THIS NON-AXIOMATIZABLE QUANTUM THEORY

FROM HILBERT'S SIXTH PROBLEM
TO THE RECENT VIEWPOINT OF GELL-MANN AND HARTLE

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Abstract. My first goal is not to add to the vast sea of speculations on the interpretation of quantum theory, but rather to reduce it by taking a common factor out of brackets. This common factor is the rivalry between the notions of *absolute reality* and *relative reality*, which is implicitly responsible for the long-standing absence of consensus.

My second goal is to argue that standard quantum theory cannot be axiomatized on the level that fundamental classical physics can be.

My third goal is to disentangle one knot from the tangle of quantum interpretation problems, showing that the knot is well known far beyond quantum physics. This is what I refer to as the "federation" of theories. The effectiveness of mathematics may be restricted by its inability to handle this problem.

> So it is a problem whether or not to
> worry about philosophies behind ideas.
> [Fey65, p. 170]

How did we live till now?

Some quotations:

> ... quantum theory can be organized into two parts: First, there is the calculus of amplitudes for histories ... Second, there are rules for deriving probabilities from these amplitudes ... [GMH93, p. 3358]

> ... the form of the effective classical equations of motion ... may be as much influenced by the character of the coarse graining and the mechanisms of decoherence as by the fundamental equations of motion ... [GMH93, p. 3346]

> The Schrödinger equation, in itself, is rather like a perfect computer program without output commands. [Dio92, p. 299]

So, in the spirit of Diosi's metaphor, we can say that the need to install an "output device" for our "quantum theory machine" has been acknowledged by the mainstream of physics ... many decades after the machine was first used!

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Puzzle (to be understood metaphorically). How can a computer center appear to seriously start realizing the need to buy printers only after a lengthy period of successful work?

Hint (the same metaphor). A quarter of a century ago, I worked on a computer, whose printer printed numbers on a narrow paper ribbon, with the speed of two numbers per second. Columns were raised above the ribbon and I was almost able to read the printed numbers according to the height of the columns, which were driven directly by the computer. So, it was almost possible to do without the printer, by joining a simple indicator to the corresponding interface and writing the numbers by hand! Let us imagine a scenario in which we had become used to saving money on a printer. One day we received a new computer, as usual, without any printer. This time, however, we were unable to decode signals on its printer interface, and were forced to spend money on a printer. It appeared to be a postscript printer, having a built-in auxiliary computer more powerful than the one with which I started the story!

Another hint.

> ... the standard way in which quantum mechanics operate: When in doubt, enlarge a system to include its immediate environment. ... Average over unobserved variables to produce nonunitary, dissipative time evolution ... I have to confess that I would be disappointed if this were all there is to interpreting quantum mechanics at the cosmic level ... [Amb93, p. 82]
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> An interpretation becomes necessary when no pedagogical method can be found to reexpress the basic concepts of the theory within the categories of common sense. [Omn92, p. 376]

My solution. The right interpretation of the calculations performed in the framework of the usual quantum theory, in most cases, is simply guessed. Sometimes the interpretation raises doubts, and one has to apply quantum theory to the system "the object plus the nearest part of the apparatus." Even a very simplified model for the quantally described part of the apparatus, combined with a very primitive guess at the interpretation of the wave function of this part, gives excellent results. The obvious lack of isolation of the system, preventing it from a quantum description, is simply ignored without harm to the obtained results. For one of the first examples see [Mot32]. The mathematical formalization of this very primitive trick, providing apparent validity to its crude simplifications, forms the well-known von Neumann Measurement Theory [Neu32].

> Many physicists deny the so-called Measurement Problem since the Quantum Mechanics as well as the von Neumann Measurement Theory are generally thought to be perfect. However, according to John Bell [Bel90], they are not quite perfect, only *for all practical purposes* (FAPP in Bell's irony) are they perfect. [Dio92, p. 299]

The growing dissatisfaction of this FAPP is caused by (1) the tendency towards logical completeness of quantum theory; and (2) the failure of the FAPP in quantum cosmology.

HOW MANY TIMES TO SOLVE THE PROBLEM?

Some old quotations:

> Objections have been raised in the past to the conventional or "external observation" formulation of quantum theory on the grounds that its probabilistic features are postulated in advance instead of being derived from the theory itself. We believe that the present "relative-state" formulation meets this objection, while retaining all of the content of the standard formulation. [Eve57, p. 462]

> The preceding paper puts the principles of quantum mechanics in a new form. [Eve57] [Whe57, p. 463]

> Observations are treated as a special case of normal interactions that occur within a system, not as a new and different kind of process that takes place from without. [Whe57, p. 463]

> No escape seems possible from this relative state formulation if one wants to have a complete mathematical model for the quantum mechanics that is internal to an isolated system. [Whe57, p. 465]

> ... there is no need to employ the "reduction of a wave packet." [Wak62, part II, p. 140]

> In this series of papers, I and II, we have already shown that there is in principle no difficulty in the problem of measurement in quantum mechanics. [Wak62, part III, p. 1156]

