the recognition of the role of learning extends to their particular domains of interest.

Arguing from a formalism, such as Rosen used, to existence is definitely different from arguing from existence (seeing, hearing, binding of the visual and aural, etc.) to a formalism. A vast amount of work (concerning tickling, e.g., [Blakemore et al. 1998](#); posture control, e.g., [Gahery 1987](#), [Melzer et al. 2001](#), [Adkin et al. 2002](#)); gait control ([Sahyoun, et al. 2004](#)) exemplifies the latter. In the same category, reference can be made to [König, and Krüger 2006](#) on the subject of predictions about future stimuli (the frog spotting a flying insect, and the process of filling in the informational gap in order to define the position where its tongue will capture it in a swift move). The authors allude to patterns of behavior. Such patterns are also suggested in the research of [Roth \(1978\)](#) as he analyzes the prey catching behavior of *Hydromantes genei*.

The very encouraging aspect in such research is that measurements of trigger-based experiments reveal what happens before the trigger (obviously if the measurement itself is not set off by the trigger). In other words, what happens in anticipation of stimuli (can be guessing, prediction, noise, for example), not as a result of them begins to be examined. Preparation (cf. [Gahery 1987](#)) is part of the anticipatory process. I doubt that a theory of anticipation, or at least some amendments to the available theories, could emerge from these rich sets of data. But such experimental evidence is encouraging first and foremost because it consistently supports the fundamental idea expressed in Rosen's modeling relation: If a modeling relation between a natural system and a formal description can be established, the formal description (of vision processes, of tickling, of tactility, of sound and image binding, etc.) is a model, and the domain knowledge is a realization of such a description subject to further investigation. Moreover, arguing from computation – which is more and more a gnoseological mode – might impress through even broader sets of data and much more detail, but still not substitute for the lack of a theoretic foundation. As impressive as applications in neural networks ([Homan 1997](#), [Knutson et al. 1998](#), [Kursin 2003](#), [Tsirigotis et al. 2005](#)), artificial intelligence ([Ekdahl et al. 1995](#), [Davidsson 1997](#)), adaptive learning systems ([Butz et al. 2003](#)), among others, are, they can at most make us even more aware of the need to define our terminology and practice scientific discipline. [Rosen \(1991, p. 238\)](#) pointed out quite clearly that the more constrained a mechanism, the more programmable it is. Albeit, reaction is programmable, even if at times it is not exactly a trivial task to carry out. Modeling and simulation, which are intensive computational tasks, are no more anticipatory than any other mechanisms. They embody the limitation intrinsic in the epistemological horizon in which they were conceived. Neural networks and anticipation, followed by impressive achievements in animation and robot motion planning ([Balkenius et al. 1994](#), [Christensen and Hooker 2000](#), [Fleischer et al. 2003](#)), only allow us to realize again the difference between purposive activities (where there is a finality; a “final cause”) and deterministic activities, of a different causal condition.

This observation brings up the effort known under the name *CASYS conferences* (organized by Daniel M. Dubois, in Liège, Belgium since 1997). Dubois builds upon [McCulloch, and Pitts \(1943\)](#) “formal neuron” and on von Neumann's suggestion that a hybrid digital-analog neuron configuration could explain brain dynamics

(cf. 1951). It is tempting to see the hybrid neuron as a building block of a functional entity with anticipatory properties. But from the premise on, Rosen followed a different path, quite convincingly, that recursions could not capture the nature of anticipatory processes (since the “heart of recursion is the conversion of the present to the future”). Neither could incursion and hyperincursion (an incursion with multiple solutions that Dubois advanced) satisfy the need to allow for a vector pointing from the future to the present. Rosen warned about the non-fractionability of the “(M,R)-Systems”; and this is of consequence to the premise adopted in Dubois’ work. When [Dubois \(2000\)](#) defines “...the main purpose...is to show that anticipation is not only a property of biosystems, but is also a fundamental property of physical systems,” he argues with Rosen’s fundamental ideas from a position that basically ignores the distinction between the ontological (of which anticipation is a characteristic) and the epistemological. Within science, difference of views is perfectly acceptable, provided that the concepts are coherently defined. This provision is ultimately not met. For particular applications, Dubois’ take is quite convincing. His conference usually serves as a good opportunity for featuring contributions inspired by his work. Addressing issues of autonomous systems (i.e., they self-regulate), [Collier \(2008\)](#) builds on Dubois’ conjecture in addressing autonomy and viability. Suffice it to say that such a contribution is in itself relevant for the richness of the dialog that Rosen’s book and its subsequent interpretations triggered.

