INTERVIEW

This interview is scheduled for the June-2014 issue of the European Association of Theoretical Computer Science.
It will be reproduced in a volume titled *Conversations with Famous Scientists* to be published by Imperial College Press, London.

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is a physicist, mathematician and epistemologist specialised in the foundations of quantum mechanics, probability theory, and information theory. 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 probl√®mes des sciences", Gauthiers Villars, Paris, 1964, two years before Bell's invalidation.
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\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 specialize in theoretical physics. For political reasons my studies have suffered an interruption that seemed to be fated to keep irreversible. But later the events evolved and I finally was allowed to resume my studies. So I graduated by a master in theoretical physics.
The 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 on which I prefer not to focus attention again.
As for the general atmosphere, after 1948 I perceived it as 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 invited 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 have tried – very hard indeed – to recruit Louis de Broglie.
When I graduated, my former Professor of Atomic Physics, Horia Hulubei (who had been a pupil of Jean Perrin, in Paris, and then after the war had been called back to Romania

in order to create an Institute of Atomic Physics) obtained my inclusion in the team of theoretical physics of the new Institute.

The subject of research assigned to me was to calculate the interaction between three spins inside the framework of quantum mechanics and accordingly to the method established by van Vleck. 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 why I was calculating in precisely the prescribed way. This was a new feeling. The Newtonian mechanics had 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 this state of mind, I found in a textbook of quantum mechanics translated from Russian the assertion that a certain von Neumann had *proved* by a famous theorem that 'hidden parameters' that would 'complete' the quantum mechanical formalism making it intelligible, are *impossible*. The proof was not given. But immediately I reacted by a mixture of satisfaction and astonishment. I felt happy to learn that other persons also perceived non-intelligibility and were investigating upon that. But I was unable to imagine *how* it could be possible to ***prove a definitive im****possibility*. 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 had been first exposed. When the book arrived it was made accessible only in the basement. I inscribed myself for consulting a Russian review that was equally available only in the basement and, once there, I found von Neumann's book and I went back home with it.

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 the treatment of my subject. But 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 me 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 features oh scientific thought.

Meanwhile I kept improving the text. And when I finally thought it to be achieved I asked an audience from Professor Hulubei and I asked him to do me the enormous favour to send the manuscript by diplomatic pouch to Louis de Broglie of whom I had learned indirectly that he believed the theorem to be erroneous. Professor Hulubei accepted, though insuring me that I certainly would never receive an answer.

During the period that followed my husband (who was a Professor of resistance of materials at the Polytechnic from Bucharest) decided that both of us had to abdicate our professional positions in order to demand a passport for leaving the country without creating a dangerous and useless small scandal. We knew quite well how very illusory was the success of such a demand. But we felt that we just *had* to try. So we coldly closed our Romanian 'careers' and we left Bucharest to start a long period of uncertainty (it lasted three full years) during which, quasi incognito, we wandered through the country assuming 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 had taken in charge to construct an irrigation system for a rice-field, I was rather miraculously reached there by a telegram from my parents telling that Professor Hulubei wanted to see me as rapidly as possible. I left a small note on the boat, I

traversed swamps in a tractor, I caught a train, I arrived in Bucharest, and at the end of that very day I stood before Professor Hulubei. He said: « Do you know what? Louis de Broglie answers 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, assert the same conclusion about von Neumann's proof. But since I had taken a logical approach and had genuinely demonstrated the vicious character of the proof, he would be happy if this work could one day become a PhD under his direction.

From that moment on I nourished only one dream: to manage to arrive in France. In 1962 this dream became truth in consequence of an unrealistically adventurous detective research on the ways of obtaining a passport. And in 1964 my PhD Thesis, titled *Étude du caractère complet de la mécanique quantique* was sustained 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 contained a French version of my initial invalidation (practically unchanged); while the second part contained 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 realized, but it might be some day).

