Posts by Alain Connes in the blog http://noncommutativegeometry.blogspot.com

Sunday, February 25, 2007
Real and Complex
I would like to discuss the "next entry" in the parallel texts that Masoud was presenting in his post.

On the function theory side we are talking about "real and complex variables". A perfect book to get introduced to that is "real and complex analysis" by W. Rudin (McGraw-Hill). It is a classic and remains one of the best entrance doors to the subject. What one learns is the constant interplay between the "real variable" techniques such as the Lebesgue integral, differentiability almost everywhere, etc., and the "complex variable" techniques. There is a saying of André Weil like "The complex world is beautiful, the real world is dirty". One might then be tempted to ignore the "real world" and only work in the complex variable set-up where "any" function is holomorphic and hence infinitely differentiable etc... That's fine, and one can go some distance with that, except that most of the deep results in complex analysis do rely on real analysis. Now what about the next entry in the parallel text ? It is

Complex variable........................	Operator on Hilbert space
Real variable...............................	Self-adjoint operator

where I have slightly rewritten the previous entry

Functions $f : X \to C$	Operators on Hilbert space

of Masoud's post to stress that the right column gives an ideal model for what the loose notion of a "variable" is... The set of values of the variable is the spectrum of the operator, and the number of times a value is reached is the spectral multiplicity. Continuous variables (operators with continuous spectrum) coexist happily with discrete variables precisely because of non-commutativity of operators.

The holomorphic functional calculus gives a meaning to $f(T)$ for all holomorphic functions f on the spectrum of T, and a deep result controls the spectrum of $f(T)$. The really amazing fact is that while for general operators T in Hilbert space the only functions $f(z)$ that can be applied to T are the holomorphic ones (on the spectrum of T), the situation changes drastically when one deals with self-adjoint operators : for $T = T^*$ the operator $f(T)$ makes sense for any function f ! You can take a pencil and draw the graph of a function, it does not need to be continuous... nor even piecewise continuous, just anything you can name will do... (at the technical level the only requirement on f is that it is universally measurable but nobody can construct explicitly a function which does not fulfill this condition !)... Moreover a bounded operator is a function of T (ie is of the form $f(T)$) if and only if it shares all the symmetries of T (ie if it commutes with all operators that commute with T).

I remember that, at a very early stage of my encounter with mathematics, it is this very fact that convinced me of the power of the Hilbert space techniques in close relation with the adjoint operation $T \to T^*$. This was enough to resist the temptation of starting directly in the "complex world" of algebraic geometry which was attracting most beginners at that time, following the aura of Grothendieck, who described so well his first encounter with that world : "Je me rappelle encore de cette impression saisissante (toute subjective certes), comme si je quittais des steppes arides et revêches, pour me retrouver soudain dans une sorte de "pays promis" aux richesses luxuriantes, se multipliant à l'infini partout où il plaît à la main de se poser, pour cueillir ou pour fouiller..."

AC said...
"anonymous" the point is that the class of arbitrary functions (real analysis) is that which operates on self-adjoint operators, while only the holomorphic ones operate on general operators T. The case of normal operators ($[T, T^*] = 0$) is just "two real variables" and has nothing to do with complex analysis. When the class of functions is smaller you expect more properties, but that does not mean that it is "easier" (rather the opposite) so the analogy is not backwards...
February 26, 2007 at 8 :42 AM

Wednesday, March 7, 2007

Le rêve mathématique

I guess one possible use of a blog, like this one, is as a space of freedom where one can tell things that would be out of place in a "serious" math paper. The finished technical stuff finds its place in these papers and it is a good thing that mathematicians maintain a high standard in the writing style since otherwise one would quickly loose control of what is proved and what is just wishful thinking. But somehow it leaves no room for the more profound source, of poetical nature, that sets things into motion at an early stage of the mental process leading to the discovery of new "hard" facts.

Grothendieck expressed this in a vivid manner in *Récoltes et semailles* : "L'interdit qui frappe le rêve mathématique, et à travers lui, tout ce qui ne se présente pas sous les aspects habituels du produit fini, prêt à la consommation. Le peu que j'ai appris sur les autres sciences naturelles suffit à me faire mesurer qu'un interdit d'une semblable rigueur les aurait condamnées à la stérilité, ou à une progression de tortue, un peu comme au Moyen Age où il n'était pas question d'écornifler la lettre des Saintes Ecritures. Mais je sais bien aussi que la source profonde de la découverte, tout comme la démarche de la découverte dans tous ses aspects essentiels, est la même en mathématique qu'en tout autre région ou chose de l'Univers que notre corps et notre esprit peuvent connaitre. Bannir le rêve, c'est bannir la source - la condamner à une existence occulte".

I shall try to involve on the post of Masoud about tilings and give a heuristic description of a basic qualitative feature of noncommutative spaces which is perfectly illustrated by the space T of Penrose tilings of the plane. Given the two basic tiles : the Penrose kites and darts (or those shown in the pictures), one can tile the plane with these two tiles (with a matching condition on the colors of the vertices) but no such tiling is periodic. Two tilings are the same if they are carried into each other by an isometry of the plane. There are plenty of examples of tilings which are not the same.

The set T of all tilings of the plane by the above two tiles is a very strange set because of the following : "Every finite pattern of tiles in a tiling by kites and darts does occur, and infinitely many times, in any other tiling by the same tiles".

