News from New Zealand

BY

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1 Scientific and Community News

The latest CDMTCS research reports are (http://www.cs.auckland.ac.nz/staff-cgi-bin/mjd/secondcgi.pl):

441. C.S. Calude, R. Freivalds and F. Stephan. Deterministic Frequency Pushdown Automata. 09/2013

442. C.S. Calude. Quantum Randomness: From Practice to Theory and Back. 09/2013

443. A.A. Abbott, C.S. Calude and K. Svozil. Value Indefiniteness Is Almost Everywhere. 09/2013

444. C.S. Calude, L. Staiger and F. Stephan. Finite State Incompressible Infinite Sequences. 11/2013

445. A. Nies. Calculus of Cost Functions. 11/2013

446 S. Figueira and A. Nies. Feasible Analysis, Randomness, and Base Invariance. 11/2013

447. K. Wei and M.J. Dinneen, Comparing Two Local Searches in a (1+1) Restart Memetic Algorithm on the Clique Problem. 12/2013

448. C.S. Calude and L. Staiger. Liouville Numbers, Borel Normality and Algorithmic Randomness. 12/2013

449. F. Ferrarotti, S. Hartmann and S. Link. Reasoning about Functional and Full Hierarchical Dependencies Over Partial Relations. 12/2013

450. J. Kontinen, S. Link and J. Väänänen. Independence in Database Relations. 12/2013

451. V.B. Tran Le, S. Link and F. Ferrarotti. Effective Recognition and Visualization of Semantic Requirements by Perfect SQL Samples. 12/2013

452. H. Köhler, U. Leck and S. Link. Possible and Certain SQL Keys. 12/2013

453. S. Böttcher, S. Link and L. Zhang. LECQTER: Learning Conjunctive SQL Queries Through Exemplars. 02/2014

454. S. Hartmann and S. Link. Normal Forms and Normalization for Probabilistic Databases under Sharp Constraints. 02/2014

455. A. Gavruskin, S. Jain, B. Khoussainov and F. Stephan. Graphs Realised by R.E. Equivalence Relations. 01/2014

456. A. Gavruskin, B. Khoussainov and F. Stephan. Reducibilities Among Equivalence Relations Induced by Recursively Enumerable Structures. 01/2014

457. S. Jain, B. Khoussainov, F. Stephan, D. Teng and S. Zou. Semiautomatic Structures. 02/2014

458. A. A. Abbott, C. S. Calude and K. Svozil. On the Unpredictability of Individual Quantum Measurement Outcomes. 03/2014

459. L. Staiger. On the Hausdorff Measure of Regular ω-languages in Cantor Space. 04/2014

460. M. Hannula, J. Kontinen and S. Link. On Independence Atoms and Keys. 04/2014

461. C.S. Calude and N. Poznanović. Free Will and Randomness. 05/2014

2 A Dialogue with Mioara Mugur-Schachter on Information, Quantum mechanics and Probabilities

Professor Mioara Mugur-Schachter, `http://www.mugur-schachter.net` *is a physicist, mathematician and philosopher specialising in quantum mechanics, probability theory, information theory and epistemology. Her PhD Thesis (supervised by Nobel laureate Louis de Broglie) contains the first invalidation of von Neumann's famous proof stating the impossibility of hidden parameters compatible with the quantum mechanical formalism. This result was included in the volume "Etude du caractère complet de la mécanique quantique", (with a Preface by L. de Broglie) published in the collection "Les grands problèmes des sciences", Gauthiers Villars, Paris, 1964, two years before Bell's invalidation.*

Professor Mugur-Schachter has founded the Laboratoire de Mécanique Quantique & Structures de l'Information at the University of Reims, the Centre pour la Synthèse d'une Épistéémologie Formalisée and L'Association pour le Développement de la Méthode de Conceptualisation Relativisée.

CC: You have been born and educated in Romania. Tell us about your time at the University of Bucharest: subjects you studied, professors, general atmosphere.