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

\MM-S

As if it were yesterday. We were toward the end of April and my first son has been born in Paris at the end of June. I had immediately announced my arrival and I had immediately obtained a *rendez-vous*. I was now waiting seated in the hall of the Academy of Science. An usher came and presented a silver tray asking me to depose 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 passage from the ocean of vague and moving inner images that had so long subsisted in my mind concerning 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 before. He never forgot and never postponed something that he had announced he would do. He never argued on 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 insert 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 ***by use of the particular way of***

representing probabilities that is specific of Hilbert spaces, not of microstates. (If the microstates 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 inacceptable *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 natures, 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 among requirements of different natures. What thus comes in is a sort of coherence that cannot be separated from a feeling of beauty, or on the contrary of ugliness when certain slopes of this global coherence 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, his 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 have shown that the asserted conclusion *does not follow*. I have even identified a trivial counterexample and I have shown how this counterexample is allowed to arise inside Wigner's structuration.

In consequence of my former experience with von Neumann's proof, as soon as I succeeded to achieve a sufficiently dense 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. Wigner himself proposed 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, his 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 the *physical* 'entropy', possesses a mathematical form that is identical with that of Shannon's purely mathematical concept of 'informational entropy', i.e. of 'entropy *of the* ***probability law*** of an alphabet of signs for transmitting messages. For it seemed to me to be a priori unconceivable that this formal identity between two concepts that are so radically different by their semantic contents, be just a coincidence.

The central idea of the approach has been to construct – inside a Kolmogorov probability space – a purely mathematical definition of: *[the probability of realization of a given statistical distribution of the elementary events from the universe of elementary events*

from the space, inside an arbitrarily long sequence of elementary events] and to try to draw therefrom some understanding of the question formulated above.

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 with 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:

- One statistical structure can arise for various lengths assigned to the sequence.
- For each given length of the sequence, a given statistical structure can arise for a whole set of mutually distinct orders.
- For a fixed length, not any statistical structure is possible.

So the following two questions can be examined:

Q1. What is the expression of the meta-probability for the realization of a sequence with a given statistical structure, abstraction being made of the order and no matter how long it is?

Q2. How does this meta-probability evolve when the length of the considered sequence is increased freely, i.e. both the order of the elementary events and their statistical structure being *not* fixed?

The questions *Q1* and *Q2* have been answered, respectively, *via* two theorems, *Th.1* and *Th.2*.

Th.1 establishes that: The Kolmogorov expression of the limit of the ratio between **[**the meta-probability of the sequence considered in the *Q1***]** and **[**the length of the sequence**]** when this length increases toward infinity, is equal to the difference between two terms, of which *the first one is the Shannon-entropy of the probability law from the considered Kolmogorov probability space*, and the second term consists of another entropic form that I called *'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 considered in Q1, with respect to the probability law from the considered Kolmogorov probability space'*.

Th.2 establishes that when the length of the considered sequence of elementary events tends freely toward infinity as supposed in *Q2*, the opacity functional ***entails*** the weak law of large numbers.

The opacity functional possesses the following specificities:

- If the the statistical structure of a sequence of elementary events is chosen such as to reproduce the acting probability law, then the modulation identifies 'statically' with the Shannon entropy of the probability law, and so the opacity functional becomes nul. If this is not done, then the opacity functional has a definite value that defines an immediately calculable numerical measure of the 'distance' between the statistical structure of any given sequence of elementary events from a Kolmogorov probability space, and the probability law from that space.
- When the length of the considered sequence of elementary events tends 'freely' toward infinity as it is supposed in *Th.2*, then the statistical structure of the sequence of elementary events identifies progressively with the probability law from the considered Kolmogorov space, and again, the modulation identifies with the Shannon entropy of the probability law and the opacity functional becomes nul.

So, via its conditions of annulation, the opacity functional acts in two distinct ways as a mathematical 'selector' of the expression of the 'informational entropy' of the probability law assigned by Shannon to the signs a_i from a source-alphabet A, regarded as elementary events from a Kolmogorov probability space.

Consider now that :

- Shannon has introduced his concept of *informational entropy* isolately, by a direct posit, quite independently of any definition constructed deductively inside the theory of probabilities.