This means that it is impossible to decide locally with which tiling one is dealing. Any pair of tilings can be matched on arbitrarily large patches and there is no way to tell them apart by looking only at finite portions of each of them. This is in sharp contrast with real numbers for instance since if two real numbers are distinct their decimal expansions will certainly be different far enough. I remember attending quite long ago a talk by Roger Penrose in which he superposed two transparencies with a tiling on each and showed the strange visual impression one gets by matching large patches of one of them with the other... he expressed the intuitive feeling one gets from the richness of these "variations on the same point" as being similar to "quantum fluctuations". A space like the space T of Penrose tilings is indeed a prototype example of a noncommutative space. Since its points cannot be distinguished from each other locally one finds that there are no interesting real (or complex) valued functions on such a space which stands apart from a set like the real line R and cannot be analyzed by means of ordinary real valued functions. But if one uses the dictionary one finds out that the space T is perfectly encoded by a (non-commutative) algebra of q-numbers which accounts for its "quantum" aspect. See this book http://alainconnes.org/docs/book94bigpdf.pdf for more details. In a comment to the post of Masoud on tilings the question was formulated of a relation between aperiodic tilings and primes. A geometric notion, analogous to that of aperiodic tiling, that indeed corresponds to prime numbers is that of a Q-lattice. This notion was introduced in our joint work with Matilde Marcolli and is simply given by a pair of a lattice L in R together with an additive map from Q/Z to QL/L. Two Q-lattices are commensurable when the lattices are commensurable (which means that their sum is still a lattice) and the maps agree (modulo the sum). The space X of Q-lattices up to commensurability comes naturally with a scaling action (which rescales the lattice and the map) and an action of the group of automorphisms of Q/Z by composition. Again, as in the case of tilings, the space X is a typical noncommutative space with no interesting functions. It is however perfectly encoded by a noncommutative algebra and the natural cohomology (cyclic cohomology) of this algebra can be computed in terms of a suitable space of distributions on X, as shown in our joint work with Consani and Marcolli.

There are two main points then, the first is that the zeros of the Riemann zeta function appear as an absorption spectrum (ie as a cokernel) from the representation of the scaling group in the above cohomology, in the sector where the group of automorphisms of Q/Z is acting trivially (the other sectors are labeled by characters of this group and give the zeros the corresponding L-functions).

The second is that if one applies the Lefschetz formula as formulated in the distribution theoretic sense by Guillemin and Sternberg (after Atiyah and Bott) one obtains the Riemann-Weil explicit formulas of number theory that relate the distribution of prime numbers with the zeros of zeta. A first striking feature is that one does not even need to define the zeta function (or L-functions), let alone its analytic continuation, before getting at the zeros which appear as a spectrum. The second is that the Riemann-Weil explicit formulas involve rather delicate principal values of divergent integrals whose formulation uses a combination of the Euler constant and the logarithm of 2π, and that exactly this combination appears naturally when one computes the operator theoretic trace, thus the equality of the trace with the explicit formula can hardly be an accident. After the initial paper an important advance was done by Ralf Meyer who showed how to prove the explicit formulas using the above functional analytic framework (instead of the Cauchy integral).

This hopefully will shed some light on the comment of Masoud which hinged on the tricky topic of the use of noncommutative geometry in an approach to RH. It is a delicate topic because as soon as one begins to discuss anything related to RH it generates some irrational attitudes. For instance I was for some time blinded by the possibility to restrict to the critical zeros, by using a suitable function space, instead of trying to follow the successful track of André Weil and develop noncommutative geometry to the point where his argument for the case of positive characteristic could be successfully transplanted. We have now started walking on this track in our joint paper with Consani and Marcolli, and while the hope of reaching the goal is still quite far distant, it is a great incentive to develop the missing noncommutative geometric tools. As a first goal, one should aim at translating Weil's proof in the function field case in terms of the noncommutative geometric framework. In that respect both the paper of Benoit Jacob and the paper of Consani and Marcolli that David Goss mentionned in his recent post open the way. I'll end up with a joke inspired by the European myth of Faust, about a mathematician trying to bargain with the devil for a proof of the Riemann hypothesis. This joke was told to me some time ago by Ilan Vardi and I happily use it in some talks, here I'll tell it in French which is a bit easier from this side of the atlantic, but it is easy to translate...

La petite histoire veut qu'un mathématicien ayant passé sa vie à essayer de résoudre ce problème se décide à vendre son âme au diable pour enfin connaître la réponse. Lors d'une première rencontre avec le diable, et après avoir signé les papiers de la vente, il pose la question "L'hypothèse de Riemann est-elle vraie ?" Ce à quoi le diable répond "Je ne sais pas ce qu'est l'hypothèse de Riemann" et après les explications prodiguées par le mathématicien "hmm, il me faudra du temps pour trouver la réponse, rendez-vous ici à minuit, dans un mois". Un mois plus tard le mathématicien (qui a vendu son âme) attend à minuit au même endroit... minuit, minuit et demi... pas de diable... puis vers deux heures du matin alors que le mathématicien s'apprête à quitter les lieux, le diable apparaît, trempé de sueur, échevelé et dit "Désolé, je n'ai pas la réponse, mais j'ai réussi à trouver une formulation équivalente qui sera peut-être plus accessible !".

Tuesday, March 20, 2007
Time
I will try to describe in loose terms the steps that lead to the emergence of time from noncommutativity in operator algebras. This hopefully will answer the questions of Paul and Sirix (at least in parts) and of Urs.

