

Laws, Constraints, and the Modeling Relation – History and Interpretations

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The modeling relation and models of complex systems expressed by non-integrable constraints were developed during *ca.* 1970–1987, when I worked most closely with *Robert Rosen*. I contrast the modeling relation *within the organism* itself as a necessary condition for life and evolution, as *Rosen* developed it in his fundamental work '*Anticipatory Systems*', with the modeling relation *within our brain* as a necessary condition for understanding life, as *Rosen* developed it in '*Life Itself*'. Our approaches to the modeling relation were complementary. *Rosen* focused on the formal relational conditions necessary for life, and on the limitations that formal mathematical-symbol systems impose on our models. I focused on the physical conditions necessary for these abstract relations to be realized, and on the symbolic control in organisms that allows open-ended evolution. I contrast *Rosen's* views on physics and evolution in '*Anticipatory Systems*' and later papers with his views in '*Life Itself*', and I speculate on why they differ so greatly.

1. Personal History. – I had a close association with *Robert Rosen* from our early interest in hierarchical-system theory in the late 1960s and early 1970s [1–4] to the late 1980s. In 1971, *Rosen* invited me to join the Center for Theoretical Biology (CTB) at the State University of New York at Buffalo as a visiting professor. That year, he was on leave at the Center for Democratic Institutions in Santa Barbara. When he returned, we worked together at CTB from 1972 to 1975, when CTB was dissolved. We continued to communicate and visit each other after he moved to Dalhousie University in Halifax, Nova Scotia, and I moved to State University of New York at Binghamton. Many of the topics we discussed appeared in our publications in the 1970s and 1980s, when our concepts of constraints and the modeling relation were developed.

Rosen and I had two closely related motivating interests: one was the nature of life, and the other was the epistemology of models. The nature of models was a basic interest for both of us because we believed that all *living organisms persist and evolve by virtue of their heritable internal models*. We focused on understanding how models are created, on their physical and logical limitations, and what they can or cannot tell us about reality. During the time of our discussions, *Rosen* viewed the genetic system as a form of internal model that directs metabolism and reproduction, as well as predictive behavior acquired by evolution and emergence of new functions. We both independently discovered why this genetic model could not be adequately described by the formalism

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of Newtonian state-determined dynamics. An *alternative description* was both logically and physically necessary. *Rosen's* focus was on the formalization of predictive models that could describe informational and functional organization. He was less interested in the possible physical realizations of these models.

I viewed genes as molecular symbol systems in which the cell's genetic model is encoded, but my focus was on the necessary physical conditions that allow symbolic control of the cell's dynamics and evolutionary emergence, while always obeying inexorable fundamental physical laws [5][6].

Following our discussions at CTB and through the late 1980s, we both came to the conclusion that all informational concepts, like measurement, memory, prediction, and the modeling relation itself, can be formally modeled by physical laws, *provided* that they are augmented by local kinematic constraints that can be formally described only by inexact differential forms. As a consequence, these constraints are formally non-integrable and logically impredicative. Later, however, *Rosen* took an entirely different approach when he wrote '*Life Itself*' (LI) [7]. I shall trace how *Rosen's* views changed, by quotations from his publications.

2. The Modeling Relation as Control and as Knowledge. – To help understand *Rosen's* change of view, it is necessary to clearly distinguish our two motivations for studying the modeling relation. The first motivation was to understand life as a physical system under the *control* of the organism's internal predictive model. The second motivation was to understand our *knowledge* of life, that is, the epistemology of cognitive models of life. Both control and knowledge depend on models. The cognitive models in brains have evolved over billions of years from the internal genetic control models of the earliest cells, but even though there are enormous differences, there are also common physical and functional requirements. In particular, we understood why physical degeneracy of informational constraints is necessary for symbolic encodings of models that allow novelty and emergence in the evolution of cells and in the creativity of brains.

The first motivation for studying the modeling relation is apparent in the work '*Anticipatory Systems*' (AS) [8], where *Rosen* focuses on the predictive *model within the organism* itself. As *Rosen* states in the Preface of AS [8] (there on p. vii): '*The present volume is intended as a contribution to the theory of those systems which contain internal predictive models of themselves and/or their environment, and which utilize the predictions of their models to control their present behavior.*'

In AS, *Rosen* develops the evolutionary consequences of the fundamental concept of *error* in predictive models and its relation to *system complexity* (AS; Chaps. 5.6 and 5.7). *Rosen* states there an important, but often neglected, fact of life, that evolution depends on '*the proliferation of inequivalent models*' within the organism, and that, in this sense, '*biology is the science of mutability; i.e., the science of error*' [8] (there on p. 319). *Rosen* then concludes: '*The relation between them [error and complexity] can be summed up in the proposition «simple systems do not make errors»*' [8] (there on p. 321). This is because errors occur only in models, not in the systems being modeled. It follows that '*a complex system is one in which errors can occur*' [8] (there on p. 323), and this leads to *Rosen's* concept of a *complex system* as a *system that contains an internal model*. One of his measures of complexity is a system's capacity to form

inequivalent models, or, as he says, ‘*the capacity for making errors*’ (*ibid*). This is commonly interpreted as a measure of semantic-information capacity.

As I will explain, *Rosen* took a different approach for studying the modeling relation in LI [7], where he focuses on our human cognitive models of organisms *within our brain*. It was a lifelong desire of *Rosen* (following *Rashevsky*) to formulate a pure relational model of life. This is the primary motivation for LI, where *Rosen* takes a completely different view of physics and evolution than he developed in AS. In LI, *Rosen* no longer characterized life by its internal predictive models that allow adaptive evolution. Instead, he develops a timeless relational concept of organisms based on natural and inferential ‘*entailments*’ derived from *Aristotle*’s causal categories. He then defines life by a topological closure: ‘*A material system is an organism if, and only if, it is closed to efficient causation*’ [7] (there on p. 244).

Contrary to his earlier papers, showing that complex predictive informational systems can be modeled by non-integrable constrained dynamical systems, *Rosen* takes a radical and controversial view in LI that none of the present scientific formalisms – neither physical, nor mathematical, nor computational – are rich enough to include biology, and that this inadequacy necessarily betokens a profound change not only in physics and biology, but in all of science.

In LI, *Rosen* no longer considers evolution as a necessary property of life, neither does he consider error in adaptive predictive models, as he did in AS. About evolution he says: ‘*Ever more insistently over the past century, and never more than so today, we hear the argument that biology is evolution; that living things instantiate evolutionary processes rather than life; and ironically, that these processes are devoid of entailment, immune to natural law, and hence outside science completely.*’ And he continues: ‘*To me, it is easy to conceive of life, and hence biology, without evolution. But not of evolution without life. Thus, evolution is a corollary of the living, the consequence of specialized somatic activities, and not the other way around. Indeed, it may very well be more a property of particular realizations of life, rather than of life itself. Thus it is that the word «evolution» has hardly been mentioned in the preceding pages*’ [7] (there on pp. 254–255). In other words, the central issue in LI is no longer the evolving predictive model *in the organism*, but the limitations of formal models *in our brain*.

3. Early Sources: the Problem of Hierarchy Levels. – We were both writing about the nature of hierarchical organization before we were thinking explicitly about the modeling relation. The central problem in hierarchy theory is the evolutionary emergence of new levels of control in organisms. In principle, all levels of organization have a lawful description because we believe the details at every level still obey physical laws. The problem is that the concept of control does not exist at the detailed level of universal laws [3][4]. Consequently, we need alternative models for higher levels of organization.

