

# The challenge of complexity: Unfolding the ethics of science In memoriam Ilya Prigogine

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## Introduction

The attendees at the Complexity and Philosophy gathering in Havana this past January were honored to hear a keynote from Isabelle Stengers the noted philosopher of science who teaches at the Free University of Brussels. While Stengers is perhaps best known for her decades long partnership with Ilya Prigogine, what struck most of the delegates to the conference was the creative strength of Stengers's own positions. This should have been no surprise to those who read French: for Stengers's work is vast and well read in France. But for those of us who are limited to English, the breadth and suggestiveness of Stengers's thoughts were new and provocative.

When Stengers writes "...scientists' practical position in relation with what they address must change. The problem is no longer one of deduction, but of wondering what is relevant and how. Scientists no longer address a system as explained by what they know about it, even if they know it perfectly well, because it is a model. Their questions imply an open situation: 'what will it be able to produce?' 'What kind of behavior will emerge?' And the question must be asked each time, with each new situation," she is describing an era of complexity and uncertainty. Her description seems equally applicable politics and current events as it is to science. It is a description which the manager confronts daily and the school teacher every hour.

Stengers is thus reminding all of us of the implicit links between the natural sciences and the social sciences, between science and the humanities, and between science and every day life. It is only fitting that such a description finds its way into this transitional issue of *Emergence* where the journal itself is trying to reassert linkages between science and every day life.

At the risk of committing the very reductive act against which Stengers warns us, let me truncate her words into what seems to be the valued 'takeaway':

*"[to those who say] Just give me the data, well defined data, and I can make a model [come the reply] True scientific simplicity is never reductive; it is always a relevant simplicity that is a creative achievement... The true grandeur of science is not power but the demanding*

*quest for relevance...How to learn? How to pay attention? How to acquire new habits of thinking? How to concentrate or explore other kinds of experiences? Those are questions that matter."*

Michael R. Lissack

The name of Prigogine, and of his collaborators, Paul Glansdorff, Grégoire Nicolis, René Lefever and so many others, is part of the very origin of the discovery by scientists of the huge, unexplored continent whose name is 'complexity'. When we wrote together the book which has been translated as *Order out of Chaos*, but whose French original title was *La Nouvelle Alliance*, this title was an answer to the molecular biologist Jacques Monod's announcement that "l'ancienne alliance est morte": Men had to finally recognize they were alone in a world foreign to their music and values, because life, as understood by molecular biology, was only a matter of chance and blind natural selection. But *La Nouvelle Alliance* also had strong biblical resonances. This was not a matter of chance, since Jacques Monod's idea was that molecular biology was indeed striking a deadly blow against any faith, excepting only the scientific one. But it possible to go further, and find among the "new covenants" which are proposed in the Bible a strangely relevant one with regard to Prigogine's ethical stance. It is after the great flood, and God swears that whatever Mankind's sins, he will never flood away his world again. As a memento of this covenant, He establishes the rainbow.

Such a commitment, if accepted by scientists, who so often have explained away what they later discover to be very interesting and the source for many fruitful developments, is a challenge indeed. I will propose that this challenge can be associated with 'complexity'. It may seem obvious, since complexity gives the lie to the motto which was so often used in order to claim that everything is clear, at least in principle. "This is the same thing that we already understand, just more complicated." This was precisely Jacques Monod's claim: the study of bacteria had produced the secrets of life; the royal road, the only scientifically relevant one, had been opened. For the mouse or the elephant, or man, it would be the same questions, the same road.

Ethics, as I feel it is relevant for the scientific adventure, is first linked with keeping alive the sense of wonder. It is, already apparent in the eighteenth century, this sense of wonder that the philosopher Diderot proposed, in an imaginary conversation, to the great physicist D'Alembert. D'Alembert defended with calm certainty that matter is nothing more than inert masses and indifferent physical interactions, and that, as a result, "Time means nothing to nature." To quote Diderot passionately pleading for D'Alembert to resist the temptation to turn the open advance of knowledge into a closed vision, a scientific one maybe, but as dogmatic as the theological and religious ones Diderot was fighting against:

*"You see this egg? That's what enables us to overturn all the schools of theology and all temples on the earth. What is this egg?... Instead of answering me, sit down, and let's follow these effects with our eyes from moment to moment. And first there's a point which oscillates, then a thread which grows and takes on color, flesh forms, a beak, the tips of wings, eyes, and feet appear, a yellowish material which unwinds and forms intestines. It's an animal. This animal moves, agitates itself, cries. I hear these cries through the egg shell... And then it breaks the shell. It comes out, it walks, it flies, it responds to a stimulus, it runs off, it comes closer, complains, suffers, loves, desires, rejoices. It has all your moods and goes through all your actions. Do you claim, with Descartes, that this is a purely imitative machine? But small children will make fun of you, and philosophers will reply that if that's a machine, then you are another machine. If you admit that between you and the animal there is merely a difference in organic structure, you'll be following good sense and reason, acting in good faith. But people will conclude from all this, in opposition to you, that from an inert material arranged in a certain manner, impregnated with another inert material, and subject to heat and movement, we get sensibility, life, memory, consciousness, passions, and thought."*

At the end of the conversation, D'Alembert evades the argument with those final words: "I want to sleep. Good night." But it is Diderot who is holding the pen, and he chose not to give D'Alembert an easy night. What will follow is the famous Dream of D'Alembert, a wild dreaming speculation about swarming bees. I quote again, this time D'Alembert babbling in his sleep:

*"Mademoiselle de L'Espinasse! Mademoiselle de L'Espinasse! Have you sometimes seen a swarm of bees going out of their hive?... The world, or the general mass of matter, is the large hive... Have you seen them move out to the end of a tree branch to form a long cluster of small winged animals, all hooked to one another by their feet?... This cluster is a being, an individual, an*

*animal of some sort... Have you seen them?... If one of these bees decides somehow to pinch the bee to which it is hanging, what do you think will happen?... You don't know, but the philosopher knows... yes, he does. If you ever see him, he'll tell you that the second bee would pinch the one next to it, that in the entire cluster there would be as many sensations aroused as there are small animals, that everything will get aroused, shift itself, change position and shape, that a noise will arise, small cries, and that someone who had never seen a group like that arrange itself would be tempted to assume it was an animal with five or six hundred heads and a thousand or twelve hundred wings."*

I will not claim that this philosopher already knew, more than two centuries before the beginning of the study of complex systems, how to define the behavior of a swarm acting like an emerging whole from the many interactions of its constitutive parts. There is a long way between an idea and the mathematical definition of a problem, between what Diderot describes and today's computer simulations of swarms and flocks. Science and philosophy are not rivals; Diderot's dream was that they would become partners, and that this would happen if, and only if, they shared, each with its own means, the same interest in the diversity and multiplicity of questions that nature and human societies may inspire in us.

## Equilibrium

It was the great achievement of thermodynamics to describe equilibrium states as characterized by the extreme, minimum or maximum, value of an adequate potential function, and this achievement brought to light the importance of a new, quite abstract, physical function - entropy. I recall the event the definition of entropy constituted for nineteenth century physicists because this is already a great example of scientific creativity. For Diderot, the philosopher, it was obvious that D'Alembert was wrong, that time means something for nature, that time matters. However the first definition of the way time may matter, the spontaneous increase of entropy associated to natural processes, was an answer that only mathematical physics could provide. Only the precise definition of the demands that a potential function must satisfy could lead to the conclusion that the relevant characterization of thermodynamic equilibrium states cannot derive from energy, the central function for understanding dynamical motion, but requires this new function Clausius named entropy and which he associated with the famous second law of thermodynamics. As we all know, if all natural processes conserve energy, they cause entropy either to increase or to remain constant.

It is understandable that so many physicists considered the case as closed. There was on the one

side the indefinite diversity of particular macroscopic configurations and, on the other, their common fate, an equilibrium state that the physicist is able to characterize. Furthermore, this characterization did include the reason why, whatever the particular evolution, it would spontaneously lead to equilibrium and would be unable to have the system leave equilibrium. Indeed it is the second principle of thermodynamics that directly defines the equilibrium state as the stable one. Any evolution which would cause the system to deviate from a state defined by the extreme value of its defining potential function would do so in opposition to the second principle.