> ... we have developed a quantum theory of measurement, which ... avoids the well-known, undesirable infinite regression of standard von Neumann's treatment. [DLP66, p. 119]

> Our theory constitutes an indispensable completion and a natural crowning of the basic structure of present-day quantum mechanics. We are firmly convinced that further progresses in this field of research will consist essentially in refinements of our approach. [DLP66, p. 127]

> It is shown that the two ways of the change of state vectors can be understood without introducing von Neumann's 'ultimate observer' and without abandoning the linear law of the time evolution ... Thus the problem ... which has haunted quantum mechanics from the beginning dissolves into a pseudoproblem. [Jau64, p. 293]

And some rather recent quotations:

> The interpretative scheme which results is applicable to closed (isolated) quantum systems, ... has no need for wave function "collapse," makes no reference to process of measurement ... [Gri84, the abstract]

> ... the Copenhagen interpretation is incomplete, its consistency is very questionable, and its treatment of phenomena is much too superficial. [Omn92, p. 340]

> ... there is now a workable complete and consistent interpretation of quantum mechanics. [Omn92, p. 359]

> The origin of the phenomenological deterministic laws that approximately govern the quasiclassical domain of familiar experience is considered in the

> context of the quantum mechanics of closed systems ...
> [GMH93, p. 3345]

> We should be able to deal, for example, with the classical behavior of the Moon whether or not any "observer" is looking at it. [GMH93, p. 3346]

> ... recent years have seen a growing consensus that progress is being made in dealing with the measurement problem. [Zur91, p. 37]

> Zurek's consensus certainly didn't include John Bell ... [GGP93, p. 13]

> ... what we have to conclude is rather surprising. Gell-Mann and Hartle's coarse-graining is equivalent to performing von Neumann measurements, their decoherence criterion is equivalent to assuming a chain of *nondisturbing* von Neumann measurements. ... This is an old dream of measurement theorists. If a nontrivial chain of nondisturbing measurements have had ever been constructed we would have eliminated the Measurement Problem long ago. [Dio92, p.302]

> Instead of merely summarizing the results ... let us relate them to the problem (of understanding quantum mechanics and its relation to quasi-classical experience) in which we have been engaged, and which involves many elements elucidated by other authors over the last 35 years.
> [GMH93, p. 3376]

Puzzle. (a) How many times do we need to expel the reduction, caused by an external observer *a la* von Neumann, thus solving the Measurement Problem and revealing classical features within a closed quantum system?

(b) Are we closer to the goal now than 35 years ago, or not?

Hint.

> The quantum description of macroscopic systems ... attracts great attention from the "optimists," who have made a number of important observations and stated more than once that a positive solution of the problem has been found. However, all the suggested solutions were met with criticism from the "pessimists," who emphasize a failure in resolving the problem so posed. [KT92, p. 901]

> ... we agree with D'Espagnat [DEs90] that the origin of the disagreement between the "optimists" and the "pessimists" is the fact that the "optimists" investigate the emergence of classical reality relative to a class of observers, whereas the "pessimists" acknowledge only absolute (independent) classical reality.[1] [KT92, p. 904]

> In contrast with the "independent reality" concept, that of empirical reality does, at least in part, take into account in its very definition the nature of the human (or should we say of human *and* animal?) modes of apprehension. [DEs90, p. 1167]

> The concept of *world* in MWI[2] is not part of the mathematical theory, but a subjective entity connected to the perception of the observer ... such

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[1] These "optimists" and "pessimists" are referred to by D'Espagnat as "measurement theorists" [DEs90, p. 1148] and "physical realists" [DEs90, p. 1160], respectively.

[2] MWI = many-world interpretation.

> that it corresponds for human beings to our usual notion of the world. [Vai93, p. 15]

> ... certainly a physical system does not come with a subsystem containing a little sign reading, "I am the environment: Trace over me." [GGP93, p. 13]

> Since one cannot pose the problem of measurement without recognizing that systems exist, there is no need to apologize for assuming their existence in searching for its resolution. [Zur93, p. 88]

Another hint (to be used by analogy).

> While the computational aspects of *Mathematica* are essentially identical on all kinds of computers that run *Mathematica,* the external interface to *Mathematica* inevitably varies somewhat from one computer system to another.
>
> *Mathematica* is usually divided into two parts: the *kernel* that actually does computations, and a *front end* that deals with interaction with the user. The kernel is set up to be as similar as possible on every different kind of computer; the front end varies from one computer to another, taking advantage of particular features of each computer. [Wol88, p. 581]

My solution. Our description of the world is naturally divided into two parts: a *kernel* that is intended to describe the world as it is, irrespective of us (as far as possible), and a *front end* that deals with the way we are embedded into the world. The kernel should be the same for all possible observers; the front end is designed for us people, taking advantage of the particular features of our way of observing the world.

During the epoch of classical physics it was possible to keep the front end outside physics. It becomes impossible in the quantum epoch, because:

> ... on the one hand, the physical theory is in need of the concept of a generalized observer deprived of individual features, and on the other hand, idealizing the real observer in quantum theory inevitably misrepresents the dynamics of an observed object! [KT92, p. 905]

It is natural to believe that the notion of objective reality, being highly fundamental, belongs to the kernel. Alas, it appears to be inconsistent with quantum theory. We have to become accustomed to treating our objective reality in the framework of the front end only.