- Shannon did not work out a general 'probabilistic' distance between : [a statistical structure that emerges inside a random sequence of elementary events] while some probability law is supposed to act, and on the other hand, [that probability law] (he has made use of other 'distances', posited for other descriptional elements and for more local aims).

In this perspective the results *(Th.1)* and *(Th.2)* appear as elucidating, in the following sense:

The opacity functional absorbs Shannon's main mathematical treatments, into Kolmogorov's theory of probabilities, thus accomplishing an abstract unification between the probabilistic and the informational approaches. This unification permits to *construct* deductively inside the theory of probabilities, 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 mentioned formal identity can now be clearly distinguished from the semantic specificities (physical, informational), while the relations between formalism and semantic are clearly defined in each case.

So the problem raised initially is entirely solved.

\CC Your work on "formalized 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...¬ª. Can you describe your method of <<relativized conceptualisation>>?

\MM-S

It is a method of relativized conceptualization (*MRC*).

It has a nature similar to that of a grammar, or of 'a formal logic', that give syntactic rules for making use of a set of signs. But instead of concerning this or that 'language' or this or that symbolic way of constructing 'rational truths' (conclusions established deductively), *MRC* concerns the whole of the human processes of conceptualization: it is a general syntax for normalized creation of consensual knowledge. I say 'normalized' in the sense of 'methodologized': indeed, like any method, *MRC* is organically tied with aims. And the major specific aim is expressed in the following assertion:

> The system of norms organized by *MRC* insures the realization 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 that, from my initial investigations – exclusively critical and achieved by referrence to norms that worked only implicitly and were devoid of a general organization – leads to my present research, decidedly constructive and developed accordingly to a quite general and explicitly organized methodological framework.

Lert me now detail a little more.

Any piece of knowledge that can be communicated without restriction of space or/and time is 'description' (while pointing toward something restricts to copresence on a same place at the same time, and it is not 'description'). What is not 'described' cannot be communicated in unrestricted ways, even if it is known by someone. So it can also be said that *MRC* is a method of describing scientifically and safely.

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

According to current language a 'description' consists of some 'qualification' – in a certain generalized *adjectival* sense – of some 'entity-to-be-qualified'.

Accordingly to the *MRC*-norms any description has to be realized inside a previously defined 'epistemic referential' *(G,V)* that consists of an explicitly defined *operation of generation G* of the object-entity $œ_G$ to be 'qualified' ('described'), and a concept denoted *V* that is called a *view* and consists of a structure that, in each case, can be conceived so as to realize precisely the desired sort of qualification.

The operation of generation *G* can consist of just *selecting* a pre-existing entity and to assign to it the role of object $œ_G$ for future qualifications; but *G* can also be a *radically* creative operation (as it happens indeed for a free microstate to be studied accordingly to quantum mechanics); etc.

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 $œ_G$ stays in a one-to-one relation with its operation of generation *G* (this is expressed by the index *G* from the denotation $œ_G$). This *a priori posit* constitutes inside *MRC* an essential 'methodological decision'.

As for a view *V*, its basic nature is analogous to that of a grammatical predicate. But its structure is far more complex, precise and general. A view *V* consists by definition of a finite union $V=\cup_g V_g$ 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 *gk* where *g* is fixed and *k* varies inside a finite set (for instance, on the semantic dimension of 'colour', one could place just green, red and yellow, or these and also other 15 colors, etc.). An aspect-view V_g is 'blind' with respect to the semantic dimensions different from its own one, as well as with respect to any value *gk* 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 do constitute an act of 'examination by V_g'; *(b)* of what the observable result of any given act of examination does consist ; and (c) how this result is translated into a value *gk* of V_g (when $œ_G$ is not directly perceivable, this requirement is highly non-trivial).

For the sake of effectiveness in the sense of computer science, everything inside *MRC* is specified operationally and is finite by construction.