At the time Prigogine was trying to extend thermodynamics to permanent out-of-equilibrium situations, where active exchanges with its environment make it impossible for the system to reach equilibrium, many were satisfied with a simple elegant answer to the question of the relation between the second principle and the kind of stable order characterizing living beings. The answer was that such an order corresponds to a production of *negentropy* that is 'negative entropy'. This is indeed a perfectly satisfying answer since it corresponds to the old vitalist idea that one way or another, life must be defined against physics, the rules of physics corresponding to death, the loss of the active coherence of life.

### Dissipative structures

If Prigogine could not accept negentropy as satisfying the problem of life, defining life as an exception or as some kind of antagonist power opposing the general power of the second principle, it is not because he wanted to explain life in terms of physics, but because for him it was not a solution, just another name for the problem. In order to characterize non-equilibrium situations, you can no longer rely on the second principle to define stable states. All situations in an open, far-from-equilibrium system, that is, a system whose exchanges with its environment keep the processes going on, do satisfy the second principle. The question has changed: it is no longer that of deducing from the definition of the system the stable behavior it will take on. It is asking about the possibility that a stable behavior becomes an unstable one if pushed further away from equilibrium, and looking for the new kind of stable behavior the system can then reach. And as we know, there is no general answer to this problem. It depends on what the general definition of equilibrium thermodynamics could safely ignore - that is, the coupling among processes. Only nonlinear coupling can lead to the possibility of what was called 'bifurcation points', when a system undergoes a global change, when we deal with the emergence of the kind of coherent collective self-organized activity Prigogine and his co-workers named 'dissipative structures'.

The very naming of dissipative structures was important. For the first time a precise meaning was associated with the old idea of emergence and novelty. Since the nineteenth century the interpretation of phase transitions - for instance what may appear as the 'emergence' of a liquid from a gas - was clear, at least in principle. Even if physicists still struggle with the precise characterization of the transition itself, liquid, gas and solids are all equilibrium states, exemplifying the power of definition of their adequate potential functions. But the emergence of out-of-equilibrium order can be associated with a kind of surprise no explanation can eliminate. I still remember Prigogine explaining with an always renewed emotion the marvel that billions and billions of molecules, devoid of any global coordination, just randomly interacting with their neighbors, would enter together a collective coherent behavior, producing together time or spatial orders with macroscopic dimensions. "Look at this liquid periodically changing its color!"

Diderot wanted D'Alembert to recognize that we have to accept matter as characterized by a general property of sensitivity. But the scientific answer is much stranger indeed than the philosopher's imagination. No need for a new property. Sensitivity, if I may use Diderot's term, is emerging; it cannot be attributed to the molecules, the individual behaviors of which remain impeccably random. Sensitivity is, as such, a far-from-equilibrium property. And as a result the definition of equilibrium states has changed. At equilibrium what physicists define as 'correlations', measuring the kind of togetherness among randomly interacting constituents, are by definition of zero range and intensity.

As a philosopher, an inheritor of Diderot, I cannot but rejoice. But I wish also to carry on with Diderot's plea for D'Alembert to accept just looking at the egg. I would like scientists to accept looking at one particular aspect of the tale I just told. There is an old Chinese proverb saying that when the wise man points to the moon, the fool looks at the finger of the wise man. Unfolding the ethics of science means that it is also worth looking at the scientific finger. Not in order to suspect it, but in order to produce a more complete celebration of the achievement. Those billions and billions of out-of-equilibrium correlated molecules are not like the moon, a beautiful presence in the sky just waiting for us to contemplate it. Or, if it is like the moon, then we must celebrate together with the presence of the moon in the sky the greatness of biological evolution that produced not only eyes to see but also the aesthetic sense that beauty matters, that the presence of the moon is worth looking and feeling. This is presupposed by the finger of the wise man and by the Chinese proverb. For, regarding a scientific achieve-

ment, the more complete celebration should include the description of the new practical questions that scientists learned were relevant in order to successfully characterize what they address. Unfolding the ethics of science means here not being fascinated by the advent of a new scientific answer, as if a new moon was coming within sight, for everybody to contemplate. It means knowing that there is no new scientific answer without the creation of new relevant questions, and a new practical definition of the kind of relevant relationship those questions entail between scientists and what they address.