[Chrisley \(2002\)](#) is among those aware of the contradictory situation in which proponents of “computing anticipatory systems” are. He is explicit: One can go further and inquire as to the extent to which such causal anticipatory systems are computational. This is very important since, as Chrisley notes, the model is essential, not the data. The so-called “transduction of present data into future data (i.e., into predictions) through the agency of a model of the world “does not turn the probabilistic prediction into anticipations. Indeed, the anticipation, expressed in action, is, after all, part of the system from which it originates. In order to address this aspect, [Dubois \(2000\)](#) distinguished between weak anticipation – more or less along Rosen’s idea of a model-based process – and strong anticipation “when the system uses itself for the construction of its future states,” (Dubois and [Leydesdorff 2004](#)). Leydesdorff (a distinguished researcher of social systems examined from an anticipatory perspective) argues that “the social system can be considered as anticipatory in the strong sense,” (it constructs its future). [Leydesdorff \(2008\)](#) further enlarges upon this observation in examining intentionality in social interactions.

Along this line, I want to mention some very convincing attempts to relate perception and motoric response ([Steckner 2001](#)) to address issues of predictive model generation ([Riegler 2001](#)), to associate anticipation with decision-making processes ([Nadin 2009a](#)), to deal with interaction as it results in a variety of anticipatory processes ([Kinder 2002](#)). [Riegler \(2004\)](#) focuses on “Who (or what) constructs anticipation?” It is, as he defines it, a challenge to the implicit assumption of Rosen’s model-based definition. The “decision maker” whom he is after remains an open question. In the area of applied interest (automobile driving, assessing the impact of emerging technologies, extreme events assessment, the whole gamut

Fig. 2 Rosen’s model of anticipation

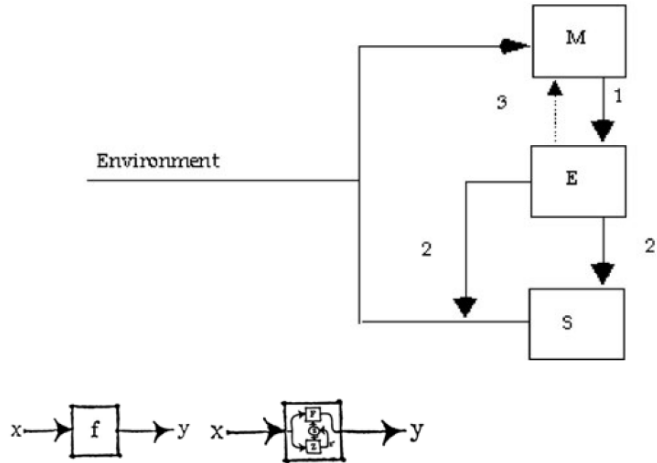


Fig. 3 “Roadsigns Definitions Postulates Aphorisms, etc.,” [sic]. Heinz von Foerster (1995)

very close to his own views of the living and on the constructivist Condition of Knowledge. In his famous formulation, “Die Ursache liegt in der Zukunft” (The cause lies in the future), von Foerster gives what is probably the most concise (or expressive) definition of anticipation. But what prompts my decision to bring up von Foerster is the striking analogy between Rosen’s anticipatory model (1985a, p. 13) and von Foerster’s (2002) concept of non-trivial machines (Figs. 2 and 3).

Let us only make note of the fact that non-trivial machines are dependent on their own history (which is the case with Model M in Rosen’s model), cannot be analytically determined, and are unpredictable (cf. 2002, p. 58).

If the suggestion holds – and we should dedicate more time to it – it is quite clear how from Rosen’s original definition of anticipation many more were derived as alternative, non-trivial, machines (in von Foerster’s sense). This brings up important epistemological questions, such as hybrid computation (the human being or a living entity connected somehow to computers), or even quantum computation in the sense Feynman defined it; interactive computation, membrane computations, DNA computation. It is necessary to realize that we are focused on non-algorithmic data processing, sometimes combining analog and digital representations. Howard Pattee, Rosen’s colleague at the Center for Theoretical Biology in Buffalo – and with whom he had many passionate discussions – still cannot accept Rosen’s intransigence in dealing with von Neumann’s universal constructor – a construct that could achieve unlimited complexity. Pattee is willing to concede that formally von Neumann’s model was incomplete. But he argues that ultimately, von Neumann and Rosen agreed (“life is not algorithmic”), moreover, that self-assembly processes characteristic of the living do not require complete genetic instructions. The reason I bring up this point is rather practical, and Pattee (2007) expressed it convincingly: we should avoid getting diverted from Rosen’s arguments only because, at times, they do not conform with the accepted notions (in this case, von Neumann’s replication scheme).