When the constructed view *V* from a given epistemic referential *(G,V)* acts accordingly to the definitions of the aspect-views contained in it, upon the entity-to-be-qualified $œ_G$ generated by the operation of generation *G*, there emerges inside the epistemic referential *(G,V)*, a *relative description of* $œ_G$ denoted $D/G,œ_G,V/$. The description *D itself* consists exclusively of a set $D\equiv\{(gk)\}$ of *(gk)-values* each one of which belongs to a semantic dimension *g* involved by the definition of the view $V=\cup_g V_g$ that has been made use of. This set $D\equiv\{(gk)\}$ of *(gk)-values* is represented inside a conveniently constructed *representation-space of V*. But – via its genesis – the obtained description $D\equiv\{(gk)\}$ emerges essentially relative to the triad $/G,œ_G,V/$ that worked to construct it. So, in order to constantly remind of the essential role played by the triad $/G,œ_G,V/$ throughout the emergence of $D\equiv\{(gk)\}$, this triad is incorporated to the current denotation $D/G,œ_G,V/$ chosen for *D*. For maximal clarity one can also write $D/G,œ_G,V/\equiv\{(gk)\}$.

Just like in a grammar or in a logical or a mathematical structure, *'G'*, *'$œ_G$'*, *'V'*, *'D'*, are ***roles*** that are *not* indelibly tied to the entity or action that realizes that role inside this or that particular global descriptional action: an operation of generation from a previously achieved description, or that description itself as a whole, can be treated inside another

descriptional action as the entity-to-be-qualified, etc.

The relativized genesis of any *MRC*-description induces a quite definite global structure into the whole evolving volume of the conceptualized. This structure possesses the character of *a network of chains of increasingly complex cells of relativized description* subjected to explicit rules of mutual connection. Each relative description from this network reproduces, with a sort of fractal character, the same basic epistemological structure $D/G,\text{œ}_G,V$.

Here and there two or more chains meet in a descriptional meeting point consisting of a common relativized description.

And each chain of *MRC*-descriptions, when followed down to its very first sources, appears to be rooted into the a-conceptual physical reality via some set of 'primordial transferred descriptions' where the operation *G* extracts – directly from the a-conceptual physical reality – a fragment denoted œ_G that is still entirely *unknown in its singularity*. It is only labeled a priori by a word, a name of a whole category (a 'microstate', a 'sample of rock' from Mars, a 'sample of tissue' for a biopsy, etc.), in order to permit to communicate on it. The acts of examination defined in a primordial view *V* from a primordial transferred description, are called *measurement-interactions*. These produce for the fragment of reality œ_G generated by *G* the very *first* 'qualifications' of the sort that is desired. These consist of just a set of observable marks 'transferred' by the measurement interaction upon registering devices of the examining-apparatuses.

Now, this set of marks – by itself – *carries no meaning* (as do the marks produced by light on the retina of a human observer who calls them, say, 'red'), it is strictly devoid of any 'qualia' that for human mind would signify something connected with the entity œ_G that has to be qualified. So these marks must be translated into some meaning tied with $\boldsymbol{\text{œ}_G}$ and with the view *V* that acted in the measurement-interaction. These marks have to be somehow expressed in terms of values *(gk)* of the qualifying dimension *g* involved by the measurement-interaction with œ_G, that has produced the marks. If not, there is no 'primordial step of conceptualization'. Indeed the structure and the denomination of the view *V* (for instance a 'momentum'-(aspect-view)), together with the definition of the operation of generation *G* and the denomination chosen a priori for the corresponding entity-to-be-qualified œ_G ("microstate", etc.), constitute only a local abstract device of pre-conceptualized descriptional instruments that compose a sort of conceptual 'elevator' for hoisting the unknown fragment of physical reality labeled *'œ_G'*, into the volume of the already conceptualized before. They are a conceptual investment for transforming the unknown fragment of physical reality labeled *'œ_G'*, into a very first, a *primordial description*, a minimal element from the volume of the conceptualized. But this sort of conceptual elevator yielded by the epistemic referential *(G,V)* does not qualify *'œ_G'*. Only the registered marks qualify *'œ_G'*, iff they have been translated in terms of a value *gk* of an aspect-view V_g from V.

