On the relevance of negative results

Giuseppe Longo

On the relevance of negative results

Giuseppe Longo, *Influxus*, [En ligne/Online], mis en ligne le 21 novembre 2012. URL : http://www.influxus.eu/article474.html - Consulté le 5 mars 2013.

Résumé

L'accès à la connaissance scientifique est une construction de l'objectivité qui nécessite l'aperçu critique de «résultats négatifs». Ceux-ci consistent en la construction explicite des limites internes aux théories et les méthodes actuelles. Nous ferons allusion au rôle de certains résultats qui, en logique, en physique ou en informatique, ont ouvert de nouveaux domaines de connaissances en affirmant : «Non, nous ne pouvons pas calculer cela, nous ne pouvons pas décider que ...». L'idée est que les sciences de la vie et de la cognition, en particulier dans le cadre des mathématiques et de l'informatique, ont besoin de résultats similaires afin de fixer des limites au transfert passif de méthodes physico-mathématiques dans leur construction autonome de la connaissance, et d'ouvrir la voie à de nouveaux outils et perspectives. Nous comparerons cette perspective avec l'exigence, tant au niveau national qu'européen, de finaliser la plupart des activités de recherche (toutes ?) pour des applications industrielles prévisibles.

Abstract

The access to scientific knowledge is a construction of objectivity which needs the critical insight of "negative results". These consist in the explicit construction of internal limits to current theories and methods. We shall hint to the role of some results which, in Logic, in Physics or Computing, opened up new areas for knowledge, by saying "No, we cannot compute this, we cannot decide that…". The idea is that both the sciences of life and of cognition, in particular in connection to Mathematics and Computing, need similar results, in order to set limits to the passive transfer of physico-mathematical methods into their autonomous construction of knowledge and open the way to new tools and perspectives. We will compare this perspective with the requirement, both at the national and European levels, to finalize most (all ?) research activities into foreseeable industrial applications.

On the Relevance of Negative Results[1]

Giuseppe Longo

CNRS et Dépt. d'Informatique.
École Normale Supérieure, Paris
et CREA, École Polytechnique
http://www.di.ens.fr/users/longo

Abstract
The access to scientific knowledge is a construction of objectivity which needs the critical insight of "negative results". These consist in the explicit construction of internal limits to current theories and methods. We shall hint to the role of some results which, in Logic, in Physics or Computing, opened up new areas for knowledge, by saying "No, we cannot compute this, we cannot decide that…". The idea is that both the sciences of life and of cognition, in particular in connection to Mathematics and Computing, need similar results, in order to set limits to the passive transfer of physico-mathematical methods into their autonomous construction of knowledge and open the way to new tools and perspectives. We will compare this perspective with the requirement, both at the national and European levels, to finalize most (all?) research activities into foreseeable industrial applications.

1. Scientific knowledge and critical insight.

The analysis of concepts, conducted on a comparative level if possible, as well as the (tentative) explanation of the philosophical project, should always accompany scientific work. In fact, critical reflections regarding existing theories are at the core of positive scientific constructions, because science is often constructed *against* the supposed tyranny and autonomy of "facts" which in reality are nothing but "small-scale theories". Science is also often constructed by means of an *audacious interpretation* of "new" (and old) facts; it progresses against the obvious and against common sense (le "bon sens"); it struggles against the illusions of immediate knowledge and must be capable of escaping from already established theoretical frameworks. For example, the very high level of mathematical technicity in the geometry of Ptolemaic epicycles constructed from clearly observable facts strongly perplexed numerous Renaissance thinkers such as Copernicus, Kepler and Galileo…: in order to account for the movements of the stars and for the "obvious" immobility of the earth, circles that were added to circles, centers of new circles, were established with and extraordinary geometrical finesse and gave way to uncountably many "publications" (of very high Impact Factor, at least till the middle of the XVII century). Yet they failed to convince the aforementioned revolutionary critical

[1] Invited paper at the conference "**Négation, dualité, polarité**", Marseille, 2009 (proceedings to appear in 2012). A preliminary version in French of the first part of this paper appeared in **Intellectica**, vol. 40, n. 1, 2005.

thinkers. And, as Bachelard rightly puts it, the construction of knowledge was then founded, as was Greek thought, upon an epistemological severance, which operates a separation with the previous ways of thinking.