Rosen (1966) was firmly opposed to von Neumann's understanding that there is a "threshold of complexity" that can be crossed by finite iterations (analogous to the notion that infinity can be reached from a finite number, simply by adding one more). Rosen brought up the need to account for the characteristics of the organism as evolvable, adaptive. Nevertheless, in hindsight we can say that both realized, although in different ways, that if complexity is addressed from an informational perspective, we end up realizing that life is ultimately not describable in algorithmic terms. Chu and Ho (2006) noticed that in Rosen's view, "living systems are not realizable in computational universes." – whatever such computational universes might be. They provided a critical (negative) assessment of Rosen's proof, which Louie (2007) convincingly refuted. Louie's argument in some ways confirms that non-algorithmic self-assembly (epigenetic progresses) is of such a condition that it does not require full descriptions either of the functions or of the information involved in living processes.

Computation and Anticipatory Processes

Given the implications of this observation, let us take a closer look at what it means. Along the line of the Church-Turing thesis – i.e., that every physically realizable process is computable – von Neumann (1963, p. 310) went on out a limb and stated, "You insist that there is something a machine cannot do. If you will tell me precisely what it is that a machine cannot do, I can always make a machine which will do just that." If von Neumann was convinced that telling *precisely* what it is a machine cannot do – emphasis on *precisely* – is a given, he was not yet disclosing that *telling precisely* might after all require infinite strings, and thus make the computation to be driven by such a description impossible (*intractable*, in computer science lingo). It is easy to show that if you completely map a process, even the simplest computation can reproduce its function. But can we completely map even simple biological functions? And given the fact that "To live is to change" (cf. Chorda 2010), to map completely is, in the final analysis, to create a *living representation*, a virtual process in which *matter* is replaced by *information*. Actually, von Neumann should have automatically thought of Gödel (and maybe he did) in realizing that a complete description, which would have to be non-contradictory, would be impossible. Descriptions, in words (as he expected, cf. "anything that can be completely and unambiguously put into words..."), or in some other form (e.g., numbers), are, in the final analysis, semiotic entities. They stand as signs for something else (the represented), and in the process of interpretation they are understood as univocally or ambiguously defined (Nadin 1991).

Representations of the world are always incomplete; they are not fragments of the world. It is such incomplete representations that are processed in an algorithm-driven computation or in some non-algorithmic computational process. Until the development of brain imaging, we could not capture the change from sensorial

energy to the cerebral re-presentational level. And even with images of the brain, semiotic processes are still not quantified. What is quantified are information processes, because information was conveniently defined in relation to energy (cf. [Shannon and Weaver 1949](#)). And we are able to measure energy quite precisely. But meaning is more than information; things make sense or not, not on account of bits and bites, but rather on qualitative changes. It is the re-presentation of things, not things themselves, that is subject to processing and understanding. Re-presentations (like a picture of a stone, or its weight, or the chemical formula/formulae describing its composition) are renewed presentations (of the stone) as signs, which means: dematerialized, extracted from the thermodynamic context, from the dynamics in which they are involved. Interpretations are attempts to associate a sign (a semiotic entity) to an object (a physical entity) and to conjure the consequences that the sign might have on our activity. Re-presentations can be of various degrees of ambiguity – from very low (indexical signs, as marks left by the object represented) to very high (symbols, i.e., conventions). Lightning arouses a sense of danger associated with phenomena in the world. The black cat can bring up false associations (superstitions) with dangers in the world. They are of different levels of ambiguity. The living can handle them quite well, even if, at times, in a manner we qualify as irrational (cf. [Dennett 1991](#) on anticipation).

