Quentin Rodriguez, Philosophies et Rationalités, Université Clermont Auvergne.

Back to Prigogine: An ambiguous relation with reductionism at the dawn of complex systems field

The Belgian chemical physicist Ilya Prigogine (1917-2003) has been an early proponent of complex systems approaches (Nicolis & Prigogine 1977). As such, he has often been presented as an opponent to physicalist reductionism. A critical examination of his scientific work shows nevertheless an ambiguous relation to reductionism. Moreover, this relation deeply changed over his career, allowing to distinguish between two Prigogine. The “first Prigogine” (1940s – mid-1970s) tried to reactivate Pierre Duhem’s dream of a “generalized thermodynamics,” an autonomous and phenomenological theory unifying the entire field of the macroscopic phenomena. The “second Prigogine” (starting from mid-1970s) engaged in an alternative microscopic dynamics, departing from the standard statistical mechanics, in order to deduce the laws of thermodynamics from a microscopic theory. However, Prigogine did not underline this change of viewpoint, blurring the meaning of his work in the second part of his career (see Lombardi 2012, or Chibbaro, Rondoni & Vulpiani 2014). I argue that these ambiguities are useful to understand the shaping of modern complex systems field, whose relation to reductionism is more complicated than promotional speeches often claim.

First, I will state what remained *unchanged* throughout his eclectic intellectual career, and show what, in contrast, did changed. Prigogine’s scientific work and thought indeed remained durably defined by the pursuit of a *thermodynamic evolutionism*. By evolutionism I here mean philosophical evolutionist thought, that preceded Darwin’s own theory (with which it was confused, despite notable discrepancies). Philosophical evolutionism can be defined as the doctrine according to which the whole world, from matter to the universe, including species and societies, evolve according to the same cosmological law, from “simple,” or “homogeneous” structures, towards more “complex,” or “heterogeneous” ones. It postulates the existence of a natural law of “increasing complexity” through time, modern complex systems scientist would say. Herbert Spencer is the most famous author to have systematized and promoted such a doctrine, but Bergson can also be associated with this school of thought, the later having a greater influence on Prigogine than the former. Unlike Bergson and Spencer, however, Prigogine pursued a *physicalist* evolutionism, which he sought to base on thermodynamics. His entire scientific career can be summed up as the search for the recognition of the fundamental character of the second law of thermodynamics (about irreversibility), and the fact that the evolution of all natural phenomena (and especially the appearance of life) could follow from it. What strongly distinguishes the “first Prigogine” from the second one is the way to achieve such goal. During the first part of his career, he tried to reduce the entire field of the macroscopic phenomena to an extended thermodynamics, trying to include, for instance, hydrodynamics, biochemical reactions, biological evolution, or embryogenesis into a thermodynamic formalism. In that sense, the first Prigogine can be said to stick to a kind of epistemological reductionism like Duhem’s one, reluctant to use explanations talking about constituents (say, molecules or atoms), to prefer the aim of subsuming all experimental observations under a common abstract mathematical formalism. The most achieved expression of this duhemian Prigogine is to be found in his 1971 book *Thermodynamic Theory of Structure, Stability and Fluctuations* (Glansdorff & Prigogine 1971). However, contrary to Duhem which famously opposed atomism at the turn of the twentieth century, Prigogine could not dismiss the success of microphysics. The second law of thermodynamics being, strictly speaking, impossible to deduce from standard microphysics, he let imprecise the link between this principle of macroscopic irreversibility and the commonplace understanding of physical phenomena in terms of microscopic constituents. Although emergence was not part of his vocabulary, this relationship between the micro and the macro level could be understood as a kind of synchronic emergentism: thermodynamic irreversibility is a universal macroscopic property that does not exist at the atomic scale, but statistically emerges in some way. Starting from the mid-1970s, he searched on the contrary for a *microscopic* understanding of his evolutionist principle, postulating its universal validity from the atomic scale. When talking about the relation between levels of description, this "second Prigogine" then exhibits a strongly micro-reductionist flavor, at odds with some popular misconceptions about his position. However, trying to inscribe thermodynamic irreversibility inside the microscopic dynamics, he aimed to dissolve in the same time determinism "from the start." This allows to call this second viewpoint as a diachronic emergentism, in some sense. Organisms and societies are strictly and only ruled by the microphysical laws ruling their ultimate constituents, but an intrinsic contingency of microscopic phenomena forbid us to *temporally* reduce the future evolution of these constituents to their past.