First I'll explain the basic formula due to Tomita that associates to a state L a one parameter group of automorphisms. The basic fact is that one can make sense of the map $x \to s(x) = LxL^{-1}$ as an (unbounded) map from the algebra to itself and then take its complex powers s^{it}. To define this map one just compares the two bilinear forms on the algebra given by $L(xy)$ and $L(yx)$. Under suitable non-degeneracy conditions on L both give an isomorphism of the algebra with its dual linear space and thus one can find a linear map s from the algebra to itself such that $L(yx) = L(xs(y))$ for all x and y.

One can check at this very formal level that s fulfills $s(ab) = s(a)s(b) : L(abx) = L(bxs(a)) = L(xs(a)s(b))$.

Thus still at this very formal level s is an automorphism of the algebra, and the best way to think about it is as $x \to LxL^{-1}$ where one respects the cyclic ordering of terms in writing $Lyx = LyL^{-1}Lx = LxLyL^{-1}$. Now all this is formal and to make it "real" one only needs the most basic structure of a noncommutative space, namely the measure theory. This means that the algebra one is dealing with is a von Neumann algebra, and that one needs very little structure to proceed since the von Neumann algebra of an NC-space only embodies its measure theory, which is very little structure. Thus the main result of Tomita (which was first met with lots of skepticism by the specialists of the subject, was then succesfully expounded by Takesaki in his lecture notes and is known as the Tomita-Takesaki theory) is that when L is a faithful normal state on a von Neumann algebra M, the complex powers of the associated map $s(x) = LxL^{-1}$ make sense and define a one parameter group of automorphism s_L of M.

There are many faithful normal states on a von Neumann algebra and thus many corresponding one parameter groups of automorphism s_L. It is here that the two by two matrix trick (Groupe modulaire d'une algèbre de von Neumann, C. R. Acad. Sci. Paris, Sér. A-B, 274, 1972) enters the scene and shows that in fact the groups of automorphism s_L are all the same modulo inner automorphisms !

Thus if one lets $\mathrm{Out}(M)$ be the quotient of the group of automorphisms of M by the normal subgroup of inner automorphisms one gets a completely canonical group homomorphism from the additive group R of real numbers $\delta : R \to \mathrm{Out}(M)$ and it is this group that I always viewed as a tantalizing candidate for "emerging time" in physics.

Of course it immediately gives invariants of von Neumann algebras such as the group $T(M)$ of "periods" of M which is the kernel of the above group morphism. It is at the basis of the classification of factors and reduction from type III to type II + automorphisms which I did in June 1972 and published in my thesis (with the missing III_1 case later completed by Takesaki).

This "emerging time" is non-trivial when the noncommutative space is far enough from "classical" spaces. This is the case for instance for the leaf space of foliations such as the Anosov foliations for Riemann surfaces and also for the space of Q-lattices modulo scaling in our joint work with Matilde Marcolli.

The real issue then is to make the connection with time in quantum physics. By the computation of Bisognano-Wichmann one knows that the s_L for the restriction of the vacuum state to the local algebra in free quantum field theory associated to a Rindler wedge region (defined by $x_1 > \pm x_0$) is in fact the evolution of that algebra according to the "proper time" of the region. This relates to the thermodynamics of black holes and to the Unruh temperature. There is a whole literature on what happens for conformal field theory in dimension two. I'll discuss the above real issue of the connection with time in quantum physics in another post.

AC said... Urs : Yes, what happens in fact is that for any quantum system with infinitely many degrees of freedom the hamiltonian H does not belong to the algebra of observables. Thus the corresponding automorphisms are not inner. To see what happens it is simplest to take the case of a system of spins on a lattice. The algebra of observables is the inductive limit of the finite tensor products of matrix algebras one for each lattice site. The hamiltonian H is, even in the simplest non-interacting case, an infinite sum of the hamiltonians associated to each lattice site. Thus it does not belong to the algebra of observables and the corresponding one parameter group is not inner(both in the norm closure ie the C^*-algebra, and in the weak closure)... In QFT the situation is entirely similar and has of course infinitely many degrees of freedom from the start...
March 26, 2007 at 8 :53 AM

Dirac and integrality
In the first paper on "second quantization", namely the paper of Dirac called "The quantum theory of the emission and absorption of radiation" the process of second quantization is introduced and is related again to "integrality". This time it is not the Fredholm index that is behind the integrality but the following simple fact : if an operator a satisfies $[a, a^*] = 1$, then the spectrum of a^*a is contained in xN, the set of positive integers (as follows from the equality of the spectra of aa^* and a^*a except possibly for 0)... Second quantization is obtained simply by replacing the ordinary complex numbers a_j which label the Fourier expansion of the electromagnetic field by non-commutative variables fulfilling $[a_j, a_j^*] = 1$... (more precisely the 1 is replaced by $\hbar\nu$ where ν is the frequency of the Fourier mode). This example shows of course that integrality and non-commutativity are deeply related... While the Fredholm index is a good model of relative integers (positive or negative), the aa^* for $[a, a^*] = 1$ is a good model for positive integers...

Wednesday, April 25, 2007
Another very striking recent development was described in the talk of U. Haagerup on his joint work (I think it is with Magdalena Musat but am not sure, the paper is not out yet) on the classification of factors modulo isomorphism of the associated operator spaces. He gave an amazing necessary and sufficient condition for the class of the hyperfinite III_1 factor : that the flow of weights admits an invariant probability measure. (One knows that this holds for the von Neumann algebra of a foliation with non-zero Godbillon-Vey class). This special case suggests that the general necessary and sufficient condition should be the "commensurability" of the flow of weights, and the idea of Mackey of viewing an ergodic flow as a "virtual subgroup" of the additive group R should be essential in developing the appropriate notion of "commensurability" for ergodic flows. I was off at the beginning of the week for a short sobering trip in Sweeden (Atiyah's "Witten" talk always has a sobering effect) and heard a really interesting talk by Nirenberg which suggests that the Holder exponent $1/3$ which enters as the limit of regularity for the winding number formula of Kahane corresponds to the $3 = 2 + 1$ of the periodicity long exact sequence in cyclic cohomology. There is yet another conference taking place the whole week in paris, organized by Vincent Rivasseau.