> But converting observable marks, into a pre-defined sort of qualification researched for *'œ_G'*, is a highly non-trivial process. And if no adequate construction of such qualification (meaning) can be defined for the registered sets of transferred marks then no description does arise, no knowledge whatever is generated.

Let us note immediately that in the case of the descriptions of microstates, the construction, for the observable registered marks, of meaning in terms of *mechanical* qualifications, is a quite crucial problem. A theory of the microstates that does not solve explicitly this problem is not a theory of the microstates.

All the primordial transferred descriptions that are available at a given time constitute together, at that time, the primordial stratum of the human conceptualization. This primordial

stratum has an evolving content, but it is endowed with stable epistemological peculiarities. And these stable epistemological peculiarities entail a permanent universal *cut* between the primordial stratum of conceptualization, and a second, subsequently constructed stratum, where each primordial transferred description is drawn into a process of progressive 'modeling' that generates an increasingly complex and unbounded volume of conceptualized. Inside this second stratum are achieved all the models that are regarded as "objects" in the classical sense. This general *MRC*-'cut' includes in particular the famous 'quantum-classical' cut.

Here I stop detailing, to just assert some results and some major general features.

Inside the framework of *MRC* :

- The classical logic moves into a 'genetic relativized logic' *that entails a calculus with relative descriptions*.
- The classical probabilities move into relativized genetic probabilities.
- Genetic logic and genetic probabilities become essentially unified.
- Shannon's theory of communication of information, of which it has been said that nowhere it encapsulates *meaning*, becomes relativized when it is absorbed into the relativized theory of probabilities, and in consequence of this it becomes apparent that in fact it *is* endowed with quite definite receptacles of potential meaning.
- Inside *MRC* what is called 'complexity' resolves into a set of relativized numerical 'measures', in the mathematical sense. These – for physical entities – can be established by acts of measurement, which offers an alternative with respect to the concept of 'algorithmic complexity', tied exclusively with signs, with language.
- The concept of time acquires an explicit – bi-dimensional – representation.
- A relativized concept of 'system' is in course of being constructed (H. Boulouet [2014].

Etc. New possible applications of *MRC* flow constantly into view. It thus becomes clear that:

> Relativ*izations* are not relativ*ism*. Quite on the contrary, the descriptional relativizations are a necessary condition for precision, whereas the absence of descriptional relativizations entails false absolutes, false problems, and paradoxical circularities.

Furthermore, I have to stress that the concept of 'transferred description', that inside *MRC* plays such an essential part, is entirely ignored in the current thinking and the current languages. It is equally ignored in classical logic, classical probabilities, and in the classical sciences. In particular it is ignored by the whole macroscopic classical physics, Einstein's theory of relativity being included. The classical disciplines are all constructed and exposed as if the descriptions 'mirrored' things and facts that pre-exist quite independently of the human conceptor (even Wittgenstein's so extraordinary analyses do not clearly contest this naïvely realistic conception). Everywhere works a tendency to occult the physical operations that in so many current macroscopic circumstances (medical analyses, detective researches, social organizations, study of entities from other planets, etc., etc.) create more or less radically the entity-to-be-qualified, and produce registered marks devoid of any qualia specifically related with the entity-to-be-qualified and with the acting view.

> *MRC*, on the contrary, is explicitly founded of the transferred descriptions.
> And via the stratum of conceptualization consisting of basic transferred descriptions, *MRC* penetrates beneath all the pre-existing languages, whether natural or specialized, and implants operationally its roots directly into the a-conceptual physical reality. This

is a genuine conceptual leap.

I dare assert that *MRC* is the first scientific general method of deliberate human conceptualization.

\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 analytical 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 concerning probabilities. (The opacity functional can be relativized and discretized).