But it is recent examples that interest us, where the critical view finds expression on a more punctual basis, by means of "negative results". Let's explain.

When Poincaré was working on the calculi of astronomers, on the dynamics of planets within their gravitational fields, he produced, by purely mathematical means, a great "negative result": formal (equational) determination *does not* imply mathematical predictability. The result is negative – such is how Poincaré calls it: *one cannot predict, or calculate,* the evolution of a planetary system, even if it is formed by only two planets and a sun, despite having a dynamics which is still perfectly determined by the Newton-Laplace equations. This is the origin of what will later be called "deterministic chaos": systems where determination is compatible with, if not underlying, random evolutions. It was a true revolution, which destabilized a science that positively expected the "great equation" of knowledge of the world, as a potentially complete tool for scientific prediction.

Poincaré's result is, of course, important in itself, but its role will be better understood in time, when the *techniques* of the proof (of the theorem of the three bodies) will have spurred a new field of knowledge, the geometry of dynamical systems, of which the applications are quite important within contemporary science. It is not a coincidence if it took 70 years for these techniques to be developed (with the exception of the works by Hadamard and of a few isolated Russian scientists, it took up till the 50s and 70s with the Kolmogorov-Arnold-Moser theorem and the works of Ruelle): a negative result destabilizes positive expectations and does not necessarily indicate where to go from there. "The new methods" were there in Poincaré's writings, it is true, but the negation of an expectation does not immediately fall within the expected positivity of science: the delay for applications seems to demonstrate that it is necessary to first assimilate (philosophically) the critical standpoint and the boundaries which a negative result imposes upon existing knowledge in order for a new construction of objectivity to follow.

On the other hand, the critical viewpoint precedes Gödel's incompleteness theorem. Gödel did not believe in Hilbert's hypothesis of completeness and decidability of sufficiently expressive formal theories. He thus explored a syntactical variant (through arithmetic) of the liar's paradox, demonstrably equivalent to the coherence of arithmetic: both statements are unprovable, if arithmetic is consistent. The impact of this is also huge. On the one hand, the enunciation of the theorem, as in the case of Poincaré, surprises and fascinates, on the other, the techniques of proof open up at least one new field: the theory of computability. More precisely, the notion of Gödelization, the class of recursive functions, defined within the proof, the reflexivity of the meta-theory within the (arithmetic) theory will be at the center of analyses of deduction and effective computations, from the 30s onwards. The equivalence of the approaches of formal calculi (and deductions), the works of Church, Turing, Kleene, etc., will spur, by means of the methods of proof of Gödel's negative theorem (*one cannot decide…)*, a new discipline, the science of computability and of computers, which is in the process of changing the world: in order to say that one cannot decide, it was necessary to specify what is meant by "effective procedure of calculus" (and of decision).

In both cases, a theorem which says “no” imposes boundaries upon a form of scientific knowledge (Laplacian determination, formal deduction) and, at the same time, highlights the techniques for progress (quantitative or geometrical methods) or for a better construction of the field thus delimited (effective calculus). Because there actually is a difference: Poincaré’s New Methods already contained, we were saying, the seeds of the geometry of dynamical systems, whereas Gödel’s theorem is “only” a (diagonal) theorem of undecidability (see [Longo, 2011]), saying nothing about the possible proof of the undecidable statement (actually, on the coherence of arithmetic). We will have to wait for Gentzen (ε_0-induction, ‘36), Gödel’s 1958 article, or even Girard’s type of normalization in the 70s in order to have and closely analyze the proofs of coherence. Both theorems therefore set boundaries, but one of them also suggests what can be done “beyond”, while the other constructs, rigorously, all which is doable “from within” these boundaries.

Let’s now recall another immense negative “result” for science. It is not a mathematical theorem, but a change of theoretical viewpoint, following physical experiments. The result consists in the theoretical interpretation of these experiments and the proposition for a radical turnabout in the construction of physical objectivity. In microphysics, *it is impossible to determine,* at the same time, and with as great a precision as one would want, the position and momentum of a particle. Plank, Bohr, Heisenberg… impose a change of viewpoint, thus erecting boundaries that are insurmountable for classical physics: the atom *is not* a little planetary system, upon which to apply the classical methods. The classical “field” ends where begins a new analysis based upon the essential indetermination and the correlations of probabilities instead of classical field and causality… leading to the non-locality, the non-separability of quantum phenomena. It is not an issue of the unpredictability of a deterministic system, as for Poincaré, nor of the incompleteness of formal theories (Gödel), but the intrinsic indetermination of a complete system for microphysics.