During the last decades, the main motivation for searching for reality within a closed quantum system was a matter of principle rather than a practical need. The aim was arrive at a form of quantum theory, self-contained (at least) to the same extent as the classical theory was. But the aim is unattainable.[3]

> There are unlikely to be any physically interesting variables that decohere independent of circumstance. [GMH90, p. 451]

> Expressing disappointment ... is a bit like complaining about the absence of an absolute time in special relativity ... [Zur93, p. 88]

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[3] As follows from the title "My solution," I am expressing my current view. A number of authors will surely disagree.

Physical examinations of our reality, performed by "optimists," led to the emergence of some forms of classical reality, but these forms were in fact relative rather than absolute.

Logical examinations of the problem, performed by "pessimists," forced us to conclude that the problem of finding an absolute reality within a closed quantum system is insolvable. The absence of clarity on the desired goal resulted in repeated "openings" and "closings" of the desired form of quantum theory.

The above is my "metatheory," not only explaining what happened, but also predicting what will happen, as follows.

The chief motivation for searching for reality becomes practical. Accordingly, the treatment of reality as relative becomes acceptable. Hence, the principal approaches found long ago will shift from rejected to accepted (maybe, after being re-opened). It will appear as if they were shifted from false to true, without essentially being changed!

Needless to say, the current work of Gell-Mann and Hartle [GMH93] is much more advanced in many technical aspects than the above-cited old works. Nevertheless, [GMH93] contains nothing protecting it from the criticism of principle, that was leveled at these old works.

Another, for whom we are not real

It is natural for any physical theory, both classical and quantum, to try to get rid of its dependence on the specific features of a definite observer. To this end, there exists the great equalizer — physical apparatus. If designed for the blind, it can inform the blind as well as the sighted; if designed for humans, it can equalize them with creatures able to hear gravitational waves and to touch single atoms.

In fact, we go slightly further than apparata allow us, and strive to describe physical reality from the point of view of an "optimal" observer.

> For the coarse graining defining a quasiclassical domain to be an emergent feature of the universe characterized by H and ρ rather than an artifact chosen by some IGUS,[4] it should be as refined a description of the universe as possible consistent with the requirements of decoherence and quasiclassicality. [GMH93, p. 3376]

But two coarse grainings may be inconsistent, that is, may not admit a third coarse graining more refined than both of them. The problem is that quantum theory does not exclude the possibility of the existence of an observer capable of observing some interference between histories that are definitely different for us people. In his world we are not real!

This problem arises repeatedly in various forms: from the Schrödinger cat paradox and the Wigner friend paradox to the "future generations" problem [GYS89, p. 680], and was not avoided by Gell-Mann and Hartle:

> We have posed the question as to whether there could be various kinds of essentially inequivalent quasiclassical domains or whether any quasiclassical domain is more or less equivalent to any other. The former case poses some challenging intellectual puzzles, especially if we imagine IGUS's evolving

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[4] IGUS = "information gathering and utilizing systems" capable to function as observers within a quasiclassical domain ([GMH93, p. 3376]).

> in relation to each of the essentially inequivalent quasiclassical domains. [GMH93, p. 3377]

This is *The Question.* Not one of the proposed solutions of the measurement problem escaped it. But times have changed: in the 60-s "optimists" proposed solutions whereas "pessimists" defeated them via the question. Today we observe the coexistence of a solution and the question in [GMH93]. Does it mean that they have become consistent? No — if a solution is treated as introducing *Absolute Reality,* independent of observers. Yes — if it concerns *Relative Reality,* depending on observers.

But why believe in the existence of Another, for whom we are not real? Each of the works of "optimists" proves that it is impossible! Yes, it proves; but — postulating the irreversibility of some physical process in a finite region of space-time, namely, registering sensory data by a recording device [Eve57]; "spreading" macroscopic information into the microworld [Wak62]; passing from a metastable state to a stable one [DLP66]; and so on. Being satisfactory "for all practical purposes," these postulates of irreversibility contradict the well-known reversibility of all elementary fundamental processes. In practice, we may be as sure of these irreversibilities as in our own reality. Theoretically, we have no hope of proving either the former or the latter from first principles — and therefore no hope of finding a well-defined boundary, i.e. the number of particles, or other parameter, which determines the threshold of appearance of irreversible phenomena. Accordingly, each solution of the measurement problem leaves some vagueness — allowing the problem to still be considered as unsolved. Needless to say, [GMH93] also does not answer the question, for which parameter values it becomes true that "History is approximately permanent ... The reason is that decoherence there is essentially local in time" [GMH93, p. 3358].

Estimations were made, showing that Another, for whom we are not real, should use a monstrous amount of matter, time, or other resources; see for example [Omn92, p. 356] and [PR64]. Being satisfactory "for all practical purposes," these proofs add a number of premises to first principles. They do not seem reliable in the presence of some "latent order" as discussed in [GYS89].