Machines operate also on representations. But if we expect a certain output – such as the visualization of a process, or the processing of a matrix (a mathematical entity that does not correspond to processes in the world) – we have to provide representations that are unambiguous. Machines do not *dis-ambiguate* representations. For this reason, we conceive, design, and deploy artificial languages of no or very low ambiguity. The living operates, most often effectively, with representations regardless of their ambiguity. The machine is “protected” from ambiguity. (We endow machines with threshold identifiers: is the ignition turned on or not? Neither intermediate values nor intentions count! Ambiguity would be a source of error in their functioning.) Von Neumann’s claim that he could conceive a computation for any *precisely* described entity (i.e., complete description) means nothing more than that he proceeds to segregate between the semiotic of the unambiguous and the semiotics of ambiguity. In von Neumann’s thinking, to be precise means to be also unambiguous (in addition to reducing the measurement error to zero).

Moreover, computational reductionism does not acknowledge the fundamental role of time in the dynamics of the living. Time is reduced to interval. There is a clock that keeps track of sequential processes; and a clock is necessary in order to support the rigid synchronicity of parallel computation. It has been repeatedly demonstrated that an anticipatory system has at least two clocks, i.e., correlated processes unfolding at different times scales ([Nadin 2009b](#)). At various levels of the living, several clocks are at work: some very fast (at nanosecond speed); others in the domain of the “gravitational” clock; and yet others are very slow. Therefore, Rosen’s model unfolding at faster than “real time” is probably a distributed anticipatory process with many models operating at various time scales. [Rosen and Kineman \(2004\)](#) examine the characteristics of complexity in (Robert) Rosen’s view, realizing

correctly the central role played by the modeling relation. “The internal predictive models” are, in their view, hypotheses about future behavior. Finally, Feynman’s understanding of the integration of past, present, and future in the computation (meant to simulate Nature) is probably closer to Rosen’s understanding of anticipation than is von Neumann’s implicit anticipatory dynamics.

With all these considerations in mind, the reader of AS in its second edition should now be in a better position to understand that at the level of algorithm-driven machines (digital or not), anticipation is not possible. Such simple machines operate in the interval domain of causes and effects, in a non-ambiguous manner. As [Werbos \(2010\)](#) noticed, “The brain possesses a kind of general ability to learn to predict.”

Rosen – and not only Rosen (even von Neumann, cf. [Pattee 2007](#)) – ascertained that the living is not representable through a computational process. There are good reasons to accept that the living is not reducible to a deterministic machine, no matter how sophisticated such a machine might be. As we have seen with Feynman’s take on quantum computation, the situation changes: this is qualitatively a different machine; it is a *sui generis* process. Actually, quantum mechanics as a construct originates in response to the deterministic view of nature. Therefore, the argument that a quantum type of computational process can represent life is somehow circular: the quantum representation was constructed in order to overcome the limitations inherent in the classical deterministic concept of machines. The same holds true for DNA computing.

Be this as it may – and with the warning that such discussions cannot be relegated to well-intentioned half-baked scientists (cf. [Penrose 1989](#)) – the question to be posed is, after all, not whether we can ever come up with machines that are not machines in the sense propagated since Descartes (and de la Mettrie), but rather: If enough computational resources are available – theoretically an inexhaustible amount – wouldn’t the aggregate computation be sufficient to become a “life-like” process? Brute force computation – the relatively common practice in almost all instances of computation used to deal with complicated processes – means to throw as many computing cycles as possible at a problem and to work on as much data pertinent to the problem as we can get. IBM’s Deep Blue, which beat Kasparov in a chess game, met Turing’s test of intelligence without even being close to a living chess player (master or beginner). Seemingly, Venter’s attempt at modifying life is also one based on brute force. While [Hacker \(1978, 2005\)](#) addressed anticipation (properly defined as *Vorwegnahme*, the German word that describes *antecipare*, the Latin word behind *anticipation*) in relation to trajectory evaluation – a frog catching prey is the classic example – the *Star Wars* effort was supported by brute force computation. *Star Wars* was not about anticipating an attack, rather integrating information (descriptions of attack circumstances) and providing a reaction based on the fact that the speed of even the fastest ballistic missiles, carrying nuclear warheads or other lethal charge, is limited, and lower than the speed of electrons. If a system can get information as close as possible to the launch pad, given the fact that there is a huge difference in speed between the missile’s movement and the propagation of information, it can generate predictive models and effectively target – like the frog catching an insect – the trajectory load. “Elementary, my dear Watson” – nothing more to add.

