How to learn to ask good questions in physics.

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February 26, 2012

History of physics is full of psychological and philosopical moodswings: in the middle ages, nature was mystical and magical, from the 18th century onwards an irrational rationality installed itself culminating in the arrogance of modern science that the entire world can be described in a symbolic language. Of course, a notorious austrian Kurt Godel put an end to such aspirations at the beginning of the last century which ultimatly drove him to the paradoxical attempt to prove the existence of God. Godel, from all people, should have understood that some things are beyond proof and classical logic and that irrationality is the very foundation of everything we do and accept as true. There were other giants at the beginning of the previous century who were contemplating similar topics such as Albert Einstein and Wolfgang Pauli. Einstein was obsessed with what is but at the same time the old man was wise enough to use *words* to describe his thoughts; he threw his equivalence principles and the principle of general covariance into the physics world while never caring to really *define* them. Of course, when writing a paper, you better make something precise so that your collegues feel comfortable that they have a chance to understand and to judge you and during the process, they might even get the entirely false feeling that they actually understand what you are saying. Specifically, Albert used the language of manifolds and of Riemannian geometry to realize his ideas and in one breath gave away the field equations any graduate student knows today. So, people today believe that they know what local Lorentz covariance and general covariance *mean* while this understanding is at most contextual within a certain mathematical representation. Einstein never cared too much about representations because from relativity he evolved into general two covariant tensors (gravity with an antisymmetric tensor), theories with an affine connection (Einstein-Cartan theory) and later on, he even contemplated the discrete manifolds Riemann thought about before. There remains an infinity of other possibilities to be explored and Riemannian geometry is just the beginning of the road; again, the crucial thing here is that Einstein valued his undefined principles much more than all the easy mathematics which followed.

Likewise it has been so with quantum theory; the ideas of the founding fathers such as Heisenberg, Von Neumann, Dirac and Wigner suffered the same destiny. This is a social phenomenon which is grounded in the simple fact that when ordinary people write about the thoughts of a genius, they never are able to capture the full richness of the exposed ideas. Hence, a lot of information gets lost and people start actually to narrow their scope until a new genius comes along and slaps them on the wrist. To illustrate this phenomenon, let us consider the example of Johnny Von Neumann who united Heisenbergs with Schrodingers approach to quantum physics. Now, Von Neumann did not do anything really special at first, he simply observed that both approaches could be written within the language of densly defined self-adjoint operators on Hilbert space. He was himself very aware of this and later paved the ground for the more axiomatic approach of quantum logic which has been extended by Constantin Piron later on and which is still subject of study in the Brussels-Geneva approach to quantum mechanics. Likewise, he suggested the possibility of a quaternionic Hilbert module; an idea which has been developped in the margins by Finkelstein, Jauch, Adler and others. Von Neumann was what I could call a finitist, he was fascinated with bounded operators and constructed weakly closed subalgebras of the full $C^{\star}$ algebra of bounded operators on Hilbert space H. Likewise was Weyl: his stomach must have felt rather akward when dealing with subtle domain issues, symmetric densly defined operators and self-adjoint extensions of such exotic beasts. Weyl felt much more comfortable in his finite world and simply exponentiated these operators so that he found himself into the realm of the safe unitary kindergarten. Heisenberg however took Cantor's and Godel's lesson for real and tried to understand infinity when it came to him; later on in his life he appears to have been playing with even more essential unbouded operators in Nevanlinna space (indefinite norm). Mathematicians such as Krein never accepted this kind of exotics and Krein space is still too close to Hilbert space as it stands. The tension between the finitists and people who understand the meaning of infinity culminated when the standard model was developped including its machinery of renormalization. Now, this is the kind of infinity Dirac did not sign for and I don't think Heisenberg did either but I am not sure here. Evidence for my position here is that Heisenberg was developping a radical spinor theory with plenty of non-renormalizable terms. My point being here is that all the eminent founding fathers were aware that Hilbert space was not adequate nor compelling, the $\mathbb{C}$ numbers were not that special as they are generally believed to be, the approach embracing unbounded (and even later on distributional) operators has been proven to be the most fruitful and as an aside, consciousness had an essential role to fullfill in the quantum formalism.