It is of course possible to obtain a mathematical description of some irreversible process by a preliminary passing to a limit in some parameter. However, no parameter can be infinite for a real process in a bounded domain of space-time; and just such processes support our reality: "In that case, for every photon near us, there would be a correlated photon ... on the other side of the universe. The local physics, however, would be accurately described by a nearly thermal reduced density matrix in which the distant photons has been traced over" [GMH93, p. 3356]. Clever use of these correlated distant photons may be a part of the super high technology of Another, for whom we are not real.

We may imagine a definition of reality based on moving photons to infinity rather than far enough.

> Indeed, in many circumstances where the phases are carried off to infinity or lost in photons impossible to retrieve, the probability of recovering them is truly zero and the situation perfectly irreversible ... [GMH90, p. 453]

But it leads to "delayed choice" problems: our reality depends on a future decision of Another to measure one or another observable.

An irreversible dynamics can be invented, which despite being so close to the

Hamiltonian quantum dynamics that experimental discrimination is extremely difficult, still ensures irreversibility of the usual macroscopic processes; see [GGP90, Dio92]. Moreover, some reason independent of the measurement problem was pointed out [Dio92]. Such theories exclude the existence of Another, for whom we are not real, and enable the introduction of absolute reality. But such a theory can hardly be accepted without experimental confirmation, which at present does not exist.[5] Such theories are not considered in this work.

But, again, why believe in the existence of Another, for whom we are not real? We have neither empirical evidence of his existence, nor theoretical reasons (such as, say, the Penrose theorem on inescapable singularity after satisfying some conditions). Moreover, we (we, not Another) cannot obtain empirical evidence of our non-reality![6] And a theory predicting an empirically non-verifiable fact can hopefully be improved excluding the dull prediction. Thus, we may accept an axiom excluding the existence of Another, for whom we are not real, thereby facing neither internal contradictions in the theory, nor its disagreement with facts. Does the problem dissolve into a pseudoproblem? Alas, it does not.

The problem is not that we fear the existence of Another. The problem remains in force for a man completely convinced of his reality and of the non-reality of Another.

A theory proving our reality (absolute, for any Other) from first principles has to contain an exact definition of "our reality" in terms of first principles. This, and not the certitude of our existence, is what is important to obtain. Unfortunately, we cannot obtain it from quantum theory. Adding the axiom "we are real" is a useless diversion, since before adding this axiom we have either to add the new primary notion of "we" (parallel with notions of space-time, superstring, and so on??), or to express it in terms of available primary notions. But this is precisely the problem!

Is a simulated Moon there when the display is off?

A non-dissipative computer is an interesting combination of classical and quantum features [Ben73, Fey86, Deu85b, Per85, Mar89]. Like any contemporary computer, the non-dissipative computer can be used as a classical machine. But it does not involve anything posing physical obstacles to putting it into a superposition of classical states, and the superposition can survive for a long duration. Though unavailable now, such computers should become feasible in the future.

Lay people playing computer games will surely consider personages on the display as a part of reality[7] without inquiring whether the hardware is dissipative or not. The display is connected via a controller performing quantum measurements. The controller's output, being a macroscopical dissipative current, is surely a part of reality. But the computer's internal working does not depend on output devices. We may switch the display off together with its controller, have a meal, then switch them on again and, judging from appearances, it is clear that simulated events were moving during our meal. However, for the nondissipative case, no decoherence was occurring!

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[5] See [Bal91, p. 10] for a contradiction between a theory of [Dio92] and observed facts.

[6] Or, anyway, our theories are not intended to serve us in very strange situations, such as, say, serving a person on drugs.

[7] Small children may consider them living, but we adults mean the reality of corresponding records in the computer's memory.

Clearly, "hydrodynamical" coarse graining of Gell-Mann and Hartle will not detect the activity of the non-dissipative computer without output devices. This fact shows that such coarse graining can be "as refined as possible" only within a restricted class of competitors. We may imagine a civilization rejecting the "hydrodynamical" coarse graining on the same grounds as we people would reject the "astronomical" coarse graining discerning only planets and stars.

An appropriately programmed nondissipative computer may also be considered an observer ("IGUS," following [GMH93]). We can make contact with such an observer, but we can also perform interference experiments with it (with him, as far as it is ethical and legal); see [Deu85a, Sect. 8] and [Alb83]. It means that we can play for it (or for him) the role of Another ...

> If there are many essentially inequivalent quasiclassical domains, then we could adopt a subjective point of view, as in some traditional discussions of quantum mechanics ... [GMH90, p. 454]

The nondissipative computer is not quasiclassical, but deserves the same approach.

Federating physical theories

The relation between the two components of theory, the kernel and the front end, is not as new to physics as it may seem. "Federative" relations are rather typical between more fundamental and less fundamental theories. By "federative" I mean that when joining the theories, it involves some arbitrariness not present when taken separately. It resembles continents: solid but slightly mobile.

As an example we may take acoustics and electrodynamics. It is clear that electrodynamical interaction is responsible for acoustic phenomena, so equations of acoustics have to result from electrodynamics at the expense of some specific premises and the neglect of some small quantities. Each of the two theories has its own difficulties, but can be represented as a mathematical theory following from a few definite principal equations. Alas, this definiteness is lost when attempting to join the two theories.