### Far from equilibrium

I have already emphasized that, far-from-equilibrium, the power of potential functions must be renounced. But I have not sufficiently emphasized the change it produces in the very meaning of what is a scientific, objective definition. Indeed what was so satisfying with the equilibrium thermodynamic definition was that it extended, but at the price of describing equilibrium states only, the kind of power then associated with dynamics. Today, dynamics is also transformed, as instability, the so-called sensitivity to initial conditions and the study of dynamic chaotic systems, have entered the picture. But the classical ideal fulfilled by both classical dynamics and thermodynamics was that it is sufficient to identify the system, in terms of a well-defined set of variables, in order to acquire the power of deducing its possible behavior. This is why D'Alembert could indeed say that "Time means nothing to nature." What the physicist deduces may well be an evolution, but this evolution is just a transition among states physicists are able to deduce from their definition. And in equilibrium thermodynamics, it can take time for a system to reach equilibrium, but the thermodynamic definition allows physicists to know that it does not matter: sooner or later, the system will get to the state they have the power to deduce from their definition.

Far-from-equilibrium physicists must adopt a rather different position. They have to pay attention to the details of the processes, and their coupling, knowing that something, which does not matter at equilibrium, may come to matter far from equilibrium. A change in the characterization of what is relevant, a 'becoming relevant', means that in general scientists have to question their definition, not to trust its power. The "finger" and the "moon" both acquire an open, problematic dimension.

Indeed, in abstract models, constructed in order to exhibit and explore features of far-from-equilibrium order, physicists know how to question. They know which couplings, highly nonlinear ones, are crucial; they can even deal with new questions. I still

remember my wonder when it was shown that gravitation, the effect of which is only an insignificant noise at equilibrium, may become quite relevant far from equilibrium, entering into the definition of structures that would not have been possible without it - that is attaining a new meaning, no longer a general meaning. It is the very role conferred to gravity by the coupled processes that produces this meaning. We cannot say any longer that the system is defined as submitted to gravity, as I feel myself to be (if I loose equilibrium I will just fall down), we must say that gravitation participates, takes part in, and is not a mere factor but an actor in the production of what becomes possible far from equilibrium, just as it plays a part in a dancer's moves - she does not fall down, she knows how to play with gravity.

Thus, even when they themselves have defined their model, physicists may be surprised by what they discover. They must learn what their model is able to produce, not deduce it. At equilibrium the system was defined by its variables, while far from equilibrium it is the very functioning of the system which will determine how processes and interactions will matter, and matter together, because of their far-from-equilibrium togetherness. And this difference becomes quite obvious when we turn, for instance, to models reproducing what we know of a biochemical pathway. Here it is indeed worth looking at the fingers, at the careful tact needed in order to try and disentangle what a cellular process does entangle. Indeed the one who creates the model is not just describing all the possible far-from-equilibrium behaviors, but meeting the contrast between what is a living cell and what would spell its death sentence. How does a cell escape chaos, the possibility of which is inscribed in the equations describing the pathway? How does a regular behavior succeed at being stabilized? This becomes a vital part of the problem of the cellular pathway.

As a result, the entangled nonlinear set of reactions, which are the rules in biochemistry, can no longer be defined simply as a mean to transform and utilize food. What scientists have to understand has indeed this function of transformation, but they must also deal with what I would call a production of existence, the existence of an ordered pattern on which life depends. They discover the crucial fact that the speed of this reaction is low in comparison with that other one, that this nonlinear coupling is much more intense than that one. They can no longer see life and death as opposed, but are led to wonder, recognizing their proximity, at the very small transformation that would be sufficient for the pattern to be destroyed, for the cell to enter into a chaotic and probably deadly regime. Just as we wonder at and admire the skill of the dancer, understanding how close her gracious moves take her to the risk of

falling down, seeing in the dance how close together the double possibility of harmony and failure come.