The origin of new levels of organization and control is a very old problem. *Lucretius* (96–5 BC) asked the question [9]: ‘*Again, if all movement is always interconnected, the new arising from the old in a determinate order – if the atoms never swerve so as to originate some new movement that will snap the bonds of fate, the everlasting sequence of cause and effect – what is the source of the free will possessed by living things throughout the earth?*’ In his 1892 book ‘*The Grammar of Science*’ [10], *Karl Pearson* devotes a

chapter to the relation of life to Newtonian physics. There he asks: ‘*How, therefore, we must ask, is it possible for us to distinguish the living from the lifeless if we can describe both conceptually by the motion of inorganic corpuscles?*’ [10] (there on p. 287). He also makes a distinction between mere *description* using physical laws and an *explanation* of why life is different: ‘*Clearly, those who say mechanism cannot explain life are perfectly correct, but then mechanism does not explain anything. Those, on the other hand, who say mechanism cannot describe life are going far beyond what is justifiable in the present state of our knowledge*’ [10] (there on p. 289). Rosen and I, along with many physicists, were stimulated by Schrödinger’s work ‘*What Is Life*’, published in 1944, and which is still interesting reading, even if it is dated, and although some of his speculations do not agree with our present knowledge.

4. Our Differing Approaches. – Our different ways of approaching the nature of life are similar to the long-standing philosophical controversies between *idealism* and *materialism*. We subscribed to neither extreme view, but rather believed that the two views must be epistemically related by the modeling relation in an irreducible and complementary sense. Rosen and I had different approaches about models, partly because of our different backgrounds. Rosen thought most naturally as a mathematician; I thought most naturally as an experimental physicist. Consequently, Rosen usually thought more like an ‘idealist’, and I thought more often like a ‘materialist’, but we regarded both as necessary for the modeling relation, as I shall explain in *Sect. 12* below.

For brevity and lack of adequate words, I will simply speak of Rosen as a formalist, and of myself as a materialist. (Note that I am using *formalist* roughly in a Platonic idealist sense, *not* in the Hilbert-logical sense.) To be more precise, Rosen, following Rashevsky’s relational biology, sought abstract and formal models of life. That is, he was not concerned with the possible material realizations of formal models, but with the abstract organization and function that characterizes the modeling relations necessary for life. Rosen says: ‘*I am persuaded that our recognition of the living state rests on the perception of homologies between the behavior exhibited by organisms, homologies that are absent in non-living systems. The physical structures of organisms play only a minor and secondary role in this; the only requirement which physical structure must fulfill is that it allows the characteristic behavior to be manifested*’ [8] (there on p. 3). Rosen felt that focusing on the particular material embodiments necessarily leads to reductionism that ‘throws away’ the underlying organization. As he states: ‘*The relational alternative to this [reductionism] says the exact opposite, namely: when studying an organized material system, throw away the matter and keep the underlying organization*’²⁾ [7] (there on p. 119). Consequently, in order to represent a model’s relations, Rosen often used the formal mathematical languages of continuum dynamics, set theory, and category theory.

I understood Rosen’s relational approach that required ‘throwing away the matter’ in order to discover the essential relational properties of life; but as a materialist, I needed a *complementary alternative model* to make empirical contact with whatever

²⁾ General note: plain text (not in italics) within quotes indicates *stressed* words or phrases in the original.

material embodiments exist. I appreciated the relational approach because, before meeting *Rosen*, I was impressed by *von Neumann*'s relational model of self-replication that he used to understand '*the completely decisive property of complexity*'. Understanding how complex organization can continually evolve was the motivation for his well-known model. His informal model was impressive because he developed it by 'throwing away the matter'. He did not introduce any material structures, physical laws, or known biological components – the conditions for a pure relational model. However, *von Neumann* recognized that when he formalized the model, it could not answer important questions. He said: '*By axiomatizing automata in this manner one has thrown half the problem out the window and it may be the more important half. One does not ask the most intriguing, exciting and important questions of why the molecules or aggregates that in nature really occur [...] are the sorts of thing they are, why they are essentially very large molecules in some cases, but large aggregations in other cases*' [11].

Rosen and I agreed that, although description of life by physical laws was fundamentally inadequate, nevertheless, living systems at all levels of organization never violate any physical laws; so while I agreed with the last clause of *Rosen*'s statement, '[...] *the only requirement which physical structure must fulfill is that it allows the characteristic behavior to be manifested*', I also agreed with *von Neumann* that formalization is only half the problem. I argued that the lawful nature of matter must play more than a *minor role* in the nature of life, but, of course, my argument depends on what is meant by 'the nature of life'. I was concerned with the actual physical requirements that any material structure must fulfill in order to satisfy *Rosen*'s and *von Neumann*'s abstract symbolic relations. In particular, I worried about *Lucretius*' question: how does symbolic control of matter arise and open-ended evolution occur when all matter, including organisms, must obey state-determined physical laws in every detail? [6].

5. Basic Agreements. – In spite of our different approaches, *Rosen* and I were discussing the same fundamental questions, like those asked by *Lucretius*, *Pearson*, and *Schrödinger*. We agreed on several basic positions:

- 1) Most important was our belief in the necessity of *multiple alternative models* to understand complex systems. This view was essential for hierarchic systems, and, in fact, this was *Rosen*'s *measure of complexity*, *i.e.*, the number of inequivalent models necessary to adequately rationalize a system [8] (there on p.322). By two *inequivalent models* we meant that neither model is reducible to, or derivable from, the other by formal means.
- 2) State-determined temporal dynamics alone (the Newtonian paradigm) is inadequate for describing life, as well as any informational concepts such as measurement, prediction, and control.
- 3) The choice of observables and their measurements (encoding) is not determined (entailed) by natural laws or by formal logic. This choice is created by the organism or the observer by natural selection, imagination, or an emergent process like creative thought. Consequently, there is no algorithm for this type of process.
- 4) All models are incomplete in the sense that all possible observables cannot be consistently incorporated into a single model (*i.e.*, a corollary of 1).

- 5) It follows that the *philosophy of reductionism is untenable* (but this does not exclude specific reductive models as one essential type of alternative model). Often, these alternative models will be hierarchical and inequivalent (not reducible or derivable, one from the other). We agreed that this is an epistemological, not an ontological, condition. As *Rosen* says: ‘*complementarity* [of inequivalent models] *is entirely a property of the formal systems and not of the natural ones*’ [8] (there on p. 83). That is, there can be no contradiction in Nature itself. Contradiction can occur only in our formal systems, and may occur when one tries to combine formally inequivalent models.

During my time at CTB and into the 1980s, we understood why the inexorability and universality of physical laws is based on principles of invariance or symmetry. We also agreed on *Hertz*’s requirements for an empirically testable model (as will be discussed in *Sect. 12*), and with the hierarchical models of other non-reductionists like *Michael Polanyi* [12] and *Herbert Simon* [13].

Discussion is not productive with someone who agrees with everything you say; neither is it productive with someone who disagrees with everything you say. *Rosen* and I had a very productive balance. I will focus here on specific issues where we disagreed for the same reason that *Rosen* and I focused on them – *i.e.*, because discussing our disagreements was more interesting and productive than restating our agreements.

6. Early Sources of *Rosen*’s Views. – In his published works, *Rosen* gave few references to show how his own ideas of non-integrable constraints and the modeling relation evolved. In AS [8], he says that the original germinal ideas for this work came to him in 1972, when he was at the Center for Democratic Institutions in Santa Barbara, CA. *Rosen*’s AS manuscript was written in 1979 after we had left CTB. It was not published until 1985, but he says, ‘[...] *none of the fundamental material has changed in the intervening five years. I have, however, added an Appendix. It has turned out that the initial anxieties which generated the book were unfounded, and since that time, I have been able to push some of the fundamental implications of the relation between system and model much further. In the Appendix, some of these more recent developments are sketched, and some of their rather startling implications are pointed out*’ [8] (there on p. vi). *Rosen* adds that he plans to discuss these implications in future manuscripts, as he did, *e.g.*, in [14–16], as will be summarized below. As a matter of fact, it was just these ‘*initial anxieties which generated the book*’ that were later assuaged by his understanding of *Hertz*’s non-holonomic constraints, and the ‘startling implications’ were that complex informational and biological systems can be modeled by such constraints.

