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ROBERT ROSEN'S RELATIONALIST UNDERSTANDING OF BIOLOGICAL STATES AND QUANTUM MECHANICS

SUMMARY: *Robert Rosen's intriguing ideas of a formalized framework to understand biological systems have been discussed across the life and cognitive sciences. Yet his crude account of physical states, quantum states in particular, seems to be irreconcilable with his account of biological states, thus preventing a pursuit of his framework as a general ontological account. A more subtle understanding of quantum states, however, leaves room for a relationalist understanding of physical states in general agreement with Rosen's framework of biological states.*

KEYWORDS: *Theoretical Biology; Quantum Mechanics; Ontology; Relationalism.*

1. Introduction[1]

In his pursuit of the ontology of natural systems, Robert Rosen argued that an entire body of modern physics is predicated on an ontological framework which he found deeply unsatisfying for understanding biology. More specifically, he said, biological systems defy two defining characteristics of the Newtonian paradigm which has dominated physics and shaped special sciences. These two characteristics are the Turing computability and the classical ontology of states. Generally speaking, the classical ontology of states, Rosen argued, introduces a provisional distinction between organization and underlying matter (entities), only to discard the former as an epiphenomenon reducible to the properties of the latter. Rosen developed a detailed formal theory of relational systems as an alternative to better capture the nature of biological systems. In his theoretical framework, the analytic units of a given natural system cannot be identified as the system's ontological units. Instead, the system is

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accounted for by the analysis of the organizational scaffolding, as it defines the nature of the analytic units in relation to each other.

Although Rosen's ontology is compelling for biological systems, if we wish to devise a synoptic view of natural systems based on it, we are faced with a dilemma. Is it more plausible to "physicalize biology," based on the classical ontology of states, while dismissing the relationalism of living systems as an "exotic" and merely heuristic account limited to theoretical biology? Or should we try to "biologize physics" by postulating Rosen's relational ontology as basic, while treating the systems explained by physics as approximations of an essentially biologistic/relationalist nature? *Prima facie*, the former option seems more attractive, since an acceptance of the latter requires the reinterpretation of the classical ontology of states - an ontology pertinent, according to Rosen, to the entire body of modern physics - as a special case of Rosen's relationalist ontology.

Central to Rosen's assessment of the framework of modern physics is his classification of both Quantum Mechanics (QM) and Newtonian Mechanics (NM) as predicated on the classical ontology of states. I argue, however, that the prospects of successfully arguing for a general ontology of nature along the lines of Rosen's ideas are much brighter than his own argument implies, if it turns out that the assumptions of the classical ontology of states do not apply to QM and physical states in general.

Rosen's understanding of QM was influenced by his reading of the so-called Copenhagen interpretation as an amalgam of views of Heisenberg's instrumentalism and a skewed interpretation of Bohr's complementarity. If interpreted this way, QM may be classified, together with NM, as yet another theory predicated on the classical ontology of states where organizational features reduce to matter-entities. But the notion of the physical state is understood differently if quantum phenomena are interpreted along the lines of Schrödinger's interpretation – recently revived in philosophy of QM through various derivatives of relationalist accounts of quantum states. This understanding of QM is inherently compatible with Rosen's understanding of biological systems, while it renounces a traditional (Humean) account of efficient causation as a foundation of classically defined states. If such an interpretation of QM is sound, when we approach "small" on the ontological scale, we end up with relationalist systems of QM analogous to similar systems in biology. And going in the opposite direction, when we approach more complex systems, we encounter inherently relational organizations, such as living systems.

2. Robert Rosen's relationalist account of living systems

Twentieth-century theoretical biology has been dominated by the study of numerous aspects of the theory of evolution, leaving other theoretical advances, such as those attempting to approach living phenomena in general terms and delineate them from non-living matter, in the shadows – even if they can be understood as an extension of Darwinian theory. Advances such as Von Bertalanffy's General Systems Theory (1950, 1974), Artificial Life (Langton 1997) and Maturana and Varela's (1974) theory of Autopoiesis have been applied to fields outside biology, but they either have never or only recently reached the level of theoretical precision and rigor that the application to the experimental approach in biology requires; biologists have not fully recognized their potential to understand living phenomena, as they have not been presented in terms closer to their own practices.

