Chapter 16

Giuseppe Longo: Computations and Natural Sciences



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The Human Face of Computing

nals: "Mathematical Structures in Computer Science" (editor-in-chief), "Information and Computation", "Theoretical Informatics and Applications", "JUCS", "La Nuova Critica", "The European Review", "Journal of Mind Theory", "Biology Forum". Professor Longo has supervised 33 (research-oriented) Master Theses and 19 PhD Theses. He has been an invited lecturer at 40 international conferences and he gave more than 160 seminar talks in universities or research institutions in Europe, USA and Asia. Professor Longo is a member of the Academia Europaea since 1992.

CC: Your academic career took you from Pisa (where you graduated "Laurea" (cum laude) in Mathematics and then spent 15 years as an academic) to US (UC Berkeley, MIT and Carnegie Mellon for three years), and then to ENS in Paris (since 1989, as an invited professor). How enriching have these moves been?

GL: Learning from others, the exchange with others is crucial, in scientific work. Very few researchers can do relevant work without interaction. I learned a lot from these very enriching contexts, beginning with the extraordinary milieu of mathematics and informatics in Pisa, in the '70s and '80s, and the subsequent American experience. Some collaborations, in particular at MIT and Carnegie Mellon with many, but also in Britain (R. Hindley) and Holland (H. Barendregt) were fundamental for me. And then the complex network of interactions with colleagues of three disciplines I am enjoying in Paris, in particular with the physicists F. Bailly and T. Paul. The main lesson I try to give to my students is that "two interacting brains think and produce much more than the double". But collaborations and the exchange itself is difficult, in particular the interdisciplinary exchange.

There is a growing fashion instead in the use of words referring to "competition" in research. But this is not how science goes: the difficult and productive side is collaborating, exchanging, learning from others and ... to go further on, together. If, time to time, one has to compete for finite resources or for a position, this is part of the game, not the purpose nor the joy of science.

Stressing competition of teams and individuals is a real disaster for scientific research. And it is largely borrowed from the current cultural hegemony of the financial markets, where traders are in continual competition and they compete on a mostly empty economic/productive content. As a further imitation, many institutions entrust "Independent Evaluation Agencies" (like

those that evaluated with the highest score Enron in 2001 and Lehman Brothers in 2008, until the "week before" their default ...) for judging scientific work. And self-appointed, "science independent" agents provide automatic indexes for classifying researchers. I proposed an "Editors' Note: Bibliometrics and the Curators of Orthodoxy" (downloadable from my web page) for *Mathematical Structures in Computer Science*, the CUP journal I direct, on this theme. It was approved by all the 34 members of the board, from 11 different countries. A more recent invited lecture on these issues (and bibliometrics), at an Academia Europaea meeting, is downloadable from my web page. Yes, in contrast to competition, collaboration and exchange are fundamental and moving enhances them greatly.

CC: Moving is a joy and pain. How hard was for your family, especially, your daughter?

GL: Hard, but stimulating. My wife started a new carrier, first by a master in Pittsburgh, when I was teaching at CMU, then a PhD in Paris, finally a university position in France, but this was tough on her. My daughter moved between the age of 5 and 8 between three very different school systems (USA, Italy, France), not easy for a child. Now she is trilingual, though, and she can ... "adjust" to almost any life context. And she is completing a beautiful thesis work on Italian Quattrocento paintings, in Paris, with very frequent trips to Italy.

CC: We share two main interests: incompleteness and randomness. Let's talk about incompleteness first.

GL: OK, but I prefer to tackle the issue by relating it to some Mathematics of Physics, in a preliminary way. In a short note of 2001, I suggested that Poincaré's three-body theorem is an epistemological predecessor of Gödel's undecidability result, in particular because Hilbert's completeness conjecture is a meta-mathematical revival of Laplace idea of the predictability of formally (equationally) determined systems. For Laplace, once the equations are given, you can completely derive the future states of affairs (with some, preserved, approximation). Or, more precisely, in "Le système du monde", he claims that the mathematical mechanics of moving particles, one by one, two by two, three by three ... compositionally and *completely* "covers" or makes understandable the entire Universe. And, as for celestial bodies, by this progressive mathematical integration, "We should be able to deduce all facts of astronomy", says he.

The challenge, for a closer comparison, is that Hilbert was speaking about a *purely mathematical "yes or no"* questions, while unpredictability shows

The Human Face of Computing

up in the relation between a physical system and a mathematical set of equations (or evolution function). That is, in order to give unpredictability, Poincaré's Negative Result, as he called his proof of the non-analyticity of the equations for three-body system, needs a reference to physical measure. Measure is always, in classical (and relativistic) physics, an interval, that is an approximation. And non-observable initial fluctuations may give *observable*, thus deterministic, but unpredictable, evolutions, in presence, typically, of non-linearity of the mathematical modelling (main reasons: the initial interval expands exponentially - this is measured by Lyapunov exponents—and it is "mixed"—its order is not preserved; the choice of a path, in presence of a bifurcation, depends on minor fluctuations).