Rosen was modeling biological systems already in 1958 in his pre-doctoral publications [17], but he did not make the concept of a modeling relation explicit. However, as early as 1959, *Rosen* independently discovered the fundamental problem with physical modeling of informational constraints. As he put it: ‘*The shock was in discovering that the families of observables I characterized in that [informational] way could not contain anything which behaved like a Hamiltonian. And, of course, without a Hamiltonian you cannot even get started in doing traditional quantum mechanics*’ [18] (there on p. 12). The Hamiltonian is an expression of the total energy of a system, and

this recognition of the energy independence (degeneracy) of informational models is the reason why it is necessary to introduce non-integrable constraints, the point that *Rosen* later developed during our discussions.

We also found that, in 1950, *R. J. Eden* had addressed the mathematics of non-integrable constraints in classical Hamiltonian and quantum-mechanical systems (at the suggestion of *P. A. M. Dirac*). These constraints introduce quasi-observables that must take on values without the intervention of an actual measurement process. Those who are interested in a rigorous treatment of non-holonomic systems, should refer to *Eden's* papers [19].

In Chapt. 2.3 of AS [8], where *Rosen* first describes the modeling relation, he gave no references to how his idea of a modeling relation arose, and in Chapt. 3.2 ('*Specific Encodings between Natural and Formal Systems*'), he refers only to his '*Fundamentals of Measurement and Representation in Natural Systems*' (FM) [20], which was published in 1978, only a year before he wrote AS. However, as early as 1972 at the CTB, we were discussing *Hertz's* modeling relation and his concept of non-holonomic constraints in '*The Principles of Mechanics*' [21]. *Rosen* left no record of how much his concept of a model in AS was influenced by *Hertz*, or how much it only confirmed what he had already pictured. In any case, it is not possible to untangle all the influences on our thoughts from our many seminars and discussion groups at CTB.

We both recognized from our earlier work that measurement in its most general sense – including detection, pattern recognition, encoding, and observation – is a fundamental epistemological problem. We were well aware of the measurement problem in quantum theory and had both written papers suggesting a dependence of organisms on a non-classical theory of measurement [22][23]. It has been clear in physics, at least since *von Neumann's* discussion, that measurement cannot be described by laws, but requires an *inequivalent alternative description* [24]. *Rosen's* work FM [20] is his detailed formal analysis of measurements and the inherent ambiguities of their interpretations. *Rosen* defines the measuring instruments ('meters') by their functions [20] (there on p. 28), but does not discuss the hardware constraints of specific measuring instruments, which is a central issue in the quantum-measurement problem. FM focuses on what one can infer from the formal *symbolic results* of measurement, not the measuring devices.

However, in FM, *Rosen* made the fundamental point that *measurement constraints do not have state-determined models*. His point was that a measurement constraint '[...] invariably contains other quantities besides those which enter into the state description, which are variously described as parameters, or rate constants, or controls. These are quantities which are, by definition, not functions of the state description, yet they enter the dynamical equations on the same footing as the state description itself. The presence of such gratuitous «parameters» make it absolutely clear that our initial state descriptions were not complete and do not allow us to understand problems of dynamical interactions [of measurements] in terms of them alone' [20] (there on p. 59).

It is just these gratuitous *parameters* that are not functions of the state-determined dynamics that *Rosen* later recognized as capable of expressing all forms of local informational constraints that could function as controls, and even as computer programs, as he explained in references [15] and [16]. I had reached the same conclusion, but I disagreed with *Rosen's* introduction of *Aristotle's* causal categories to

distinguish simulations from models. I will discuss our different views of causality in *Sect. 13* below.

By the time we left CTB (1975), *Rosen* and I agreed that Newtonian physics and even quantum physics [19] can be augmented by non-integrable constraint formalisms to model informational processes. This includes complex biological systems, measurement, predictive model-based controls, open-ended evolution, and symbol systems like natural, formal, and programming languages. I will document this from *Rosen's* publications below.

7. The Necessity of Alternative Descriptions. – The recognition of the necessity of alternative descriptions was central to *Rosen's* concept of a complex system. Our earliest recognition of the inadequacy of state-determined Newtonian dynamics arose because of its inherent determinism, while *alternative descriptions* are required by measurement and control (*Lucretius' problem*). To quote *Rosen*: '[...] *the idea of a hierarchical organization simply does not arise if the same kind of system description is appropriate for all of [its activities]*' [2] (there on p. 180). Later he says: '[...] *we recognize [hierarchical] structure only by the necessity for different kinds of system description at various levels in the hierarchy*' [2] (there on p. 188). These descriptions are generally inequivalent. *Rosen* and I were also familiar with *von Neumann's* analysis [24] that measurement is also a process that cannot be described by time-symmetric reversible laws, but requires an irreversible alternative description that recognizes a 'before and after'. This raised the epistemic questions: *what is a description?* and *how are symbolic descriptions related to the natural world?* Understanding the nature of description played a fundamental motivational role in our discussions about the modeling relation.

This is where our different metaphysical beliefs produced different views of symbolic descriptions. The concept of a symbolic description implies some form of language, or at least a system of symbols that have both syntactic and semantic attributes. As a formalist, it was *Rosen's* long-standing desire to create a pure relational biology independent of the details of specific material realizations. This required describing his model in a *formal* symbol system. Consequently, he was concerned with the *syntactic limitations* of formal mathematical systems, like Gödelean incompleteness and *Turing* undecidability. *Rosen's* formalist belief was that these logical limits, and the non-formal *Church–Turing* Thesis, were too restrictive for adequate models of physical and biological systems, as well as mathematics [7][25][26]. In '*Essays on Life Itself*' [27], he says: '*I claim that the Gödelian noncomputability results are a symptom, arising within mathematics itself, indicating that we are trying to solve problems in too limited a universe of discourse. The limits in question are imposed in mathematics by an excess of «rigor», and in science by cognate limitations of «objectivity» and «context independence».*'

Here again, the contrast between AS and LI is apparent. *Rosen* did not mention these formal limits in AS, and discussed relational models only for his (M,R) -systems. He focused on augmenting physical dynamics to include internal predictive models. By contrast, these formal limits are of central importance in LI, while internal predictive models are not discussed. LI has a revolutionary and highly controversial goal compared to AS. It is an attempt to show that it is just because of these rigorous formal

and cognate limits that all our concepts of physical and biological models need to be profoundly revised.

As a materialist, I focused on the converse question of the *limitations* physical laws impose on the execution of the idealized syntactic rules of formal symbol systems [28]. From an empirical-physicist's point of view, the 'rigor' of formal computability theorems is not a necessity because all empirically testable models are only finite approximations. No measurement is exact in the formalist sense. Furthermore, there is simply no empirical evidence that any formal axioms or any logical theorems *necessarily* apply to Nature's laws. A materialist does not believe that symbol systems exist, except as a material embodiment in genes, brains, or computers, and, consequently, it is Nature's laws that place limits on symbol-vehicle manipulation, not the other way around.

Rosen and I recognized that these differences arose from classical idealist and materialist ontologies that are not empirically decidable. The extent to which the laws of physics impose limits on the ultimate nature of computation and thought, and conversely, the extent to which unprovable and undecidable symbolic expressions impose limits on what we can know about Nature, are still actively disputed [29].

Until the writing of LI, we used a common physical language for our discussions, and we agreed on the necessity of alternative models, even though we differed in our metaphysical views of models. However, *Rosen's* complete change of approach in LI effectively ended our discussions because he began using a novel vocabulary in which constraints could not even be expressed. Consequently, he no longer showed interest in non-integrable constraint of dynamical systems as a valid description of complex systems. In writing LI, *Rosen* changed to an adversarial view of physical and biological models, in particular computational and evolutionary models, on which I continued to work. I will illustrate these changes in *Rosen's* views in the following sections.