Saturday, May 26, 2007
The Ascona meeting on Pauli and Jung
Thanks to Masoud for keeping the blog alive in the middle of all these trips to conferences. The last one I attended ended today and was dedicated to Pauli's philosophical ideas. It was quite interesting and gave me a great occasion to get a better knowledge of these ideas. An interesting talk was given by Rafael Nunez "Where does mathematics come from ? Pauli, Jung, and contemporary cognitive science" with a brave attempt to dismiss platonism (and in particular Pauli's view) using "contemporary cognitive science". The talk was very entertaining, in particular on the representation of the future as relative motion as in expressions of the form "winter is coming" or "we are arriving at the end of the year". Or when the infamous Chilian dictator, after the coup, said successively : "Communism has taken us at the edge of the abyss" and "today we took a big step forward". Unfortunately I missed the talk of Arthur Miller "When Pauli met Jung - and what happened next"... but I could talk to him directly and got very interested with these images Jung was showing to Pauli after hearing his dreams. I had to give a rather improvised talk on Wednesday morning (about the nature of mathematical reality and also the relations with physics) and barely made it in time, since my plane to Milan had been canceled the day before (strike of Air Control) and I had to go there by train. This took the whole day and the only possible way to reach Ascona in time was to rent a car in Milan and drive there in the middle of the night. I did it since I really hate to accept giving a talk somewhere and not be able to make it at the last moment, but there was definitely a kind of "Pauli effect" making it quite difficult to reach the place in time...

Jürg Fröhlich was prevented to come for family reasons and the talk on Pauli's work was given by Harald Atmanspacher who replaced Fröhlich at the last minute. What was really striking in this meeting was that all talks were followed by long and passionate discussions which usually lasted for almost half an hour and one could learn a lot just because there was so much interaction.

AC said...
Dear Nic, yes and this was a very nice point in the talk. I completely share this view that the past is the only thing we control and in fact I believe we just keep trying to rearange the past in order to cope with whatever the present gives us...

May 30, 2007 at 10 :09 AM
AC said...
Anonymous, the lecture of Arthur Miller was based on a book which is scheduled for publication next year. So one will have to wait for that one.

As far as I know it is true that Grothendieck has been writing a lot about dreams, but my information is not reliable and the only source is the "Grothendieck Circle" website at http://www.grothendieckcircle.org/ from which you can get better information.
May 30, 2007 at 4 :41 PM

AC said...
Dear Lieven
Thanks for your comment, it is quite difficult to "maintain" the blog just Masoud and me. We really

welcome comments like yours and would be happy to have you as a "guest" blogger.
One basic concern I have is to try and bridge the gap between the various aspects of NCG. I really like the purely algebraic aspect (and did a bit of work with Michel Dubois-Violette on that). Both the purely algebraic noncommutative geometry as well as the more "operator theoretic" differential noncommutative geometry are mature enough now not to be frightened to interact more openly. When a theory is at its beginning I believe it is important to leave it a chance to grow by itself and "protect" it somehow, but obviously time is ripe now for a broader perspective and attitude. That's pretty much what is going on with the new Journal and the organized meetings such as the Newton Institute program of last fall or precisely the Chicago meeting. Since I was there only briefly it is difficult for me to write a full report but I'll do what I can (after doing that for the vanderbilt meeting) and will try to get a "volunteer" to give a better account than my partial one (because of the small number of talks I could attend, being quite tired after the many classes I had to give in Vanderbilt).
June 6, 2007 at 8 :11 PM

Tuesday, July 3, 2007
Noncommutative spacetime
As I explained in a previous post, it is only because one drops commutativity that, in the calculus, variables with continuous range can coexist with variables with countable range. In the classical formulation of variables, as maps from a set X to the real numbers, we saw above that discrete variables cannot coexist with continuous variables.

The uniqueness of the separable infinite dimensional Hilbert space cures that problem, and variables with continuous range coexist happily with variables with countable range, such as the infinitesimal ones. The only new fact is that they do not commute.

One way to understand the transition from the commutative to the noncommutative is that in the latter case one needs to care about the ordering of the letters when one is writing. As an example, use the "commutative rule" to simplify the following cryptic message I received from a friend : "Je suis alençonnais, et non alsacien. Si t'as besoin d'un conseil nana, je t'attends au coin annales. Qui suis-je ?"

It is Heisenberg who discovered that such care was needed when dealing with the coordinates on the phase space of microscopic systems.

At the philosophical level there is something quite satisfactory in the variability of the quantum mechanical observables. Usually when pressed to explain what is the cause of the variability in the external world, the answer that comes naturally to the mind is just : the passing of time. But precisely the quantum world provides a more subtle answer since the reduction of the wave packet which happens in any quantum measurement is nothing else but the replacement of a "q-number" by an actual number which is chosen among the elements in its spectrum. Thus there is an intrinsic variability in the quantum world which is so far not reducible to anything classical. The results of observations are intrinsically variable quantities, and this to the point that their values cannot be reproduced from one experiment to the next, but which, when taken altogether, form a q-number. Heisenberg's discovery shows that the phase-space of microscopic systems is noncommutative inasmuch as the coordinates on that space no longer satisfy the commutative rule of ordinary algebra. This example of the phase space can be regarded as the historic origin of noncommutative geometry. But what about spacetime itself ? We now show why it is a natural step to pass from a commutative spacetime to a noncommutative one.