In order to relate consistently unpredictability to undecidability, one needs to effectivise the dynamical spaces and measure theory (along the lines of Lebesgues's measure), the loci for dynamic randomness. This allows to have a sound and purely mathematical treatment of the epistemological issue (and obtain a convincing correspondence between unpredictability and undecidability). I will go back to the work on this while answering your next question, on randomness.

As for Gödel's incompleteness, when studying Poincaré's theorem, I understood that the two results share also a methodological point: they both destroy the conjecture of predictability/completeness from inside. Poincaré does not need to refer concretely to a physical process that would not be predictable, by measuring it "before and after". He shows, from the pure analysis of the equations, that the resulting bifurcations and homoclinic intersections (between stable and unstable manifolds) lead to deterministic unpredictability (of course, the equations are derived in reference to three bodies in their gravitational fields, similarly as Peano Axioms are invented in reference to the ordered structure of numbers). Gödel as well, by playing a purely formal game, formally constructs an undecidable sentence, with no reference what so ever, in the *statements* and *proofs* in his 1931 paper, to "semantics", "truth" or alike, that is to the underlying mathematical structure.

Modern "concrete incompleteness" theorems (that is, Gödel-Girard's normalisation, Paris-Harrington or Friedman-Kruskal theorems) resemble instead Laskar's results of the '90s, where "concrete unpredictability" is shown for the solar system. In reference to the *best possible astronomical measures*, Laskar shows that the evolution of our beloved system is provably unpredictable, globally, beyond one million years (one hundred years, when considering only Earth). Similarly, concrete incompleteness for Arithmetic

was given by proving (unprovability and) truth over the (standard) model. More generally, I view the incompleteness of our formal (and equational) approaches to knowledge a fundamental epistemological issue. And this why we permanently need new science: by inventing new principles of conceptual constructions we change directions, propose new intelligibilities, grasp or organise new fragments of the World. There no such a thing as "the final solution to the foundational problem" in mathematics (as Hilbert dreamed—a true nightmare), nor in other sciences.

And finally, then, my other current interest, biology. The "incompleteness" of the molecular theories for understanding life phenomena is a similar issue. No way to understand/derive completely embryogenesis nor phylogenesis (evolution) by looking only at the four letters of the bases of DNA (the formal language of molecular biology). More precisely, in this very different context, "completeness" philosophically corresponds to the largely financed myth that the *stability* and the *organisation* of the DNA and the subsequent molecular cascades completely determine the *stability* and the organisation of the cell and the organism. This is false, since the stability and the *organisation* of the cell and the organism causally contribute to the stability and the organisation of the DNA and the subsequent molecular cascades. Thus the analysis of the global structure of the cell (and the organism) must parallel the absolutely crucial molecular analyses. The hard philosophical point to explain now, to my friends in molecular biology, is that "incomplete" does not mean useless: formal systems are incomplete but ... very useful, of course (well, I worked most of my life in Type Theory, lambda-calculus and related formal systems ...). The point is that we badly need also an autonomous theory of organism and further develop the (fantastic) Darwinian theory of evolution.

By the way, randomness plays a crucial role in evolution, but also, and it is increasingly believed so, in embryogenesis. But ... what kind of randomness? Physics, classical/quantum, proposes two distinct notions of randomness

CC: What aspects of randomness interest you?

GL: Classical (physical) randomness is unpredictability of deterministic systems in finite time (dice trajectories are perfectly determined: they follow the Hamiltonian, a unique geodetics; yet, they are very sensitive to initial and contour conditions ...: it is, in general, not worth writing the equations). Now, Martin-Löf's (and Chaitin's) number-theoretic random-

The Human Face of Computing

ness is for infinite sequences. How may this yield a connection then between Poincaré's unpredictability and Gödel's undecidability?

As I said, physical randomness, as deterministic unpredictability, is a matter at the interface "equations/process" by measurement and shows up at finite time. Yet, also physical randomness may be expressed as a limit or asymptotic notion and, by this, it may be soundly turned into a purely mathematical issue: this is Birkhoff's ergodicity (for any observable, limit time averages coincide with space averages). That is, physical randomness, as a mathematical limit property, lives in formal systems of equations or evolution functions: in their measurable spaces, they may engender infinite random trajectories or generic points, in the ergodic sense. And this sense applies in (weakly chaotic) dynamical systems, within the frame Poincaré's geometry of dynamical systems.