8. Universal Laws, Initial Conditions, and Constraints. – I understood *Rosen's* desire to create a pure relational model of life, but I could not go along with his revised view of physics in LI. The symmetry principles and the *inequivalent categories of initial conditions, boundary conditions, and constraints* are so fundamental that, without them, the meaning of natural law would be lost entirely. From symmetries, we can derive the *conservation laws* (*Noether's* theorem); e.g., time-translation symmetry gives conservation of energy, space-translation symmetry gives conservation of momentum, and rotation symmetry gives conservation of angular momentum.

In LI, *Rosen* dismisses these foundations claiming that '*physics has long beguiled itself with a quest for what is universal and general*'. From my perspective, along with other physicists, knowing the enormous range of empirical successes of physical theories, the claim that physicists have been fooling themselves in their search for general laws certainly needs a lot more-powerful supporting evidence than *Rosen* provided. *Rosen* only offered a metaphysical quasi-Platonic view that the limits of formal mathematics '[...] betokens the most profound changes in physics itself' [7] (there on p. 13). He also took a narrow view of physical laws, equating them to Cartesian mechanisms [7] (there on p. 213), ignoring their basis in symmetry principles. His novel vocabulary, in some cases, results in metaphors that are enlightening, but in other cases it is totally inadequate to express the essential inequivalent categories that

are fundamental for his previous discussions in AS and other works. In particular, in LI, *Rosen* does not distinguish *laws* and *constraints* [7] (there on pp. 93, 97, and 98). Having lost the distinction between *law* and *constraint*, he then loses the distinction between *law* and *mechanism*: ‘*Indeed, the claim that there is nothing outside (i.e., that every natural system is a mechanism) is the sole support of contemporary physics’ claim to universality*’ [7] (there on p. 213). It should be obvious why physicists would not recognize such a claim, because the universality of laws is based on symmetry principles that are the antithesis of local mechanisms in the normal sense.

9. Error in Measuring Devices. – I also found *Rosen*’s emphasis on the Cartesian machine metaphor, which has long been outdated in physics, a serious diversion from his own relational model. His failure to distinguish laws and constraints in LI results in an ambiguous use of other concepts like measuring devices, encoding, simulability, software, and hardware. I found this especially apparent in Chapt. 9 of LI titled ‘Relational Theory of Machines’. For example, by *Rosen*’s definition: ‘*Mechanism is merely one way of expressing Natural Law*’ [7] (there on p. 242). However, if laws are not distinguished from mechanisms, there can be no understanding of a measuring device (termed ‘meter’ in FM), which is certainly a type of mechanism. The epistemic necessity of this distinction is fundamental, and was well-known to *Rosen* [20] (there on p. 59). He understood why a measuring device, as a local-constraint structure, must have an *alternative description* from laws because it is the measurement that must select the state or initial conditions for the laws. In other words, a measuring mechanism treated as a state-determined lawful system is impredicative, and leads to an infinite regress, as shown by *von Neumann* [24] (there on p. 352).

In normal physical language, as well as in the language of hierarchy theory *Rosen* had used [2], machines are local constraint structures that require inequivalent alternative descriptions. Measurement devices and all machines belong to the next inequivalent model, a level above the state-determined microscopic laws. That level is statistical mechanics, where the observables are averages over the detailed states of many particles. The concept and formalism of probability is *inequivalent* to a deterministic model. *Max Planck* expressed this view as follows: ‘*For it is clear to everybody that there must be an unfathomable gulf between a probability, however small, and an absolute impossibility. Thus dynamics and statistics cannot be regarded as interrelated*’ [30]. Then, *von Neumann* concurs: ‘*In other words, we admit: Probability logic cannot be reduced to strict logics, but constitute an essentially wider system than the latter*’ [24].

For the materialist, this ‘*unfathomable gulf*’ has profound consequences not only for machine constraints, but also for all informational constraints, because *if they are irreversible, it means they are dissipative and, therefore, inherently noisy*. No matter how deterministic the detailed formal description of a system, that system must also obey the statistical laws of thermodynamics, and the Second Law does not allow ‘*absolute impossibility*’ (i.e., determinism) in dissipative machines. All measuring devices and mechanisms that are irreversible are, therefore, *subject to error*.

I know that *Rosen* was well aware of all this. He carefully discussed error in models in AS (Chapt. 5.6), its relation to complexity (Chapt. 5.7), and to thermodynamics and entropy (Chapt. 5.8). *Rosen* also emphasizes in FM [20] (there on pp. 107–114) that no

system is absolutely isolated from external noisy interactions. Nevertheless, in LI *Rosen* ignores not only measurement constraints, but also all the aspects of error that are inherent in the encodings of the modeling relation, and that are essential for evolution.

10. Non-Integrable Constraints. – My discovery of the necessity of non-integrable constraints came during my attempt to model eutactic copolymerization, a discovery of *Ziegler* and *Natta* for which they received the *Nobel Prize* in Chemistry in 1963. This is a process in which the choice of which type of monomer is added is governed by earlier monomers in the chain. This amounts to a local hereditary memory of past sequences. The problem is that each new monomer adds some new degrees of freedom and new constraints. So, in effect, the dynamics cannot be integrated without continuous modification by the new degrees of freedom that have their initial conditions constrained by monomers in the previous sequence. The same problem arises in protein synthesis, except that adding a new amino acid is controlled by a separate genetic memory. This led to several speculative papers, suggesting how primitive hereditary copolymer sequences might be replicated by similar non-integrable constraints [31–33].

Ordered polymer synthesis is only one type of constrained dynamics. *Why are all types of informational constraints not integrable?* As described above, a microscopic physical system is typically defined in terms of a *fixed* number of *degrees of freedom* that are represented as variables in the equations of motion expressed as positions (configuration) and momenta (time derivatives). Once the initial conditions are specified for a given time, integration of the equations of motion is a deterministic procedure for finding the state of the systems at any other time. This form of state-determined dynamics leaves no room for alternative behavior. Each set of initial conditions determines a unique trajectory for the system. Consequently there is no possibility of describing measurement, control, or predictive models by state-determined dynamics, because these *informational* concepts, by definition, require alternative configurations, including added degrees of freedom that can control which alternative dynamical pathway is actually followed.

In other words, informational concepts *require more configurational degrees of freedom for description of the system's structure than the number of degrees of freedom of its actual constrained motion*. *Hertz* named such constraints 'non-holonomic'. Informational constraints may have an arbitrarily large configuration space, as in non-dynamic memory storage, while reducing the dynamic velocity space, controlled by the constraint, to only a single trajectory, a programmed computation being an extreme example.

Rosen described these constraints this way: 'We may note that such partially constrained systems are basically what are called synergetic [by *Haken*]; systems in which many degrees of freedom are functionally related and hence appear to act in a coordinated way. If we impose the maximum number of nonholonomic constraints, then the velocity vector is uniquely determined by the configuration. The result is an autonomous dynamical system, or vector field on the configuration space. At this point, the impressed forces of conventional analytical mechanics disappear completely; their only role is to get the system moving [15] (there on p. 112).

Rosen then shows that partitioning the constraint structure to include arbitrary control parameters allows the dynamics to be programmable. In his words: 'These

relations, it should be noticed, involve new parameters, independent of both phase and time. These new parameters can be thought of as an alternative description of the constraints themselves, and hence can be identified with what we have called program.' And he then continues: *'Thus these systems are programmable, even though maximally constrained. It is this fact that renders such systems «plastic» and will be of crucial import to us in our subsequent discussion of machines, cells, and brains'* [15] (there on p. 113).