Combining both these insights coming from relativity and quantum mechanics, mainstream approaches towards quantum gravity seem like to be wrong from the start. Indeed, we all pretend to have an axiomatic grounding for quantum mechanics, but people knew already 80 years ago we didn't and nothing much changed since then. As an example, one can cite the attempts to formalize quantum field theory; the most well known and recent mainstream books in that direction probably constitute Steven Weinberg's excellent trilogy [2]. Weinberg uncritically accepts : (a) complex Hilbert spaces with a countable basis (b) an associative and well defined operator multiplication (c) the validity of the principles of causality and (d) cluster decomposition. He does not discuss in depth the consequences of Haag's theorem and presents the Coleman-Mandula theorem as a serious blow to theories unifying in a nontrivial way representations of the Poincaré group with other symmetries in nature. The virtue of Weinberg's presentation, however, is that it is clear and allows for deeper insight where to modify assumptions he takes for granted if one wishes to avoid some of the conclusions he is getting to. For example, he derives the spin statics theorem from the following principles: (a) causality (b) cluster decomposition (c) Fock space

(d) statistics "theorem" (e) positive energies (assuming Minkowski spacetime). It is well known that if any single one of these assumptions fail, excluding (d) of course, there is no theorem anymore and particles with spin 1/2 could be bosons as well as fermions. I am not aware however that the following suggestion has ever been made: Weinbergs proof also shows that the reverse holds, that is (b),(c),(d),(e) and spin statistics imply causality. This relation appears to be much more robust since one can drop some assumptions here such as weakining the notion of Hilbert space to Nevanlinna space. Hence, this would degrade causality to a mere computation and not a fundamental principle. This appears the right way to go since quantum causality as we know it in a spacetime background makes no sense in quantum gravity. The point is that we do not even know the right principles for quantum field theory on Minkowski! In curved spacetime the situation is considerably worse, it is known for at least 35 years that free quantum fields can only be defined in the context of spacetimes with a timelike Killing symmetry. This inherent limitation of quantum field theory is deadly for quantum gravity; recently a formulation of free quantum field theory for scalar particles has been given on a causal set which avoids this limitation and therefore, the Feynman propagator is uniquely defined here.