The work of Robert Rosen (1959, 1972, 1978, 1991, 2000), a mathematician and a theoretical biologist, belongs among the aforementioned general approaches. His work generated little interest among biologists until fairly recently, but it has now been applied to the physical chemistry of living and non-living systems (Cornish-Bowden 2015), complex systems (Varenne 2013, Kineman 2012), and bio-informatics (Mikulecky 2001). With the rise of computational power and the increasing prominence of computer simulations and *in silico* experimentation in the life sciences, Rosen's ideas have become a valuable source for understanding life, making it important to probe the philosophical ramifications of his ontological framework.

One of the most striking aspects of Rosen's work is its particular way of relying on mathematical formalism to convey ideas about living phenomena. Rosen himself saw his work as a development of a branch of mathematical biology, the area that aims at establishing the formal rules of the processes characterizing living systems. He regarded metabolism as a phenomenon central to biology and believed that our ultimate understanding of life must be predicated on our understanding of metabolism. As metabolic processes are vast and complex, it is essential to ask the right general questions about their nature. Rosen's hope as a mathematical biologist was that formalizing and modeling the metabolic cycle could deal with its complexity and thus suggest the right questions to ask. He developed his ideas primarily with respect to cellular metabolism but later extended them to other metabolic systems and biological processes.

Cellular metabolism (CM) seems a hopelessly complex network of interrelated biochemical reactions. Yet it is a highly organized system capable of maintaining its stability for a relatively long period of time. A successful explanation of CM must account for this extraordinary capability. Rosen's initial suggestion was to start off with a very general distinction of the elements of CM in order to devise an accurate model of it.

Perhaps the most striking general characteristic of CM is its cyclical nature: material constantly enters and leaves its physical boundaries in a highly organized way. This seems to be critical for the maintenance of its stability. In this process of matter exchange, some elements of the system, namely enzymes, retain their molecular structure, while they perform the function of continuously transforming the vastly varied and complex molecular structure of in-coming matter into out-going matter. The key to understanding CM, then, according to Rosen, is to understand how exactly enzymes as *components* (M) can retain their structure in such chemically volatile surroundings, while interacting and transforming up to a hundred different incoming chemical substances. A straightforward and simple answer to this problem almost immediately comes to mind: *subcomponents* (R) of the system must somehow repair the components. Indeed, such (molecular) subcomponents are present, and they do maintain the functioning state of the components.

Yet the answer is unsatisfying. The death of bacteria and the termination of CM are not preprogrammed as they seem to be in some eukaryotes, and their life span, given the favorable circumstances, is unlimited. But with the relatively long lifetime of CM, doesn't the repair system consisting of repair subcomponents sooner or later require repair itself? The answer is, yes, it does. Yet resorting to the same explanatory strategy and pointing to the existence of sub-sub-components, and so on *ad infinitum* is hardly satisfying in the case of CM, as the number of the kinds of components involved in repairing the system's components is finite and, in fact, of a fairly limited complexity. The lifespan of cells, theoretically and practically speaking, vastly exceeds the extent to which the hierarchy of the repair subcomponents would have to be produced to maintain the structure and functioning of the components. Even if this were possible, the real cells do not seem to produce elaborate hierarchies of repair subsystems. These hierarchies stop at a fairly early stage of the development of the system.

The general guiding idea behind Rosen's approach to this abstract but fundamental problem on the core nature of metabolic systems is that metabolic systems are such that the metabolic activity of their components plus the activity of their repair subsystems, i.e., the system as a whole, results in *the replication of the subsystems*. Hence, a continuous renewal of the repair systems does not require any instances other than the instances of their own kind and, of course, the initial components.

In more formal terms, the activity of CM can be represented by mapping a set of input materials and a set of output materials (Rosen 1959). Only some members of the set of mappings H (A, B) are suitable candidates to represent those with the self-replicating property needed in CM. (M, R) systems are a family of purely formal systems, the sequences of mathematical objects (A, B, f, Φ), some of which include models of metabolism. In the case of some members of the set H, an operator Φ (of repair) selects a (metabolic) function f from H, based purely on the molecular configuration of the outgoing material (B) that it has transformed from the incoming material (A).

Rosen (1972) formulated this as a theorem of closure that provides a selection procedure based exclusively on the ranges and domains of the functions in the set. Thus, the entity β that produces operator Φ from B, i.e., the outgoing material that it has previously transformed from the incoming material A, acts as the 'formal entity' of metabolic configuration. The configuration of the system is the (formal) cause of its self-repair. The molecular configuration of B (as β) is responsible for the production of the operator, yet B is the also result of the operator as the efficient cause. Only some Bs are such that H (A, B) is capable of such circular organization. Therefore, metabolism f, repair Φ and molecular configuration (i.e., self-replication) β are mutually dependent and, as such, sufficient for the maintenance of the system.[2] Metabolism is, thus, a relational system, acting as the inherent cause of its own self-replicating ability. Rosen (1972, 1991) resorted to the Aristotelian classification of causes to point out the function of the molecular configuration as both the efficient (the operator producing) and the formal cause.