MM-S: I began by studying mathematics and philosophy (especially logic and psychology). Then I chose to specialise in theoretical physics. For political reasons my studies suffered an interruption that seemed to be fated to be irreversible. But later the events evolved and I finally was allowed to resume my studies. So I graduated with a Master in theoretical physics. My Professors, as I remember them, were very remarkable indeed. Profoundly educated persons, and many among them endowed with genuine originality. The teaching was very thorough. For me however—from a subjective point of view—my student years have been a deeply troubled time about which I prefer not to focus my attention again. The general atmosphere after 1948, as I perceived it, was constantly growing more and more oppressive from a moral point of view.

CC: Your PhD Thesis was elaborated in Bucharest and sent to Louis de Broglie before you came to Paris. How did you choose your subject? Did you have any supervision in Bucharest for this work?

MM-S: During a recent public visit in a town from the South of France, a young man asked how Louis de Broglie had recruited me? I answered that in fact it was me who tried—very hard indeed—to recruit Louis de Broglie.

When I graduated, my former Professor of Atomic Physics, Horia Hulubei (who was a pupil of Jean Perrin, in Paris, and after the war was called back to

Romania to create an Institute of Atomic Physics) obtained for me a position in the team of theoretical physics of the new Institute.

The subject of research assigned to me was to calculate, using the method established by van Vleck, the interaction between three spins using the framework of quantum mechanics. While covering with matrix elements meter-long sheets of paper intended for architectural projects, I constantly suffered from a very disagreeable feeling of not 'understanding' at all what I was calculating in the prescribed way. This was a new feeling. The Newtonian mechanics seemed to me fully intelligible, and also thermodynamics, atomic physics, statistical physics, and even Maxwell's electromagnetism. But in the case of quantum mechanics I simply did not grasp *how the mathematical formalism manages to carry definite meanings*.

In that state of mind, reading a textbook of quantum mechanics translated from Russian I found the assertion that a certain von Neumann had proved a famous theorem stating that 'hidden parameters' that would 'complete' the quantum mechanical formalism, making it intelligible, are impossible. The proof was not given. Immediately I reacted with a mixture of satisfaction and astonishment. I felt happy to learn that other persons also perceived the unintelligibility and they were investigating it. But I was unable to imagine how it could be possible to prove a definitive impossibility. Inside what conceptual-formal environment could such a proof be achieved? Founded upon what assumptions? So I became very eager to examine the proof. I had a friend who worked at the library of the Academy and I convinced him to order an English translation of the German book by von Neumann where the proof was first presented. The book eventually arrived, but its access was restricted to the library basement. Using a trick, I found von Neumann's book and took it home.

During the next months I became an expert in von Neumann's book. Meanwhile the calculus of matrix elements suffered a nearly total stagnation. At the end of the year I was downgraded for not having finished my assignment. On the other hand, I had written in English the first draft of what I thought to be an invalidation of von Neumann's proof.

I then began asking teachers and colleagues to read my work. But it appeared that nobody around was interested in von Neumann's proof. At the same time everybody was *a priori* convinced that it was a 'definitive' result. This was my first collision with the social environment of scientific thought.

Meanwhile I kept improving the text. And when I finally thought it to be achieved I asked Professor Hulubei to do me the enormous favour to send the manuscript by diplomatic courier (correspondence with the West was restricted at that time in Romania) to Louis de Broglie as I learned indirectly that he believed the theorem to be false. Professor Hulubei accepted, though assuring me that I would never receive an answer.

During the period that followed my husband (who was a Professor of resistance of materials at the Polytechnic Institute of Bucharest) decided that both of us give up our professional positions in order to apply for a passport for leaving the country without creating a dangerous and useless small scandal. We knew quite well how illusory was such an action, but we felt that we just had to try. So we coldly put an end to our Romanian 'careers' and left Bucharest to start a long period of uncertainty (it lasted three full years) during which, quasi incognito, we wandered through the country with temporary jobs here and there. Which, unexpectedly, we enjoyed profoundly.

