PROBLEMS, CONJECTURES, SPECULATIONS

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"Wir müssen wissen. Wir werden wissen." (David Hilbert)

In these notes I'm going to draft some open problems, conjectures, and speculations related to my research topics, as presented in [RT]. Retrospectively, I realize how much I owe to my Göttingen years of studies, with maths and physics living together as a matter of course. Sharing Hilbert's optimistic vision of science, I do think that problems have a solution, and that conjecturing and speculating is part of mathematical activity. Since the latter are usually banished from "official" research papers, the present notes do not belong to that category. As in [RT], I shall first develop some ideas related to *conceptual differential calculus*, and second, ideas related to the *relation between geometry and algebra*. But let me start with some general remarks. (I add some hyperlinks, in grey in the text.)

1. The universe of mathematics, and the mathematical universe

1.1. **The universe of mathematics.** I have spent a good deal of my mathematical life by thinking about *associative* products $a\Box b$, and about the fact that any such product comes together with an "opposite product", say $a\Diamond b := b\Box a$, which is again associative. It is a feature of nature that there is strictly no reason to prefer using one, or the other, and they look like monozygotic twins:[1] hard to distinguish – all you can do is to say: "I have fixed *one* product, and I'm going to use it throughout the present text". You cannot be sure your fellow mathematician uses the "same" product, unless you show him a copy of your text, and both of you agree that you are both writing from left to right, using the same symbols and conventions, etc.

If you take this silly remark seriously, then you must be puzzled by the fact that virtually all modern mathematical texts speak of "the" *composition of mappings*, which, as you learn, is one of the fundamental notions of mathematics, written $g\circ f$ and "defined" by $(g\circ f)(x) = g(f(x))$. But there is no way to distinguish this from the notation $f(g(x))$, except "showing" your text to your students or collegues. For instance, we cannot know if mathematicians on distant galaxies use the "same" definition of composition of mappings as we do: even if we assume that "left" and "right" have the same meaning for them as for us, we cannot know if they write from left to right, or from right to left. Thus the term "composition of mappings" is well-defined only in the local context of a civilization where members are connected to each other via e-mail, internet, reading the same books, or something similar.

You may say, as almost all mathematicians do, that this is not a big deal: it's just a question of notation; or: "the set of all products" is the same as "the set

[1] In the dictionary I found that in English one also says: "identical twins". Amazing!

of all opposite products", or: mathematics based on the opposite composition is "isomorphic" to mathematics based on the "usual" one; it's just a matter of convention how you write arrows, or morphisms, of your category. However, the fact remains that notation is something "physical", not understandable in purely mathematical terms, and that there are two ways of composing *endo*morphisms, which in general give not the same result. Thus, if you don't feel too comfortable with "big" categories, like those of all products, or of the whole of mathematics, then you may agree that it would be safer to develop our formalism in such a way that, whenever some mapping, say the product $\Box$, or the Riemann curvature tensor or whatever, is an *object of study*, you would prefer to look at it as some member of a *small* category, that is, to fix a small set, which one may call "local universe", and where the mappings that you intend to compose are assumed to live in. Of course, you need to have the notion of "composition of *arbitrary* mappings" in your tool box, since you cannot avoid using it at some level – but then you loose control over the order of composition. By properly choosing your universe you can retard this moment as long as you wish. What I say here is a plea for using the language of *groupoids* much more systematically than is done usually. Other people already argued to this end, and I just want to go further: I propose to rewrite calculus and differential geometry in this spirit. This may sound boring and pedantic, but I rather guess it's not: I expect that, done right, it will lead to interesting and important insights.

Concerning differential calculus and differential geometry, this program is about half-way carried out in my papers [37, 38, 40], cf. [RT]. What is missing, is a systematic formalization of what happens if one exchanges everywhere groupoids with opposite groupoids: "source" α and "target" β are exchanged, as well as products and opposite produces – my feeling is that this should be the most natural way towards *supersymmetry* (see below). In a certain sense this would provide a good answer to our silly problem: develop a theory that contains source α and target β as parameters and that predicts what happens if these parameters take different values.