The full action of gravity coupled with matter admits a huge natural group of symmetries. The group of invariance for the Einstein-Hilbert action is the group of diffeomorphisms of the manifold and the invariance of the action is simply the manifestation of its geometric nature. A diffeomorphism acts by permutations of the points so that points have no absolute meaning.

The full group of invariance of the action of gravity coupled with matter is however richer than the group of diffeomorphisms of the manifold since one needs to include something called "the groupof gauge transformations" which physicists have identified as the symmetry of the matter part. This is defined as the group of maps from the manifold to some fixed other group, G, called the "gauge group", which as far as we known is : $G = U(1).SU(2).SU(3)$. The group of diffeomorphisms acts on the group of gauge transformations by permutations of the points of the manifold and the full group of symmetries of the action is the semi-direct product of the two groups (in the same way, the Poincaré group which is the invariance group of special relativity, is the semi-direct product of the group of translations by the group of Lorentz transformations). In particular it is not a simple group (a simple group is one which cannot be decomposed into smaller pieces, a bit like a prime number cannot be factorized into a product of smaller numbers) but is a "composite" and contains a huge normal subgroup.

Now that we know the invariance group of the action, it is natural to try and find a space X whose group of diffeomorphisms is simply that group, so that we could hope to interpret the full action as pure gravity on X. This is the old Kaluza-Klein idea. Unfortunately this search is bound to fail if one looks for an ordinary manifold since by a mathematical result, the connected component of the identity in the group of diffeomorphisms is always a simple group, excluding a semi-direct product structure as that of the above invariance group of the full action of gravity coupled with matter. But noncommutative spaces of the simplest kind readily give the answer, modulo a few subtle points. To understand what happens note that for ordinary manifolds the algebraic object corresponding to a diffeomorphism is just an automorphism of the algebra of coordinates ie a transformation of the coordinates that does not destroy their algebraic relations. When an involutive algebra A is not commutative there is an easy way to construct automorphisms. One takes a unitary element u of the algebra ie such that $uu^* = u^*u = 1$. Using u one obtains an automorphism called inner, by the formula $x \to uxu^*$.

Note that in the commutative case this formula just gives the identity automorphism (since one could then permute x and u^*). Thus this construction is interesting only in the noncommutative case. Moreover the inner automorphisms form a subgroup denoted $\mathrm{Int}(A)$ which is always a normal subgroup of the group of automorphisms of A.

In the simplest example, where we take for A the algebra of smooth maps from a manifold M to the algebra of matrices of complex numbers, one shows that the group $\mathrm{Int}(A)$ in that case is (locally) isomorphic to the group of gauge transformations ie of smooth maps from M to the gauge group $G = PSU(n)$ (quotient of $SU(n)$ by its center). Moreover the relation between inner automorphisms and all automorphisms becomes identical to the exact sequence governing the structure of the above invariance group of the full action of gravity coupled with matter.

It is quite striking that the terminology coming from physics : internal symmetries agrees so well with the mathematical one of inner automorphisms. In the general case only automorphisms that are unitarily implemented in Hilbert space will be relevant but modulo this subtlety one can see at once from the above example the advantage of treating noncommutative spaces on the same footing as the ordinary ones. The next step is to properly define the notion of metric for such spaces and we shall indulge, in the next post, in a short historical description of the evolution of the definition of the "unit of length" in physics. This will prepare the ground for the introduction to the spectral paradigm of noncommutative geometry.

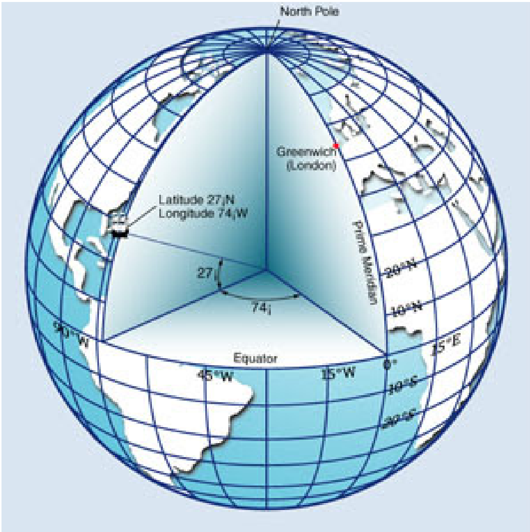
AC said...
Guy on the street, just try to permute some letters and get 4 times a name which is not so difficult to guess... what can you come up with starting with "non alsacien" for instance ?
July 4, 2007 at 6 :59 PM

AC said...
Dear Fabien
Your question is pertinent. The role of the finite space is now much better understood from the very recent papers with A. Chamseddine : "*Why the Standard Model*" (https://arxiv.org/pdf/0706.3688.pdf) and "*A dress for SM the beggar*" (https://pdfs.semanticscholar.org/765c/4b648502de8f1628258f79ca7bc7e61fe3fc.pdf) which are on the hep-th arXiv. My intention is to use this blog, this summer holidays, to explain their content in details, but one step at a time. So far I just wanted to explain why it is natural to consider NC spacetimes and not be so dependent on the "point-set" commutative view of spaces. So even if one cannot exclude that the finite space F is, as you suggest, a "remnant of a shrunk down ancestral continuous space" it will give us a lot more freedom to drop the dependence on the commutative view.
July 4, 2007 at 7 :10 PM