This breaking in principles shatters the apparent unity of physics, erects a wall between modes of intelligibility within the very field of physics itself: one physical science, centered upon trajectories, from Aristotle to Galileo, to Newton and to Einstein, could tell us very little about a microphysics where quanta do not as such have trajectories across space-time. Once this new field of knowledge constituted, the issue of the unity of science was properly stated (that of physics, at least), this time, in terms of *unification*, rather than in terms of reduction of the quantum to the relativistic field (or viceversa). One hundred years later, the progress is remarkable, but unification is still far from being achieved.

In this case, the critical approach is formed at the same time as the analysis of the experiments but, without the total freedom of “hermeneutical” thinking enabling to first establish limits to the era’s perspective, the new construction would be unthinkable; a construction, marked at the onset by a very limited recourse to mathematics in comparison to classical physics. The acritical subscription to the technicity existing in science has its predecessor in the splendid geometry of planetary epicycles, spread across whole volumes that are now completely forgotten.

From the mathematical standpoint, we believe that a great negative theorem (even several theorems) or an epistemic turnaround comparable to that of quantum mechanics is needed, in biology as in cognitive sciences. If we want to see the establishment of a new theoretical field if possible with its own mathematical autonomy (as is the case for dynamics and quantum physics), but even if we want to specify and refine the existing methods (as with Gödel), it is also necessary to target, by means of a critical standpoint, the limits of these methods.

Let's try then to ask: what are the cognitive functions or cerebral (cellular) structures which are demonstrably ungraspable by formal neural networks and statistical physics? Which boundary is to be set for the analyses of living phenomena in terms of physical criticality (dynamic and thermodynamic)? Is there, in phylogenesis, an indetermination or a randomness which is specific to living phenomena and comparable, yet different, to indetermination in microphysics (analyses in terms of physical dynamics provide us at best with a deterministic unpredictability)? Which biological phenomenon is non-measurable, in terms of any measure of physical complexity? How can one go beyond the incompleteness of the computational theories of DNA, conceived as a complete (formal-symbolic) "program" for the phenotype (do you remember Hibert's completeness conjecture?), analyzed in terms of theories which add regulating gene-program over regulating gene-program, not unlike what was done back in the age of epicycles?

In [Bailly, Longo, 2011], we have attempted to provide a few venues, although certainly in an incomplete and preliminary manner: the notion of extended critical transition differentiates the analysis of living phenomena from the current physical theory of criticality, including for the conceptualization of the temporality dimension specific to biology. Indetermination has been described in terms of changes in the very space of the evolutions, an approach which is foreign to classical physical determination and even to the mathematics of quantum physics. The notion of contingent finality has extended and enriched the usual representations of physical causality, for which the very notion of finality is actually "beyond the subject"; extended criticality is, in principle, of an infinite physical complexity. Our idea is that well beyond our little attempts, and based upon the theoretical originality of Darwinian evolution, only a conceptual or mathematical autonomy of biological theoretizing could enable the quest for a scientific unity to be constructed in relation to physical and physicochemical theories.

2. Changing frames

Many other results of a "negative nature" may be quoted in science. Let's just mention the various thermodynamic limits (no perpetual movement, no way to reach absolute 0...); A. Kastler, in *Cette étrange matière* (Stock, 1976), calls them "Actes de renoncement" and refers also to the quantum limits recalled above. Similarly, computer science witnessed a flourishing of negative results: computational and complexity limits have been shaping the discipline (it is theoretically/practically impossible to compute this or that... see D. Harel, *Computers Ltd.: What They Really Can't Do,* Oxford U.P., 2003). Yet, the results we focused on above seem to have provided an epistemological severance as they operated a particularly radical separation with the previous ways of thinking: in computer science, for example, the unfeasibility or limiting complexity results move somewhat along the lines of Gödel's (or Turing's undecidability) theorems, even though the technique and the frame may differ. In short, the results we mentioned above caused a philosophical shock in science and, in particular as for Poincaré's theorem and quantum indetermination, a robust resistance to be "digested" or accepted. In the first case, this was indirectly manifested by the major delay in developing further results along the same lines; in the second, by a persisting minority still now proposing "hidden variables" approaches of deterministic flavor, in spite of large empirical evidence (since Aspect's work on Bell's inequalities in 1980, see [Bailly, Longo, 2011]).