One morning, while we were living on a boat anchored on a void island in the delta of the Danube, where my husband was in charge of the construction of an irrigation system for a rice field, I rather miraculously got a telegram from my parents informing that Professor Hulubei wanted to see me as soon as possible. I left a small note on the boat, traversed swamps in a tractor, caught a train to Bucharest, and at the end of that very day I stood before Professor Hulubei. He said: "Do you know what? Louis de Broglie answered you! And he agrees that you have invalidated von Neumann's proof!". He handed me a very brief letter addressed to 'Mister Misare Mugur-Schächter (I abandoned that precious letter in Romania, like any other hand-written document). In essence, Louis de Broglie's letter said that it was curious to see that two minds so different as his and mine, reached the same conclusion about von Neumann's proof. But since I had taken a logical approach and had genuinely demonstrated the circularity character of the proof, he would be happy if my work could one day become a PhD under his supervision.

From that moment on I nourished only one dream: to manage to arrive in France. In 1962 this dream became true following an unrealistically adventurous detective path to obtain a passport. And in 1964 my PhD Thesis, titled *Étude du caractère complet de la mécanique quantique*[1] was defended at the University of Paris and published by Gauthier Villars in the collection "Les grands problèmes des sciences", in a volume prefaced by Louis de Broglie (`http://www.mugur-schachter.net`). The first part of the volume contains a French version of my initial invalidation (practically unchanged); the second part contains the proposal of an experiment derived from considerations on the quantum theory of measurement and from de Broglie's reinterpretation of quantum mechanics (the experiment has not been realised, but it might be some day).

CC: You arrived in Paris in 1962. Can you reminiscence about your first encounters with Louis de Broglie, the 7th duke de Broglie?

MM-S: As if it were yesterday. We were towards the end of April. I immediately announced my arrival and obtained a "rendez-vous". I was now waiting

[1] *Study of Completness of Quantum Mechanics.*

seated in the hall of the Academy of Science. An usher came and presented a silver tray asking me to put my visit card. I had no card, so I wrote my name on a piece of paper. And a little later Louis de Broglie himself arrived. He greeted me and invited me to follow him.

I shall never forget the instantaneous transition from the ocean of vague and moving inner images that had so long subsisted in my mind regarding the possible scene of my first meeting with Louis de Broglie, to that unique real scene, so radically definite in every detail, that was uncoiling with apodictic evidence. An upright, infinitely distinguished man, in a dark costume and a shirt with broken collar, was there, in front of me, confirming that he accepted me to become his "last student". He was Louis de Broglie, and I was in Paris, France, seated in an office from the Academy of Science.

During the two subsequent years we met practically every Wednesday to discuss a fragment of my work that I had left in his letterbox from Neuilly-sur-Seine, at least two days in advance. He never forgot and never postponed something that he had announced he would do. He never argued with an idea or a way of expressing something. He just stated his opinion. He also meticulously corrected my French. And very discreetly, he constantly helped me in essential ways to settle myself in France. His attitude influenced me profoundly.

CC: What was wrong with von Neumann's proof?

MM-S: It simply was circular. The hypotheses contained the conclusion. The conclusion of 'definitive' (absolute) impossibility of hidden parameters was in fact derived inside the mathematical formulation of quantum mechanics, namely using the particular way of representing probabilities that is specific to Hilbert spaces, not of micro-states. (If micro-states are represented by another mathematical syntax, different from that of Hilbert spaces—as it is indeed the case for the de Broglie-Bohm representation—then the proof ceases to hold.)

But this is not the unique insufficiency of von Neumann's argument. In my Thesis I have brought forth the unacceptable global structure of von Neumann's argument. The inadequacies of this argument overflow abundantly the strictly logical-formal aspects. They leak out into epistemology, method, and usual language. This 'proof' can be regarded as a striking illustration of the extreme difficulty to achieve a wholly and explicitly dominated mathematical representation of a domain of 'physical facts'. Such a representation involves quite essentially operations of various sorts, physical as well as abstract ones; it involves assumptions of various nature, in particular methodological choices and conventions; it involves *aims* of different natures, the aim to know in a precise way, of course, but also other aims that should be all composed under the constraint of a sort of global coherence. What thus comes out is a need of a sort of coherence that cannot be separated from a feeling of beauty, or on the contrary, of ugliness when certain

slopes of it are violated in some unspeakable way. I had tried as much as I was able to bring all these aspects together into one representation and to extract the essence of the whole. But I was very young, and this was my first research.