With all this in mind, the methodological question becomes: Is the non-algorithmic, or even the broader (ill-defined) non-computable, corresponding to a level of complexity beyond the threshold defining the living, equivalent to infinite computation? The characteristics of powerful computation of large amounts of data (which is the tendency in our days) and the characteristics of brute-force computation (on a larger scale) of small but appropriately selected data are different. Can we evaluate the difference between computing an infinite amount of data (all that we can get given the progress in sensor technology) and the infinite computation needed to process a small but relevant amount of data? Evidently, to know beforehand which data are significant is a matter of anticipation; and it might simply move the problem of computing anticipation from processing all available data to filtering what is significant.

In general, to represent life is to represent something that is in process, changing all the time. For better or worse, we can model/represent – ergo prevent – every oil spill that has already occurred, but not anticipate accurately the one that will happen next. We can similarly model/represent every terrorist attack, and every financial crisis, and every epileptic seizure that have already taken place, but not predict accurately new occurrences of the same. But since there are no laws that capture the uniqueness of extreme events, of disease, of art creation, of Shakespeare’s writings, of scientific research, we cannot build a machine – similar to cars, rockets, AI-driven surgery, etc that will anticipate such things. The regular, patterned, and repetitive can be described as infinite representations. Therefore, machines can be conceived to effectively process such representations. The unique is the subject of idiographic knowledge, which is focused on the particular, not the general. Currently, more and more science is idiographic: visualizations are not abstractions equivalent to mathematical equations. To generalize from an individual’s brain image to all brains is more a matter of faith than of science. Dynamic visualizations – i.e., “films” of certain processes – are even less so. They qualify rather as “histories” – which is the substance of biology – than as “theories.”

Anticipation, always expressed in action, is unique. Repetitive patterns, such as the frog’s behavior in chasing a moving target (and in nature, this is not an exception), or mating behavior, do not result in laws, but in “chronicles” of successful and less successful actions. In ascertaining the action as expression of anticipatory processes, we implicitly ascertain realization at the pragmatic level, but not syntactic or even semantic performance, as is the case with machines. A computation “aware” of its state is an intentional procedure. Such a teleological computation, if we could conceive of it, would have a sense of purpose. It would also be an adaptive computation. Not unlike an artist, who never knows when the goal has been reached (for that matter, every work of art is open, i.e., unfinished), such a computation could also be seen as open-ended. Anticipatory computing (cf. [Nadin 2010b](#)), as a subject, will benefit from Rosen’s many elaborations on the subject, in the sense of terminological coherence.

Once we reach the notion of complexity at which causality itself is no longer reducible to determinism, and the condition of the living integrates past, present, and future, a new form of adaptive behavior and of finality (*purposiveness*) emerges. In

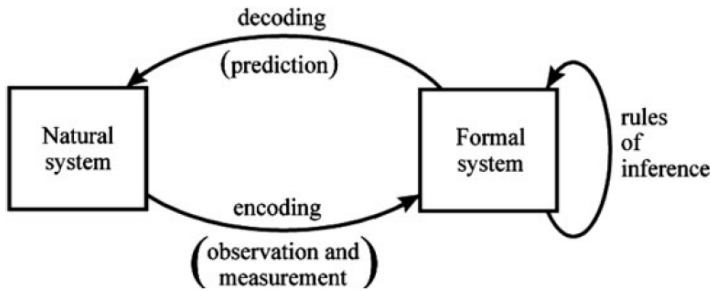


Fig. 4 Initial modeling relation

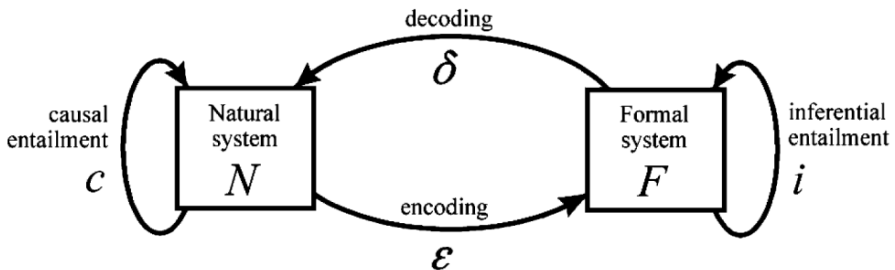


Fig. 5 Revised modeling relation

for a passionate intellectual who was fully committed to his science. Moreover, neither what defines the living nor what it means to measure, and even less what anticipation is are subjects for the do-it-yourself obsession of our time.

For something to be replicated, we need the understanding of what is needed for that entity to exist in the first place. Rosen expressed this kind of understanding in the modeling relation. This is, in a very concentrated form his epistemology (Fig. 4).