In the case of science of the living and cognition, it is possible that the philosophical "resistance" to the required changes in viewpoint, or limiting results, would be even stronger than that which has emerged with regard to unpredictable dynamics, to formal incompleteness and to quantum indetermination: we ourselves constitute living phenomena and, being monists, we want to be within this world (physical). But the unity of science is a difficult thing to achieve and is not attained by transversally forcing the same methods upon different forms of knowledge, as does the attempt to transfer the little planetary system model to the atom: it doesn't work. First, we would rather need to establish the (causal?) "field" of living phenomena and the boundaries (mathematical boundaries if possible) which define its theoretical autonomy in order to then reach a new synthesis, a unification of "fields" which would probably displace all these boundaries in order to grasp the unicity of the material world (our presumption). Of course, to start off with the available mathematical tools is a good method that is employed by numerous highly valued colleagues. But without the talent for taking some distance in order to enable critical thinking, as demonstrated by Poincaré and by quantum physicists, it will be difficult to progress much.

The resistance may not only be of a philosophical nature, but may also stem from this "culture of results" more than "of knowledge", a culture which increasingly claims to completely direct science. The accountability obligation, increasingly required by the managers who rule the scientific financing, is of an industrial type and imposes its paradigms: one must beforehand clearly set out the projected methods, the expected results (the "deliverables" ...) in order to be able, at the end of the project, to compare them with the results effectively obtained.

Scientific objectivity mostly progresses by means of "intelligibility" which may or may not be derived from "positive" results. Fundamental research may only be evaluated (and severely so, as we said) *a posteriori* and will be fundamental if *it has no foreknowledge of its methods and results*. It is without doubt that applications need a scientific and financial effort: oriented, industrial research lacks greatly in Europe, but definitely not because of an excess of fundamental research. All the while developing applicative science, it is necessary to maintain a wide platform for perfectly, absolutely independent thought with regard to any conceivable application. What would a corporate director say if the result he got from the calculation of the evolution of three bodies within a certain physical field was negative and only to yield repercussions 70 years later? And what if he had asked, as accountable objective, for the exact determination of the position and moment of certain atomic particles? Or if Gödel had been asked to build a digital machine to demonstrate all theorems of combinatorial arithmetic? The person funding that sort of work would not have been happy with Poincaré, Heisenberg or Gödel…what would he/she tell the shareholders the following year? Would he/she report a total failure regarding a project of calculus?

Today, and more so than ever, in order to get financing, it is better to propose a computational model for everything, particularly in the fields of biology and cognition, if possible by means of well established techniques, independently of the target discipline. Proposals to calculate, to decide or to determine are certainly at the center of scientific activity and highly appreciated (and rightly so). But it would be better, as history teaches us, if, in parallel, we try (and allow) to construct a critical view, with its own conceptual frameworks and negative results, that is, with the delimitations that create new fields. And this also requires a hermeneutic of scientific knowledge, as was the case for Galilean physics, for Relativity and for Quantum Physics.

An ontological monism, we have often repeated, does not imply a monism of theoretical methods, but a scientific unity to be constructed. As for within the field of physics, it is possible to aim for unification, once set the relative boundaries, once differentiated the theories, if necessary by means of negative results (even the mathematics of Relativity started off by means of a differentiation of the geometry of the space of senses from that of astrophysics, by a negation: Riemannian geometry *is not* stable by homotheties – this is the independence from Euclid's V^{th} axiom: *one cannot transfer* any Euclidian property at any space scale).