Anyhow, *MRC* is quite essentially finite, so discrete, by construction. And it is by use of *MRC* that I solved, I think, a major (though very rarely known) difficulty of the classical probabilistic conceptualization (MMS [2014]. Namely the fact that up to this very day no general procedure has ever been defined for constructing the *factual, numerical* distribution of probability to be asserted in a factual situation that is unanimously considered to be probabilistic. I called this difficulty "Kolmogorov's aporia" because, starting from already 1963, Kolmogorov himself denounced most forcefully this startling and scandalous situation, and he claimed that in these circumstances his theory of probabilities is to be regarded as only a chapter of pure mathematics, devoid of any practical applicability: Kolmogorov wrote, for instance[1] :

> « I have already expressed the view …that the basis for the applicability of the results of the mathematical theory of probability to real random phenomena must depend in some form on the *frequency concept of probability*, the unavoidable nature of which has been established by von Mises in a spirited manner…..(But) The frequency concept (of probability)[2] which has been based on the notion of limiting frequency as the number of trials increases to infinity, does not contribute anything to substantiate the applicability of the results of probability theory to real practical problems where we have always to deal with a finite number of trials ».

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

Professor Longo was aware of this work and I think that he has understood the social difficulties encountered by it. So he decided to provide a way to submit it as freely as possible.

I must mention that the special issue of MSCS devoted to *Randomness, Statistics and Probability* where the mentioned work is coming forth, contains also a contribution (C. Porter [2014]) with a very interesting historical content that I ignored while developing my work. From C. Porter's contribution I learned that quite a number of mathematicians are well aware

[1] Kolmogorov, [1963] in *Sankhya* (quoted in Segal, J., [2003], *Le zéro et le un*, Syllepse)

of what I have called 'Komogorov's aporia', but they called it long before "the applicability problem". Which clearly is a better name.

But the 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 semantic content cannot be reduced to purely syntactic structuration, nor entirely 'mimed' by it (in the sense in which a mold can 'mime' a face).

The special issue of MSCS devoted to *Randomness, Statistics and Probability* contains also a brief debate between several outstanding contributors that is focused on precisely this point, i.e. 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 furthermore, even when it is raised in quite explicit and insistent terms, it retains very little attention[3].

I believe that this state of facts deserves examination.

Human intuition is magic. Nevertheless, instauration of explicit principles and rules for working out matches between a given semantic content and the syntactic expression assigned to it, could come out to be very fertile, acting like a vehicle for rapid and precise understanding and consensus. Men have lived before Aristotle's syllogistic, but since it has been created this syllogistic has avoided heaps of sophisms in heaps of lost time and effort.

And *MRC* offers a framework for matching narrowly and safely semantic contents and syntactic structures.

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

\MM-S

It simply is devoid of a theory of measurement acceptable from a formal as well as from a conceptual point of view, and endowed 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 have 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 that weigh on estimations concerning the structure of a whole inside which one is oneself imprisoned, is a very difficult problem indeed. If the imprisonment is absolute, this problem is radically and a priori devoid of solution. This may seem trivial, but many first order 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 that 'whole'.

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[3] Kolmogorov [1963]) wrote (quoted in Segal [2003]) :
« I have already expressed the view …that the basis for the applicability of the results of the mathematical theory of probability to real random phenomena must depend in some form on the *frequency concept of probability*, the unavoidable nature of which has been established by von Mises in a spirited manner…..(But) The frequency concept (of probability)[3] which has been based on the notion of limiting frequency as the number of trials increases to infinity, does not contribute anything to substantiate the applicability of the results of probability theory to real practical problems where we have always to deal with a finite number of trials ».

And 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 microstates? 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 organized 'outside of the quantum mechanical formalism' 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 represented in it. And, as I have already stressed, the whole organized thinking that is exterior to quantum mechanics ignores the concept of primordial transferred descriptions. So with respect to this concept there cannot exist an organized 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 in the sense of consensus. This, as a fact, is manifest since tenths of years.

> What is cruelly lacking is an organized structure of reference, different from quantum mechanics itself, but constructed in a way that permits to be clearly related with quantum mechanics, that admits of a controlled comparison with quantum mechanics, in the details as well as globally.

So I constructed such an organized structure of reference. I maintained invariant that what is represented inside quantum mechanics, namely states of microsystems, 'microstates'. But I constructed another representation involving them.