In AS, the natural system is represented by a formal system: knowledge is derived as inference and eventually guides practical activities. Rosen explains: Knowledge acquisition translates as an attempt to encode natural systems (i.e., represent them based on a semiotic code) into formal descriptions (equations, diagrams, etc.). Operating on representations, we can derive inferences or even theorems (statements that can be finally proven). Such operations eventually result in statements about how the natural system might behave. In fact, a modeling relation is, *in nuce*, a “theory of prediction.” For it to become a “theory of change,” (cf. Nadin 2010d), it would have to account for how the natural system behaves over time. Causality – causal entailment, in Rosen’s original thought – is the answer to the *Why?* question definatory of relational biology. In LI, causality is expressed as causal entailment and “rules of inference” are seen as the causal entailment encoded into inferential entailment, which completes the modeling relation diagram (Fig. 5).

Anticipation itself has the condition of entailment. It is clear that there is anticipation in the natural system; but this does not translate, in a one-to-one

relation, into the formal system, and even less into the ability to replicate it. In his words:

We seek to encode natural systems into formal ones [such that] the inferences or theorems we can elicit within such formal systems become predictions about the natural systems we have encoded into them (p. 74 of the original edition).

If we associate Rosen's clear statement with Einstein's (1921) observation, "In so far as the propositions of mathematics are certain, they do not apply to reality; and in so far as they apply to reality, they are not certain," we realize that only charlatans can promise to deliver a known future event. What is possible, however, is the design of systems that allow us to consider under which circumstances our descriptions of reality will have to be endowed with adaptive properties (no matter how primitive or limited in scope).

Earlier in the Prolegomena, the point was made that hyperboles do not help us better understand Rosen's original contributions. Beyond the hyperbole in "Biology's Einstein" lies an argument we need to consider: Rosen advanced a coherent view of the living within a broad understanding of what is traditionally called Nature. If in respect to the physical, the formal system allows us to infer laws, on the basis of which machines are built, then in respect to the living, we can at best describe successions, and further relations between the events or phenomena succeeding each other. Windelband (1894), who advanced the distinction between *noematic* sciences (focused on descriptions in the forms of laws) and the *idiographic* sciences (focused on descriptions of sequences, in the form of Gestalt), would have mentioned Rosen's view as illustrative of both (if he had not been his precursor by almost a century). Rosen specifically spells out (*LI*, p. 58) that science describes the world ("that is in some sense orderly enough to manifest relations or laws"), but that it also "says something about ourselves." The "orderliness" of the world (ambience) "can be matched by, or put into correspondence with, some equivalent orderliness within the self." This is what makes science, and also the scientist, possible.

What speaks in favor of an inquiry into anticipatory processes is the need to ascertain the complementarity of noematic and idiographic knowledge. Every process of anticipation, definitory of the natural system, involves knowledge as an expression of accumulated experience, but also of art as an expression of the creativity implicit in the living. In a very telling private communication (addressing Segel, who reviewed "Organisms as causal systems which are not mechanisms: an essay into the nature of complexity"), Rosen (1985c) uses a very intuitive image: "The point of view I have taken over the past 25 years is that the way we look at systems is no different than the way we look at each other . . . dynamic interactions between the (systems) are cognate to our own observing process." His intention in explaining his view to a colleague who "did not get it" (as we would say today) was very simple: "I'm writing the above only so that we may understand each other better." This second edition of AS is a delayed "letter" to a new generation of scientists who are now in a better position than Rosen's own generation to entertain provocative ideas that justify current interest (and hope) in anticipation studies.

Final Note

Rosen wanted to write a book on complexity. Many scholars and researchers wish he did. It was not to be; but it suggests to the scientific community that progress in understanding the relation between complexity and anticipation requires that we define the situation at which anticipation becomes possible – and probably necessary.