It is therefore necessary to emphasize the role of a critical mode of thinking which does not necessarily aim for a positive result stated beforehand (to calculate this or that…) nor for a result provided by pre-explained methods (for the project to be accountable, by means of explicit and direct links between promises and results). And it is necessary to maintain an intangible space for a science which may also produce "non-results" (results that say "Sorry, but *it is not possible* to calculate, decide, determine… transfer such or such method, theorem…"). These results always present a high level of technical difficulty – and of originality, but even a controversial idea can be more interesting than a result which is heroic – and predictable.

Accountability forces us into "normal science" Kuhn would say, a science which is, *sometimes*, rich in immediate applications. But in the sciences of life and cognition, even more so than in the others, we need a new theoretical and mathematical view, which would be specific to them. And this, one century and a half after the coming of the Theory of Evolution, which constituted in its time a revolutionary way of seeing living phenomena, as the only theory truly developed within biology itself and comparable to the great physical theories (relativistic, dynamic, quantum). Thoroughly defining the relative boundaries of the other sciences, physical and mathematical, which claim to be transferable to living phenomena and its cognitive activities, could help to propose it, negatively, and by this help to establish epistemological divisions.

2. Industrially-Oriented Projects?

Europe strongly needs a major commitment in applied and industrial research. The comparisons with American research flourish everyday in the press. As a matter of fact, many research centers of the present or of the past (IBM York Town Heights, Xerox Park, ATT Bell Labs, and many others) provided both the applied and fundamental research grounds for major industrial advances in the USA. Industrial investments in Europe cannot even vaguely compare to this effort that makes the difference in today's technological gap, which, in spite of some areas of industrial excellence (mobile telephones, aerospace), remains or even widens. The question is whether public commitment in Europe, in particular the financial support by the European Commission, can replace this private investment in knowledge. Of course, public funding may help to stimulate industrial ones, but, if full-absorbing, the price to be paid is a decline in fundamental research; the medium or long-term disadvantages will be much greater than the immediate fallout from the current push towards industrially-orienting everything in research. But we will also stress below an immediate negative consequence: a reduced sensitivity to critical insights.

Since more than a decade, in National and European policies, in particular since Edith Cresson's turn on "technological education", the politically correct, as for scientific research, must always refer to "Industrially-Oriented or Motivated" projects; as currently presented, the problems of the Information Society, for example, seem only to be an issue of industrial competitiveness. And this should include even interdisciplinary projects such as research on "Human Cognition", whose aim, instead, is the invention of new theoretical approaches ranging from the analysis of human symbolic culture, as an historical (a pre-historical) issue, to the mathematics of brain activity, not excluding neurobiology and psychophysics. In particular, this is where we need an epistemological turn and, possibly, "negative results", as stressed above.

The usual and general answer to the need for autonomous support and commitment in fundamental investigations refers today to the impossibility to split fundamental and applied research, an old fashion distinction, many explain, as today the two frameworks for research are deeply entangled.

It is a fact that advanced applied and industrial research increasingly require a fundamental insight, given that the technological depth and the manifolded branching of the several applications directly raise fundamental questions. However, we argue that there should always exist, if we want further advances, a research area where the criteria for novelty should be the following:

"Is there a foreseeable application for this project? *No, not a single one!*"

This may give *some* chances to the theoretical originality of a proposal, a guarantee that it may produce *radically new applications* in the future: exactly the ones that we *cannot see* now.

We hinted above to some results whose actual meaning was, when they were proved: "No way to use this theory or results for an application in the intended frames (such as computing or constructively deciding, as required by the mainstream conjecture at the time)".

So, besides the major role that fundamental research may have when it is developed in direct connection to applied research, we must reserve an area where the criteria for financial or any other type of support is the exact opposite of the chances of resulting in a "foreseeable industrial product": if we want new technologies in the future, as *unexpected* as the ones that Computability Theory or Quantum Mechanics gave us, we would now need an original theoretizing, far removed from any expected applications. Better if they are grounded on several "noes", possibly based on "critical" insights. And this also for one more reason.

As a matter of fact, fundamental research must be largely based on critical or alternative insights into problems. As suggested by the case analysis above, the major advances were due to scientists who thought: "no, it doesn't work that way" (the way pursued by the majority, at the time). This critical attitude, when it is in the heads of extraordinary (and rare) scientific personalities, may open entirely new ways. But it may also provide an immediate, even industrial, fallout, in the more ordinary cases, as we shall argue.