As for algorithmic randomness, Martin-Löf randomness is a "Gödelian" notion of randomness, as it is based on recursion theory and yields a strong form of undecidability for infinite 0-1 sequences (in short, a sequence is random if it passes all *effective statistical tests*). Recently, M. Hoyrup and C. Rojas, under Galatolo's and my supervision, proved that dynamic randomness (a la Poincaré, thus, but at the purely mathematical limit, in the ergodic sense), in suitable *effectively given* measurable dynamical systems, is equivalent to (a generalisation of) Martin-Löf randomness (Schnorr's randomness). This is a non-obvious result, based also on a collaboration with P. Gacs, spreading along two "entangled" doctoral dissertations (defended in June 2008, a nice example on how two collaborating individuals may produce more than the "double").

In the last few years, I have been teaching a course at ENS, in Paris (and once in Rome III), along these parallel lines, from Poincaré and Gödel to algorithmic randomness. The course (the program and one video-recording is on my web page) was first organised with a colleague in quantum mechanics at ENS, Thierry Paul: we alternated one two hours lecture each and he introduced the EPR paradox (Einstein's and others' paper on entanglement) and its modern consequences, quantum computing. As Thierry moved to Polytéchnique, I took up part of his lectures since we have been doing some joint work on a logical and (modern) physical understanding of EPR. By the way, EPR is dedicated to prove the incompleteness (!) of QM. Their argument is (beautiful, but) wrong as it is based on the impossibility of entanglement.

As for quantum randomness, note now that, because of entanglement, it differs from classical: if two classical dice interact and then separate, the

probabilistic analysis of their values are independent. When two quanta interact and form a "system", they can no longer be separated: measures on them give correlated probabilities of the results (mathematically, they violate Bell's inequalities).

CC: What is the link between computability, continuity and Church-Turing thesis?

GL: The idea hinted in the book and in several papers with Bailly and Paul, two physicists, is that the mathematical structures, constructed for the intelligibility of physical phenomena, according to their continuous (mostly in physics) or discrete nature (generally in computing), may propose different understandings of Nature. In particular, the "causal relations", as structures of intelligibility (we "understand Nature" by them), are mathematically related to the use of the continuum or the discrete and may deeply differ (in modern terms: they induce different symmetries and symmetry-breakings, see a 2014 paper with Montévil).

But what discrete (mathematical) structures are we talking about? I believe that there is one clear mathematical definition of "discrete": a structure is *discrete* when the discrete topology on it is "natural". Of course, this is not a formal definition, but in mathematics we all know what "natural" means. For example, one can endow Cantor's real line with the discrete topology, but this is not "natural" (you do not do much with it); on the other hand, the integer numbers or a digital data base are naturally endowed with the discrete topology (even though one may have good reasons to work with them also under a different structuring).

Church's thesis, introduced in the '30s after the functional equivalence proofs of various formal systems for computability, concerns only computability over integers or discrete data types. As such, it is an extremely robust thesis: it ensures that any sufficiently expressive *finitistic formal system* over integers (a Hilbertian-type logic-formal system) computes exactly the recursive functions, as defined by Gödel, Kleene, Church, Turing This thesis therefore emerged within the context of mathematical logic, as grounded on formal systems for arithmetic computations, on discrete data types.

The very first question to ask is the following: If we broaden the formal framework, what happens? If we want to refer to continuous (differentiable) mathematical structures, the extension to consider is to the computable real numbers. Are, then, the various formalisms for computability over real numbers equivalent, when they are maximal? An affirmative response could

The Human Face of Computing

suggest an extension of Church thesis to computability on "continua". Of course, the computable reals are countably many, but they are dense in the "natural" topology over Cantor's reals, a crucial difference, as we shall see. With this question, we begin to get near to physics, since it is within spatial and often also temporal continuity that we represent dynamical systems, that is, most mathematical models for classical physics. This does not imply that the World is continuous, but only that we have said many things thanks to continuous tools as very well specified by Cantor (but his continuum is not the only possible one: Lawvere and Bell, say, proposed another without points).

Now, from this equivalence of formalisms, at the heart of Church's thesis, there remains very little when passing to computability over real numbers: the theories proposed are demonstrably different, in terms of computational expressiveness (the classes of defined functions). The various systems (recursive analysis, whose ideas were first developed by Lacombe and Grezgorzcyk, in 1955-57; the Blum, Shub and Smale, BSS, system; the Moore-type recursive real functions; different forms of "analog" systems, among which threshold neurones, the GPAC ...) yield different classes of "continuous" computable functions. Some recent work established links, reductions between the various systems (more precisely: pairwise relations between subsystems and/or extensions), yet, the full equivalence as in the discrete case is lost. Moreover, and this is crucial, these systems have no "universal function" in Turing's sense. And this, for a fundamental reason, which has to be analysed closely.