AC said...
I believe that the extension of the "symplectic" framework to the NC world is simply the notion of the first order term in a deformation of the NC-algebra. This is quite clear in the commutative case where a symplectic structure (or more generally Poisson structure) is just the first term in the expansion of the

deformed product. Thus it is a semi-classical form of the deformation. In the NC case there are many examples where it is natural to use a similar starting point for deformations (for instance in Rankin-Cohen brackets generalized to deformations of NC projective structures).
July 8, 2007 at 11 :35 AM

Tuesday, July 10, 2007
A brief history of the metric system



The next step is to understand what is the replacement of the Riemannian paradigm for noncommutative spaces. To prepare for that, and using the excuse of the summer holidays, let me first tell the story of the change of paradigm that already took place in the metric system with the replacement of the concrete "mètre-étalon" by a spectral unit of measurement.

The notion of geometry is intimately tied up with the measurement of length. In the real world such measurement depends on the chosen system of units and the story of the most commonly used system - the metric system - illustrates the difficulties attached to reaching some agreement on a physical unit of length which would unify the previous numerous existing choices. As is well known, the United States are one of the few countries that are not using the metric system and this lack of uniformity in the choice of a unit of length became painfully obvious when it entailed the loss of a probe worth 125 million dollars just because two different teams of engineers had used the two different units (the foot and the metric system). In 1791 the French Academy of Sciences agreed on the definition of the unit of length in the metric system, the "mètre", as being the ten millionth part of the quarter of the meridian of the earth. The idea was to measure the length of the arc of the meridian from Barcelone to Dunkerque while the corresponding angle (approximately 9.5°) was determined using the measurement of latitude from reference stars. In a way this was just a refinement of what Eratosthenes had done in Egypt, 250 years BC, to measure the size of the earth (with a precision of 0.4 %).

Thus in 1792 two expeditions were sent to measure this arc of the meridian, one for the Northern portion was led by Delambre and the other for the southern portion was led by Méchain. Both of them were astronomers who were using a new instrument for measuring angles, invented by Borda, a French physicist. The method they used is the method of triangulation and of concrete measurement of the "base" of one triangle. It took them a long time to perform their measurements and it was a risky enterprize. At the beginning of the revolution, France entered in a war with Spain.

Just try to imagine how difficult it is to explain that you are trying to define a universal unit of length when you are arrested at the top of a mountain with very precise optical instruments allowing you to follow all the movements of the troops in the surrounding.

Both Delambre and Méchain were trying to reach the utmost precision in their measurements and an important part of the delay came from the fact that this reached an obsessive level in the case of Méchain. In fact when he measured the latitude of Barcelone he did it from two different close by locations, but found contradictory results which were discordant by 3.5 seconds of arc. Pressed to give his result he chose to hide this discrepancy just to "save the face" which is the wrong attitude for a Scientist. Chased from Spain by the war with France he had no second chance to understand the origin of the discrepancy and had to fiddle a little bit with his results to present them to the International Commission which met in Paris in 1799 to collect the results of Delambre and Méchain and compute the "mètre" from them. Since he was an honest man obsessed by precision, the above discrepancy kept haunting him and he obtained from the Academy to lead another expedition a few years later to triangulate further into Spain. He went and died from malaria in Valencia. After his death, his notebooks were analysed by Delambre who found the discrepancy in the measurements of the latitude of Barcelone but could not explain it. The explanation was found 25 years after the death of Méchain by a young astronomer by the name of Nicollet, who was a student of Laplace. Méchain had done in both of the sites he had chosen in Barcelone (Mont Jouy and Fontana del Oro) a number of measurements of latitude using several reference stars. Then he had simply taken the average of his measurements in each place. Méchain knew very well that refraction distorts the path of light rays which creates an uncertainty when you use reference stars that are close to the horizon. But he considered that the average result would wipe out this problem.

What Nicollet did was to ponder the average to eliminate the uncertainty created by refraction and, using the measurements of Méchain, he obtained a remarkable agreement (0.4 seconds ie a few meters) between the latitudes measured from Mont Jouy and Fontana del Oro. In other words Méchain had made no mistake in his measurements and could have understood by pure thought what was wrong in his computation. I recommend the book of Ken Adler (*The measure of all things : the seven-year odyssey and hidden error that transformed the world*, eds Little, Brown et compagny, 2003, ou *Mesurer le monde*, Flammarion, 2005) for a nice account of the full story of the two expeditions.

In any case in the meantime the International commission had taken the results from the two expeditions and computed the length of the ten millionth part of the quarter of the meridian using them. Moreover a concrete platinum bar with approximately that length was then realized and was taken as the definition of the unit of length in the metric system. With this unit the actual length of the quarter of meridian turns out to be 10 002 290 rather than the aimed for 10 000 000 but this is no longer relevant. In fact in 1889 the reference became another specific metal bar (of platinum and iridium) called "mètre-étalon", which was deposited near Paris in the pavillon de Breteuil. This definition held until 1960.

Already in 1927, at the seventh conference on the metric system, in order to take into account the inevitable natural variations of the concrete called "mètre-étalon", the idea emerged to compare it with a reference wave length (the red line of Cadmium). Around 1960 the reference to the called "mètre étalon" was finally abandoned and a new definition of the unit of length in the metric system (the "mètre) was adopted as 1650763.73 times the wave length of the radiation corresponding to the transition between the levels 2p10 and 5d5 of the Krypton 86Kr.