A student in engineering, say, also attending courses by teachers who are devoted to fundamental research, may be guided towards the acquisition of a critical attitude: in principle, those teachers must have a scientific habit according to which challenging the established conceptual frameworks is the priority. Reversing or at least revising the foundation of some scientific domain is they key attitude for any reasonably good theoretician. Then, that student, when he/she will later work in an industrial environment, may have assimilated the possibility of a critical attitude from someone used to analyze or even "shake the foundation" of some way of thinking. He/she may have acquired the talent to think of a radically different solution or of an original approach also regarding technical problems. In short, the talent to "take a step to the side", look at the roots of a form of knowledge or even a specific applied problem, and to see from a distance, may develop on the grounds of a previous indirect training for facing fundamental problems. Thus, by means of teaching and research training, fundamental research may have an immediate impact on applications, by forming to "critical attitudes" in tackling also technical issues in an industrial context. It is not a coincidence that the creators of the personal computer (Apple) and of Google came out from leading Californian universities and were doctoral students of top theoreticians in Computer Science: they had learned to see things differently or globally, possibly removed from local technicalities (besides being able to solve technical problems, of course).

A research activity that entirely starts with a well established industrial objective, within an *accountable* project, as clearly explained in the European application forms (tools, methods and expected results must be clearly identified in the proposal – first year, second year, third year expected results… - so that, in the end, they can be compared to the actual achievements – will be "accountable"), excludes by principle (negative) results such as those which we mentioned above. Their novelty consisted exactly in inventing unexpected tools, new methods, in obtaining unforeseeable results. Of course, researchers must be accountable for the money they receive, but in fundamental work the "accounting" must be very flexible and based on (very) severe a priori judgments on the quality of the proponents and, a posteriori, of the results obtained, whatever they are. If we exclude this kind of research activity from support, the first fallout that will be immediately impacted is the development, by teaching researchers, of the innovative critical attitude, which is mostly specific to fundamental investigations and may indirectly lead to innovation also in industrial projects. It is basically wrong to impose that such a frontier project, as one involving human Cognition, Theoretical Biology, Mathematics and Computer Science, be excluded from allowing the search for novel theories, possibly disconnected from any chance of immediate industrial fallout, possibly a consequence of results that set limits to current theoretical tools and methods, possibly "negative results", thus, far removed from foreseeable "industrially-oriented applications".

In conclusion, recall also that Darwin's Evolution and Relativity Theory (but more examples could be given) were and are perfectly *useless* theories. An "historical" one, the first, incapable of prediction, by principle, an analysis of planets' and stars' dynamics the second (who cares?). These theories, in the following decades, radically changed the ways to analyze the living and the inert state of matter, respectively, with immense indirect fall-outs.

References:

It would be impossible to insert here the immense literature on the topics hinted. Some references may be found in the following papers (downloadable from http://www.di.ens.fr/users/longo/), which present some "negative results" in Cognition, Biology and Logic:

Giuseppe Longo. *Laplace, Turing and the "imitation game" impossible geometry: randomness, determinism and programs in Turing's test*. *In* Epstein, R., Roberts, G., & Beber, G. (Eds.). **The Turing Test Sourcebook.** Dordrecht, The Netherlands: Kluwer, 2007.

Giuseppe Longo and P.-E. Tendero. T*he differential method and the causal incompleteness of Programming Theory in Molecular Biology*. In **Foundations of Science**, 12:337–366, 2007.

Giuseppe Longo. *Interfaces de l'incomplétude*, pour **"Les Mathématiques"**, Editions du CNRS, 2011 (Originale in italiano *per* **"La Matematica"**, vol. 4, Einaudi, 2010).

Giuseppe Longo. *Incomputability in Physics and Biology.* Invited Lecture, Proceedings of **Computability in Europe**, Azores, Pt, June 30 - July 4, LNCS 6158, Springer, 2010 (complete version submitted to MSCS, special issue on *"Computability in Physics"*).

"Positive" proposals and more references may be found in:

Francis Bailly, Giuseppe Longo. **Mathematics and Natural Sciences : the Physical Singularity of Life,** 333 pages, Imperial College Press / World Sci., London, 2011. (Traduction et révision du livre pour Hermann, Paris, 2006.)