Physics and chemistry are thus no longer defined only by the power of their definitions; they can also explore the cases where this power is lost, where scientists' practical position in relation with what they address must change. The problem is no longer one of deduction but of wondering what is relevant and how. Scientists no longer address a system as explained by what they know about it, even if they know it perfectly well, because it is a model. Their questions imply an open situation: "what will it be able to produce?" "What kind of behavior will emerge?" And the question must be asked each time, with each new situation. Far-from-equilibrium scientists are no longer in the position of generally defining the difference between what is possible and what is not possible - they are exploring it. And so do all others exploring purely kinetic models, where constituents are defined as active and sensitive and we can forget about equilibrium. Just like Diderot's bees, just like ants, the collective behaviors of which, as studied in his department, became one of Prigogine's favorite examples.

Such a transformation was a deep source of joy for Prigogine, because he felt that at last, physics, the practice he chose and loved, the one which kept him thinking and dreaming, was able to address a wide diversity of natural situations. He knew perfectly well that he had not explained life in physical terms, but that he had contributed to an understanding of material processes in which that life would no longer be considered as a statistical miracle, against the laws of physics, but as belonging to the same strange and wonderful world. Not to explain but to go from opposition to coherence, this was the fulfillment of Prigogine's quest. And it was also the confirmation of the guess once produced by Joseph Needham, the famous embryologist turned into an historian of Chinese civilization. Indeed, Needham wrote that if the life sciences and physics ever came to a unity, it would be physics that would have changed, much more than biology.

### Unfolding the ethics of science

**U**nfolding and not creating a new ethics, as associated with the new science of complexity. Indeed, let us turn towards the past, towards the so-called Newtonian revolution which heralded the triumph of a science able to submit everything to general laws: the science of Laplace imagined the first of the long series of demons and of Gods in relation to which physicists have claimed to discuss the ultimate truth of the world. Such a triumph was not as simple as it may appear. It meant the defeat of philosophers, like Diderot, and of scientists, like the chemist Venel, who associated Newton with quite another understanding

of science. This defeated party celebrated Newton's courageous acceptance of what he understood was relevant. Even if we cannot understand how it is possible that distant bodies attract each other, we cannot limit nature to what we conceive as possible. The chemist Venel, fighting against a reduction of chemistry to Newtonian physics, considered himself as a true follower of Newton, who "knew that nature achieves most of her effects by unknown means; and that the only real absurdity would be to wish to limit her, by reducing her to a certain number of principles of action and ways of operating."

Venel was defeated, and it would take the quantum revolution to understand that the chemical bond was indeed something very special. Repeating Joseph Needham's remark, chemistry and physics did indeed come to a unity, but it was physics which had to be deeply transformed in order to accommodate atoms as able to enter into chemical reactions.

But telling the quantum story like that is a bit unusual. The part chemistry played is usually forgotten, and physicists, as true inheritors of Laplace, prefer to talk about the final reduction of poor blind chemistry to the general rules of quantum physics, and to speculate about quantum reality. Here resounds with full power the triumphal reductionist motto. Chemistry is the same thing as physics, it is just more complicated.

Unfolding the ethics of science, as a challenge for complexity, may then be to tell tales - to make them like many rainbows, those rainbows the Biblical God created in order to remember he had to resist the power of destroying the beauty and diversity of the world. Complexity, as it started with the discovery and study of surprising properties, usually related to the irreducible importance of nonlinear relations, would thus not mark a rupture inside the sciences, the creation of an opposition between the past and the future. It would produce the opportunity to entertain a different relation with the past, emphasizing openness, surprise, the demand of relevance, the creative aspect of the scientific adventure, and not reduction to simplicity. True scientific simplicity is never reductive; it is always a relevant simplicity that is a creative achievement, like Newton's force of attraction. And, as we know from the case of unstable dynamical systems, this creation is rarely a final point. The true grandeur of science is not power but the demanding quest for relevance.