One can imagine another way out of the "$\Box$-versus-$\Diamond$-dilemma": if there is a further product, or a vector space, around, written additively, then one may form the *Jordan product*, $a \bullet b = \frac{a \Box b + a \Diamond b}{2}$, sort of avoiding to choose between $\Box$ and $\Diamond$. Even if there is no + around, you always may look at the *squaring mapping* $x \mapsto x \Box x = x \Diamond x$, which retains precisely those properties that are common between $\Box$ and $\Diamond$. But be aware that you enter a new world, the world of *non-associative products*. This world is strange in many respects, and quite fascinating – I have been working for a long time on the "geometry of Jordan structures", and I'll say a bit more on this below. Concerning the human side of the "universe of mathematics", let me just add that Jordan theory is a topic far remote from mainstream, which even can be considered as an endangered species: it lacks of young academics.

I believe that *both* "solutions" of our silly problem lead to interesting conclusions, and that they are "complementary" to each other – rather than choosing between one or the other, one should pursue both of them simultaneously.

1.2. **The mathematical universe.** As a mathematician, I feel quite comfortable with Max Tegmark's thesis that there be a bijection between physical reality and mathematics, the *Mathematical Universe Hypothesis (MUH)*. Basically, I think and work as if it were true.[2] However, I don't believe that all mathematical structures are equally important or unimportant for reality, under the hypothetic bijection, and I would rather think that the "weight in reality" of a mathematical structure is correlated with its degree of "naturality" or "unavoidability". For instance, most people having some scientific education would certainly agree that *differential calculus* is a piece of mathematics that must have a huge weight in our "mathematical universe", whatever the final theory looks like: since Newton and Leibniz, all of physics is built on calculus. Therefore, on heuristic bases given by the MUH, we may suspect that any mathematical modification or extension of differential calculus is likely to reflect profound aspects of physical reality. However, as far as I see, most physicists and mathematicians do not imagine that differential calculus could be a topic of active research, and rather consider it as a tool having reached its final form, to be taught to undergraduates without major changes during the next centuries. I'm not so sure about that: I would say that calculus is not yet dead, it still has much potential for development, and we should try to better understand it; then, by the MUH, this *may* tell us something about reality.[3]

As said above, *associative products* are another important piece of mathematics, which also should have a very big weight in our mathematical universe. Indeed, it is hard to imagine any process or motion without associativity. Pascual Jordan put forward the idea that the mathematical formalism of quantum mechanics could equivalently be based on the Jordan product $\bullet$, instead of the associative composition of operators or matrices. This might suggest that the weight of the Jordan product in our mathematical universe be even bigger than the one of associative products. On the other hand, the followers of *non-commutative geometry* would certainly defend that the weight of associative, but generally non-commutative, products, is much higher. If, as I said above, this weight somehow corresponded to "naturality" of a structure in mathematics, then the purely mathematical discussion of the mutual relation of these theories and approaches should tell us something important about reality. For instance, on these grounds, I would rather say that Pascual Jordan (and his co-workers) were completely mislead when wanting to find some "exceptional setting" of quantum mechanics, and even more beside the point is their deception when Adrian Albert discovered that there is no infinite-dimensional such setting: they should have taken this discovery as a signpost to continue on the right way, instead of a defeat leading them to abandoning![4]

[2] I guess that, for a long time, such kind of ideas has been living as folklore in maths and physics communities. As far as I remember, I first read some explicit version of it in Roland Omnès book "Converging Realities" [O2]. From its preface: *I suggest the name "physism" for the philosophical proposal that considers the foundations of mathematics as belonging to the laws of nature.*

[3] I don't claim to be the only person who considers differential calculus to be a topic of serious research – see comments in [RT], in particular on *synthetic differential geometry*. However, try yourself what you find about differential calculus in the msc2010...

[4] Cf. Kevin McCrimmon, *A Taste of Jordan Algebras*, where the story is told in that way, and some remarks on this in my book review.

2. Conceptual calculus, and elementary particles of mathematics

2.1. **Calculus.** I often feel embarrassed when explaining to other mathematicians that I'm working on differential calculus: how can such an old and well-understood theory be a topic of "serious" present-day research? What new stuff could be discovered there – everything is known, isn't it? In Sections 1.1 – 1.3 of [RT] I have tried to explain and motivate this research, by describing my way from "usual" differential calculus via *topological differential calculus* to *conceptual differential calculus*. The last stage is not yet fully accomplished, but the fundamental structure seems quite clear at present. Although much remains yet to be done, I will not subsume under "problems" the technical details and computations that remain to be worked out: this is more or less a matter of patience and time, but not a fundamental problem. To point out the problems I consider as "fundamental", and to make the following discussion more concrete, I will just recall from the introduction to [40] three formulae that suffice to cast the whole theory (see [RT] for some more explanations). In a first round, for U one may take an (open) subset in a $\mathbb{K}$-vector space or $\mathbb{K}$-module V, and, in a second round, a general manifold, by gluing pieces together. The first formula defines its *first extended domain*