It is important to distinguish Rosen's (M, R) systems from other seemingly similar systems of circular organization studied by cybernetics, information theory, and Dynamic Systems Theory. Unlike those, the (M, R) systems do not require external stimuli to be a part of the system's network of efficient causes. The idea is exactly the opposite: in metabolic systems, the causal role of the incoming material is generated by the system as such, and this acts as an efficient cause. Arguably, (M, R) systems are a more general family of systems than those studied in the theory of self-organizing systems (Letelier, Marn, and Mpodozis 2003).

In a nutshell, Rosen tried to formally demonstrate that some systems do not require anything more than a certain molecular configuration of the components and one set of repair subcomponents in order for the repair mechanism to be "spontaneously" produced by the (relational) system as such, thereby enabling the system to maintain itself and self-replicate. Presumably, CM is an instance of the special functions of the H (A, B) set.

But when we turn to the actual experimental practice of biology, how justified is this formal model? While he occasionally provided suggestions on how exactly his model could be applied to concrete biological phenomena, Rosen focused on developing the formal details. Even so, he believed it was applicable to a wide range of phenomena, including the notoriously difficult to understand phenomenon of protein folding, i.e., the transformation of the two-dimensional stretch of the DNA molecule into the three-dimensional proteins. Although this is one of the central problems in contemporary genetics, it is not a typical instance of the kinds of systems for which (M, R) formalism was devised. Grappling with the problem, Rosen introduced the idea of *active sites* (Rosen 1991, 2000), arguing: "This kind of site is an example of something nonfractionable from the scaffolding that carries it" (Rosen 1991, 274). Although the parts of the structure of polypeptide chains are defined by its molecular structure,

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[2] To put it in more formal terms, every b as an element of B caused by the component labels the mapping H(A, B). Thus, for each b, there is b*: H(A, B) → B. Some b* are invertible; that is, if b*(f1)=b*(f2), then f1+f2. This means β is defined by the following mapping: H (A, B) → H (B, H (A, B)). In other words, it replicates the initial mapping Φ. For details see Rosen (1972) and Mikulecky (2000).

they are defined within the context of an (M, R) system of organization. Active sites constitute the organizational (external) scaffolding that ties the system together and do not reduce to the (internal) molecular structure *per se*. In Rosen's words: "[A] corollary of this nonfractionability is that we cannot get at a functional description of the site from a purely structural characterization of the scaffolding" (Rosen 1991, 274). The functional components of the system, defined by their systemic function, then, unlike the molecular units outside the context of the system, "can only "exist" when "scaffolded together," and "if the scaffolding as a whole is perturbed, or disrupted … they cease to exist" (ibid.). Thus, the folded polypeptides are essentially new organizational units that cannot be reduced to the units that existed before the folding process, although there is continuity between the (inner) molecular structure before and after the folding process.

Rosen's idea of the significance of functional rather than molecular components, in particular, their replication, may prove to be indispensable in treating diverse biological phenomena. It is necessary to test the idea directly against the actual biological processes, but some important experimental work is already being done.

For example, *in silico* experimentation may clarify the potency of Rosen's model and its properties. One such experiment dealing with CM compares Hamiltonian accounting for kinetic processes as constituents of a microorganismal metabolic process, *within* and *outside* the cellular context (Boogerd et al. 2005). The results of the experiment demonstrate that in the metabolic process of glycolysis, the observed system exhibits *systemic properties* (captured by a nonlinear function), which are defined by the context of the system and are not displayed by the parts or subsystem *in isolation*.