CC: You have also challenged Wigner's proof on the impossibility of a joint probability of position and momentum compatible with the formalism of quantum mechanics. Is the theorem false as well?

MM-S: I would not say that it is 'false'. I only showed that the asserted conclusion does not follow. I even identified a trivial counterexample and I showed how this counterexample is allowed to arise inside Wigner's construction. As in my experience with von Neumann's proof, as soon as I succeeded to achieve a sufficiently compact variant of this second critical work (which took more than two years and a long preliminary publication) I sent it to Wigner himself. Wigner invited me to visit him in his wooden house in Vermont, for a direct discussion. So I went there. He recommended the work for publication in the *Foundations of Physics*.

CC: What is the "opacity functional of a statistic" and how did you use it for a mathematical unification between the theory of probabilities and Shannon's theory of communication of information?

MM-S: This has been my first constructive work. It is the result of an attempt at explaining why Boltzmann's statistical distribution tied with the Carnot-Clausius definition of physical 'entropy', possesses a mathematical form that is identical with that of Shannon's purely mathematical concept of 'informational entropy'. My motivation came from the seemingly unconceivable fact that this formal identity between two concepts, that are so radically different in their semantic contents, is just a coincidence.

The central idea of the approach has been to construct—inside a Kolmogorov probability space—a pure mathematical definition of the probability of realisation of a given statistical distribution of the elementary events of the space.

Consider a Kolmogorov space that contains a universe of elementary events and a probability law on it. Consider a very long but finite random sequence of elementary events from this universe. The elementary events emerge inside this sequence in a certain order, and each elementary event possesses a certain relative frequency inside the sequence, which defines a certain 'statistical structure' of the sequence. It is obvious that: a) a given statistical structure can arise for various lengths of the sequence, b) for a fixed length, not any statistical structure is possible.

Two questions can be examined. The first one is: What is the expression of the probability for the realisation of a sequence with a given statistical structure, abstraction being made of the order and length? We have proved that the Kolmogorov expression of the limit of the ratio between the probability of the

sequence considered and the length of the sequence, is equal to the difference between two terms, the Shannon-entropy of the probability law from the considered Kolmogorov probability space, and 'the modulation of the probability law by the fixed statistical structure'. I called this difference the *opacity* of the (fixed) statistical structure of the sequence of elementary events with respect to the probability law of the Kolmogorov probability space.

The second question is: How does this probability evolve when the length of the considered sequence tends to inifity? The answer is: If the length of the sequence of elementary events tends to infinity, then the opacity functional satisfies the weak law of large numbers.

The opacity functional realises an abstract unification between the probabilistic and the informational approaches. This unification permits to construct deductively inside the theory of probability, the identity of form between, on the one hand, the concept of physical statistical entropy introduced by Carnot, Clausius and Boltzmann, and on the other hand, Shannon's concept of informational entropy of the probability law assigned to the signs from an alphabet of an information-source regarded as elementary events. The formal identity can now be clearly distinguished from the semantic specificities (physical, informational), while the relations between formalism and semantics are clearly defined in each case.

CC: Your work on "formalised epistemology" was characterised by Jean-Paul Baquiast, editor of "Automates intelligents", as *a revolution in the way of representing the processes by which we acquire knowledge....* Can you describe your method of "relativized conceptualisation"?

MM-S: The method of relativized conceptualisation (MRC) is similar to a grammar or 'a formal logic', that give syntactic rules for making use of a set of signs. But instead of dealing with this or that 'language' or symbolic way of constructing 'rational truths' (conclusions established deductively), MRC concerns the whole of human processes of conceptualisation: it is a general syntax for normalised creation of consensual knowledge. I say 'normalised' in the sense of 'methodologies': indeed, like any method, MRC is organically tied with aims, and MRC major aim is expressed in the following: The system of norms organised by MRC assures the realisation of 'safe scientific knowledge', that is, of communicable and consensual knowledge where any possibility of emergence of false problems or of paradoxes is excluded by construction.

MRC establishes a bridge from my initial investigations—exclusively critical and achieved with reference to norms that worked only implicitly and were devoid of generality—to a quite general and explicitly organised methodological framework.