$$U^{[1]} := \{(x,v,t) \in V \times V \times \mathbb{K} \mid x \in U, x + tv \in U\}, \qquad (2.1)$$

and the second formula goes with a theorem saying that the pair (with source and target projection, units, product and invesion defined as in loc. cit.)

$$U^{\{1\}} := (U^{[1]}, U \times \mathbb{K}) \qquad (2.2)$$

is a *groupoid*. To write the third formula, we first have to make a formal copy $\{k\}$ of the symbol $\{1\}$, for each $k \in \mathbb{N}$, and then iterate the whole procedure by defining inductively the following gadget

$$U^{\mathsf{n}} := U^{\{1,2,\ldots,n\}} := (\ldots(U^{\{1\}})^{\{2\}}\ldots)^{\{n\}}. \qquad (2.3)$$

Iterating the theorem mentioned above, one realizes that U^{n} is an *n-fold groupoid*. (Please don't be afraid if you have never met such animals: nor have I, not long ago. They don't bite!) I call it the *n-fold tangent groupoid of U*. The basic principles of topological differential calculus then imply that *a map $f : U \to U'$ is smooth if, and only if, it has natural (continuous) prolongations to groupoid morphisms $f^{\mathsf{n}} : U^{\mathsf{n}} \to (U')^{\mathsf{n}}$, for all $n \in \mathbb{N}$.* Studying the structure of f^{n} and the one of U^{n} go hand in hand: that is the heart of "conceptual calculus". See my lecture notes [CG] and the papers [37, 38, 40] for unfolding these statements in more detail.

2.2. **Understand the scaloid.** Although, as I said, writing up explicit formulae for the structure of U^{n} and of f^{n} is possible and just a matter of patience, I must admit that I am still quite far from having a comprehensive and good theory of the algebraic structure of the n-fold groupoids U^{n}. To illustrate the difficulty of the problem, and without exaggerating too much, one might say that such a theory would be a "theory of everything" – at least: a theory of everything concerning infinitesimal *and* local geometry of general manifolds or varieties. But you cannot get all this for free: digging details out of the general and abstract approach costs a lot of work.

To warm up, one may develop a simplified theory, called in [38, 40] *symmetric cubic*, which consists in fixing in each of the formulae (2.1), (2.2), (2.3) the value of the scalar parameter t, and so on: in the k-th iteration step we take a fixed value t_k, so that the symmetric cubic version of U^{n} depends on a parameter $\mathbf{t} = (t_1, \ldots, t_n) \in \mathbb{K}^n$. The resulting, *symmetric* cubic, theory is much simpler than the *full cubic* one: it deals with *edge-symmetric* n-fold groupoids. By letting $t_1 = \ldots = t_k = t$, and taking the limit $t \to 0$, we get back "usual calculus". In a way, symmetric cubic calculus is "universal" for base fields and rings of characteristic zero.

However, for really understanding what is going on, symmetric cubic calculus alone does not suffice: one needs "full cubic" calculus. Fortunately, all difficulties can be concentrated in a single object, which I've named *scaloid*: let us call *naked point*, and denote by 0 the zero-subset of the zero-$\mathbb{K}$-module $\{0\}$. By definition, the *scaloid* is the family of n-fold groupoids 0^{n}, for $n \in \mathbb{N}$. Of course, 0 itself is a trivial gadget, but 0^{n} is not: already $0^{\mathsf{1}} = (0^{[1]}, \mathbb{K}) = (\mathbb{K}, \mathbb{K})$ is not trivial as a set (although it is trivial as a groupoid), but $0^{\mathsf{2}} \cong \mathbb{K}^{\{2\}}$ is already non-trivial, both as set and as algebraic structure. Indeed, the theories of 0^{n} and of $\mathbb{K}^{\mathsf{n}-1}$ are essentially the same: 0^{n} is a non-trivial $(n-1)$-fold groupoid. It secretely enters into most computations related to U^{n}. The abstract reason for this is explained in [37].