The experiment does not treat the system by computer modeling but rather by the technique of *computer experimentation*. It is practically impossible to perform an experiment that tests an actual complex metabolic system of a microorganism, as an immense number of components must be accounted for and controlled. Instead, in the experiment, a precise computer-generated kinetic replica of the process reveals which properties of metabolic regulation in the context of the cell can be compared to the properties of its parts in isolation. The outcome of the experiment suggests that although systemic properties explain *the parts* of the system, such properties are exhibited only in the cellular context. The state-dependent properties can be deduced from those Hamiltonians that describe parts of the system in a cellular context, not as context-independent. The experiment shows that these systemic properties cannot be predicted from the behavior of parts of a simpler system, which is similar to the system exhibiting these systemic properties but lacks the relevant context giving rise to the systemic properties. The components of the system that determine the behavior of the system do not exist outside its context. The systemic properties of the components responsible for the control of the system cannot be measured as such (in isolation) once the system ceases to exist, although they are critical for predictions of the system's behavior.

Thus, our understanding of the system equals our understanding of the whole's functioning in terms of the properties of the parts characterizing it, but hoping to predict or understand the behavior of these properties as isolated from the system, and subsequently put together its story, even as a matter of principle, seems to be based on an ontological assumption for which the experiment suggests no grounds whatsoever. If anything, it gives credence to Rosen's point about the nature of the organizational features of CM.

Experiments of this kind and Rosen's account of (M, R) systems are predicated on a common understanding of metabolism that sets the level of the functional significance of the cellular context as distinct from the level of its molecular parts in isolation (i.e., as chemists interested in non-biological processes understand them). Rosen's approach emphasizes the inherently contextual aspect of the model. As such, it may provide a general framework, an umbrella concept, if you will, for the kind of experimental results produced by Boogerd and colleagues. His model could serve as a starting point for understanding systems that behave the way the experiment suggests.

> What I am suggesting is that the *active sites* of folded polypeptides, or of multichain structures, are of this character. Hence that folding generates a scaffolding which, in this sense, brings entirely new chemical entities into existence, entities composed of parts drawn from residues remote from each other in terms of primary structure. Hence these scaffolded entities do not have a symbolic representation in terms of that structure at all; they are precisely the intersymbol hybrids… which are forbidden in any syntactic scheme based on those symbols. (Rosen 1991, 274)

3. Towards a relationalist philosophy of nature

3.1 Physics and biology: ontology of states and Newtonian paradigm in physics

An explicit or implicit hope of any nonreductionist-minded biologist or philosopher, including Rosen, is that a nonreductionist ontology of biology will reveal a general framework of natural systems whose significance exceeds particular explanatory interests in the nature of the living systems. The question that needs to be addressed, if Rosen's model is to become a general ontology of nature, is whether and how it captures the nature of systems other than living systems.

In very general terms, the kind of ontology that comes out of the (M, R) systems is such that analytic units of a given natural system that we may discover cannot be identified, even in principle, as the system's ontological units. Although an observer (or an experimentalist) does not arbitrarily pick units from the whole, these units still do not exhaustively account for the system. They can be accounted for only by pointing out the scaffolding that defines the nature of the analytic units in relation to each other.

Such a treatment of biological processes and the role of organization in them suggests that the nature of the relations between the components in living systems may not lend themselves to the Newtonian paradigm that has dominated physics and still remains a working hypothesis for understanding the physical framework that underlies more complex process and entities studied by special sciences. Given the nature of biological systems as characterized by Rosen's model, there seems to be a fundamental discrepancy between biological and physical explanations and, accordingly, the underlying ontology in these two sciences. The following two components define this discrepancy: a) Turing noncomputability of (at least some) biological processes; 2) the ontology of states, an underlying explanation in physics, is inappropriate when dealing with living systems.

A careful look at the state of affairs in biological sciences indicates its non-compliance with the Newtonian paradigm, although the extent of this non-compliance is open to debate. Some argue that "the Mendelian picture cannot be 'lifted' to the molecular one via an immediate, exclusive identification of DNA sequence and Mendelian factor", but Rosen says this leaves open the possibility that, in principle, "it can nevertheless be lifted" (Rosen 2000, 54). A more assertive non-reductionism substantiated by significant experimental evidence would go a step further, proposing a "more radical possibility [which] says that the Mendelian picture cannot be lifted at all, at least not within the confines of contemporary views about reductionistic particles and their interactions" (Ibid.). Or we might even go farther: "The final, and most radical possibility is closely related to complexity. It says basically that organisms cannot be completely formalized; that they have nonalgorithmic, noncomputable models, and hence no largest, purely syntactic one" (Ibid.). Rosen is explicit:

> The power of belief in reductionism [is] the scientific equivalent of the formalist faith in syntax. Though of course Newtonian mechanics has had to be supplemented and generalized repeatedly, the basic faith in syntax has not changed; indeed, it has been bolstered and made more credible by these very improvements. And there has as yet been no Gödel in physics to challenge that credibility directly. But there *is* biology. (Ibid.)