In 1967 the second was defined as the duration of 9192631770 periods of the radiation corresponding to the transition between the two hyperfine levels of Caesium-133. Finally in 1983 the "mètre" was defined as the distance traveled by light in 1/299792458 second. In fact the speed of light is just a conversion factor and to define the "mètre" one gives it the specific value of $c = 299792458$ m/s. In other words the "mètre" is defined as a certain fraction $9192631770/299792458 \sim 30.6633...$ of the wave length of the radiation coming from the transition between the above hyperfine levels of the Caesium atom.

The advantages of the new standard of length are many. First by not being tied up with any specific location, it is in fact available anywhere where Caesium is. The choice of Caesium as opposed to Helium or Hydrogen which are much more common in the universe is of course still debatable, and it is quite possible that a new standard will soon be adopted involving spectral lines of Hydrogen instead of Caesium. See this paper of Bordé for an update http://christian.j.borde.free.fr/ChB.pdf.

While it would be difficult to communicate our standard of length with other extraterrestrial civilizations if they had to make measurements of the earth (such as its size) the spectral definition can easily be encoded in a probe and sent out. In fact spectral patterns provide a perfect "signature" of chemicals, and a universal information available anywhere where these chemicals can be found, so that the wave length of a specific line is a perfectly acceptable unit, while if you start thinking a bit you will find out that we would be unable to just tell where the earth is in the universe...

Coordinates ? yes but whith respect to which system ? One possibility would be to give the sequence of redshifts to nearby galaxies, and in a more refined manner to nearby stars but it would be quite difficult to be sure that this would single out a definite place.

AC said...
Dear Apprenticing Physicist. First I do not know any real evidence of the variation in time of the constants of nature. Just look for instance at
http://www-cosmosaf.iap.fr/Cste%208%20mai%202004%20html.htm

In fact Dirac's large number idea of 1937, that was predicting a variation of G with time was not validated by experiment and one has an experimental upper bound of the order of $dG/G < 4\ 10^{-11}$ per year. Thus I am not sure that there is any convincing experimental evidence yet for what you say. If there were it would be interesting to discuss more precisely of which constant we are talking. For instance the spectral unit of length which I discussed in the post depends on the Rydberg constant and hence on the electron mass. In NCG that mass is tied up to the inverse size of the finite geometry F that will be discussed later in this blog. At least what can be said is that, in NCG, the "atomic units" are intimately tied up to the geometry of the finite space F while the astronomical units are tied up with the manifold M. The NCG model of space-time is the product M times F, but nothing prevents the geometry of F to vary over M.
July 13, 2007 at 10 :27 AM

Tuesday, July 17, 2007
Non Standard stuff
I am not sure I really know how to make use of a "blog" like this one. Recently I had to write a sollicited paper describing the perspective on the structure of space-time obtained from the point of view of noncommutative geometry. At first I thought that I could just be lazy and after the paper was written (it is available here https://alainconnes.org/docs/shahnlong.pdf) just use pieces of it to keep this blog alive during the summer vacations. However, when trying to do that, I realized that it was better (partly because of the impractical use of latex in the blog) to first make the paper available and then tell in the blog the additional things one would not "normally" write in a paper (even a non-technical general public paper such as the above). I am not keen on turning the blog into a place for controversies since it is unclear to me that one gains a lot in such discussions. The rule seems to be that, most often, people have prejudices against new stuff mostly because they don't know enough and take the lazy attitude that it is easier to denigrate a theory than to try and appreciate it. I am no exception and have certainly adopted that attitude with respect to supersymmetry or string theory. A debate will usually exhibit the strong opinions of the various sides and it is rare that one witnesses a real change taking place. So much for the "controversy" side. However I do believe that there are some points that can be quite useful to know and which, provided they are presented in a non-polemic manner can help a lot to avoid some pitfalls. I will discuss as an example the two notions of "infinitesimals" that I know and try to explain the relevance of both. This is not a "math paper" but rather an informal discussion.

When I was a student in Ecole Normale about 40 years ago, I fell in love with a new math topic called "nonstandard analysis" which was advocated by A. Robinson. Being a student of Gustave Choquet at that time, I knew a lot about ultrafilters. These maximal filters were (correct me if I am wrong) discovered by H. Cartan during a Bourbaki workshop. At that time Cartan had no name for the new objects but he had found the remarkable efficiency they had in any proof where a compactness and choice arguments were needed. So (this I heard from Cartan) the name he was using was "boum" ! ! ! Of course he knew that it gave a one line proof of the existence of Haar measure (boum...). And also that because of uniqueness of the latter it was in fact proving a rather strong convergence statement on the counting functions that approximate the Haar measure. He wanted to make sure, and wrote in a Compte-Rendu note the full details of a direct geometric argument proving the expected convergence. From ultrafilters to ultraproducts is an easy step. And I got completely bought by ultraproducts when I learnt (around that time) about the Ax-Cochen theorem : the ultraproduct of p-adic fields is isomorphic to the ultraproduct of local function fields with the same residue fields. Thus I started trying to work in that subject and obtained, using a specific class of ultrafilters called "selective", a construction of minimal models in nonstandard analysis. They are obtained as ultraproducts but the ultrafilters used are so special that, for instance, in order to know the element of the ultrapower of a set X, one does not need to care about the labels : the image ultrafilter in X is all that is needed. I wrote a paper explaining how to use ultraproducts and always kept that tool ready for use later on. I used it in an essential manner in my work on the classification of factors. So much for the positive side of the coin. However, quite early on I had tried in vain to implement one of the "selling adds" of nonstandard analysis, namely that it was finally giving the promised land for "infinitesimals". In fact the adds came with a specific example : a purported answer to the naive question "what is the probability "p" that a dart will land at a given point x of the target" in playing a game of darts. This was followed by 1) the simple argument why that positive number "p" was smaller than epsilon for any positive real epsilon 2) one hundred pages of logic 3) the identification of "p" with a "non-standard" number...