Note that *Rosen's* concept of machine here is the standard meaning. It clearly differs from his later peculiar definition of *machine* as a *simple* system, with no internal models. As *Rosen* indicates above, there is certainly no such limitation on constrained systems, as demonstrated by programmable computers, cells, and brains. *They all have internal models* and, therefore, satisfy *Rosen's* definition of a complex system, which he recognized (see *Sect. 11*).

11. Complex Information Systems. – Even before our CTB association, *Rosen* was interested in my papers on non-holonomic constraints, and he began to look into the mathematics of these constraints that are usually associated with machines. I think it is fair to say that *Rosen*, as a good formalist, was never enthusiastic about real machinery, computer hardware, or actual computer programs. His deep interest was always in formal computation theory. However, during our discussions, *Rosen* recognized that *'the initial anxieties [the limitations of Newtonian dynamics] that generated the book [AS]'* were unfounded, because the concept of non-integrable constraints allows formal modeling of non-dynamic informational controls.

Rosen first alludes to this in the Appendix of AS, where he describes the *'more general class of dynamical systems'* necessary for informational models, and he adds: *'In fact, however, there is a close relationship between the general [informational] dynamical systems and those of Newtonian mechanics; indeed, the former system can be regarded as arising out of the latter by the imposition of a sufficient number of non-holonomic constraints'*¹ [8] (there on pp. 414–428). In his footnote '1', he further explains: *'This relation between dynamics and mechanics is quite different from the usual one [Newtonian dynamics] in which the manifold of states is thought of as generalizing the mechanical notion of phase, and the equations of motion generalize impressed [unconstrained] force. In the above interpretation, however, it is quite otherwise; the manifold of states correspond now to mechanical configurations, and the equations of motion come from the reactive [constrained] forces'* [8] (there on p. 428).

Toward the end of [15] (there on p. 122) in the context of cell fabrication, *Rosen* draws the following conclusion: *'The results of our analysis above suggest that, whatever else may be true of relatively simple but nevertheless highly evolved contemporary cells, they must at least behave physically like maximally (nonholonomically) constrained programmable mechanical systems.'* Note again that *Rosen's* meanings of 'constrained' and 'mechanical' here are the standard physical usages. They are *not* his confusing usages in LI, where constraints are not distinguished from laws, and laws are characterized as mechanical. I disagreed only with the word 'maximally', which I would replace with 'optimally', because cells have evolved no more constraints than are necessary for survival.

What is even more significant here is that *Rosen* recognized that Newtonian dynamics, augmented by non-integrable constraints, satisfy his definition of a *complex*

system (as he continued to use it in LI). I will quote *Rosen* extensively, omitting the mathematics: 'As we recall, a simple system is one that (a) has a largest mathematical image or model, and (b) this largest model is a dynamical system. However some of the ideas of the preceding section [on *Hertz*'s non-holonomic constraints] indicate how we can develop a theory of systems that violate one or both of these conditions. At the same time, these ideas involve natural extensions of the causal and informational aspects of systems that we have been discussing' [16] (there in Chapt. 8 on p. 17).

There follows a mathematical discussion, where not all constraint relations can be expressed as exact (integrable) differentials, and *Rosen* concludes: 'If we consider that the chances of all these quantities defining exact differential are zero, we see that systems of such «informational» networks provide a far more general mode of system description than do dynamical equations of motion. Such systems (a) in general have no biggest description, and (b) have no image as a dynamical system, although they can be locally approximated by such systems' (cf. the work of *Eden* [19]).

'Material systems that possess such descriptions are therefore not simple systems. They have a multitude of partial dynamical images, but no largest one. In physical terms, they may be regarded as infinitely open, and thus they may have properties quite different from any of the simple systems or mechanisms that we have considered so far' [16] (there on p. 19). Note that, in this last sentence, *Rosen* has now redefined the normal meaning of 'mechanism', arbitrarily restricting his usage of the term *machine* to a system that has no internal model. It is important to understand that this new restricted definition of *machine* in no way invalidates his previous demonstration that non-integrable constraints can describe normal machines that do have internal models and that, therefore, by his definition, are complex systems (cf. [15], there on p. 113).

12. The Modeling Relation. – It was, in fact, *Rosen*'s earliest curiosity about non-holonomic constraints that led him to consult *Hertz*'s work 'The Principles of Mechanics', where, in the Introduction, *Hertz* expresses the condition for a good model and its epistemological limits: 'We form for ourselves images or symbols of external objects; and the form which we give them is such that the logically necessary (*denknotwendigen*) consequents of the images in thought are always the images of the necessary natural (*naturnotwendigen*) consequents of the thing pictured.' [21] (there on pp. 1 and 2)

This is a necessary, but not sufficient, condition. Sufficiency includes many less-objective criteria such as simplicity (*Ockham*'s razor), fecundity, elegance, and other aesthetic qualities. We discussed *Hertz*'s epistemology and concepts of constraints extensively in ca. 1973 at CTB. I believe *Rosen*'s first graphical diagram of the modeling relation (*Figure*) occurs in Chapt. 2.3 of AS, where he explains: 'We seek to encode natural systems into formal ones [such that] the inferences or theorems we can elicit within these formal systems become predictions about the natural systems we have encoded into them' [8] (there on p. 74). *Rosen* sometimes expressed this relation as the 'commutation relation', although it does not have its formal meaning in the modeling relation.

Rosen's description of the modeling relation in LI is essentially the same as it was in AS, but in LI he applies it to our brain's model of life, while in AS it was generally applied to the organism's adaptive internal predictive controls. As a basic epistemology,

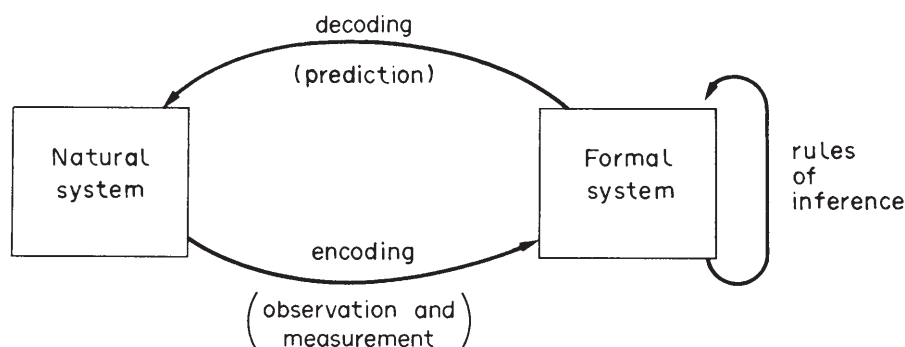


Figure. Rosen's *first diagram of the modeling relation*. A successful model is one in which the encoded observations of the natural system elicit, by rules of inference in the formal model, decodings that are predictions verified by further observations of the natural system (taken from 'Anticipatory Systems' [8], there Fig. 2.3.1., p. 74).

there is nothing novel in this view of a model. It goes back to *Plato's* shadow image on the wall of a cave, the projection metaphor still used in physics to describe measurement. The idealist is justified in claiming that all we can experience directly is this shadow (the right side in the *Figure*). On the other hand, the materialist is justified in claiming that something is casting the shadow (left side of the *Figure*). The difficult epistemic questions remain about how we project or encode images, and how we judge the correspondence between the consequents of the image in the ideal model and the image of the consequents of material nature.

There is a more-subtle epistemic limitation that *Hertz* added to his condition for a good model, which bears directly on *Rosen's* and my disagreement about causal categories. *Hertz* continues: '*For our purpose it is not necessary that they [the images] should be in conformity with the things in any other respect whatever. As a matter of fact, we do not know, nor have we any means of knowing, whether our conception of things are in conformity with them in any other than this one fundamental respect [the commutation relation]*'.