Finally, I would stress that the science of complexity also needs rainbows, in order to escape the temptation of power, in order to keep alive the learning process and the capacity not to turn a surprise into a triumph. It is also something I learned, working with Prigogine: the many people who came to his depart-

ment asking the so-called new physics to define for them the good questions in their own field, be it ecology, sociology or economy. As the house philosopher I was often obliged to receive them and talk with them, and I often had to disappoint them, telling them that physics is no universal key, that nothing can take the place of the process of creation of relevant questions in each field.

I would propose that the many complex models that are proposed be first appreciated as enlightening abstractions, precious new tools for thinking. By this I emphatically do not mean ready-made instruments. Tools modify the ones who use them. To learn how to use a tool is to enter in a new relation with reality, both an aesthetic and practical new relation. I would say that those new tools put the ones who use them in a very interesting new practical position with regards to what they address. Indeed the problem is no longer simply how to explain; it includes the problem of what is to be explained. What are the good questions? What is the relevant way to address a pattern? Is it to be directly explained, for instance, or should the relevant question be what stabilizes this pattern rather than others, or rather than chaos? And if this is the case, how are we to understand this stabilization? As the result of the past, as may be the case for biochemical patterns? As a result of the way the pattern participates in its environment? Or both? Or would it not be as a result of the patterning of the very behavior of the individual components, as it is probably the case with social situations, which heavily depend on social habits. Our situation here today, for example, heavily depends on the nice social habit we all have of listening and keeping silent, but there are many schools where the unstable, chaotic collective assembly of untamed young people produces distress and not happy surprise.

I would thus claim that the tools afforded by complexity may free us from the intellectual habit of relying on the obvious identification of what is to be explained, as obvious as the moon in the sky, and of looking for causes that would be responsible, that would have the power to directly explain what we observe. This makes a great practical difference indeed, as it creates the need for a new imagination, the cultivation of new insights, and the explicit recognition that there are no good answers if the question is not the relevant one. Tools are demanding - they do not confer the power of judging, they ask for the choice of the right tool for the right situation. In other words they oblige us to think and wonder.

### Accepting duality

**A**s we know, the production of light also means the production of darkness. It is not that the enlightened zone makes the rest measurably

darker, of course, it is just that it feels like it. Maybe the man looking for his keys would have been able, one way or another, to imagine how to look for them elsewhere. But light made him feel that elsewhere was really too dark. Feeling matters in science also - the entire history of the scientific adventure testifies to this. So very often we see the same coexistence between some who quite clearly define the scope of new results or models - what those models demand in order to be relevant - and others who will say: "we know that quite well, but it's so dark, by comparison, out here where we are; and who knows, maybe we will make some progress if we trust your questions to apply to our problems."

Since Galileo and Newton, there has been such an ongoing double identity of science, the one which relates to my definition of ethics, where the production of relevant questions is always an event, a selective and demanding creation, and the one which answers to my definition of power, where the selective, creative event becomes a new definition of a so-called scientific or rational general approach, which must be used to enlighten regions that are still so dark. Those two identities are strongly correlated. Light produces its darkness. Indeed it should never be forgotten that in fact there is knowledge about those seemingly dark regions. It is just that this knowledge - the relevant questions people have learned to ask, often since before the advent of modern science - does not look like the kind of science we desire. The feeling of darkness is produced by the contrast, and it produces the need for some light, any light.

To take only one example, among the people who dislike and criticize market mathematical economy. There are many historians of economy, who know a lot about precisely those aspects of economy which mathematical economy simplifies away. But the mathematical economists' answer is always: "yes, you historians know a lot, but it is only storytelling, not science." If we take into account everything you know, all your experience about aspects that matter, our models will be useless, but we will then be back in the darkness from which we escaped, and the chance for true scientific progress will be lost.

Unfolding the ethics of science would then mean resisting this contrast, resisting the claim that abstracting away what appears as an obstacle against a scientific approach - if we take that into account we are lost - is the condition for scientific progress, and also resisting the kind of contempt so easily directed toward scientists who learn and experience the demands of their field. Resisting this double temptation may be a challenge that may be the more specific duty and need for the complexity field to accept.