What Rosen is getting at in this passage is that a relationalist ontology that captures the nature of living systems may not be totally incompatible with the Newtonian paradigm and the ontology of states it implies. Rather, it may be an instance of more general relationalist ontology, and the natural systems of the Newtonian world are merely its limit. Rosen makes the following argument:

> A Newtonian approach starts from the premise that our cell, as a material system, is to be studied and understood in the same universal terms as any other material system. That means: it must be analyzed down to a family of constituent particles. These

> particles define of specify a formal state system, or phase space, as we have seen; the original system, the cell, is then imaged by some special set of points in this space. To find the dynamical laws, we must look empirically at the different kinds of particles we have resolved our cell into; we must determine from them, in isolation, how they can interact with things around them. Specifically, we must determine both how they respond to forces imposed on them and how they impose forces on each other. From these a set of dynamical relationships (i.e., a constraint) can be written down, which specify the necessary entailments, the necessary recursion, *valid on our whole space of states or phases*. (Rosen 1991, 118; emphasis in original)

> [If] I give you another, different cell, then the entire analysis must be repeated for that new cell. There is nothing in the fact that the subject of analysis is a *cell* that can shorten the analysis or indeed help in any way. (Ibid.)

> It is only after the organization has been specified … that its components acquire specific functions, and the resulting entailments within that organization can be analyzed and explored. All the rest lies in the specific character of particular realizations, whose particularities are not entailed relationally. … Of course, such ideas do not sit well with those who analyze only individual material realizations of particular modes of organization. In any such particular situation, the organization is "wired in" to the physics of the system from the outset. This in turn conveys a strong, but utterly mistaken, impression that the physics is entailing that wiring, rather than (in a precise sense) the reverse. (Rosen 2000, 45)

Yet the dominant contemporary ontological framework of physics, Rosen argues, fails to capture the structure of the systems and their properties across the natural world; most notably, it fails to accommodate the theoretical requirements of biology. It cannot adopt these theoretical requirements and must treat them as unwanted excess, as long as it purports to be a general ontology of the natural world, rather than a special case of it. The ontological notion of complexity and context-dependence, according to Rosen, cannot be reconciled with the contemporary understanding of physical systems.

Thus, the key challenge is to acknowledge both the need and the desire for comprehensive relationalist ontology. But the prospects look grim, given the presumed lack of reform in physics predicated on the alternative ontological assumptions of relational ontology. At the heart of physics, in Rosen's view, is the postulation of states of the system, wherein a system is a "family of structureless particles," as comprehensible only in mechanistic terms.

Ultimately, Rosen's pronouncement that physics, as it is motivated by the reductionist ontology, necessarily deals with a narrow species of natural systems while the underlying relational ontology of biological systems is more encompassing fails to convince. Worse yet, it seems a near-desperate dismissal, implying that his model cannot be justifiably generalized and will remain a tool in a moderate niche of modeling properties of living systems.

3.2 Quantum mechanics and the ontology of states

Rose's pessimistic outlook was largely driven by his assessment of QM as the best empirically confirmed theory of physics and a defining theory of the micro-physical world. QM defines properties of all elementary particles and can thus be presumed, in a sense, to be the bottom level of reduction and, hence, most relevant to any ontological pronouncements on the general nature of physical systems.

As mentioned above, Rosen portrayed QM in light of the so-called Copenhagen interpretation of it. This led him to conclude QM is only a subspecies of a broader ontology of states, with Newtonian mechanics one of these subspecies. Presumably, then,

the concept of state plays the central role in its formalism, just as it did in its classical predecessor, and the essential property of state is its recursiveness. It thus perpetuates the duality between states and dynamical laws that begin with Newton. The inferential or entailment structures in the two formalisms are different enough so that they cannot be directly compared, but they remain different species of the same genus. (Rosen 1991, 105)

Although the problem of encoding into two different formalisms, i.e., matrix formalism and wave-mechanical formalism, remains, the problem does not question the nature of a causal entailment devised in terms of the ontology of states. The situation is even worse, Rosen argued, if we interpret the theory in light of the instrumentalism of Niels Bohr and Werner Heisenberg, as it denies that any ontological points can be drawn from the experiments in quantum mechanics. Consider the following:

> [A]s far as causal entailment is concerned, the quantum-theoretic revolutions were mainly technical; the heart of Newtonian causality (recursion) has passed intact from classical to quantum mechanics. (Rosen 1991, 104)
>]W]hen quantum mechanics seemed to contradict or preclude classical ideas of causality, enormous disquiet was generated, which has still not been completely resolved. (Ibid.)
> In simplest terms, the measurement problem requires a partition to be drawn between an observer and what he observes, so that everything objective (i.e., all the physics) falls to one side of the partition. The trouble is in getting the measurement process itself, as an event in the material world, into that objective side. (Rosen 2000, 139)

Rosen states, apparently in desperation:

> The reappraisal of causality occasioned by the advent of quantum theory has left physicists without consensus on what causality is, or on how it should be encoded

> into contemporary physical formalisms. More generally, no one is today sure what the formalism of quantum theory encodes, or even if it encodes anything at all; in this latter view, advocated by Bohr under the rubric complementarity, the only thing that matters is the decoding. I believe it fair to say that the “foundations” of quantum theory remain a quagmire, to a far greater extent than has ever been true in physics before. (Rosen 1991, 105)

Before we look more closely at Rosen’s arguments, some remarks on the Copenhagen interpretation of QM are in order. The Copenhagen interpretation is an amalgam of a few independently developed viewpoints on quantum states, put together for a pragmatic reason, i.e., to derive a rough operational understanding that suffices in experimental practice. The idea of the separation of the observer and the system observed is one of the aspects of the Copenhagen interpretation. The instrumentalist attitude is another: what matters, or indeed, in a deeper reading, what can be said to exist, is the state of the system after the measurement on a microphysical system is performed. This is by no means the only interpretation taken seriously by those studying quantum theory. Rather, it is a working model of sorts that physicists use as a conventional interpretation if they do not intend to dwell on the details of its philosophical merit. This model suffices if we want to explore a new microphysical property or particle. But if we are interested in developing a general ontological account of nature that bridges the world of physics and biology, a much more careful conceptual approach to the subject is needed. In this sense, Rosen’s characterization of physical states is both hasty and crude:

> Physics … requires as an unwritten law of nature that the presumed objective side with which it claims to exclusively deal be entirely predicative (Church’s Thesis), purely syntactic, formalizable. But measurement processes or feature extractions lead either to infinite regress or to impredicativities somewhere. The presumed partition, which absolutely separates objective physical reality from a subjective observer, has to be drawn so that the requisite impredicativities fall outside the objective part and hence are put entirely into the observer. Indeed, the presumed partition is itself impredicative. (Rosen 2000, 139)

The options for drawing distinctions between the observer and the observed system are more varied than Rosen suggests. Indeed, some current interpretations insist on continuity between the two, not ultimate discrepancy. A widely discussed decoherence account (Joos et al. 2013, Zurek 2003) views the measurement process as much the same as any other unobserved interaction between two quantum systems that leads to the same result, namely to a collapse into one of the states accounted for by the wave equation. In other words, an entanglement of the observer and the measurement apparatus during the measurement process, which breaks the coherence of the mixed-state (the microphysical state accounted for by the wave equation – e.g. the outcomes on the screen in the double-slit experiment) is not a special sort of interaction process to be treated as a category of its own. The so-called collapse of the wave-function into one of the individual states of the mixed state captured by the wave equation does not have to be treated as a *sui generis* event, a special mental state of sorts, as some physicists have suggested, although it is occasionally portrayed as an integral aspect of the Copenhagen interpretation.

Rosen states:

> The problem is that the Uncertainty Principle, or more generally, the commutation relations on which Heisenberg based his quantum theory, are not compatible with the notion of *phase*. … phase is the basic idea in the Newtonian description of particulate systems; it is precisely what the recursion rules operate on to generate the trajectories that encode causality in that formalism. The Heisenberg commutation relations said that classical phase could no longer even be defined at the quantum level, let alone be recursive. (Rosen 1991, 104)