Let me detail a little more. Any piece of knowledge that can be communi-

cated without resource restrictions (space or/and time) is a 'description' (pointing toward something restricts to co-presence on a same place at the same time, so it does not give a 'description'). What is not 'described' cannot be communicated in unrestricted ways, even if it is known by someone. So, MRC is a method of scientific and safe description.

MRC is constructed in a deductive way and uses the current natural logic. It involves 1 postulate, 3 principles, 1 convention, 22 main definitions and 6 proved "propositions". That is all.

A 'description' consists of some 'qualification'—in a certain generalised adjectival sense—of some 'entity-to-be-qualified'. According to MRC-norms, any description has to be realised within a previously defined 'epistemic referential (G, V) which consists of an explicitly defined operation of generation G of the object-entity oe_G to be 'qualified' ('described'), and a concept denoted V that is called a view which consists of a structure thatrealises precisely the desired sort of qualification.

The operation of generation G can consist of just selecting a pre-existing entity and assigning it the role of object oe_G for future qualifications; but G can also be a radically creative operation (as it happens indeed for a free micro-state to be studied according to quantum mechanics).

On the basis of very careful analyses, it appears that in order to avoid any arbitrary a priori restriction it is unavoidable to posit—even if a posteriori this posit is modified—that the object-entity oe_G stays in a one-to-one relation with its operation of generation G (this is expressed by the index G from the denotation oe_G). This a priori posit constitutes inside MRC an essential methodological decision.

The basic nature of V is analogous to that of a grammatical predicate. But its structure is far more complex, precise and general. A view V consists of a finite union $V = \bigcup_g V_g$ of aspect-views V_g. Each aspect-view V_g introduces a freely chosen 'semantic dimension g' (for instance the trivial one indicated by the word 'colour', but also any other more unusual or sophisticated one) endowed with a finite set of 'values' denoted g_k, where g is fixed and k varies in a finite set (for instance, for the semantic dimension of 'colour', one could place just green, red and yellow, or these and also other 15 colours, etc.). An aspect-view V_g is 'blind' with respect to the semantic dimensions different from its own, as well as with respect to any value g_k with which it has not been endowed by its definition: it is a filter. Moreover, each aspect-view V_g states explicitly (a) what conceptual-physical operations constitute an act of 'examination by V_g'; (b) what is the observable result of a given act of examination, and (c) how this result is translated into a value g_k of V_g (when oe_G is not directly perceivable, this requirement is highly non-trivial).

For the sake of effectiveness in the sense of computability, MRC operates with operationally specified entities using finite constructions.

The relativised genesis of any MRC description induces a definite global struc-

ture for the whole evolving volume. This structure possesses the character of a network of chains of increasing complexity, subject to explicit rules of mutual connection. Each relative description from this network reproduces the same basic epistemological structure $D = \{(g_k)\}$.

In the framework of MRC classical concepts and theories get a new form.

- The classical logic corresponds to a 'genetic relativized logic' that entails a calculus with relative descriptions.
- Classical probabilities correspond to relativised genetic probabilities.
- Genetic logic and genetic probabilities become essentially unified.
- Shannon's theory of communication of information, which by construction does not talk about the meaning of information, becomes relativised when it is embedded into the relativized theory of probabilities; some meaning can emerge.
- The MRC 'complexity' can be expressed by a set of relativised numerical 'measures' established by measurements.
- The concept of time acquires an explicit bi-dimensional representation.

New applications of MRC are developed. For example, a relativized concept of 'system' was constructed in H. Boulouet's Ph.D. Thesis *Relativized Systems Theory* to be submitted to the University of Valencienne (2014).

Furthermore, all classical disciplines are constructed and presented as if the descriptions 'mirror' things and facts that pre-exist quite independently of the model (even Wittgenstein's extraordinary analyses do not clearly challenge this conception). In contrast, MRC is explicitly founded on transferred descriptions. I dare assert that MRC is the first scientific general method of deliberate human conceptualisation.

CC: In which way did you recently collaborate with Giuseppe Longo, an expert in computability theory and discrete mathematics, areas seemingly far away from your main interests? Is this an indication that quantum physics might benefit from an interaction with these areas?