Now, what has this all to do with the question of this contest you may ask ? Why did I indulge in repeating some too often forgotten or misunderstood history ? Because I wanted to sharpen up the question and therefore also make my answer more comprehensible. Einstein suggested that quantum mechanics is not complete and should be supplemented with hidden variables, but what is never said is that Quantum Field theory is a theory of hidden variables. Indeed, reality consists here in Fock space, an ill defined Hamiltonian (except when the theory is free) and some inertial reference system which we imagine to be tied to the observer which is not described by our quantum theory of "everything" on flat spacetime. Apart from the fact that Quantum Field Theory does not allow for measurement of particles inside the universe, the rigorous description of infraparticles and so on, the interpretation of the asymptotic state is still contextual. That is, what we imagine to be particles and states with a definite particle number are not always what they appear to be. In theory and probably also in practice, one may construct detectors such that a "single photon state" really gives two particle clicks instead of one. This puts a death sentence on approaches like Bohm-de Broglie whose practitioners would have to squeeze themselves in all corners to affirm that a measurement of two particles with the ontology of a single particle is not a logical contradiction, but a mere paradox. So quantum mechanics says that reality is in a bi-module over some ring (to generalize immediately the quantum formalism) and in right linear distributional operators over it represented on some fixed spacetime. So, if quantum mechanics were universally true, we should also be in this structure but then we meet the "logical incompleteness" of how to measure oneself from within. If we are within the state, then a mere state description is incomplete because that simply gives an arrangement of elementary particles; it doesn't contain information concerning nonlocal variables such as human, table and so on. I mean, an outside conscious observer can assign such nonlocal variables, but from the inside this is impossible. For example, there is nothing in the formalism which even allows for an identification of the same cat within a superposition of monomials in creation beables applied to the vacuum state (even on Minkowski) since this would require the extra imput of catX observable as well as the interpretation of it. But then, one arrives at the conclusion that in order for such system to work, the theory should be supplemented with observables for all living entities which were and will be in the entire universe. Still, one is left with the issue of consciousness, what or who projects my state to the state of observation of my computer screen. Is this a kind of personal consciousness or is it a universal awareness such as religious people imagine God to be? It is impossible to prove such scheme wrong by experimental verification and therefore many people are content with this situation. However, science does not operate like this: there is a more stringent criterion called Occam's razor, which values theories by means of a balance between their assumptions and validations. The picture above is infinitely open and hence a complete description does not exist: indeed, it appears that the whole of Platonic space should be represented in operators and there is no way to describe the former. Humans would be drones and the Mona Lisa would be ordered in the initial state of the universe by means of a Mona Lisa projection operator; moreover, there would exist something outside spacetime which we have no means of describing at all. Suppose moreover now that spacetime itself is quantum, then this super observer not only measures me and you, but also the causal relations between us. But how is this possible ? We never ever measure causality, a spacetime metric or a distance between two points; these are all imaginary concepts which are needed to discribe the quantum world but they are not observables in the quantum mechanical sense. They are "classical", meaning non-operational, hidden variables; so the principle of superposition does not apply to them. However, there is still no logical contradiction, it could be that a super observer measures these things but that we as humans are incapable of appreciating this because our brain states do not contain this information. Again, I believe this is an unrealistic attitude and therefore my answer to the question whether spacetime is digital in the sense that it is measured somehow as we measure particles is a resounding no. Of course, we still did not make full use of the information in the first three pages and we proceed on the road to exploit this to a full extend. So, we came to the conclusion that spacetime has a pure ontological status; now we have to ask what attributes we might wish to impose upon its structure. In particular, we may wonder wether it is (a) a continuum (b) discrete (c) neither of both or (d) it does not matter what it is. We could broadly assume Einsteins attitude here and say that it is not important what it is, that it might be a matter of representation of deeper underlying undefined principles which do not need any backbone whatsoever. I think that this is unlikely and I will try to say something more intelligent about this later on. So, let us concentrate for now on options (a), (b) and (c). One always has to start out conservative and study if there are compelling reasons to give up the "easiest" option, that is (a). Typical reasons which you hear are the following : (a) since all Quantum Field Theories require a UV regulator, there should be a natural physical mechanism to provide an upper scale of observation, therefore one has a shortest distance scale which suggests discreteness (b) atoms are discrete and quantum theory is all about atoms, therefore spacetime should be discrete (c) Brownian motion and quantum diffusion can be modelled by a (quantum) random walker which requires a discrete lattice like structure (d) path integrals are only well defined on a finite lattice and regularization happens while taking the continuum and thermodynamic limit, which might indicate that the lattice is more fundamental than the spacetime is (e) (classical) gauge theories have a natural representation in terms of Wilson loops and electric field variables (surfaces), so spacetime is a relational theory of lines and surfaces. Anyhow, these are the ones which immediately pop up in my mind and we shall continue now to deconstruct every one of them carefully. I think (a) is the most important reason and we have prepared the ground for a in depth discussion of why renormalization problems do not force us to abandon the continuum but allow for more conservative and controlled strategies. Renormalization is in my view a problem of field theory and not of the continuum, so logically we have to abandon one of the premises behind field theory. As indicated before causality is a good candidate to sacrify and the cluster decomposition principle will be violated generally too. The latter is indeed in conflict with the Machian principle in which everything is connected to everything else certainly in the bulk of spacetime where real measurements happen. Also, traditional Fock space will be replaced by a generalized Fock like construction allowing for negative norm particles. In this context, it is natural to extend quantum mechanics beyond the real, complex and quaternion numbers to generic Clifford algebras; this will involve negative probabilities automatically. I believe locality can be saved and it is worthwile to understand what happens if we give up the continuum. In causal set theory for example, locality and cluster decomposition fail just as unitarity does. It is a this moment even unknown what quantum causality means within this context so asking the question is not even opportune at this moment in time. Let us go further into (b), atoms are not "discrete" at all, actually all representations of the Poincaré e group have a mixed unbounded continuous and bounded discrete spectrum asso- ciated energy-momentum and spin respectively (within a single particle species). Discreteness is only an approximation in the semi-classical limit where fermionic particles are quantized but the gauge field is static and classical. So, I dont see why atomisticity should imply discreteness at all. Concerning (c), these results merely concern alternative ways to understand an equation which is well defined in the continuum, likewise is (e), so again this is far from being a compelling argument. Perhaps the best argument is still (d) but this is in my opinion a "problem" of path integrals and not of physics. Now, let us study negative arguments why alternatives to the continuum are not appealing: (a) the issue of local Poincar é invariance becomes problematic, in causal set theory for example this is a statistical statement concerning sprinklings of points in Minkowski, there is no intrinsic causet notion of local Lorentz covariance at all (b) in approaches like causal dynamical triangulations, local Lorentz covariance is clearly broken at the bones (c) discretization sacrifies many things as we have just seen and it hasnt rewarded anybody in a substantial way until now.