An acoustic wave is a well defined notion within an imaginary "acoustic world" filled with continuous elastic matter without long-range interactions. What, however, corresponds exactly to the acoustic wave within an "electrodynamical world" (also imaginary, but far more realistic)?

Oscillations of charged particles have to be "dressed" by corresponding oscillations of the electromagnetic field. But to what extent? Too weak a dressing leads to deterioration because of the intensive high-frequency momentum exchange between the particles and the field. Too strong a dressing results in a too global picture: each acoustic mode includes oscillations both on Earth and on the Moon because of their electromagnetic interaction. So, establishing an acoustic-electromagnetic correspondence is a matter of compromise.

A similar "federate relation" takes place between celestial mechanics and general relativity. Considering the orbiting of Earth we have to choose how to dress it with a gravitational field.

Yet more problems are caused by statistical physics. What corresponds to a definite temperature? Well, the Gibbs measure. But what exactly corresponds to a heat transfer described by the heat equation? And what corresponds to a metastable state? A clever answer to the last question is known [PL71], with some arbitrariness being evident.

We are used to the federation of physical theories. But we are also used to the fact that the most fundamental theory is distinguished at any moment among all elements of the federation. It is this element that provides us with a principal picture of our physical reality. Others give an approximate description for some typical patterns arising within the reality.

The new and unexpected fact introduced by quantum theory, is that the most fundamental theory no longer gives us a picture of physical reality. This mission passes to other physical theories.

Thus, "the infamous boundary" [Bel71] still exists, but now it is not the boundary between classical and quantum worlds. It is rather the boundary between the most fundamental physical theory and other theories responsible for physical reality.

Clearly it is connected with the relativity of reality. Absolute reality, within theories where it exists, is defined on the most fundamental level.

Constructing fragments of our relative reality

Being introduced on a non-fundamental level, our relative reality has no definite, simple and universal expression in fundamental terms. In itself it is a random process, that is, a probability distribution on a space of histories. A mitigated notion of a "quasi-probability distribution" is also in use, the additivity being approximate. Some of the components of the random process should in most cases be close to determinism. The deterministic process can be the Hamiltonian dynamics of a classical mechanical system. It can also be non-Hamiltonian, including friction, heat transfer, and so on. Discrete components may emerge from the decay of metastable states [DLP66], including that of unstable particles [Boh93]. Discrete deterministic components may form complicated algorithmic systems (computers). A kind of discrete reality based on a brain-like structure is also proposed [Don93].

> Reality is a subtle concept that has been debated over the years in many physics texts and does not have a clear definition. [PZ93, p. 2737]

The diversity of forms of reality should find its expression in the corresponding diversity of mathematical schemes of description. Besides, the inherent arbitrariness in reducing each form of reality to fundamental notions, gives rise to competing mathematical schemes describing the same form of reality. At present, attention is focused on schemes for Hamiltonian mechanics with small dissipation local in time. An optimal choice of coarse graining parameters is discussed in [GMH93]. But the available arbitrariness is in no way limited to a choice of parameters.

> ... the theory of decoherence, at least as it stands now, is a rather blunt mathematical tool ... It looks attractive, therefore, to retain the program while looking for more adequate mathematics. [Omn92, p. 359]

It was shown [KT87, KT92] how the remaining arbitrariness can be used for gaining additional advantages:

- the exact (rather than approximate) additivity for probabilities;
- an exact factorization of a quantum state for interacting collective degrees of freedom.

Federating mathematical structures: to derive or to enrich?

From a mathematical viewpoint, little wonder that physicists do not know some key ingredients of their theory, such as the true Lagrange function, or even the number of fundamental fields. Anyone knows that the ultimate physical theory is not achieved. However, it is surprising that a far more general question remains vague: what kind of mathematical structure is nominated by fundamental physics as the description of the physical world? A classification of possible mathematical structures into an infinite variety of types (a finite number of them already being used) is presented in Chapter IV "Structures" of the first book [Bou] of the famous Bourbaki series. Some examples are: a Hilbert space; a topological space; a Riemann space; a symplectic space; a fiber bundle; a C^*-algebra; a representation of a group in a Hilbert space; a net of local algebras; and many others. Seeing a physicist trying this or that Lagrange function, a mathematician concludes that the physicist at least knows that what he needs is a Lagrange function and not, say, a Banach space.

As far as the kernel of physics is concerned, the mathematician's conclusion is essentially true: several, but not many approaches using different structures exist. However, the kernel in itself does not describe our reality. So, the following question remains:

Which kind of mathematical structure should describe our physical reality?

This point remains very vague, which is usually considered a specific feature of quantum theory. But the knot can be disentangled into two more tractable parts. The first part is that the theory of our reality has to be a "federation," as was mentioned before. The second part, the problem of "federated" mathematical structures, is really vague, but fortunately is far more general than its quantum case. Hence we hope to enlist some intuition from abroad. A relevant example follows (free of quantum theory).