Now, here is a question, admittedly speculative: let us assume that some version of the MUH is true; then the family of scaloids $(0^{\mathsf{n}})_{n\in\mathbb{N}}$ must correspond to "something real" – what is it? It must be something "very small", indeed corresponding on the level of base sets to a single point. The most natural speculation would be that it somehow represents "elementary particles", or maybe better: some "classification system of elementary particles". Namely, for each n, the scaloid 0^{n} is a collection of 2^n sets, each of which is a free finite dimensional $\mathbb{K}$-module, and the highest of these sets (the "top vertex set") is rather big – it has dimension $2^n - 1$. So there is plenty of room for interesting mathematics to happen: one may try to break up these spaces into "irreducibles", which each could play the rôle of some more specific type of "elementary particle" – for instance, each of these 2^n spaces certainly decomposes into irreducible subspaces under the action of certain symmetry groups. Such groups could enter the stage on the levels $n = 1, 2, 3$, and they could be related to the groups $\mathrm{SU}(3), \mathrm{SU}(2), \mathrm{U}(1)$ that physicists like to use in gauge theories. But let me say again, even if this speculation were entirely ill: for purely mathematical reasons, understanding the scaloids really is the key to understanding conceptual calculus, and these "elementary particles" may play a useful rôle in mathematics, even if physics does not want them.[5]

2.3. Discrete *versus* continuous, and difference *versus* differential calculus.

It seems to be a mad idea to develop "differential calculus" over discrete, possibly finite, fields or rings: you can do *difference calculus*, that is, look at finite difference quotients, but there is no way to take their "limit". Nevertheless, if you have not yet abandoned reading this text, you should be prepared to find it less crazy: the "conceptual" approach is algebraic in nature, and it suffices to upturn things.

[5] This discussion is related to the "functor of points approach" from algebraic geometry, but should not be confused with it. As far as I see, the scaloid never appeared in algebraic geometry. Explaining the precise relation of these theories with each other is part of our problem list.

Indeed, this can be done ([38)]): in topological calculus, we used topology to prove certain algebraic properties; now let us take these algebraic properties as starting point. That is exactly what algebra people do with *polynomials*: they replace the analytic concept of a *polynomial function* by the algebraic concept of an abstract polynomial.

But why should one want to do this? First, there are purely mathematical reasons: e.g., modern algebra cannot do without abstract polynomials, for good reasons, and likewise, some day, conceptual analysis will not waive abstract smooth functions neither. For present day analysts this day may seem still very far away, and all this will sound exotic and extremely formal. But, second, let's add some speculation: quantum physics suggests that the universe be discrete in nature; so, if we want to continue using differential calculus in physics, we may need something like "discrete differential calculus". Maybe there are other, better, propositions of how this can be done – but it least it seems to me that the "conceptual" approach raises questions that have to be answered in this context. For instance, what is the rôle of *infinity*? As far as I see, "infinitesimal calculus" needs to use "infinite mathematics" somewhere. In usual, topological calculus, this infinity comes in via the (uncountable) infinity of the point set underlying the topological spaces (real manifolds, typically), and this infinity allows to recover all desired limits, as usual in analysis. If the underlying point set is discrete, or even finite, this is no longer possible. Then either we must give up the idea of "derivatives of all orders", and content ourselves with the poor information of zero-jets, or we store the infinite information somewhere else: in a sort of "attached file" to the possibly finite base space M, we carry along all higher order finite spaces $(M^{\mathsf{n}})_{n\in\mathbb{N}}$. The (uncountable) infinity of the base set is now replaced by letting each point be carrier of a (countable) infinity of information. Thus, there are two different kinds of infinity in play, and even in a "physically finite" universe we will need infinite mathematics.

Of course, pursuing this approach, a lot of new problems will appear. For instance, relating the differential notions of *length and volume* with their local or global analogs, in a discrete space, raises entirely new questions – the answers from usual real differential geometry do not carry over. To add some more speculation: it would seem unreasonable to give up topological notions completely; one may rather wish to "combine" both approaches, using some discrete-valued distance function on the base space, in order to modelize quantum foam; and doing so, the "ratio" between the two kinds of infinity mentioned above should be some fixed value, resembling something like a "Planck constant" associated to a discrete space.