Rosen clearly recognized this limitation. In reference to computer simulation, he says: '*Our second observation is also epistemologically important. It says that all relevant features of a material system and of the model into which it was originally encoded (Fig. 1) [see Figure above], are to be expressed as input strings to be processed by a machine whose structure itself encodes nothing. That is to say, the rules governing the operation of these machines, and hence the entire inferential structure of the string processing system themselves, have no relation at all to the material system being encoded. The only requirement is that the requisite commutativity hold, as expressed in 1 above [the modeling relation] between the encoding on input strings and the decoding of the resultant output strings*' [26]. This last sentence is just *Hertz's* limitation when you replace *Rosen's* 'only requirement' by *Hertz's* only 'means of knowing'.

Rosen, by definition, distinguished *models* from *simulations* by the condition that models preserve *Aristotle's* causal categories, while simulations do not. As *Rosen* says, programmed computation does not *in itself* encode any causal or physical categories of

what it simulates. *Rosen* saw this failure to preserve causal categories in a computer program as a key problem: ‘[...] all the information regarding the system to be simulated must go into the input sequences [the program] to the simulator. This includes its initial conditions (material cause), parameter [program] values (formal cause), and equations of motion (efficient cause). It is thus very easy to see now that the causal structure in what we are simulating cannot be preserved in the simulation itself’ [16] (there on p. 17). *Rosen* also makes this a central objection in LI. He argues that it is necessary to preserve causal entailments in a model; consequently, he objects that ‘[...] in simulation, the hardware of the simulator [the computer] needs to have nothing whatever to do with the hardware [physical system] that it simulates [7] (there on p. 193). That is certainly the case; but I think this is the case for all models, those in cells and in our brains as well as those in computers. The problem is that *Aristotle’s* ‘metaphysical’ causal categories do not appear to be associated with physical categories by any observable test and, therefore, *Hertz’s* epistemic limitation prevents *Rosen* from making an empirical distinction between simulations and models.

13. Physics and Metaphysics of Causality. – Clearly, *Rosen* and I differed on the ontological status of causality. In AS, *Rosen* used *Aristotle’s* causal categories in the Appendix only as an analogy to illustrate the inequivalence of genotype and phenotype [8] (there on p. 413), but none of his arguments in AS depended on this analogy. On the contrary, in his arguments in references [15][16], as well as in LI [7], *Rosen* placed heavy reliance on the ontological status of *Aristotle’s* causal categories. I agreed that *Aristotle’s* description of material, efficient, formal, and final causes was, at least metaphorically, useful to illustrate the concept of the inequivalence of models, but I did not see that *Rosen* provided any persuasive reasoning or evidence for his association of these metaphorical, and rather ambiguous, Aristotelian categories of causality with the physicist’s well-defined inequivalent categories of initial conditions (states), measurement constraints, and natural laws. *Rosen* simply asserted that *Aristotle’s* causes are ‘tacit’ in the physicist’s categories: ‘Actually three out of the four of *Aristotle’s* causal categories are tacit in the Newtonian formalism. Suppose we ask why a certain mechanical system is in a state (phase) $(x(t), v(t))$ at a time t . We can answer this question in the following different ways: 1) Because it was in phase $(x(0), v(0))$ at a time $=0$ [state or initial conditions]. This corresponds to what *Aristotle* called material cause. 2) Because of the constraints which convey upon the system an «identity», allowing it to go from the given initial phase to the later phase. This corresponds to what *Aristotle* called formal cause [constraints include measurement]. 3) Because of the character of the environment, as manifested in impressed forces and what we call inputs. This is what *Aristotle* called efficient cause [state-transition laws], and it is embodied explicitly in the operator $[\phi_g]$ above’ [15] (there on pp. 119 and 120).

Rosen further said: ‘Inequivalence of the causal categories is of central importance to us in the distinction we have drawn between modeling (which preserves the causal structure) and simulation (which does not)’ [15] (there on p. 121).

For any physical theory, initial conditions, measurement constraints, and laws are indispensable, necessary inequivalent categories, but *Rosen* does not explain how *Aristotle’s* causes can be related empirically to these categories. Physicists cannot objectively attribute causality to the basic laws of Nature for many reasons. One of

them is that the concept of causality is certainly *irreversible*. Effects do not precede causes, by definition; but the fundamental microscopic laws of physics are time-symmetric and, therefore, *reversible*. Logically, therefore, causality cannot be attributed to the laws themselves.

Furthermore, to an empiricist, the category of *phase* (*state* or *initial conditions*) has no causal powers at all because it depends simply on the observer's choice of *when* to measure the system. Clearly, when to make an observation is the observer's choice, not a causal necessity of the system being observed. One could only say that measurement and calculation are actions initiated or *caused* by the observer, not by laws, because, as *Rosen* often emphasized, measurement is unentailed by laws. *Rosen* also said that final causes are excluded from physics; but measurement is clearly a *purposeful* or *teleological* act of an observer; therefore, according to *Aristotle*, it would qualify as a type of *final cause*.

Causality has always been a metaphysically contentious issue, because there are so many ways of thinking about it [34]. For the above reasons, from a philosophical and empirical point of view, *Rosen's* distinction between modeling and simulation, which is based on preserving causal categories, appear to me as only a question of how the modeler chooses to interpret the syntax and bits of a programmed computation or the base sequences in DNA, or for that matter, how he interprets the images in his own brain.

14. Formalist and Materialist Views of the Modeling Relation. – My labeling of *Rosen* as a 'formalist' and of myself as a 'materialist' clearly oversimplifies our views. Like most philosophers, physicists, and mathematicians, our beliefs were more complex, and we shared both formalist and materialist positions to some degree.

Nevertheless, generally a materialist would characterize the right-hand formal side of the modeling relation shown in the *Figure* as symbolic, but *only* by virtue of an *interpretation* by some physical structure within cells or brains. Its separation from the left-hand side of the modeling relation is *only* required by the epistemic necessity of separating the knower from the known. These symbol vehicles can be described by local, non-integrable constraints that, while obeying laws, can also implement arbitrary syntax or 'formal' rules. Interpreted as information or instructions, the *only* significant link of symbols to laws is that they constrain the dynamics of genes, brains, and computer hardware. Because they are irreversible, they dissipate energy and are subject to error.

As a formalist, *Rosen* would generally accept this material assessment, *except* for the three uses of 'only'. *Rosen*, like all good formalists, would not accept the fact of material error in computers or brains as a persuasive argument that matter is the *only* fundamental reality. On the contrary, to a formalist, *error* is persuasive evidence that material structures do not have as clear and distinct existence as do abstract forms. The results of formal proofs or computations appear secure to a formalist, even though we cannot be absolutely sure that material neurons, counters, switches, and memories are error-free. In particular, to the formalist, conclusions like *Gödel's* incompleteness and *Turing's* undecidability proofs are thought to be true and inescapable, no matter how genes, brains, and computers actually function in their material embodiments.

I say, we shared aspects of both views, because, in practice, almost all physicists as well as mathematicians tacitly *act like formalists*, and seldom do they concern

themselves with the possible physical limitations on their symbolic expressions. However, unlike most mathematicians, *von Neumann* fully appreciated that symbolic manipulations as well as measurements are irreversible, dissipative, and, therefore, not free of error. However, he recognized that what saves the day for formal proofs and computation is not that they exist in a formally inerrant ideal Platonic realm, but that the *probability of error* can be kept as low as we require by the use of logical checks, redundancy, and error-detection and -correction codes, techniques that he pioneered [35].