Imagine a physical system that is observed having two states only, and jumping spontaneously from one state to the other. Suppose that its phenomenology is perfectly well described by a two-state Markov process with an exponential probability distribution for the time interval between jumps. Imagine that a microscopic theory is proposed for the system, describing it as a one-dimensional diffusion process governed by a stochastic differential equation

$$dx(t) = -U'(x(t))\,dt + a\,dw(t)\;,$$

where $dw(t)/dt$ is the white noise, $\langle dw(s)dw(t)\rangle = \delta(s-t)\,dsdt$, and U is a function with two minima at ± 1, say,

$$U(x) = c(x^4 - 2x^2)\;.$$

The constants a, c are such that $x(t)$ fluctuates around -1, the fluctuations being of very high frequency, and then, after a neither small nor large period of time, $x(t)$ leaves its metastable state around -1, entering the other metastable state around $+1$; the process then repeats itself.

Suppose that the variable x is hidden. We have no experimental evidence of its existence. But we have some convincing reasons for accepting the microscopic theory. For example, it is successful in predicting alterations of macroscopic parameters under the influence of external fields by adding relevant terms to the potential function $U(x)$. We accept the microscopic theory as the fundamental one. But we cannot interpret it without mediation of the macroscopic theory. Does it emerge from the microscopic theory, or does it supplement it?

From the mathematical viewpoint, we have two structures which both happen to be stationary probability distributions in functional spaces. (Some alternatives exist, but they are essentially equivalent.) One measure μ_{micro} is concentrated on continuous functions $(-\infty,+\infty) \to (-\infty,+\infty)$ and corresponds to the diffusion process. The other measure μ_{macro} is concentrated on (discontinuous) functions $(-\infty,+\infty) \to \{\pm 1\}$ and corresponds to the Markov process. Both measures are uniquely determined. What exactly is the connection between them?

The first, most straightforward attempt is to put $y(t) = \operatorname{sign} x(t) = x(t)/|x(t)|$ expecting that $y(\cdot)$ will be distributed according to μ_{macro}. But the attempt fails immediately. It is enough to note that each t such that $x(t) = 0$ is not a jump point for $y(t)$, but a more essential discontinuity. In fact, the set of all such t is of cardinality continuum, and each such t is a discontinuity for $y(t)$.

We see that a local-in-time dependence between $x(\cdot)$ and $y(\cdot)$ comes to nothing. We may try for $y(t)$ to be a dependence on $x(s) : s \in [t-\varepsilon, t]$ with a small ε. A more radical departure from the straightforward approach may be a search for a probability distribution for a two-component random process $\bigl(x(t), y(t)\bigr)$, returning μ_{micro} and μ_{macro} as marginal distributions, thus waiving the tenet that $y(\cdot)$ should be uniquely determined by $x(\cdot)$, in favour of the tenet that $y(\cdot)$ should somehow accompany $x(\cdot)$. But we are not ready to be absorbed in this work; we have to clarify its purpose.

It is not a problem to prove that the two theories agree in their rough predictions:[8]

$$\left\langle \exp\left(i \int f(t) x(t)\, dt \right) \right\rangle_{\mu_{\text{micro}}} \approx \left\langle \exp\left(i \int f(t) y(t)\, dt \right) \right\rangle_{\mu_{\text{macro}}}$$

for any smooth function $f(\cdot)$. The argument is quite convincing for the usual, working physics, that interprets it as a derivation of the macroscopic theory from the microscopic theory without inquiring about a path-to-path (that is, history-to-history) correspondence. Nevertheless it is questionable on the level adopted for the debate about the interpretation of quantum theory.

Can we state that $y(\cdot)$ emerges from $x(\cdot)$? Or that μ_{macro} emerges from μ_{micro}? Are the two macroscopic states $(y = \pm 1)$ defined unambiguously in terms of $x(\cdot)$? I am posing these questions not to examine the example with x and y (which is relatively clear), but to examine such notions as "emerge," "define unambiguously in terms of" and so on (that are far more vague in the context of the "quantum debate"). I am sure that the debate will remain unsuccessful until consensus is reached on the better understanding of these vague notions. And I propose to sharpen the notions on relatively simple examples before applying them to the great enigma.

This is why I believe that a mathematical examination of "federation" would be helpful for physics. And now, resting on the example with x and y, I can illustrate what I call the federation of mathematical structures.

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[8] But not in all their delicate predictions. This is especially important when federating physical theories, as was emphasized to me by L.A. Khalfin.

We have two intuitive ideas, each being successfully formalized (by μ_{micro} and μ_{macro}, accordingly). We also have an intuitive idea of their connection. We are able to construct one or another formalization of this third idea, but in any case we feel that the constructed formalization is far more definite than the idea, and the excess of definiteness is filled with artificial constructions.

The vague idea of federating mathematical structures should be compared with the well-understood idea of deriving them. The last idea is successfully formalized, see [Bou], Chapter IV. One example follows. A linear structure and a topology may be naturally linked by the usual demand that linear operations be continuous. In the case of a finite-dimensional linear space, the corresponding topology is determined unambiguously. So, the topology is derived from the linear structure. When the dimension is infinite, this is no longer the case; for example, two different topologies on a Hilbert space are well known, strong and weak. Hence, the structure of a linear topological space is a true enrichment of the underlying linear structure for an infinite dimension, but for a finite dimension both structures are equivalent.