MM-S: I think so. For historical reasons, the beginnings of quantum mechanics have been marked by contributions expressed in terms of continuous mathematics; but also of contributions expressed in algebraic terms. I believe that in the future a discrete and finite, algebraic approach will predominate.

And I think the same is true for probabilities. (The opacity functional can be relativised and discretised.)

Anyhow, MRC is quite essentially finite, so discrete, by construction. With MRC I solved, I think, a major (though rarely discussed) difficulty of the classical probabilistic conceptualisation (see my paper "On the concept of probability", *Mathematical Structures in Computer Science*, special issue on "Randomness, Statistics and Probability", 2014 (in press)). Namely, the lack of a general procedure for constructing the numerical distribution of probability to be used in a factual situation that is generally considered to be probabilistic. I called this difficulty "Kolmogorov's aporia" because starting from 1983 Kolmogorov himself denounced this startling and scandalous situation. For example, in the paper "Combinatorial foundations of information theory and the calculus of probabilities", *Russia Mathematical Surveys*, 38 (1983) 29–40, Kolmogorov says:

> The applications of probability theory can be put on a uniform basis. It is always a matter of consequences of hypotheses about the impossibility of reducing in a way or another the complexity of the descriptions of the objects in question. Naturally this approach to the matter does not prevent the development of probability theory as a branch of mathematics being a special case of general measure theory.

The MRC solution to Kolmogorov's aporia consists of an explicit finite procedure for constructing, in a given factual probabilistic situation, the corresponding finite distribution of a numerically defined law of probability. Furthermore, an equation has been worked out, that expresses the formal consistency between the finite data that characterise the above-mentioned procedure and the mathematical theorem of large numbers.

Professor Longo was aware of this work and I think that he has understood its social difficulties. I must mention that the same special issue contains a very interesting discussion of probability from a historical perspective, C. Porter, "Kolmogorov on the role of randomness in probability theory", of which I was unaware while developing my work. In this way I learned that quite a number of mathematicians are well aware of what I have called Kolmogorov's aporia, but they called it long before "the applicability problem", clearly a better name.

Mathematicians seem to believe that the applicability problem can be solved by purely mathematical means, while I believe that this is fundamentally impossible. I believe that the semantic content cannot be reduced to pure syntax, nor entirely "mimed' by it (in the sense in which a mould can 'mime' a face).

The special issue referred above contains also a brief debate between several outstanding contributors about the ways of connecting factual data with mathematical syntax. This debate brings into evidence that the applicability problem—even though Kolmogorov himself considered it so essential—not only is surprisingly little known, but, even when it is raised in quite explicit and insistent terms, it captures very little attention.

I believe that this state of facts deserves closer examination. Human intuition is magic. Nevertheless, the introduction of explicit principles and rules for matching a given semantic content and an assigned syntactic expression, could be very fertile, acting like a vehicle for rapid and precise understanding and consensus. People have lived before Aristotle's syllogistic, but its creation has avoided heaps of sophisms in heaps of lost time and effort. MRC offers a framework for matching safely semantic contents and syntactic structures.

CC: Your last book *Principles of a 2nd Quantum Mechanics* (arXiv:1310.1728, in French) presents yet another quantum mechanical formalism. What is wrong with the "1st quantum mechanics"?

MM-S: It is simply devoid of a theory of measurement acceptable from a formal as well as from a conceptual point of view, with general factual validity.

The von Neumann-Hilbert theory of measurement is, both, fallacious and devoid of general validity. As long as one is confined inside the formalism itself it is very difficult to fully perceive this. (Personally, I am startled to discover what an incredibly long time I needed in order to acquire what I now believe to be a clear and coherent view on the global structure of the quantum mechanical formalism.)