Of course, showing that abandoning the continuum is troublesome and that there is no compelling reason to do so of course doesn't prove that the theater of spacetime is analog. Let us assume a postive attitude now, in the spirit of Einstein that it might all just be a matter of representation (perhaps locality has to be given up too), towards non-manifold like structures and see what they might imply. There exists only a limited number of acceptable structures between discreteness and the continuum which are :(a) the natural numbers (b) the rational numbers and (c) the p-adic fields for p a prime number. The rational numbers differ from the natural ones in the following aspects : (a) $\mathbb{Q}$ is a field (b) $\mathbb{Q}$ is totally ordered but there is no numeration of elements of $\mathbb{Q}$ preserving the order. So, on $\mathbb{Q}$ we can perform a lot more arithmetic and it doesn't contain any holes like $\mathbb{N}$ does. The p-adic fields are number theoretic completions of $\mathbb{Q}$ with respect to a particular norm attached to a scale a. For the p-adic completions, one sees that the norm is not an Euclidean one: that is, it cannot be derived from a bi-linear scalar product. This destroys the properties of quasi-local linearity in the continuum and must have far reaching consequences what the small scale geometry is concerned. In such cases, it might be natural to give up the bi-linearity properties of the scalar product in quantum mechanics too which would seriously complicate the probability interpretation and again, I do not see any indication that something that radical is really needed. Locality is a notion which is tied to the continuum and for a certain extend to $\mathbb{Q}$; everything else which moves apart from this does appear to violate this sacrosant principle. So, it is possible to formulate a physical principle which does exclude all these exotic spacetime structures and therefore it seems that the continuum has a deeper meaning. Now, one may ask if this also applies to the manifold structure; here I see some possibilities for generalization. The original idea of a manifold basically is that it locally looks like a real vector space which is of primordial importance since it allows one to add vectors and perform scalar multiplication. These concepts are required to express things like curvature tensors and so on; although the notions of scalar and sectional curvature bounds can be generalized to general metric spaces, a real local definition of scalar curvature is not possible since the necessary limiting operation is usually not well defined. However in quantum mechanics, we can do more than just add, we can also multiply (since one disposes of an algebra of operators) and likewise, one could change the local spacetime vector space structure over R to a local Clifford or Grasmann algebra structure over $\mathbb{R}$. This is already done partially of course in the context of supermanifolds, but this line of thought may be extended to more general structures. To stress the importance of local algebraic structures, let me launch the following idea: in the deformation approach to quantum mechanics one has the ordinary commutative product structure and the deformed Moyal star product as implementing the canonical commutation relations. Indentically, geometry may be seen as a deformed nonassociative and noncommutative sum sructure on the vectorfields X, Y ; indeed, define