2.4. **Connections.** The main progress, and the main challenge, related to conceptual calculus is to understand the link between *infinitesimal* and *local* (or even: global) features of "space" in an algebraic (conceptual, chart-independent) way, and not, as in the usual approach, in an analytic way, via some chart-dependent topological "limit process". Once you start looking at things this way, you will wish to do something similar with various structures appearing in differential geometry. The most important of these are probably *connections* (affine, principal, or whatever): you will wish for a *local (or global) concept of connection.* Remarkably, such a theory already exists, although it is hardly known, even among specialists: L.V. Sabinin has developed, in a long series of papers, an approach to (affine) connections based on the algebraic concepts of *loops and quasigroups*, see his book *Smooth Quasigroups and Loops*. On page 5 of his book, Sabinin writes: *Since we have reformulated the notion of an affine connection in a purely algebraic language, it is possible now to treat such a construction over any field (finite if desired)... Naturally, the complete construction needs some non-ordinary calculus to be elaborated.* You will not be surprised reading me claim that Conceptual Calculus is precisely the "non-ordinary calculus" that Sabinin was dreaming of, and that nothing prevents us now from putting his conjectures into practice. Moreover, doing this should shed new light both on his theory, and on conceptual calculus itself. If ever you have the courage to read his book, you will agree that it would be useful to reformulate things in a more modern and conceptual way (he heavily uses coordinate-computations in which one quickly gets lost). I've been working on this since quite a while. The manuscript is still unfinished, both for reasons of time, and because there are still several fundamental issues to be better understood. See also Section 6.3.6 of [40].

2.5. **Basic symmetries: CPT.** An observation that may be pure coincidence, or not: the most basic symmetries of the tangent groupoid $U^{\{1\}}$ (and hence also of all higher groupoids U^{n}) strongly resemble CPT symmetry from physics:

(C) is inversion in the groupoid $U^{\{1\}}$, given by $i(x,v,t) = (x+tv, -v, t)$; as every groupoid inversion, it is an antiautomorphism: it exchanges α and β, and the groupoid product $*$ and its opposite product $*^{\mathrm{opp}}$.

(P) is the map $f^{\{1\}}(x,v,t) = (-x,-v,t)$ induced by the spacial point reflection at the origin, $f : V \to V$, $x \mapsto -x$. In fact, for each point $p \in V$, the point reflection at p induces such a groupoid automorphism. The collection of point reflections encodes the structure of V as a *symmetric space*, that is, V together with its natural flat connection. But the same definitions could be made for symmetric spaces *with* curvature, or even for any affine connection.

(T) is the map $\sigma(x,v,t) = (x,-v,-t)$ which is the same as the *rescaling automorphism* Φ_s for $s = -1$, as defined in [38, 40].

Is there something special about the anti-automorphism CPT that sends (x,v,t) to $(-x-tv,-v,-t)$? I don't know.

2.6. **Supersymmetry?** I have never seriously worked on (mathematical questions of) supersymmetry. But, knowing people working in this domain, and since the formalism is interesting and intriguing, I have regularly read books and papers about this topic. The main reason distracting me from going to work in this domain is that its principal thread, the "sign rule", lacks a more profound motivation: the motivation mentioned most often is that "it works", but I would like to know, *why* does it work? For my taste, the most natural, and most satisfying, solution of this question would be that supersymmetry is a kind of answer to the $\Box$-versus-$\Diamond$-dilemma that I mentioned in the very beginning. Whatever you do, there is no reason to prefer $\Box$ or $\Diamond$. And, even if you have fixed your choice, there is no abstract way to "communicate" it: you can communicate it only by using physical procedures (like those called "notation", using ink and paper, or computers). Thus the dilemma seems to be not only a mathematical one, but rather a joint problem of maths and physics. Taken seriously, this remark should have a number of consequences, among them supersymmetry.

Of course, you may hope that, in the whole of maths, a "usual" composition, named $\circ$, somehow will win the game – that is, you may hope there is some kind of "mathematical parity violation". But to me that would be a great surprise! Rather, my feeling is that the "axiom" saying "$\circ$ exists" is comparable to Euclide's parallel postulate: logically sound, but independent of the remaining structure, and there may be life beyond it (non-Euclidean life. Finally, that question was settled from the physics side.) As long as we don't know, and in order to better understand the logical status of a possible mathematical parity violation, I propose to work with local universes: small sets M, the small groupoids M^{n} built on such sets, and so on, step by step. Since (bijective) mappings $f : M \to M$ are nothing but (bi)sections of the pair groupoid $(M \times M, M)$ of M, you eventually get all maps that you may be interested in, and you avoid speaking about "all maps in all of mathematics".