If one endows non-trivial space continua with the interval topology (the "real" topology), there is no way to have an isomorphism between spaces of different dimension (see below). This isomorphism, instead, is needed for having the universal function (Turing's Universal machine) and, in general, for computability on the discrete. Its work spaces may be of any finite dimension: they are all effectively isomorphic, "Cartesian dimension" does not matter!

This is highly unsuitable for physics. First, the dimensional analysis is a fundamental tool (one cannot confuse energy with force, nor with the square of energy ...). Second, dimension is a topological invariant, in all space manifolds for classical and relativistic physics. This is shown by the fact that if two such spaces have isomorphic open subsets, then they have the same dimension. This is one of the most beautiful correspondence between mathematics and physics. Take physical measure, which is always an interval (it is approximated, by principle, classically), as a "natural"

starting point for the metric (thus the interval topology), then you *prove* that this crucial notion for physics, dimension, is a topological invariant. Discrete computability destroys this: a cloud of isolated points has no dimension, per se, and you may, for all theoretical purposes, encode them on a line. When you have dimension back, in computability over continua, where the trace of the interval topology maintains good physical properties, you loose the universal function and the equivalence of systems. Between the theoretical world of discrete computability and physico-mathematical continua there is a huge gap. One cannot even extend to the second a sound form of Church Thesis. While I believe that one should do better than Cantor as for continua, I would not give a penny for a physical theory where dynamics takes place only on discrete spaces, departing from physical measure, dimensional analysis and the general relevance of dimensions in physics (again, from heat propagation to mean field theory, to relativity theory ... space dimension is crucial).

As for the relevance of the discrete, quantum mechanics started exactly by the discovery of a key (and unexpected) discretisation of light absorption or emission spectra of atoms. Then, a few dared to propose a discrete lower bound to measure of *action*, that is of the product energy \times time. It is this physical dimension that bares a discrete structure. Clearly, one can then compute, by assuming the relativistic maximum for the speed of light, a Planck's length and time. But in no way space and time are thus organised in small "quantum boxes". And this is the most striking and crucial feature of quantum mechanics: the "systemic" or entanglement effects, which yield inseparability of observables. No discrete space topology is natural. That is, these quantum effects are the opposite of a discrete, separated organisation of space, while being at the core of its scientific originality. In particular, they motivate quantum computing (as well as our analysis of quantum randomness above). As a matter of fact, Thierry Paul and I claim that the belief in an absolutely separable topology of space continua is Einstein's mistake in EPR.

A final remark. In general, the discrete is not an approximation of classical continua. In even weakly chaotic systems, a difference by approximation (below measure, typically) quickly leads to major (observable) differences in evolutions. And the approximation relation is at most reversed. In some, not all, dynamical systems, the "shadowing lemma" holds: for any discrete trajectory, there is continuous one approximating it. The quantification is the other way round. This is an important result in Numerical Analysis, as it guaranties that a discrete trajectory on the screen is not meaningless:

The Human Face of Computing

one can find a continuous one approximating it. Of course, it does not start with the same initial point, in general.

In summary, continua, Cantorian or not, take care rather well (they are not an absolute) of the approximated nature of physical measure, which is represented as an interval: the unknowable fluctuation is within the interval. Classically, I insist, the relevance of measure is derived from Poincaré's results (changes below measure induce major differences over time). And physical measure is our only form to access "reality". The arithmetising foundation of mathematics went along another (and very fruitful) direction, based on perfectly accessible data types. Poincaré firmly opposed to the underlying philosophy of knowledge, by deep, but informal, reflections on "action" in the physical world.

CC: We have entropy and negative entropy. You invented anti-entropy.

GL: Traditionally, information is considered as negentropy (Brillouin). Then, by definition:

- (1) the sum of a quantity of information (negentropy) and an equal quantity of entropy gives 0;
- (2) information (Shannon, but also Kolmogorov) is "insensitive to coding" (one can "encrypt" and "decrypt" as much as one wishes but the information content will not be lost/gained, in principle).

I believe however that this notion, of which the applications are numerous, is not sufficient for an analysis of the living state of matter. DNA (usually considered as digital information) is the most important component of the cell, as I said, but it is necessary to analyse the *structure* of the organism, as an observable specific to biological theorisation.

Here, the collaboration with Francis Bailly, a physicist also interested in biology, has been very important. He actually was my teacher in many aspects of natural sciences (Francis recently passed away: a recorded conference in his memory may be accessed from my web page). Concerning biological (morphological) complexity, we have proposed the notion of anti-entropy to define it (or quantify it in terms of complexity of cellular, functional and phenotypical differentiation). In short, biological complexity may be understood as "information specific to the form", including the intertwining and enwrapping of levels of organisation. Its use in metabolic balance equations has produced a certain number of results mentioned in a recent long article (and summarised in the 2014 book with Montévil). We have, in particular, examined systems far from equilibrium and analysed diffusion equations of

biomass over biological complexity as anti-entropy, following Schrödinger's "operational method" in quantum mechanics. This has enabled to operate a mathematical reconstruction of this diffusion, which corresponds to the paleontological data presented by Gould for the evolution of species.