$$X \oplus Y = X + Y + \epsilon \nabla_X Y - \frac{\epsilon}{2} [X, Y]$$

for some $\epsilon > 0$. Then, the commutator $X \oplus Y - Y \oplus X$ is proportional to the torsion tensor and a nonzero associator is related to the Riemann curvature. This suggests that quantum gravity may be founded on algebra's with a deformed sum and product structure. So, I believe there are compelling reasons why spacetime must be analog and representational ambiguities do not appear to arise at this level although the concepts of manifold, general covariance and local Poincaré invariance may have infinitely more useful implementations which remain currently unexplored.

So, again, infinity (in terms of the infinite small) seems to vindicate and the finitist attitude is not as deeply motivated as one may think and problematic to say the least. Therefore, the remaining useful principles to construct a theory of quantum gravity are (a) spin-statistics (b) local Poincaré invariance (c) a generalized Fock module bundle (d) general covariance and (e) locality. Causality and cluster decomposition are both abandonned and the degree to which they are violated depends upon the semiclassical gravitational field. Indeed, as we have suggested, the problems of Quantum Field Theory reside in some very basic principles which need to be given up, but the continuum isn't one of them. So far, we have argued our case only from the perspective of quantum field the- ory; are the infinities showing up in relativity imposing us to reconsider the continuum ? Likewise, the answer is a resounding no and the issue may be dealt with in several ways. First of all, there are the standard Penrose-Hawking singularity theorems which apply only to Riemannian geometry. They do not apply to the affine connections used in Einstein-Cartan theory and Trautman has provided evidence that the big bang singularity dissapears in a gravitational theory with torsion. Intuitively, it is clear why this should be the case, torsion is associated to angular momentum and the latter offers "resistance" to the gravitational attraction. Therefore, by restoring the full local Poincaré group and by not merely taking into account the translations (diffeomorphisms), the singularities might evaporate in thin air in a classical setting. A second attitude to infinities in general relativity is simply accepting them: in my view they do not pose any problem for the classical theory at all but they merely destroy the modern view on the theory. Indeed, I have heard too often people express the view that "silly" Einstein was not aware of the superior formulation of relativity in terms of action principles which allow for a more direct calculation of the Einstein tensor and the conservation equations than by contracting the second Bianchi identities. Well, in my view, it is clear who is "dumb" here and it isn't Einstein; let me explain why. Einstein's formulation in terms of the field equations is a truely local one and suggests more a boundary value point of view rather than an initial value perspective. The manifold is not fixed at all in this picture and may be seen as an evolving entity by pasting together local coordinate patches. Nowhere does the boundary of a coordinate chart enter the formulation, the field equations are constraints and nothing more. A Lagrangian point of view is nonlocal and specifies the manifold $\mathcal{M}$ immediately from the beginning; moreover, the action needs a boundary term in case $\mathcal{M}$ has a nontrivial closure. Therefore, a singularity is troublesome for the Lagrangian formulation (and hitherto for the initial value formulation) since the York-Hawking-Gibbons boundary term is not well defined; however, there is no problem in Einstein's view. Albert's picture is not one of an evolving universe which starts somewhere and develops towards the future; his interpretation of affairs is that a universe simply organically "forms" in a four dimensional way. It is clear to me that a solution of the problem of quantum gravity will require a truely local formulation of quantum mechanics which dismisses the philosophy of Feynman and Heisenberg which are based upon global considerations and not local ones. Therefore, it is fair to say that the limitations of general relativity imply an Einsteinian view and not the modern one.