Science is not about doctrine – even less about faithfulness. Rosen’s work should therefore not be seen in a light different from his own: It is yet another hypothesis – probably one whose time has finally come. In publishing a second edition of *AS*, those involved in the process did not intend to suggest a return to the initial book, rather to stimulate further dialog and more probing scientific investigation. Judith Rosen has her own well-defined identity; she is also passionate about her father’s legacy and understands why dialog is important. She never hesitated in providing answers to questions I had or volunteering details I would not have access to. George Klir is dedicated to this book, and even more to the pursuit of academic dialog of integrity. A.H. Louie, who has also supported the effort of seeing a second edition of *AS* published, disseminates Rosen’s thoughts in his own distinguished publications. His teacher would have been proud of him. I have benefited from their competence and wisdom, and I admire their knowledge and appreciation of Rosen’s ideas. I also benefited from contacts with Peter Cariani, Roy Chrisley, Winfried Hacker, G. Hoffman, S. Kerckel, Dobilas Kirvelis, Andres Kurismaa, Loet Leydesdorff, Helmut Loeckenhoff, Alexander Makarenco, E. Minch, H.H. Pattee, Dean Radin, Marion Wittstock (who made me aware of the work of the Dresden group), and Olaf Wolkenhauer, and want to express my gratitude for their help. In particular, my respect to Lotfi Zadeh for some very useful conversations, and to Otthein Herzog for making possible my research at the Information Technology Center (TZI, Bremen University, 2009–2010). Lutz Dickmann was a patient sounding board for many of my hypotheses. He challenged me in the spirit of his own research for his Ph.D. To what extent my research of anticipation enriched the work of everyone I interacted with (in the project Anticipation-Based Intelligent Multiagent Societies for Logistics) remains to be seen. A Fellow award at the Hanse Institute for Advanced Study (Delmenhorst, Germany) allows me to focus on the relation between anticipation and its representation. A first concrete result is the study dedicated to the expression of anticipation in innovation (Nadin 2012c) and the interaction I had with a distinguished musician (Tibor Szász, Freiburg) regarding anticipation expressed in musical creativity (in particular Enescu and Bartók). But this was also a time for putting the finishing touches on these Prolegomena. Publication of the second edition of *AS* does not do any favor to Rosen – his work stands on its own – but to everyone who is authentically interested in the subject. My prediction is that the book will stimulate more attempts to integrate the anticipatory perspective in our understanding of the world.

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Chapter 1

Preliminaries

1.1 General Introduction

The original germinal ideas of which this volume is an outgrowth were developed in 1972, when the author was in residence as a Visiting Fellow at the Center for the Study of Democratic Institutions in Santa Barbara, California. Before entering on the more formal exposition, it might be helpful to describe the curious circumstances in which these ideas were generated.

The Center was a unique institution in many ways, as was its founder and dominating spirit, Robert M. Hutchins.¹ Like Mr. Hutchins, it resisted pigeonholing and easy classification. Indeed, as with the Tao, anything one might say about either was certain to be wrong. Despite this, it may be helpful to try to characterize some of the ambience of the place, and of the remarkable man who created it.

The Center's spirit and *modus operandi* revolved around the concept of the Dialog. The Dialog was indispensable in Hutchins' thought, because he believed it to be the instrument through which an intellectual community is created. He felt that "the reason why an intellectual community is necessary is that it offers the only hope of grasping the whole". "The whole", for him, was nothing less than discovering the means and ends of human society: "The real questions to which we seek answers are, what should I do, what should we do, why should we do these things? What are the purposes of human life and of organized society?" The operative word here is "ought"; without a conception of "ought" there could be no guide to politics, which, as he often said, quoting Aristotle, "is architectonic". That is to say, he felt that politics, in the broadest sense, is ultimately the most important thing in the world. Thus for Hutchins the Dialog and politics were inseparable from one another.

For Hutchins, the intellectual community was both means and end. He said,

The common good of every community belongs to every member of it. The community makes him better because he belongs to it. In political terms the common good is usually defined as peace, order, freedom and justice. These are indispensable to any person, and no person could obtain any one of them in the absence of the community. An intellectual community is one in which everybody does better intellectual work because he belongs to a community of intellectual workers. As I have already intimated, an intellectual community cannot be formed of people who cannot or will not think, who will not think about anything

in which the other members of the community are interested. Work that does not require intellectual effort and workers that will not engage in a common intellectual effort have no place in [the intellectual community].

He viewed the Dialog as a continuation of what he called “the great conversation”. In his view,

The great conversation began with the Greeks, the Hebrews, the Hindus and the Chinese, and has continued to the present day. It is a conversation that deals – perhaps more extensively than it deals with anything else – with morals and religion. The questions of the nature and existence of God, the nature and destiny of man, and the organization and purpose of human society are the recurring themes of the great conversation. . .