First, it may be considered as a duty, since what is so important with complex systems is that they are characterized by features which were traditionally explained away as non-scientific, the kind of things that had to be eliminated away in order for scientific progress to proceed. It is a matter of ethics that complex-systems people do not side with the kind of conception that eliminated what they now know how to address. This is why the example of Prigogine is so precious to me. His work was inspired by a strong feeling of duty. He felt obliged by the world physics claims to understand and which should not be destroyed by this claim, and he felt obliged by the experience and knowledge embryologists had produced about the mysterious, spontaneous enfolding of living order, even if they did not know how to relate this enfolding to the usual laws of physics and chemistry.

But it is also a need. Indeed the many models of complex systems, as can now be produced with computer simulation, lead to such a proliferation of diverse behaviors that there is a strong temptation to use them as a kind of universal key, able to serve whatever purpose we like. Just give me the data, well-defined data, and I can make a model! Or, inversely, I have got an interesting behavior, who has got data which would fit with it? Such use of abundant models is nice and playful. What makes the models and their use potentially dangerous is the strong claim that the business of science is to explain away what is only subjective opinion and illusion. On that claim rests the authority associated with modern science and that authority is anything but playful. How are we to avoid taking a simulation as scientific theory, explaining away what the model had no need to take into account?

### **On consciousness**

I will make such forebodings more explicit by taking, as a final example, what appears now to be the last scientific fashion, the claim that “consciousness is just about the last surviving mystery”, to use the words of Daniel Dennett. It strikes me as the worse sign towards the direction I fear, transforming precious tools for thinking into a universal source of answers.

Indeed, if we follow Dennett, it seems on the one hand, that we now have the answer for everything but consciousness, which means for instance that we would also know how to describe biological evolution or the relation between what we call genes and the development and differentiation of the vast, proliferating population of cells that together constitute the body, including the brain. It would be just a matter of finding a complex algorithm.

But on the other hand, the so-called ‘mystery of consciousness’ is described in a quite abstract man-

ner, exactly in the same way that Leibniz, some three centuries ago, declared an impasse with no hope for escape. Never would something like experience emerge from a reality described in terms of a matter devoid of experience. Leibniz was thinking of Cartesian matter, but he would certainly have repeated the same thing about the complex pattern of neuronal interactions.

And, finally, what is completely ignored, as consciousness is reduced to simple experiences like “I see red, I feel hot” is that those experiences are probably not what matters. Whatever the story of evolution, redness and hotness, or even consciously seeing that table or that face, are probably not the kind of achievement that gives evolutionary meaning to the production of the possibility of conscious awareness. Consciousness as such - abstract consciousness, the abstract fact that we are conscious - indeed appears as a mystery if you feel that the knowledge we have produced about nature truly defines nature. But what is then forgotten is the finger, the very conscious finger that was needed in order to create and verify those definitions. And what is ignored, despised, and left in the darkness is consciousness as the stake for so many human questions and techniques. How to produce new kinds of conscious awareness? How to learn? How to pay attention? How to acquire new habits of thinking? How to concentrate or explore other kinds of experiences? Those are questions that matter, that have always mattered, and that the brain cannot answer as such, because those questions imply that humans are social beings, learning how to learn, experimenting how to become, not because of their brains, but because whatever we call the brain makes them able to relate with others, with tools, with language, with writing.

### **What matters**

The menace here is that the complexity field may play exactly the same role as the traditional sciences of simple, linear systems: “Let us begin with the complex, the rest is the same, just still more complex.”

I feel a bit like Diderot in front of D’Alembert, so many ‘D’Alembert’s who will tell me that I am only a philosopher and that only philosophers may speculate outside the bonds of serious science. I could obviously answer like Diderot that small children may well make fun of the idea of conscious beings consciously trying to explain consciousness, and that philosophers are entitled to reply that if consciousness is the product of blind, algorithmic complex processes, then you, and also the entire scientific adventure - all your hopes to understand and explain - are also such a product. Why would I, as a product of complex algorithms, care to listen to other algorithmic productions? But I learned to love science with Prigogine, and to honor it as a

demanding adventure, not something to deride. This is why I trust that the field of complexity may answer the challenge of resisting power and accept this new version of Joseph Needham's remark: if there is ever to be any coherence between the brain as a very, very complex system and the production of experience, it will be complex-systems theory that will have changed.