To make these speculations more concrete, juste note that, exchanging the "usual" composition and its opposite, amounts to exchange "everywhere in mathematics" the target map β and the source map α. To make sense out of this, let's do this already on the level of our groupoids $M^{\{1\}}$: what happens if we exchange source and target there? Well, everything seems to look as before: we just have applied the inversion (C) from above, which is an automorphism from the groupoid onto its opposite groupoid. (But maybe time (T) is now going backwards? Or are we reading it backwards?) Anyhow, forgetting motivation of supersymmetry from physics, one would like to know for purely mathematical reasons how a theory like conceptual calculus behaves if we exchange source and target, at least for a fixed "universe" M: what happens if $M^{\{1\}}$ is replaced by its opposite groupoid? and if we do the same exchange when applying the next copy $\{2\}$: combining, do we get $2 \times 2 = 4$ different versions of M^2? and so on: do we get some kind of $(\mathbb{Z}_2)^n$-grading on M^{n}?

3. Geometries, Algebras, and Coquecigrues

While the preceding section was about a part of general language of mathematics (differential calculus), let's now turn to something more specific:

3.1. **Coquecigrues.** The coquecigrue is a specious and farcical imaginary creature mentioned first by Rabelais in "Gargantua". The wikipedia page does not mention the use of this word in mathematics: indeed, it is not part of the official language of maths, and I use it to render homage to Jean-Louis Loday, who not only liked mathematics but also associating colorful and fancy words and names to our activity. So, here is the "general coquecigrue problem": *given some class of algebras (defined in the sense of universal algebra by identities, such as: Lie-, Jordan-, associative, alternative, or other kinds of algebras), is there a correspondence between algebras of this type and certain geometric objects (the coquecigrues), which sort of "integrate" the algebra?* As far as I know, Loday never raised the problem in this degree of generality, but just for a special kind of algebras (the Leibnitz algebras). Of course, he had in mind the best known such correspondence, the one between Lie groups and Lie algebras, and he was quite enthusiastic when I told him (later) that for *Jordan* algebras the coquecigrue more and more transmutes from an imaginary creature to a real one – and hopefully no longer appearing specious and farcical to the common mathematician. Although I'm aware that the "general coquecigrue problem" sounds a bit vague and ill-defined, I'm inclined to think that it is well-posed and has a solution. However, at present there is not even the slightest beginning of a general theory dealing with the general problem, but only a collection of examples where the coquecigrues are more or less tamed and have entered into the zoo of respectable mathematical beings. Thus, before attacking the general problem, it seems wiser continuing to glean more and more animals in order to better understand this zoo.

3.2. **The associative coquecigrue, and NCG.** Noncommutative geometry (NCG) is a mathematical theory started by Alain Connes and concerned with a geometric approach to associative, noncommutative algebras, and with the construction of *spaces* that are locally presented by them (possibly in some generalized sense). Its dream is to generalize the classical duality to the duality between noncommutative algebras and geometric entities of certain kinds, and give an interaction between the algebraic and geometric description of those via this duality. In other words: the dream of NCG is to find and tame the "coquecigrue of associative (possibly non-commutative) algebras"! Well, it's not – this would be a profound misunderstanding. See Section 2.4 of [RT] for some comments on this point: there are two powerful paradigms in maths, relating geometries and algebras, the "Lie paradigm" and the "Grothendieck paradigm", to pin names on them, and you cannot follow both of them at the same time (at least, not today). This does not mean that one of them were "wrong": rather, they seem to be "complementary", shedding light on mathematical structures in a very different way. I would be very interested to know the opinion of NCG-ers on this, but so far I did not succeed.