Anti-entropy is compatible with information as negentropy, but it must be considered as a *strict* extension, in a logical sense, of the thermodynamics of entropy. Typically, the production of entropy and that of anti-entropy are summed in an "extended critical singularity", an organism, never zero, in contrast to Brillouin's and others' negentropy. As it is linked to spatial forms, anti-entropy is "*sensitive to coding*", contrarily to digital information (it depends on the dimensions of embedding manifolds, on folds, on singularities ...).

In short, over the last six or more years, in several collaborations and by (co-)supervising several theses, we have compared physical (dynamic) randomness with algorithmic randomness (at the centre of algorithmic theories of information); we worked at a theory of "extended criticality" (living objects persist in an "extended critical state"); we have added anti-entropy (a "geometrical extension" of the notion of information) to thermodynamic (in)equalities and balance equations; we modelled biological rhythms and time in two-dimensional manifolds, a sort of non-trivial geometrisation of time (and, perhaps, a quite useful one, for the digital simulation of cardiac rhythms, developed by M. Montevil). The scientific finality of this work may also entail some epistemological consequences, I hope: it should participate to the epistemological debate regarding the notion of information, the updating of its theoretical principles, as part of the many existing interactions with physics and biology. A a possible outcome of these interactions could be to start thinking to ... the next machine. Aren't we a little tired of this, nice, but mathematically rather old "Discrete Data Types Machine"?

CC: Is it possible to summarise the ideas of your book *Mathématiques et sciences de la nature. La singularité physique du vivant* (Hermann, Paris, 2006) with F. Bailly? Is there an English version?

GL: Yes, there is a translation in English (published in 2011, by Imperial College Press, London). In the book, Francis and I attempt to identify the organising concepts of some physical and biological phenomena, by means of an analysis of the foundations of mathematics and of physics, in the aim of unifying phenomena, of bringing different conceptual universes into dialog. The analysis of the role of "order" and of symmetries in the foundations of mathematics is linked to the main invariants and principles, among

The Human Face of Computing

which the geodesic principle (a consequence of symmetries), which govern and confer unity to the various physical theories. Moreover, we attempt to understand causal structures, a central element of physical intelligibility, in terms of symmetries and their breakings. The importance of the mathematical tool is also highlighted, enabling us to grasp the differences in the models for physics and biology which are proposed by continuous and discrete mathematics, such as computational simulations.

As for biology, being particularly difficult and not as thoroughly examined at a theoretical level, we propose a "unification by concepts", an attempt which should always precede mathematisation, that we later tried in some papers. This constitutes an outline for unification also basing itself upon the highlighting of conceptual differences, of complex points of passage, of technical irreducibilities of one field to another. Indeed, a monist point of view such as ours should not make us blind: we, the living objects, are surely just big bags of molecules or, at least, this is our main metaphysical assumption. The point though is: which *theory* can help us to better understand these bags of molecules, as they are, indeed, rather funny (singular?), from the physical point of view. Technically, this singularity is expressed by the notion of "extended criticality", a notion that logically extends the pointwise critical transitions in physics. Further work is summarised in the book with M. Montévil (*Perspectives on Organisms: Biological Time, Symmetries and Singularities*, Springer, 2014).

CC: I like this statement: "Further work is summarised" in a book.

GL: The idea is to work towards a Theory of Organisms analogue and along the Theory of Evolution, where ontogenesis could be considered as part of phylogenesis. As a matter of fact, the latter is made out of "segments" of the first: phylogenesis is the "sum" of ontogenetic paths and they should be made intelligible by similar principles. To this aim, we look at ontogenesis from different perspectives: the peculiar role and structure of time, the relevance of critical transitions, the increasing complexity of many phenotypes in evolution, the structure of causality in phylogenesis and ontogenesis. By this, we tried to shed some light on the unity of the organism from different points of view, yet constantly keeping that unity as a core invariant. As a matter of fact, the analysis of *invariance*, as the result of theoretical symmetries, and of *symmetry changes*, is a key theme of the approach in the book

CC: In a recent paper you rightly say that

... randomness ... is not in the world, it is in the interface between our

theoretical descriptions and 'reality' as accessed by measurement. Randomness is unpredictability with respect to the intended theory and measurement.

Randomness like determinism, chance, number, speed are only theoretical concepts with no *direct* counterpart in nature.