Quite independently of quantum mechanics, I constructed a resolutely qualitative but formalized representation of the way in which a human being can generate knowledge about 'microstates'. I brought into evidence just the necessary and sufficient conditions for constructing a communicable and consensual representation of microstates, but nothing more than these conditions, in order to offer no place for leakage of attention. What I achieved in this way is an epistemological-operational-methodological representation of the geneses of human very first pieces of knowledge on microstates. I called this *infra-(quantum mechanics)* (to be understood as 'beneath the formalism of quantum-mechanics').

Infra-(quantum mechanics) finally enables to establish a quite definite account on quantum mechanics, on its capabilities of representation, on the sources of these capabilities, whether factual, or strictly formal (like the Hilbert-space capabilities of representing probabilities), and on its epistemological-conceptual specificities. Indeed, if quantum mechanics is found to lack a feature that inside infra-quantum mechanics belongs to the set of necessary and sufficient conditions for constructing a communicable and consensual representation of microstates, then this is a lacuna of quantum mechanics.

This puts a term to trying again and again to solve this or that particular 'interpretation problem', with one foot settled inside the formalism itself, and the other foot dragging around outside the formalism, on a no-man's land that – with respect to the formalism of quantum mechanics – is just a pool of conceptual mud that hinders any definite meaning to gain a stable and consensual status.

Now, by systematic reference to infra-(quantum mechanics), the formalism of quantum mechanics reveals unexpected deficiencies:

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

- It does not represent at all, neither mathematically nor only by words, the way in which a microstate to be described, is generated. The process of generation of a physical and individual microstate 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 microstate that is involved.

- Notwithstanding a huge number of non-hamiltonian situations, quantum mechanics asserts an obviously debatable definability of a Schrödinger equation for any microstate that one would like to study; and it furthermore presupposes the rather debatable general possibility to calculate the corresponding infinite family of solutions. Correlatively, it defines no general way for specifying the initial state vector. It just posits that this can always be done. More generally, it strongly overstates calculability.

- It does not singularize mutually distinct classes of microstates. It rules directly on 'any' microstate and so it overlooks class-specificities that entail essential differences concerning the effectively possible ways of representing them.

- Etc.

But above all, as asserted already:

- Quantum mechanics is devoid of a generally valid theory of measurement. The crucial lacuna consists of a radical unawareness of the problem how the observable marks produced by an act of measurement, can be translated into a meaning expressed in terms of a given value of the measured 'mechanical' quantity. And when this problem is considered and examined, it appears that:

(a) Von Neumann's now ubiquitous concept of 'measurement evolution of the state-vector' is fallacious. The fallacy can be compensated for a particular category of free microstates, but on the basis of certain modifications that are quite non-trivial from a conceptual point of view.

(b) The bounded microstates escape the fallacy because particular features of the bounded states inactivate it.

(c) There remains a whole category of free microstates for which the fallacy cannot be resolved inside the formalism as it now stands because *no eigenstates of the measured quantity can emerge*. For such free microstates only a radically different sort of measurement operations can be conceived, and this withstands incorporation into a von Neumann-Hilbert-Dirac representation of the processes of measurement.

(Let us immediately note that this last-stated lacuna brings unexpectedly into evidence a fact that it might be useful to know when applicability of the quantum mechanical formalism is researched outside microphysics: In many cases this applicability consists of just making use of the Hilbert space representation of probabilistic results of measurements. Which is characteristic of Hilbert spaces mathematics, but has nothing to do with, specifically, microstates).

> To close the above enumeration, I dare ask: What meaning can have a theory of a domain of physical entities that cannot be perceived directly, when this theory does not define for any physical entity from that domain, measurements with observable results that can always be expressed in terms of a value of the measured quantity?

I have sketched a *2nd Quantum Mechanics* where the deficiencies enumerated above are dissolved. This new representation – not a 're-interpretation, a new representation – proposes

to try to introduce in the case *(c)* measurement operations that are founded on the de Broglie-Bohm guidance relation, but assumed to be an *observable* process, not an only conceived process. Nothing interdicts a priori this assumption. 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 put it on arXiv of quantum physics in replacement of the former one. 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: Many thanks.

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

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