More specifically, regarding the Dialog at the Center, he said,

Its members talk about what ought to be done. They come to the conference table as citizens, and their talk is about the common good. . . It does not take positions about what ought to be done. It asserts only that the issues it is discussing deserve the attention of citizens. The Center tries to think about the things it believes its fellow citizens ought to be thinking about.

The Dialog was institutionalized at the Center. Almost every working day, at 11:00 a.m., the resident staff would assemble around the large green table to discuss a pre-circulated paper prepared by one of us, or by an invited visitor. At least once a month, and usually more often, a large-scale conference on a specific topic, organized by one or another of the resident Senior Fellows, and attended by the best in that field, would be held. Every word of these sessions was recorded, and often found its way into the Center’s extensive publication program, through which the Dialog was disseminated to a wider public.

It might be wondered why a natural scientist such as myself was invited to spend a year at an institution of this kind, and even more, why the invitation was accepted. On the face of it, the Center’s preoccupations were far removed from natural science. There were no natural scientists among the Center’s staff of Senior Fellows, although several were numbered among the Center’s Associates and Consultants; the resident population, as well as most of the invited visitors, consisted primarily of political scientists, journalists, philosophers, economists, historians, and a full spectrum of other intellectuals. Indeed, Mr. Hutchins himself, originally trained in the Law and preoccupied primarily with the role of education in society, was widely regarded as contemptuous of science and of scientists. Immediately on assuming the presidency of the University of Chicago, for instance, he became embroiled in a fulminating controversy on curricular reform, in which many of the faculty regarded his position as anti-scientific, mystical and authoritarian. At an important conference on Science and Ethics, he said, “Long experience as a university president has taught me that professors are generally a little worse than other people, and scientists are a little worse than other professors”.

However, this kind of sentiment was merely an expression of the well-known Hutchins irony. His basic position had been clearly stated as early as 1931:

Science is not the collection of facts or the accumulation of data. A discipline does not become scientific merely because its professors have acquired a great deal of information. Facts do not arrange themselves. Facts do not solve problems. I do not wish to be

distinct disciplines, there were a number of most important practical consequences. For instance, our investigation of biological organisms places us almost always in the position of an external observer, attempting to characterize the infinitely rich properties of life entirely from watching their effects without any direct perception of underlying causal structures. For instance, we may watch a cell in a developing organism differentiate, migrate, and ultimately die. We can perceive the roles played by these activities in the generation and maintenance of the total organism. But we cannot directly perceive the causal chains responsible for these various activities, and for the cell's transition or switching from one to another. Without such a knowledge of causal chains, we likewise cannot understand the mechanisms by which the individual behaviors of billions of such cells are integrated into the coherent, adaptive behavior of the single organism which these cells comprise.

On the other hand, we are ourselves all *members* of social structures and organizations. We are thus direct participants in the generation and maintenance of these structures, and not external observers; indeed it is hard for us to conceive what an external observer of our society as a whole would be like. As participants, we know the forces responsible for such improbable aggregations as football games, parades on the Fourth of July, and rush hours in large cities. But how would an external observer account for them?

It is plain that a participant or constituent of such an organization must perceive and respond to signals of which an external observer cannot possibly be aware. Conversely, the external observer can perceive global patterns of behavior which a participant cannot even imagine. Certainly, if we wish to understand the infinitely subtle and intricate processes by which biological organisms maintain and adapt themselves, we need information of both types. Within the purely biological realm, we seem eternally locked into the position of an external observer. But if there were some way to effectively relate biological processes to social ones; if, more specifically, both biological and social behaviors constituted alternate realizations of a common relational scheme, it might become possible to utilize our social experience as a participant to obtain otherwise inaccessible biological insights. Indeed, this capacity for transferring data and information from a system in which it is easy to obtain to a similar system in which it is hard to obtain is a unique characteristic of the relational approach. This was my basic hope; that I as a theoretical biologist could learn something new about the nature of organisms by judiciously exploiting the cognate properties of social systems.

The other side of that coin was equally obvious; that by exploiting biological experience, obtained from the standpoint of an external observer, we could likewise develop entirely new insights into the properties of our social systems. At that time, however, my detailed knowledge of the human sciences was essentially nil; to explore the possibilities raised above would require what appeared to me to be a major educational effort, and one which at first sight seemed far removed from my own major interests and capabilities.

It was at this point that I perceived the benefits of the community of scholars which Robert Hutchins had created. At the Center I could explore such ideas, while at the same time it was possible for me to learn in the most painless possible fashion