GL: These concepts remarkably organise nature. And this is far from arbitrary: our knowledge construction is designed by us on a phenomenal veil, by a concrete friction on the world and "reality" canalises our "endeavour towards knowledge", to put it in Herman Weyl's words. So, our theoretical proposals may be incomplete for several and different reasons: impredictability, whether deterministic, like in classical dynamics, or indeterministic (the determination of a law or amplitude of probability), is a form of "incompleteness", in a broad sense. It is so by principle, because the approximated nature of classical measurement and quantum indetermination are fundamental principles of the interface between us and the world. We have no other form of access to the world, but by measurement, and this is what it is: always approximated or indeterminate. Then the theory says what unpredictability or randomness is, exactly, in that intended frame: classical deterministic unpredictability, quantum indetermination ... or the one that future theories may give us.

CC: How does randomness appears in biology?

GL: In some writings in collaboration, I focused on the various possible notions and instances of this essential component of biological dynamics, contingency as randomness. Randomness in Biology deserves a proper analysis, along the lines but well beyond the many deep ones carried out within the various theories of the inert. Classical and quantum physics each propose different forms of randomness, as we know. These, in our view, are both relevant for understanding biological randomness, as they are both present at the molecular level, at least.

In short, in the classical Darwinian perspective on selection, the unfit or the less adapted is eliminated. However, low or high adaptations are relative and may depend, of course, on the possible presence of the other avatars of competitive species or variants within a population which, locally and on specific aspects, may perform better or worse in reproduction, possibly in correlation to access to limited resources. However, even minor changes in the environment, undetectable at any particular time, may subsequently show that different causes may render the organism or species more or less adapted, adaptation standing as the only general criteria for natural

The Human Face of Computing

selection. And in no way, by looking at one specific evolutionary moment, may one predict which individuals or species will be less or more adapted in thousands or millions years.

So, we compared the different notions of randomness in Physics with those implicit in current Biology, for the purposes of the study of contingency in the two main biological processes, development and evolution. Quantum and "quantum-like" effects may happen jointly to classical dynamics and their proper forms of randomness, i.e. they take place simultaneously and affect each other in intracellular processes, as shown by robust evidence today. Moreover, different levels of organisation, in a multicellular organism (as well as in 'colonies' of unicellular ones), may interact and produce stabilising and destabilising effects. This form of integration and regulation also includes random phenomena or amplification of randomness, and we called it 'bio-resonance', in analogy to the well-established notion of resonance in physical non-linear dynamics.

CC: What is the relation between randomness and diversity in biology?

GL: Small differences may be present also in the DNA, at each cell reproduction. The sensitivity to the context at transition may contribute to cell differentiation; it surely contributes to diversity (in unicellular, first, but also in multicellular organisms). Note that disorder may go from molecules (mutations, typically, but also proteome changes or irregular split) to the whole organism, the latter being the key component for the exploration of diversity, proper for instance of the Evo-Devo systems.

Moreover, the analysis of randomness, in Biology, cannot be isolated from a context: it is always a more or less highly canalised phenomenon. Even the Brownian motion of molecules, in a cell, is highly canalised by membranes and compartmentalisation or even by the coherence structure of water. The role of randomness is a crucial and fully general point. In particular, the many-folded forms of randomness, which manifest themselves within integrating and regulating activity, are at the core of variability and diversity, thus of evolution and development. That is, they also contribute to the peculiar stability of phylogenesis and ontogenesis. As a matter of fact, these are possible since they are adaptive and constantly explore possibly diverging paths, under different and ever changing constraints.

Our work opens the way to a new conceptual and mathematical challenge: describing biological randomness also as unpredictability of the very phase space, well beyond the existing analyses in physics, where randomness is given in a predefined space of possibilities (phase space). And this

is so, in presence of dynamics where variability, adaptability and diversity are among the main invariants and contribute to the structural stability of organisms and species. And variability is (largely? only?) a consequence of randomness.

CC: In what sense do you think physical or biologically processes "compute"?

GL: The Discrete State Machines that compute are a remarkable invention, based on a long history. As I hint in the paper "Critique of Computational Reason in the Natural Sciences", this story begins with the invention of the alphabet, probably the oldest experience of discretisation. The continuous song of speech, instead of being captured by the design of concepts and ideas (by recalling "meaning", like in ideograms), is discretised by annotating phonetic pitches, an amazing idea (the people of Altham, in Mesopotamia, 3300 B.C.). Meaning is reconstructed by the sound, which acts as a compiler, either loud or in silence (but only after the IV century A.D. we learned to read "within the head", in silence !).

I insist that the crucial feature of alphanumeric discretisation is the invention of a discrete coding structure, which is far from obvious. Think also of the originality of Gödel-numbering, an obvious practice now, but another remarkable invention. Turing's work followed: the Logical Computing Machine (LCM), as he first called it, at the core of our science (right/left, 0, 1 ...). Of course, between the alphabet and Turing, you also have Descartes "discretisation" of thought (stepwise reasoning, along a discrete chain of intuitive certitudes ...) and much more.