It is peculiar to a federation that we feel unable to realize whether we are dealing with equivalent structures or with an enrichment.

> It is, however, not clear to me whether they assume that the Eq. (1) itself contains the wanted coarse-grained decohering structure or, on the contrary, GMH impose "alternative decohering histories" upon the Wheeler-DeWitt equation (1) from outside, i.e. they admit a modified dynamics."
> [Dio92, p. 300]

Essentially the same question was asked of each "optimist."

A mathematician may say: either you fix a definite topology on your infinite-dimensional linear space, thus imposing a new structure from the outside, or you consider the set of all topologies (conforming to the given linear structure), thus dealing with an equivalent structure. The second approach is similar to that of Omnes, and is definitely rejected by Gell-Mann and Hartle:

> ... enumerating the sets of decohering histories of a given Hilbert space has no physical content by itself. [GMH90, p. 441]

They attempt to single out a definite superstructure without enriching the original structure:

> We obtain a description of the sets of alternative histories of the universe when the operators corresponding to the fundamental fields are identified.
> [GMH90, p. 441]

Unfortunately, their efforts narrow the field of superstructures vastly but without singling out one of them. The best we can hope for is to make the field narrow enough in some sense that is not yet specified, but which can hopefully (a second hope!) be specified:

> It would be a striking and deeply important fact of the universe if, among its maximal sets of decohering histories, there were one roughly equivalent group with much higher classicities than all the others. That would be *the* quasiclassical domain, completely independent of any subjective criterion ... [GMH90, p. 454]

> ... were there some fundamental reason to restrict the completely fine grained sets, as would be the case if sum-over-histories quantum mechanics were fundamental ... [GMH90, p. 446]

It remains unclear whether the fundamental structure is meant to be enriched or not. This problem is peculiar to a federation. If someone succeeds in upgrading the federation to a union, then absolute reality will be restored. Otherwise only relative reality will be available.

I emphasize that these problems of federation are much older than quantum theory. In fact they are older than classical physics, too! To the best of my knowledge, the first author concerned with federation was Zeno, with his famous paradox of a heap of sand. We have some intuitive idea of a "small" number of grains of sand not forming a heap, as opposed to a "large" number capable of forming a heap. What may the corresponding mathematical structure be? A decomposition of $\{1, 2, \dots\}$ into $\{1, 2, \dots, N\}$ and $\{N+1, N+2, \dots\}$ with a definite N? A set of some (or all?) such decompositions? Or some kind of fuzzy set? We feel that the idea escapes formalization. We know of some attempts at building a mathematics acknowledging that "some integers are more finite than others," see [Vop79, Nel93]. But these attempts do not seem to be helpful in understanding the concept of a federation.

> You can recognize truth by its beauty and simplicity. . . . If you cannot see immediately that it is wrong, and it is simpler than it was before, then it is right. [Fey65, p. 171]

A physicist may figuratively imagine a successful union of mathematical structures[9] forming a bounded state, while a federation is rather like a metastable resonance, belonging to a continuous spectrum. We are accustomed to (or even spoiled by) the "discrete spectrum" of apt mathematical models. The absence of this discreteness is typical of a "federation."

Federative relations readily arise in discussions about the reduction of one science to another, say, biology to physics. Is it possible to express, say, the notion of "a cat" in fundamental physical terms? Is the notion emergent, or imposed from the outside? Only recently such relations arose between fundamental physics and its interpretation.

> . . . the essential lesson of quantum theory is that the conventional physical reality cannot be considered independently of the fundamental concepts of relevance[10] and reduction. [Zeh79, p. 813]

> It does not appear to be easy to explain in purely physical terms a concept of relevance. [Zeh79, p. 810]

> . . . all the sciences . . . are an endeavour to see the connections of the hierarchies . . . To stand at either end, and to walk off that end of the pier only, hoping that out in that direction is the complete understanding, is a mistake. [Fey65, p. 125.]

AXIOMATIZATION

> If geometry is to serve as a model for the treatment of physical axioms, we shall try first by a small number of axioms to include as large a class as possible of physical phenomena, and then by adjoining new axioms to arrive gradually at the more special theories. [Hil00, p. 15]

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[9] as the finite-dimensional linear topological space

[10] relevance = generalized coarse-graining.

> A great physical theory is not mature until it has been put in a precise mathematical form, and it is often only in such a mature form that it admits clear answers to conceptual problems. [Wig76, p. 158]

> ... we do not have an exact and unambiguous formulation of our most fundamental physical theory. [Bel75, p. 98]

> ... it would be natural for me to talk instead about the sixth problem that Hilbert might have posed if he were alive today. [Wig76, p. 147]

To be specific, we will not be concerned with the following points:

- Some local fragments of physical theory are axiomatized successfully;
- No physical statement has a guarantee of eternally surviving in the progress of physics;
- As we learned from mathematical logic, any "serious" (not too simple) axiomatic system admits non-isomorphic models.