3.3. **The geometry of quantum mechanics.** The rest of this section is devoted to speculations. Quantum theory, the heart of present day physics, has been formulated axiomatically by Paul Dirac and John von Neumann, with some subsequent updates by them and by other persons, as an algebraic theory in terms of associative (C^*-) algebras, or (P. Jordan: see above) in terms of Jordan algebras, or (G. Emch) in terms of Jordan-Lie algebras. Given that quantum theory can be formulated axiomatically in the framework of some class of algebras, and given a positive answer to the coquecigrue problem for that class of algebras, it follows that *the axioms of quantum theory can be formulated in a geometric way, in terms of the geometry of the "coquecigrue"*. This is a purely mathematical result; it remains of course an open question if this axiomatic geometric formulation leads to new insights. Assuming some variant of the MUH, there may be arguments in favor of that; I have written them up in my texts [23] and [O8], and the interested reader should have a look at these. As a mathematician, I find it quite curious that there are over twenty "interpretations" of quantum mechanics, and thousands of pages of discussion which one is the "correct one", but the Dirac-von Neumann axioms seem perpetual and out of range of questions. Maybe after another 2000 years shall we find, as for Euclide's axioms of geometry, that they have some flaws, and that we can play around with the fifth postulate and get something new and very interesting? In this sense, proposing a "coquecigrue approach" is just playing around with mathematics. Maybe we will find something, or maybe not.

In the following, let me briefly take up those topics where my regard has changed since I wrote [23] and [O3], be it because of mathematical progress made meanwhile (in particular, better understanding of associative geometries, which I now prefer to Jordan geometries) or because of reading other books and other opinions (in particular, I would like to mention books by David Deutsch and Carlo Rovelli).

3.4. **Duality.** *Duality* is a pervasive feature of our most fundamental mathematical theories – see, e.g., this beautiful paper by Michael Atiyah. If you look at the wikipedia list of dualities, you find that mathematics is most fond of cooking them in various ways, followed by physics, whereas in philosophy there is basically "just one duality", which then re-appears in various implementations. For people interested in science, maybe the most salient implementation of these philosophical dualities is the "mind-body dualism": as in particular David Deutsch stresses in his books, the activity of the mind, and the role played by "information", become more and more a part of physics, and having an increasing influence on the cosmological evolution. Sooner or later, physics will have to take seriously account of this – and hence maths, too, if we believe in the MUH!

My own speculation in this realm is that, also in physics, there should essentially be just *one*, fundamental, duality, and which should translate the most classical duality of mathematics: the duality between *(topological) vector spaces* V *and their dual space* V', or its geometric avatar, *duality in projective geometry.* Mathematically, this duality is one of the origins of the *Jordan Pair* concept by Ottmar Loos, whose geometric avatar is in turn the *duality of generalized projective geometries*, as explained in [23]. One advantage of a geometric coquecigrue-description of quantum theory could be that the rôle of duality becomes much more eminent and fundamental: you really better understand projective geometry if you learn to distinguish a space and its dual space, even if at the beginning you were only interested in "space", and not in its "dual space". This is precisely what Roland Omnès in [O], p. 527, says about physics: *The pure philosopher may start from a postulated unity and call it Being. He may then concede the necessity of distinguishing two modes of being and call them reality and logos, or whatever else... A physicist groping with his science is after all following the same path.* In [23], I proposed the terms *observable* and *observer* for elements of space and dual space. Of course, by "observer" is not meant a human being. I just propose to concede the necessity of distinguishing two modes of being, and hope that this may help to better understand and describe our theory of reality. Since the mind-body problem belongs to reality[6], it may be hoped that we get a bit closer to understanding it.

3.5. **Monism.** Dualism is not the end of the story. I do not want to "decide" between dualism and monism, or triism, or polyism. On purely mathematical grounds, the distinction between these isms can be questioned. Indeed, this is a beautiful lesson that mathematics can tell philosophers: Plato, and every other geometer,

[6] I cannot abstain from quoting Hermann Weyl at this point ([W], p. 215): *The body-soul problem belongs here too. I do not believe that insurmontable difficulties will be encountered in any unprejudiced attempt to subject the entire reality, which undoubtedly is of a psycho-physical nature, to theoretical construction.*

would concede that duality in projective geometry is an extremely useful and beautiful principle, and he won't waive it. However, there is one special case where this duality breaks down, and the distinction between space and dual space really becomes a fiction: this is the one-dimensional case, the case of the *projective line*. A hyperplane in a projective line is a point, and so the "space of hyperplanes" then really *is* "the space of points": space and dual space literally are the same. You may say that this case is degenerate and uninteristing. This is true for the ordinary projective line $\mathbb{RP}^1$, or $\mathbb{CP}^1$. But there are other, very interesting "generalized projective lines": namely the *projective lines over possibly non-commutative algebras* $\mathbb{A}$, denoted by $\mathbb{AP}^1$ (see [O8] for their definition). And much more: these generalized projective lines, and their associated "Hermitian projective lines", turn out to be precisely the coquecigrues belonging to associative (involutive) algebras, hence are exactly the "geometric home" for quantum theory! Thus we end up with an apparently paradoxal situation: we have to distinguish "space" and "dual space", but in the end we find out that the distinction is a (useful) fiction. The lesson is that sometimes the "same" thing has to be seen double if you want to understand it. Mathematics tells us that there is no logical contradiction about this.