Summarizing, I think the main lessons are the following: locality implies the continuum (or $\mathbb{Q}$) and local formulations of the laws of nature give room to infinities. Therefore, I conjecture that the renormalization problems in QFT will evaporate once a superior local formulation of a truely relativistic quantum theory has been found (which is accomplished in [1]). Such formulation cannot be based upon causality, a global Hamiltonian, the cluster decomposition principle and so on but it needs to take into account that unitarity is a local principle;

indeed, all conservation laws in GR are quasi-local and so has to be the conservation of probability. In this context, infinities in the quantum theory are just coordinate infinities by extending the group properties of unitary relators too far and hence, they have no local physical meaning at all (but nevertheless a global one) [1]. Since one simple principle as locality has such constraining consequences what the laws of physics are concerned and such virtues what the interpretation of infinities is about, I think there is little room for doubting that the continuum is a fundamental property of physics. In order to remain fair to the reader and not merely refer to a book in progress which will still take a while to "complete" in a way which I am not too unhappy about, let me present a few core physical principles and equations how these ideas come together in one coherent entity. Haags theorem reveals that the only quantum field theory which exists is a free one; in other words, the interaction picture does not exist and we have to find a fundamentally new way of treating interactions. The idea how to do this mainly comes from relativity; how did Einstein proceed from special relativity to the general theory? He simply put special relativity as a kinematical entity on tangent space and the dynamics (interaction) was all in the vierbein. Well, let us put free quantum field theory on tangent space then; this implies that the theater of reality is not the four manifold $\mathcal{M}$, but its tangent bundle $T\mathcal{M}$ or some higher jet bundle if one wishes to take into account accelerations of the vierbein. Hence, the geometry of gravity is not Riemannian geometry anymore but a subtle generalization of Ehresmann connections in the context of Finsler geometry. That is, $T\mathcal{M}$ is equipped with dynamical charts (associated to the vielbein) and is dynamically split into a horizontal and vertical distribution (often called a nonlinear connection). The free theory is in the vertical part and gravitational interactions in the horizontal piece. So, each vertical space contains a generalized Fock module (meaning Clifford-Nevanlinna bi-modules closed by means of the Guichardet construction) and a representation of the Poincaré algebra attached to the vierbein. Now, locally on $\mathcal{M}$, one can demand the existence of a group of unitary relators with trivial homology; that is, there exists unitary operators $U(x, y, e_b(x), e_b(y))$ which depend upon the points and local frames of reference and which map local particle notions to local particle notions and local vacua to local vacua. The homology condition is that for any x, y, z in the coordinate chart $\mathcal{O}$ on $\mathcal{M}$ the following holds:

$$U(z, x, e_b(z), e_b(x))U(y, z, e_b(y), e_b(z))U(x, y, e_b(x), e_b(y)) = 1$$

which allows for the introduction of a unitary potential $U(x, e_b(x))$ with respect to some reference point x_0 so that

$$U(x, y, eb(x), eb(y)) = U(y, e_b(y))U^{\dagger}(x, e_b(x))$$

locally. It is good to know that unitary operators on Nevanlinna spaces can be genuinely unbounded and therefore composition is generally not well defined globally; a minimal requirement is that this is so locally. The dynamics of the unitary potential is governed by two Dirac type equations where both equations are needed to preserve unitarity. This necessitates dynamical Clifford bundles and indeed, the Clifford numbers enter in a vital way the unitary potential once a nonzero gravitational field is present. Obviously, the Dirac equations have to be locally Lorentz covariant implying the presence of a quantum gauge field satisfying a generalized Yang-Mills equation. This scheme fully solves Haag's objection, avoids the Coleman-Mandula theorem (because the action of the Lorentz group is local and the representations of the Poincaré algebra differ from one point and reference system to another) and throws a different light on the Weinberg-Witten theorem.

Acknowledgements
Instead of spawning a whole literature list, let me simply thank those people whose input and frequent communication have somehow been of value at this point to me. These include Rafael Sorkin, Arkadiusz Jadczyk, Stefan Nobbenhuis and Diederik Aerts. I am grateful to Steve Adler and David Finkelstein for the interest shown in my work [1].

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