When, after 1948 or so, Turing gets again interested in physics, he changed the name to his LCM: in the 1950 and 1952 papers, he calls it Discrete State Machine (this is what matters for its physical behaviour). And twice in his 1950 paper (the "imitation game"), he calls it "Laplacian". Its evolution is theoretically predictable, even if there may be practical unpredictability (too long programs to be grasped, says he).

So, we invented an incredible stable processor, which, by working on discrete data types, does what it is expected to do. And it iterates, very faithfully. Primitive recursion and portability of software are forms of iterability: iteration and update of a register, do what you are supposed to do, respectively, even in slightly different contexts, over and over again. For example, program the evolution function of the most chaotic strange attractor you know. Push "restart": the digital evolution, by starting on the same initial digits, will follow exactly the same trajectory (on a paper on

The Human Face of Computing

Turing's imitation game I discuss the simulation of the double pendulum, a chaotic device). This makes no physical sense, but it is very useful (also in meteorology: you may restart your turbulence, exactly, and try to better understand how it evolves ...). Of course, you may imitate unpredictability by some pseudo-random generator or by ... true physical randomness, added ad hoc. But this is cheating the observer, in the same way Turing's *imitation* of a woman's brain is meant to cheat the observer, not to "model" the brain. He says this explicitly, all the while working in his 1952 paper, at a *model* of morphogenesis, as (non-)linear dynamics. Observe, finally, that our colleagues in networks and concurrency are so good that programming in network is reliable: programs do what they are supposed to do, they iterate and ... give you the web page you want, identically, one thousands time, one million times. And this is hard, as physical space-time, which we better understand by continua and continuous approximations, steps in, yet still on discrete data types, which allow perfect iteration.

Those who claim that the Universe is a big digital computer, miss the originality of this machine of ours. It is like believing that, when we speak, we produce sequences of letters: this is a cartoon's vision of language and misses the originality of our invention, the alphabet, an early musical notation (Chinese children have a different view: their cartoons' bubbles evoke concepts). When we construct computers, we make the far from obvious construction of a reliable, thus programmable, physical device, iterating as we wish and any time we wish, even in networks: this happens very rarely in physics. One should not miss the principles that guided this invention, as well as the principles by which we understand physical dynamics.

By the way, a question for those ho claim that the Universe is a big Turing Machine: are the main physical constants, G, c, h, computable (real numbers)? They appear in all equations and all dynamics Of course, it depends on the choice of the reference system and the metrics. So, fix h = 1. Then, you have to renormalise all metrics and re-calculate, by equations, dimensional analyses and physical measurement, G and c. But physical measurement will always give an interval, as we said, or, in quantum frame, the probability of a value. If one interprets the classical measure interval as a Cantorian continuum, the best way, so far, to grasp fluctuations, then ... where are G and c? Non-computable reals, or even Martin-L UTF009A" of random reals form a set of Lebesgues measure 1, so ... there is probability 1 that the main physical constant (except one, the one you prefer) are random reals (an asymptotic game, the one preferred by God, for sure).

Giuseppe Longo: Computations and Natural Sciences

Yet, the most striking mistake of many "computationalists" is to say: but, then, some physical processes would super-compute (compute noncomputable functions)! No, this is not the point. Most physical processes, simply do not *define* a mathematical function. In order to have a classical process to define a function, you have to fix a time for input, associate a (rational) number to the interval of measure and ...let the process go. Then you wait for the output time and measure again. In order, for the process, to define f(x) = y, at a rational input x it must always associate a rational output y. But if you restart, say, your physical double pendulum on x, that is within the interval of the measure which gave you x, a minor (thermal, say) fluctuation, *below that interval* x, will yield a different observable result y' after very short time. So, a good question would be, instead: consider a physical process that *defines* a (non-trivial) function, is this function computable?

The idea then would be that the process is sufficiently insensitive to initial conditions (some say: robust) as to actually define a function. But, then one should be able to partition the World in little cubes of the smallest size, according to the best measure as for insensitivity (fluctuations below that measure do not affect the dynamics). If the Accessible World is considered finite (but ... is it?), then one can make a list out of the finite input-output relation established by the given process. This is a "program": is it compressible?

As for biology, what can I say? 40% of fecundations in mammals fail (they do not reach a birth): a very bad performance for the DNA as a program. While iterability is at the core of software (and hardware) design, our fantastic invention, the key principle for understanding life, at the phenotypic level, is *variability*, a form of non-iterability. It is crucial for evolution, but also ontogenesis, that a cell is never identical to the mother cell. So, the principles of intelligibility are the exact opposite: the failure of lots of fecundations corresponds to the possibility that a mutant better fits a changing environment (affecting the mother's womb, say). Of course, some molecular processes iterate, but there is an increasing tendency to analyse molecular cascades in terms of statistical phenomena (and this is where good computational imitations may help to understand, by some use of pseudo-randomness or by networks interactions). This opens the way to an increasing role of epigenetic and, thus, to the relevance of downwards regulating effects, from the cell and the organism to DNA expression.