3.6. **The relational aspect.** Relational quantum mechanics (RQM) is an interpretation of quantum mechanics which treats the state of a quantum system as being observer-dependent, that is, the state *is* the relation between the observer and the system. This approach has been developed by Carlo Rovelli since 1994, and presented to a large readership in some recent quite popular books. It would fit very well with our geometric coquecigrue-approach, and possibly even gain in acuteness: a point ("state") should be understood not as a "point x in space", but as a *pair* (x, a) consisting of a "point x in space" and a "point a in dual space" (observer). The "space of quantum mechanics" is the space of such pairs (x, a), *i.e.*, the cartesian product $X \times X'$ of space X and dual space X'. The space-point x can only be described via its relation to an observer-point a. Indeed, as in usual projective geometry, the dual point a (hyperplane) defines an "affine chart" of the point-space X, and only the chart-picture is accessible to physicists (measurements, description by numbers, etc.). And vice versa.

According to RQM, the error of the standard description of quantum mechanics is to consider one observer to be fixed once and for all, and to consider all others as unreal fictions. The geometric picture might carry this approach even further than Rovelli did: as Rovelli was inspired by the analogy with *special* relativity, the coquecigrue-approach would suggest an analogy with *general* relativity: the whole structure is a geometric, *non-linear* object; the affine and linear structure also is observer-dependent, as is the case for true, "curved", manifolds. Different observers may observe different linear structures. (Here it is important to use the precise mathematical language: the linear structures may be *different*, but they will always be *isomorphic*. Possibly, much confusion in mathematical physics goes back to not carefully distinguishing between "different" and "non-isomorphic": "being identical" is context-depending – it depends on the category you are working in).

3.7. **How many worlds?** I must admit that, until quite recently, I did not really take seriously the Many-worlds interpretation (MWI) of quantum mechanics – if at all the "interpretation" of quantum mechanics was mentioned as a topic, my Göttingen teachers rather depicted some abridged Copenhagen interpretation as the final state of the story (the shadow of Heisenberg over Göttingen... and maybe even more the one of his student Carl Friedrich von Weiszäcker, who is hardly known outside Germany, but whose books had highest ranking and large readership in old Western Germany). I still feel unable to judge and to "choose" between various "interpretations" of quantum mechanics; but I find convincing the arguments in favor of the MWI formulated by David Deutsch in his books.

As a mathematician, I would be even more convinced if the MWI came out as a natural way of reading the mathematical structure of quantum theory – and in this respect, my hope would be that playing around with the axioms of quantum mechanics, as said above, would clarify the picture, just like Hilbert's "Grundlagen der Geometrie" clarified the picture, more than 2000 years after Euclide. Playing around with a geometric "coquecigrue-approach" might open interesting possibilities, and at least it may give an indication into which direction one might play the game. In fact, the arguments supporting a compatibility with MWI are the same as those supporting a compatibility with RQM, so that, in the end, I wonder if MWI and RQM are really so much different from each other. Indeed, what would distinguish the "world" from the "world observed by an observer"? Our world is the world observed by us. And hence, if we agree that there are many observers, and that our description of things really is depending on the observer, we must also agree that there are many worlds. (Let me say once again that by "observer" is not meant a human being, but just a mode of being belonging to the dual space X' of the point space X. Because of the problem of self-reference, human beings are "too big as categories" to be treated in the language of small sets and small categories. Or, to put it once again with the words of Hermann Weyl, [W], p. 215: *The real riddle, if I am not mistaken, lies in the double position of the ego: it is not merely an existing-individual which carries out real psychic acts but also a 'vision', a self-penetrating light (sense-giving consciousness, knowledge, image, or however you may call it).*)

4. Conclusion

I think this is enough for today. Comments are welcome.

W.B., Villers-lès-Nancy, june 2017

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