The problem of 'interiority', i.e. of ways of transgressing the limitations inside which one is yourself imprisoned, is a very difficult problem indeed. If the imprisonment is absolute, this problem is radically devoid of solution. This may seem trivial, but many fine authors act as if they were unaware of it, in particular, all those who make assertions concerning the entire Universe. Wittgenstein stressed this epistemological fact in various contexts. He repeated that in order to be able to think of a 'whole' one has to be able to be inside as well as outside of that 'whole'. To which he added his well-known injunction: *Whereof one cannot speak, thereof one must be silent.*

Now, what happens when one wants to size up globally, as well as in its details, the structure of the quantum mechanical representation of micro-states? The imprisonment inside this representation, of course, is not absolute. One can place oneself outside it. But what is available outside, on which one can place the feet of one's mind? There is the classical physics and the whole classical thinking, with its "objects", its space-time and causal structures. But everybody says that quantum mechanics violates all this and nevertheless—marvellously—'is working'. An organised formalism (outside of the quantum mechanical one) permitting to perceive consensually expressible specificities, or necessities, or impossibilities, does not exist.

And this is quite understandable. Indeed, quantum mechanics is the very first physical theory that introduces—-implicitly—what I have called 'transferred descriptions' of the physical entities. And, as I have already stressed, the whole organised thinking that is exterior to quantum mechanics ignores the concept of

primordial transferred descriptions. So with respect to this concept there cannot exist an organised outside.

As long as these conditions persist nothing can be asserted on the formalism of quantum mechanics in terms endowed with a precise meaning and with a character of objectivity. This, as a fact, is manifest since tenths of years. What is cruelly lacking is an organised structure of reference, different from quantum mechanics itself, but constructed in a way that permits to be clearly related with quantum mechanics, that admits a controlled comparison with quantum mechanics, in the details as well as globally.

So I constructed such an organised structure of reference. I maintained invariant that what is represented inside quantum mechanics, namely states of micro-systems, 'micro-states', but I constructed another representation involving them. Quite independently of quantum mechanics, I brought into evidence just the necessary and sufficient conditions for constructing a communicable and consensual representation of micro-states, but nothing more.In this way an epistemological-operational-methodological representation of the geneses of human very first pieces of knowledge on micro-states is obtained. I called this infra-(quantum mechanics) to be understood as 'beneath the formalism of quantum-mechanics'.

By systematic reference to infra-(quantum mechanics), the formalism of quantum mechanics reveals unexpected deficiencies. Here are three of them:

- It does not distinguish clearly between the individual level of conceptualisation, and the statistical one. In fact it almost entirely occults the individual level.

- It does not represent at all, neither mathematically nor informally, the way in which a describable micro-state is generated. The process of generation of a physical and individual micro-state is confused with something radically different, namely the process of 'preparation for measurement of the mathematical state vector' that represents the statistics of results of measurement obtained with numerous replicas of the physical micro-state that is involved.

- Quantum mechanics lacks a generally valid theory of measurement.

I have sketched a 2nd quantum mechanics where the deficiencies enumerated above (and some others) have disappeared. This new representation—not a re-interpretation—introduces measurement operations based on the de Broglie-Bohm guidance relation, but assumed to be an observable process, not an only conceived process. And whether the process is indeed observable, or not,... can be observed.

CC: Are you preparing an English version?

MM-S: I have already notably improved the French version and I shall soon update it on arXiv of quantum physics. As for the English version, it will be available before the end of July, I hope. Meanwhile I shall try to publish somewhere an extended abstract in English.

CC: Do you believe in the possibility of a grand unification between quantum mechanics and relativity?

MM-S: One can postulate that *if* one could directly observe micro-systems via signals travelling with a universally invariant velocity, then we would construct descriptions of them that would obey Einstein's theories. There is a very strong tendency to extrapolate into absolute generality an approach that has produced remarkable successes in some given domain.

But—personally—I do not see any reason why that postulate should be particularly fertile. I do not believe that what is called a "grand unification" is the best choice of an aim of today's Physics. I believe that the unique sort of a genuinely fertile unification of scientific rationality—in its entirety—can only be of a purely methodological nature. The contents should be left free of a priori constraints. They should emerge explicitly from all the specific conditions that are brought into play, so marked by unlimited diversity.

CC: As a researcher you had good moments and bad moments . Can you recall one of them?

MM-S: By far the best moments that I have had as a researcher—-and not very seldom—have been those that have emerged unexpectedly, when without any expressible specific cause I have suddenly felt a sort of inner certitude to have finally "understood" something that before, and for a long time, had stubbornly resisted my understanding.

CC: Many thanks.

MM-S: The thanks, indeed, are from my part.