What we would like to do is to take the current fundamental physical theory and to embed it into mathematics as a structure defined uniquely up to an isomorphism.

We hope that the goal will be achieved as far as the kernel of the physical theory is concerned. But the theory will be "rather like a perfect computer without output commands," and guessing will be the bridge to empirical facts.

> However, just the proper choice of the primitive concepts and axioms and the proper use of the metalanguage demand a complete mastery of the actual physical situation which it is the purpose of the axiomatic analysis to clarify, and it is therefore a fatal illusion to hope that the really deep difficulties of this situation could be skipped or in any way alleviated by any process of axiomatization. [Ros65, p. 223]

The skepticism concerns the hope of extending an axiomatization beyond the kernel. The concepts of relative reality and federation help us to understand the nature of the obstacle.

> The reductionist approach — explaining physical phenomena in terms of simple, mathematically precise, quantities — has been extraordinarily successful in almost all areas of physics. It goes against everything we have learned about nature to propose a theory in which complicated macroscopic objects, whose precise definition must ultimately be arbitrary, are fundamental quantities. [Ken90, p. 1754]

> A philosopher once said 'It is necessary for the very existence of science that the same conditions always produce the same results'. Well, they do not. ... Yet science goes on in spite of it ... [Fey65, p. 147]

> In the history of physics, ideas that were once seen to be fundamental, general, and inescapable parts of the theoretical framework are sometimes later seen to be consequent, special, and but one possibility among many in a yet more general theoretical framework. ... Examples are the earth-centered picture of the solar system, the Newtonian notion of time, the exact status of the laws of the thermodynamics, the Euclidean laws of spatial geometry, and classical determinism. In view of this history, it is appropriate to ask of any current theory "which ideas are truly fundamental and which are 'excess baggage' " [Har91, p. 235]

With great embarrassment I am forced to conclude that the time is ripe to abandon the hope that our physical reality can be described[11] axiomatically.

CONCLUSIONS

The protracted crisis in the interpretation of quantum theory, forces us to pay special attention to each obstacle that we can disentangle. Doing so we find out that some obstacles extend far beyond quantum theory in various directions, involving epistemology and mathematics, and should be discussed in their corresponding contexts.

As long as the linear nature of quantum theory is not refuted, we have no fundamental substitute for us observers. This is why the observer-independent part of the theory, its *kernel,* gives no explicit testable predictions (though we are experienced at guessing some of them implicitly).

Our physical reality, being nothing but the totality of the testable predictions, is observer-dependent, that is, a *relative reality.* This is why it cannot be defined unambiguously in fundamental terms. It is formed by the observer-dependent part of the theory, its *front end.* An example of a reality not contained in a quasiclassical domain in the sense of Gell-Mann and Hartle is provided by non-dissipative computers.

The kernel is intended for describing the universe by means of a mathematical structure defined unambiguously or up to some dimensionless parameters.

The mode of using mathematics by the front end is different and somewhat unclear. Sometimes a fragment of our physical reality is described by a mathematical structure defined unambiguously and obtainable from the fundamental (kernel-made) structure by restricting it to some (more or less definite) class of cases, and by (more or less well-founded) neglecting some terms in the equations. Even in the most successful cases such a non-fundamental fragment of the theory cannot be united with the kernel to form an unambiguously defined mathematical structure. It can rather be *federated* to the kernel. The mathematical nature of such a federation remains vague.

Fragments of reality pictured by the front end are often considered emergent from the fundamental structure, but this viewpoint is frequently questioned. It remains vague which requirements have to be fulfilled by a connection between two mathematical structures to enable us to consider one structure as emergent from the other. It is even unclear, whether the "emergent" structure should be derivable from the "emerging" one, or should enrich it! These questions usually evade explicit discussion, but present obstacles to progress. It should be helpful to discuss them in relation to some simple models. One such model, of classical (non-quantum) nature, is outlined here.

"Finally, we suggest a framework which should allow different approaches to coexist." [JQ93, p. 1]

- Authors dealing with quantum/classical correspondence should state explicitly whether they deal with relative or absolute reality. As a rule, works based on linear quantum dynamics, deal with relative reality (for example: [Eve57, GMH93]), whereas authors conjecturing some fundamental nonlinearity deal with absolute reality (for example: [Dio92, GGP90]).

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[11] in a fixed stage of the development of physics.

- Authors rejecting the very idea of relative reality, or (what is eventually the same) rejecting at least one of its immanent features, should direct their criticism not against one or another work, but against the trend as a whole. (An example: the above cited question of [Dio92] to [GMH93], whether coarse graining is imposed from the outside.)
- Authors elaborating on relative reality should not reject works on absolute reality on the only ground that the problem can be solved without sacrificing linearity. It is not the same problem.
- Authors examining one or another physical mechanism of decoherence as a basis for relative reality should neither proclaim the mechanism as the ultimate solution of the problem, nor ignore other mechanisms proposed before. The concept of an emerging structure is very vague and is in need of clarification.

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