Rosen, on the other hand, believed that any limitations of formal systems would carry over to physical reality. As he says in LI: “Hence, if a formalism, whose entailments are in congruence with causal entailments in a natural system via such a diagram [see Figure], can be replaced by an equivalent formalization, that would say something quite drastic about causality itself. Specifically, since all entailment in a formalization is algorithmic, this fact alone would place profound limitations on the laws of nature, and hence on the things that can sit on the left-hand side of the diagram’ [7] (there on p. 191). On the contrary, materialists follow the earlier *Einstein*’s statement of the disjoint nature of the left and right sides of the modeling relation: ‘In so far as the propositions of mathematics are certain [on the right side] they do not apply to reality; and in so far as they apply to reality [on the left side] they are not certain.’ There is also no empirical reason that the manipulation of symbols on the right side need to be algorithmic.

A materialist always finds that the results of a measurement can be represented by a finite, usually small, number of discrete symbols. In practice, it is usually difficult to get a numerical result of an actual measurement that gives more than 20 significant bits of information (a limiting accuracy of one part in a million). Results of over 40 significant bits are exceedingly rare [36]. Consequently, computer simulations of physical systems rarely need to represent numbers with more than 40 bits. For this reason alone, a materialist sees little relevance in formal proofs of *Turing*’s non-computability and *Gödel*’s incompleteness, which are based on infinite sets. Instead, the practical problem is finding enough processing time and memory capacity to reach the desired accuracy of their approximate, often non-algorithmic, simulations.

15. Material *von Neumann* vs. Formal *Turing* Machine. – Of course, *Rosen* and I were not the only ones to differ on these distinctions between the *material* machine of *von Neumann* and the *formal* machine of *Turing*. It has always been a source of materialist vs. idealist disputes, often involving the *Church–Turing* Thesis, an issue that *Rosen* often used in his arguments [25][26] (*cf.* also [7], there on p. 191). These are philosophical disputes that are empirically and logically unresolvable, largely because of their ambiguity [37]. In any case, for the reasons mentioned in the previous section, scientists who program real computers regard these metaphysical disputes over formal computability and the *Church–Turing* Thesis as empirically irrelevant, while clearly, for the formalist and logician, *Turing*’s proofs of non-computability are central.

Turing’s most important insight for material computer hardware is that all the symbol manipulations that are formally computable can be done by *one* machine. The essential trick for universality was to show that one machine head can be instructed by symbols on its tape to follow the rules of any machine head, and, therefore, by a

symbolic description of that machine, the universal machine could calculate the same function as any machine.

Usually, *von Neumann* is credited as the first to fully understand the practical significance of this formal principle: that to achieve open-ended computational generality with a single hardware device, the symbolic instructions must be free of the hardware dynamics, and, at the same time, the symbols must control the syntax of symbol manipulations of the hardware. This principle allows in practice a *material approximation* of a *formal* ‘Universal Turing Machine’. This approximation can be realized only if we separate the dynamically unconstrained *symbolic* memory (or software) from the symbolically constrainable dynamic, symbol-changing hardware. This is now called a computer with a *general-purpose, memory-stored program*.

It is this same fundamental principle that *von Neumann* used to justify his ‘*threshold of complexity*’. That is, by separating symbolic descriptions from dynamic construction in his kinematic self-reproducing automation, he argued that a single self-replicating construction process could have ‘general-purpose’, open-ended evolutionary potential. By separating the non-dynamic ‘quiescent’ *symbolic description* that controls the dynamics of a *universal constructor*, he showed that this one construction process could evolve unlimited complexity, because the controlling description was open-ended [11] (there on p. 86).

Notably, *von Neumann*’s informal argument was influential, because it qualitatively satisfied the modeling relation with respect to real cell replication. As I already indicated in *Sect. 4*, it was a pure relational model that did not require any material observables, and yet it helps to rationalize why all existing cells replicate and evolve as they do. His model had a major influence in the field of artificial-life research based on computer models to discover new insights into biological organization, evolution, and behavior.

I do not believe that *Rosen* ever appreciated *von Neumann*’s *informal* thinking about complexity. Instead, *Rosen* focused only on the formal aspects of *von Neumann*’s (admittedly incomplete) attempt to axiomatize replication, which he felt competed with his own (M,R) -model. He missed *von Neumann*’s basic concept of a complex evolvable system that, like his own concept, was a system controlled by an internal symbolic description.

Early in his career, *Rosen* wrote a formal criticism of what he thought was *von Neumann*’s argument. Briefly, *Rosen* argued that a formal replication mapping had to be an element of its own range, leading to an impredicative infinite regress [38]. There was nothing wrong with *Rosen*’s formal argument, but it simply did not apply to *von Neumann*’s non-formal discussion of his kinematic model, which he stated was only an ‘*analogy closely related to Turing’s principle* [of universality]’, not a logical inference or formal model [39] (there on p. 315). *Rosen* later claimed that *von Neumann*’s argument was invalid, based on another misunderstanding. *Rosen* stated that *von Neumann* ‘*argued that computation (i.e. following a program) and construction (following a blueprint) are both algorithmic processes, and that anything holding for one class of algorithmic processes necessarily hold for any other class*’ [8] (there on p. 419). *Rosen* repeated this criticism in reference [16] (there on p. 114) and in other papers.

This is certainly not what *von Neumann* had argued. He made the *opposite* point that ‘*formalizing was only half the problem*’, the other half being the material-

construction problem [11] (there on p. 77). He further warned that it was ‘*clear at which point the analogy with the Turing Machine ceases to be valid*’ [39] (there on p. 318), which was the fact that *construction is not algorithmic*. Further, *von Neumann* stated that any analogy of a formal *Turing Machine* with a material organism would break down just because algorithmic computation requires complete, unambiguous instructions for every step, while genetic description is *not* complete and, therefore, *not algorithmic*. Genes provide only enough information to functionally constrain the physical dynamics.

It is apparent that *Rosen* missed *von Neumann*’s point that life is not algorithmic, because, had he seen it, he would certainly have agreed. *Rosen* made the same point many times that non-algorithmic *self-assembly* (epigenetic) processes that do not require complete genetic instruction are characteristic of life. These self-assembly processes are what most clearly distinguish organisms from man-made machines and other artifacts. The most fundamental example is the dynamics of protein folding, which is under constraint of a passive, linear-coded (symbolic) sequence, and which results in dynamic rate-controlling enzymes.

I think that it was *Rosen*’s later narrow concept of *machine* that obscured his own earlier arguments that a memory-stored program is certainly one form of internal model that can control a material machine’s dynamic behavior. In fact, *Rosen*’s earlier definition of a *complex system* – *i.e.*, a system controlled by an internal model [8] (there on p. 323) – is conceptually the same condition that *von Neumann* required for his *complexity threshold*. Ironically, had *Rosen* understood *von Neumann*’s informal discussions, he would have found agreement not only with his own concept of complexity, but also with the non-algorithmic nature of life. Instead, his continued misunderstanding of *von Neumann*’s arguments only diverted readers from *Rosen*’s otherwise valid arguments.

16. ‘Anticipatory Systems’ Contrasted with ‘Life Itself’. – I found the adversarial stance in ‘*Life Itself*’ (LI) [7] completely out of character with the *Rosen* with whom I carried on such fruitful discussions for over 20 years. I know of only one lengthy published record of our discussions [40], but its non-adversarial tone is typical of all our discourses. *Rosen* sometimes criticized specific statements of individual biologists, as he did in the Appendix of ‘*Anticipatory Systems*’ (AS) [8] (there on p. 430), but he never expressed the general irritation and revolutionary attitude towards physics, biology, and evolution that appears in LI. The blunt and non-specific objections to what he previously considered as valid and necessary alternative models is also unique to LI. The first 23 pages of LI, the first eight pages of Chapt. 5, and all of Chapt. 11 contain broad criticisms of physics, biology, evolution, and individual scientists that do not come close to a fair assessment of the knowledge gained in these fields or of the contributions of these scientists.