In a second part, I will focus on the pivotal period 1965-1975, when the first conception of his thermodynamic evolutionism began to crack, but the second one had not yet been fully invested. It is at this time that he built, with his colleague Paul Glansdorff and his doctoral students Grégoire Nicolis and René Lefever, the concept of *dissipative structure*, and put the word "self-organization," taken from embryology, in circulation among physicists and chemists (almost at the same time as Manfred Eigen, but a few years before, based on the published literature; first occurrence for both phrases in Prigogine & Nicolis 1967). The concept of dissipative structure, that earned him the Nobel Prize in Chemistry in 1977, defines a kind of order, supposedly different from *equilibrium structures*, characterized by symmetry breakings whose necessary condition is a sufficient distance from the thermodynamic equilibrium. By contrast, "self-organization" was not used in his work as a precisely defined concept, but worked more as an umbrella term. Although it has not first been actively promoted by Prigogine, "self-organization" was widely picked up again by physicists and chemists interested in his work, becoming a kind of rallying cry in the 1970s. He used it in turn as a way to put back his specialty, chemical thermodynamics, at the center stage of physics. His place in Bruxelles, his specialty, and his way of doing theoretical physics are all characterized by a glorious past that was lost when he started his career in the 1940s, marginalized in the new regime of Big Science. The forefronts of physics became nuclear and particle physics, with its big instruments, big teams, big public funding, and the scramble to the ultimate constituents became the dominant narrative of science. "Self-organization" had then been used by Prigogine to challenge this supremacy, and to claim another way to do "fundamental physics," from the study of *macroscopic* phenomena. As we saw in the first part, he adjusted later his alternative, getting closer to micro-reductionist approaches in the second part of his career. But starting from the end of the 1960s, his theoretical narrative became difficult to unify: explanations in terms of dissipative structures located the "increasing complexity" in an unlimited range of symmetry breakings impossible to tackle by a unified formalism, and not anymore on a purely thermodynamic quantity derived from entropy. Instead of the theoretical unification he was looking for, I analyze the success of the concept of dissipative structures by its use as a *boundary concept* (Star & Griesemer 1989), that connected different scientific communities at the boundaries of physics, chemistry and biology (thermodynamics of irreversible processes, biochemistry, chemical physics, mathematical biology, condensed matter physics…), through a common language based on a network of analogies between exemplary cases belonging to these different communities.

In the last part, I will discuss what could be called Prigogine's *negative legacy* in the complex systems field. Not negative in the depreciative sense, but like the negative of a photo: the shape one can see by the place let free, and not by what has been filled. The *positive* legacy of Prigogine's work is indeed most contested among physicists, especially since his positions in the second part of his career underwent a gradual marginalization. Meanwhile, whereas the work of the first Prigogine is the most recognized, the overall project of a generalized thermodynamics encompassing all macroscopic phenomena, that was motivating it, failed. Actually, it is precisely when his first project cracked that his work had been considered the richer. Complex systems scientists have inherited I think the tension in his work about reductionism and the alternative avenues to unify the understanding of natural phenomena. The failure of the first Prigogine's project reveals the fact that there is no precisely defined measure of complexity whose maximization would control the historical development of all natural systems. Instead of a unifying theory, the concept of dissipative structures and the notion of self-organization led to the splitting up of types of modeling, loosely connected by an overarching discourse about the limitations of reductionism, something we still find in the archipelago of current "complex systems science." Complex systems physicists have continued to search for a kind of alternative unification that would carry on his fight against the supremacy of microphysics, albeit no longer the theoretical unification he treasured. The inclination is more to a methodological unification, relating to the properties of models used to grasp phenomena (scale invariance, scaling laws…; Cat 1998). Current physicists have probably learned from him being inevitably drawn towards microscopic understanding of natural phenomena. The search for a duhemian reductionism is not anymore conceivable in the field of physics. They need to negotiate between, or try to reconcile, a unification autonomous from microphysics, with a compatibility with microscopic explanations, to escape scientific marginalization. This is something one can see in the famous paper by Anderson "More Is Different": Anderson claimed to be (micro-)reductionist, but not "constructionist" (Anderson 1972). What does that precisely mean? Discussion is still going on. It had been indeed let wide open by Prigogine's attempts towards other avenues.

Bibliography

ANDERSON, P. W. (1972). More is different. *Science*, n. ser., 177(4047), pp. 393–6.

CAT, J. (1998). The physicists' debates on unification in physics at the end of the 20th century. *Historical Studies in the Physical and Biological Sciences*, 28(2), pp. 253–99.

CHIBBARO, S., RONDONI, L., VULPIANI, A. (2014). *Reductionism, emergence and levels of reality: The importance of being borderline*. Springer.

GLANSDORFF, P. & PRIGOGINE, I. (1971). *Thermodynamic theory of structure, stability and fluctuations*. Wiley-Interscience.

LOMBARDI, O. (2012). Prigogine and the many voices of nature. *Foundations of Chemistry*, 14(3), pp. 205–19.

NICOLIS, G. & PRIGOGINE, I. (1977). *Self-organization in nonequilibrium systems: From dissipative structures to order through fluctuations*. John Wiley & Sons.

PRIGOGINE, I. & NICOLIS, G. (1967). On symmetry-breaking instabilities in dissipative systems. *The Journal of Chemical Physics*, 46(9), pp. 3542–50.

STAR, S. L. & GRIESEMER, J. R. (1989). Institutional ecology, “translations” and boundary objects: Amateurs and professionals in Berkeley’s museum of vertebrate zoology, 1907-39. *Social Studies of Science*, 19(3), pp. 387–420.