> Giving up the notion of phase did not mean giving up the notion of state. It merely required an encoding of that notion into a more complicated mathematical or formal object (wave function) whose relation to actual observation chronicles was now (to say the least) indirect. Formally, in quantum mechanics, the wave functions that encode state remain completely recursive, governed now by Schrödinger's equation (or its equivalents) rather than by Newton's Second Law. The guts of classical causality therefore passed intact to the new mechanics. It is so happened that the new encoding, into a formalism of wave functions and Schrödinger's equation, could be related only in a statistical way to the old, classical encoding, so that the two *inferential structures in the formalisms* could not be brought into a complete homology. But as we have seen, this is an entirely different matter; causality encodes differently into the two kinds of formalisms, but that only says something about the encodings, and not about causal entailment itself… My main point is, however, unarguable: that the concept of state plays the central role in its formalism, just as it did in its classical predecessor, and the essential property of state is its recursiveness. It thus perpetuates the duality between states and dynamical laws that begin with Newton. The inferential or entailment structures in the two formalisms are different enough so that they cannot be directly compared (and indeed, attempts to directly compare the two formalisms have created much of the confusion to which I alluded above), but they remain different species of the same genus. (Rosen 1991, 104-5)

Rosen draws the following strong conclusion:

> Let us now turn briefly to another matter connected with these formalisms. As I argued above, they serve to replace the vague word "event" with the apparently more precise "state *x* of system *N*", and ultimately, to replace "system *N*" by "family of structureless particles." This last replacement, which as we have seen is at the heart of reductionism, basically constitutes a redefinition of the term "system" (more specifically, of "natural system"). Indeed, it says that the terms "natural system" and *mechanism* are to be synonymous. (Rosen 1991, 105)

All in all, in a very crucial respect, QM deals with the states the same way as NM does - although probabilistically and with an instrumentalist attitude to reality – i.e. irrespective of the organizational structure that is causally reducible to the configuration of the underlying entities.

Now, the Copenhagen interpretation of quantum theory has been dominant in physics for several decades. Yet recent work in history and philosophy of quantum mechanics reveals that this interpretation has been a convenient label for a number of diverse and opposing interpretations (Faye 2014, Bokulich and Bokulich 2005, Howard 2004). Its instrumentalist nature is one of these interpretations. More importantly, what also emerges from these new studies is a theoretically and experimentally plausible understanding of quantum phenomena that has been from the outset conceptualized in terms of relationalist/holist ontology.

Simply stated, the instrumentalist attitude is not a necessary component of interpreting QM. It is true that we can treat the wave equation as a new way of encoding causal events as inherently statistical in nature, thereby denying Newtonian mechanics. In other words, the Born rule interprets formalism as a resource for probabilistic reasoning about physical events. Yet once again, this does not necessarily have to be read in an instrumentalist way: the fact that causal interactions at the microphysical level are inherently indeterministic does not mean we give up on the reality of microphysical states. This was certainly not the intention of Niels Bohr who is seen as the father of the Copenhagen interpretation. In fact, as we will see, this non-instrumentalist treatment of the quantum state actually brings quantum mechanics much closer to Rosen's account of biological systems than he ever realized.

As I have pointed out, Rosen's rather hasty account of QM causes a dilemma. If we are keen to devise a synoptic view of natural systems, is it more plausible to "physicalize biology", along the lines of reductionism, or to "biologize physics" by arguing for nonreductionist relational/holist ontology, where the physical systems are subsystems of an essentially biologistic relational/holist world? Given Rosen's account of physics, it is hard to find any good reasons inherent to the body of physics to support the latter over the former. This makes considering a more general ontological framework an unattractive option, rendering the relationalism of living systems an exotic account in theoretical biology.

The case would be stronger if such reasons were inherent to our knowledge of the non-living physical world. Moreover, we would not need to advocate in favor of the option that physical systems are subsystems of the biologistic world; rather, we could argue that both biological and physical laws are best characterized in terms of relational ontology, that the universe is characterized by bio-friendly physical laws, providing proper conditions for the much more elaborate properties inherent to relational systems. But to substantiate this view and to make a strong (ontological) claim, we would need to show that (a major portion of) physics, not just biology, converges on the family of nonreductionist ontological frameworks.

In fact, the family of relational interpretations of quantum mechanics is compatible with Rosen's idea of relational systems. In biology, Rosen states:

> A *component is entailed by its function*, in any particular abstract block diagram in which it appears. … There is nothing unphysical about functional entailment. What is true is that functional entailment has no encoding into any formalism of contemporary physics; it represents a notion of final causation that is *unencodable* in any such formalism from the outset. (Rosen 2000, 110; emphasis in original)

Rosen elaborates on this notion, explaining his notion of the physical states implied in modern physics:

> It is only after the organization has been specified, by means of positing a definite abstract block diagram that its components acquire specific functions, and the resulting entailements within that organization can be analyzed and explored. All the rest lies in the specific character of particular realizations, whose particularities are not entailed relationally. Of course, such ideas do not sit well with those who analyze only individual material realizations of particular modes of organization. In any such particular situation, the organization is "wired in" to the physics to the system from the outset. This in turn conveys a strong, but utterly mistaken, impression that the physics is entailing that wiring, rather than (in a precise sense) the reverse. (Rosen 1991, 140)

In fact, precisely the opposite is true. What Rosen says about function and entailment echoes the point made by those pursuing relationalist interpretations of quantum mechanics. And it is also the spirit in which Schrödinger originally pursued his own interpretation. To Schrödinger, the fundamental continuity of the physical system limits the applicability of wave-mechanical formalism. Initially, along with the development of the wave equation, in his first version of the interpretation, any individual microphysical state is actually a continuum of further micro-physical states; singling out a particular observed state and appropriate causal inferences following this, is real, but this inference defined in terms of local interactions belongs to as real larger micro-physical system.

Schrödinger**’s initial interpretation in the late 1920s was** sidelined, justifiably so (Perovic 2005), by Niels Bohr’s interpretation, but it then went through another two stages of development. He revised his general view and introduced the notion of entanglement to better capture the experimental evidence emerging at the time. Yet in all his interpretations, the nature of the interactions between the components of the system must be somehow incorporated as basic in the ontology, as must the individual states.

Thus, in the case of microphysical systems, we must treat what appear to be individual entities, such as two distinct particles, as a *nonseparable emergent whole*, rather than as a reducible whole whose causal power is reducible to that of its entities (Humphreys 1996, 1997). It is possible to specify such an emergent whole only in terms of its entities’ *relations* – or in the case of quantum systems, in terms of the particles’ relations, i.e., in terms of the quantum field. Similarly, the proponents of *relational holism* argue the physical world is a web of nonseparable systems characterized by relational properties, where non-relational properties arise from or supervene on them (Healey 1991, Teller 1992; Silberstein and McGeever 1999). Either way, causal powers of such systems as wholes do not reduce to the properties of individual entities, as the reductionist would have it, but represent an additional causally relevant dimension of the system.[3]

Thus, the atomic individual properties of basic entities postulated by reductive physicalism may be secondary instances, not genuinely basic physical properties of physical systems (French and Krause 2006; French 2000). They arise only in observational and experimental situations, where the individual states are “constructed” and could, in principle, be eliminated from quantum theory. Each individual property instance is essentially an infinitesimal continuity[4] that can be singled out only heuristically. In such systems, any principle that aims to capture the causal relations of (quasi)individual property instances is relevant only as an operational principle, as it fails to capture the causally relevant component of system’s inherent relations.[5] Healey (2007) has argued for this sort of conceptual framework as the foundation of understanding both quantum mechanics and quantum field theory, a more general theory that unities the account of fields (electromagnetic and potentially gravitational) and quantum mechanics.

I cannot discuss the important technical details of this approach to QM here – such discussions are readily available in the literature - but the answer to the question of whether these relational ontologies are best suited to account for quantum phenomena is relevant to the justification of an overall relationalist ontology of Rosen’s type. There

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[3] See also a special issue of *Studies in History and Philosophy of Modern Physics* Vol. 35, No. 4(2004).

[4] This view of microphysical individuals is relevant beyond quantum mechanics (Bitbol 1996); it was also a received view of particles in the electromagnetic field theory up to the end of the 19th century (Heimann 1970).

[5] Quantum systems are not only counter-examples of the principle’s universality; they also pervade the microphysical world.

are substantial experimental, theoretical, and ontological reasons why we should accept relationalist interpretations of quantum mechanics rather than instrumentalist ones.

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Relacionističko razumevanje bioloških stanja i kvantne mehanike Roberta Rozena

(Apstrakt)

Intrigantne ideje Robertana Rozena o formalizovanom okviru za razumevanje bioloških sistema razmatrane su u biologiji i kognitivnim naukama. Ipak, njegovo kruto razumevanje fizičkih stanja, a posebno kvantnih stanja, suštinski je nespojivo sa njegovim objašnjenjem bioloških stanja, što ne dopušta da se njegovo stanovište shvati kao opštiji ontološki okvir. Međutim, jedno suptilnije razumevanje kvantne mehanike koje ćemo razviti ostavlja prostor za relacionističko razumevanje fizičkih stanja u saglasnosti sa Rozenovim shvatanjem bioloških stanja.

KLJUČNE REČI: Teorijska biologija; Kvantna mehanika; Ontologija; Relacionizam.