It may be difficult for *Rosen*’s readers who have concentrated on LI [7] and ‘*Essays on Life Itself*’ [27] to recognize this change in *Rosen*’s views and attitudes. Nevertheless, it is clear that certain statements in LI – *e.g.*, that ‘*theoretical physics has long beguiled itself with a quest for what is universal and general*’, or that the inadequacy of state-determined models ‘*betokens the most profound changes in physics itself*’, or that ‘*we cannot expect contemporary physics to successfully cope with problems other than those*

with which it has already coped' [7] (there on pp. 12 and 13) – are neither in substance nor style anything like what *Rosen* expressed during any of our years of discussions, nor are they consistent with the views of his earlier papers that I have quoted.

Rosen says explicitly in the conclusion of AS that the Newtonian paradigm is inadequate only because it is too 'confining', but he also says that 'it will continue to serve us well, provided that we recognize its restrictions and limitations as well as its strengths' [8] (there on p. 425). His normal-physics description of cells – as '(non-holonomically) constrained programmable mechanical systems' [15] (there on p. 122), which 'are therefore not simple systems', are 'infinitely open', and have 'a multitude of partial dynamical images, but no largest one' [16] – is certainly a different view of physics than his statement that 'the machine metaphor is not just a little bit wrong; it is entirely wrong and must be discarded' [7] (there on p. 23).

The same complete change of view of evolution appears in LI from that in AS. *Rosen* never suggested that evolution was anything but central to life in any of our discussions or in his extensive publications on the subject. I am at a loss to understand how he could so mischaracterize evolution theory as 'outside science completely' [7] (there on p. 255). His assertion, 'hence we are driven to expunge entailment from evolution entirely, not on any intrinsic scientific grounds, but because of the psychological requirements of biologists' [7] (there on p. 257), cannot have come from the same frame of mind that wrote so extensively about evolution. After all, this is the evolution theory that *Rosen* discussed in depth in AS [8] and in many earlier and later publications as a fundamental and inevitable biological process, and to which he contributed many ideas about error, adaptation, and, especially, the emergence of new levels of organization and function. In fact, *Rosen* focused on the modeling relation in AS because he recognized that an internal predictive model was the source of adaptation not only in evolution, but also in social, cultural, and cognitive systems. It is, therefore, a striking change in *Rosen*'s view that the concept of an evolving internal predictive model that was central to AS has virtually disappeared in LI. Also missing is his well-developed analysis of non-integrable constraints for representing complex models and all types of informational systems.

These contrasts raise the obvious question: why, in presenting the relational model in LI, did *Rosen* apparently ignore the central concepts on which many years of his work are based? Specifically, why did he ignore or dismiss his long-standing beliefs in the necessity of alternative models, symmetry principles as the basis of universal physical laws, complex informational systems modeled by non-integrable constraint-controlled dynamics, and the necessity of internal predictive models for adaptive evolution?

17. My Interpretation of 'Life Itself'. – I have no doubt from our discussions as well as from *Rosen*'s own words that his dominating motivation was to articulate a pure relational model of biology – a desire he held for all of his scientific life, beginning with his (*M,R*)-systems. *Rosen* had a quasi-Platonic belief that, 'the mathematical universe comprises systems of entailment (inferential entailment) no less compelling than the causal relations governing objective events in the external world' [41] (there on p. 95). In his autobiographical reminiscences, he says: 'But I rather believe that the corpus of mathematics is the only other thing which shares the organic qualities of life, and

provides the only hope for articulating these qualities in a coherent way' [18] (there on p. 17).

This belief in the power of mathematical forms surely provided *Rosen*'s deepest motivation; consequently, I interpret LI as *Rosen*'s final effort to produce just such a pure relational model of biology. *Rosen* was not satisfied with anything less, which by his own definition meant an inequivalent model that is not derivable from, or reducible to, material structures, physical laws, constraints, biochemistry, genetics, or evolution theory. As I have shown from his publications, *Rosen* clearly understood that non-integrable constrained dynamics provided a formalism that could describe all the aspects of complex systems as he defined them in LI, and that they also could provide a physically consistent model of life and open-ended evolution; but clearly this did not satisfy his deepest desire for a pure relational model. This approach depended too much on normal physics for a pure relational model. That objection is understandable, because a pure relational model, *in principle*, abstracts away time, matter, energy, and physical laws. *Rosen* says: '*As I have developed it [the LI relational model] so far, there is no time parameter, no states, no state transition sequences. There are only components (mappings) and the organizations, the abstract block diagrams, which can be built from them*' [7] (there on p. 134).

I think *Rosen* felt that his strong criticisms of established models would help justify his pure relational model. To some extent, this might be the case, indeed, but I think he went to far. The main problem with LI is that instead of viewing them as valid, though limited, alternative models, *Rosen* has presented them as conceptually misguided approaches, especially the many valuable reductionist and computational models that he felt were the nemesis of relational models. I know from our discussions that formalization was so intrinsic to his thinking that even when he was developing the physics of non-integrable constraints, *Rosen* was interested only in the abstract mathematics, and not in the physics of the material structures that were necessary for implementing the constraints.

Instead of beginning LI by stating these principled requirements for a pure relational model, *Rosen* begins in the Prolegomena with blanket criticisms of physics and biology. Instead of offering his model as an *alternative* to other models, he makes the grandiose and empirically unsupported claim that his relational model will revolutionize physics, biology, and all of contemporary science. He asks the rhetorical question: '*What if physics is the particular and biology is the general, instead of the other way around? If this is so then nothing in contemporary science will remain the same. For then the muteness of physics arises from its fundamental inapplicability to biology and betokens the most profound changes in physics itself*' [7] (there on p. 13). One answer is obvious: this is not the case, because *Rosen* has already explicitly demonstrated in his earlier papers that the physics of constrained systems is neither particular, nor mute, nor inapplicable to living systems.

Another reason why this is not the case is that *Rosen*'s subsequent definition of *generality* says nothing about physical models. In fact, all that can be concluded from *Rosen*'s usage of *general*, as he discusses it in LI (Chapt. 2), is that the *formal mathematics* of state-determined dynamics required for describing fundamental 'simple' physical laws is less general than the *formal mathematics* of non-integrable constrained dynamics required for describing complex systems and life. This is clearly

the case, but that is not the meaning of *generality* used in physics or any science. Rather, the generality of a scientific model is not about the formal mathematics of the model. The generality of a scientific model is an *empirical issue* that depends on the size of the domain of Nature over which the *modeling relation* can be satisfied by observation and measurement.

In spite of his gratuitous criticisms of other sciences, I believe *Rosen's* relational model of life can contribute unique perspective to existing models, not because of the generality of its formalisms, but precisely because it is *inequivalent* to law-based and constraint-controlled dynamic models, as well as to genetic models, and all the other levels of alternative descriptions, from reductionist molecular models and computer models to developmental and evolutionary models. All these models have limitations, but I see no reason to expect any of these physical laws and biological models to be invalidated by the formal and metaphysical arguments *Rosen* presents in LI. *They can be invalidated only if they do not satisfy the modeling relation.*

In any case, whatever the reasons for *Rosen's* adversarial stance in LI, I think it is important to focus on his relational approach itself, and ignore his uncharacteristic generalized objections to physics, biology, and alternative metaphysical views. To do so would be entirely consistent with his long-held and well-supported arguments for the epistemic necessity of alternative models. What I think would contribute most to biology, as well as best serve *Rosen's* legacy, is to develop his primary goal of forming a pure relational model of life, *but not in opposition* to the many existing empirically supported biological models, including his own. LI is an entirely original alternative approach. There is no question that *Rosen's* relational ideas in LI are provocative, but they need critical examination and development, especially in recognizing new types of qualitative, non-fractionable observables that can satisfy the modeling relation. As his *Essays on Life Itself* indicate, *Rosen* knew that LI was only a beginning. Hopefully, other contributors to this Special Issue of *Chemistry & Biodiversity* will focus on some of these needed developments.

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