CC: You argue that incomputability phenomena are more important to

The Human Face of Computing

physics than computable ones? After all, the laws of physics seem more computable than incomputable?

GL: Your second question refers to the effectiveness of our *mathematical writing* of physical invariants: of course, equations, evolution functions ... are given by sums, products, exponents, derivations, integrations ... all effective operations. Moreover, no one is so crazy to put an incomputable real as a coefficient or exponent in an equation (even if h could be so ..., so we just take an approximation of it, don't we?). This gives us remarkable approximations and, most often, qualitative information: Poincaré's geometry of dynamical systems or Hadamard's analysis of the geodetic flow on hyperbolic surfaces, do not give predictions, but very relevant global information (by attractors, for example, or regularities in flows ... that we beautifully see today, as never before, by fantastic approximations, "shadowed" on our computers screens).

By the way, to those that believe that "nature computes" like Turing in his child's note book, I would like to ask whether Planck's h is a computable real number. Well, set h = 1, then renormalise c, G. As Martin-L

UTF009A" of-Chaitin random numbers have measure 1 on the reals, I bet that these physical constants are random reals ... an asymptotic notion of randomness, thus an infinitary game, the only one adequate to God.

I do not know (absolute) laws of Nature, but our constructive theorising on the phenomenal veil, at the interface between us and the World. This active constructions are of course effective (we use the alphabet, effective operations and codings, I insist). While predictable processes are not many in Nature: you can predict a few forthcoming Eclipses, at human time scale, but the Solar System is chaotic in astronomical times, as Poincaré proved and Laskar quantified (and computed!). Unpredictable ones are the mathematical and computational challenge. And a computable physical process is, by definition, deterministic and predictable. In order to predict (pre-dicere, "to say in advance" in Latin), just "say" or write the corresponding program and compute in advance; more precisely, the results discussed above, by showing the equivalence of unpredictability and (strong) undecidability, ML-randomness, prove this fact, by logical duality. Unpredictability may pop-out in networks and this because of *physical* space-time (we then make them computable and predictable-reliable by forcing semaphores, handling interleaving ...). In Nature, many (most, fortunately) processes escape predictions, thus our computations. Fortunately, otherwise there would be no change, nor life in particular: randomness is crucial. And when we

compute unpredictable evolutions, we just approximate their initial part, as I said, or give qualitative information, both very relevant tasks. But engineers put some more cement than computed, to take care of vibrations below measure

Now, the only mathematical way I know to define randomness, in classical physics, is Birkhoff's ergodicity. But it is very specific (certain dynamics). Otherwise, randomness is given in terms of probability measure. But this is unsatisfactory, as probability gives a *measure* of randomness, not a definition. It is the theory of algorithms, thanks to Martin-Löf, Chaitin and you, that gave a fully general, mathematical, notion of randomness, as a strong form of incomputability, independently (or on top) of probability theory. Again, physical (classical) randomness is deterministic unpredictability and, by the results above and more in the literature, the role of computational randomness further comes to the limelight. In particular, it provides a very flexible theory of randomness: you can adjust the class of effective randomness tests (Martin-Löf, Schnorr ... and many more). Our joint hope is that this may help to better grasp, for example, the mathematical difference between classical and quantum randomness.

CC: If all papers and books would be destroyed by a disaster, but you could keep just one, which one would you choose? Why?

GL: I do not think I would survive to this, but, just to give a partly random answer: Weyl's "Philosophy of Mathematics and of Natural Sciences". Along the lines of "Das Kontinuum", it radically departs form the Hilbertian alphabetic myths. Mathematics, actually human thought, for formalists and computationalists, is reducible to the matching and replacement of sequences of letters: no geometric judgements, no association of gestalts ..., this is why this Laplacian mechanics of thought is incomplete. The proof has also a "geometric structure", a remark by Poincaré, and, by this, it is "sensitive to codings". This is also why its formal coding is incomplete. Reasoning is not a chain whose strength is that of the weakest ring, as Descartes claimed, but a network, a rope made of many interlaced wires, as suggested by Peirce, reinforcing each other and coupling to meaning and to forms of life. And Weyl globally develops a deep and broad philosophy of knowledge, well beyond the parody of his views in predicativist or intuitionistic terms.

CC: If all your papers and books would be destroyed by a disaster, but you could keep just one, which one would you choose? Why?

GL: The paper on "anti-entropy", where some (minor) aspects of Evo-

The Human Face of Computing

lution are mathematically described, because \dots it is the less quoted one (too "strange" a paper) and because, while working at it, I increasingly learned to love Darwin.