At first I attributed my inability to concretely get "p" to my lack of knowledge in logics, but after realizing that the models could be constructed as ultraproducts this excuse no longer applied. At this point I realized that there is some fundamental reason why one will never be able to actually "pin down" this "p" among non-standard numbers : from a non-standard number (non-trivial of course) one canonically deduces a non-measurable character of the infinite product of two element groups (the argument is simpler using a non-standard infinite integer "n", just take the map which to the sequence a_n (of 0 and 1) assigns its value for the index "n"). Now a character of a compact group is either continuous or non-measurable. Thus a non-standard number gives us canonically a non-measurable subset of $[0, 1]$. This is the end of the rope for being "explicit" since (from another side of logics) one knows that it is just impossible to construct explicitely a non-measurable subset of $[0, 1]$!

It took me many years to find a good answer to the above naive question about "p". The answer is explained in details here. It is given by the formalism of quantum mechanics, which as explained in the previous post on "infinitesimal variables" gives a framework where continuous variables can coexist with infinitesimal ones, at the only price of having more subtle algebraic rules where commutativity no longer holds. The new infinistesimals have an "order" (an infinitesimal of order one is a compact operator whose characteristic values μ_n are a big O of $(1/n)$. The novel point is that they have an integral, which in physics terms is given by the coefficient of the logarithmic divergence of the trace. Thus one obtains a new stage for the "calculus" and it is at the core of noncommutative differential geometry.

In Riemannian geometry the natural datum is the square of the line element, so that when computing the distance $d(A, B)$ between two points one has to minimize the integral from A to B along a continuous path of the square root of $g_{\mu\nu}dx^\mu dx^\nu$. Now it is often true that "taking a square root" in a brutal manner as in the above equation is hiding a deeper level ofunderstanding. In fact this issue of taking the square root led Dirac to his famous analogue of the Schrödinger equation for the electron and the theoretical discovery of the positron. Dirac was looking for a relativistic invariant form of the Schröodinger equation. One basic property of that equation is that it is of first order in the time variable. The Klein-Gordon equation which is the relativistic form of the Laplace equation, is relativistic invariant but is of second order in time.

Dirac found away to take the square root of the Klein-Gordon operator using Clifford algebra. In fact (as pointed out to me by Atiyah) Hamilton had already written the magic combination of partial derivatives using his quaternions as coefficients and noted that this gave a square root of the Laplacian. When I was in St. Petersburg for Euler's 300'th, I noticed that Euler could share the credit for quaternions since he had explicitly written their multiplication rule in order to show that the product of two sums of 4 squares is a sum of 4 squares.

So what is the relation between Dirac's square root of the Laplacian and the above issue of taking the square root in the formula for the distance $d(A, B)$. The point is that one can use Dirac's solution and rewrite the same geodesic distance $d(A, B)$ in the following manner : one no longer measures the minimal length of a continuous path but one measures the maximal variation of a function : ie the absolute value of the difference $f(A) - f(B)$. Of course without a restriction on f this would give infinity, but one requires that the commutator $[D, f]$ of f with the Dirac operator is bounded by one.

Here we are in our "quantized calculus" stage, so that both the functions on our geometric space as well as the Dirac operator are all concretely represented in the same Hilbert space H. H is the Hilbert space of square integrable spinors and the functions act by pointwise multiplication. The commutator $[D, f]$ is the Clifford multiplication by the gradient of f so that when the function f is real, its norm is just the Sup norm of the gradient. Then saying that the norm of $[D, f]$ is less than one is the same as asking that f be a Lipschitz function of constant one ie that the absolute value of $f(A) - f(B)$ is less than $d(A, B)$ where the latter is the geodesic distance. For complex valued functions one only gets an inequality, but it suffices to show that the maximum variation of such f gives exactly the geodesic distance : ie we recover the geodesic distance $d(A, B)$ as Sup $f(A) - f(B)$ for norm of $[D, f]$ less than one.

Note that D has the dimension of the inverse of a length, ie of a mass. In fact in the above formula for distances in terms of a supremum the product of "f" by D is dimensionless and "f" has the dimension of a length since $f(A) - f(B)$ is a distance.

Now what is the intuitive meaning of D ? Note that the above formula measuring the distance $d(A, B)$ as a supremum is based on the lack of commutativity between D and the coordinates "f" on our space. Thus there should be a tension that prevents D from commuting with the coordinates. This tension is provided by the following key hypothesis "the inverse of D is an infinitesimal".

Indeed we saw in a previous post that variables with continuous range cannot commute with infinitesimals, which gives the needed tension. But there is more, because of the fundamental equation $\mathbf{ds} = 1/D$ which gives to the inverse of D the heuristic meaning of the line element. This change of paradigm from the $g_{\mu\nu}$ to this operator theoretic **ds** is the exact parallel of the change of the unit of length in the metric system to a spectral paradigm. Thus one can think of a geometry as a concrete Hilbert space representation not only of the algebra of coordinates on the space X we are interested in, but also of its infinitesimal line element **ds**. In the usual Riemannian case this representation is moreover irreducible. Thus in many ways this is analogous to thinking of a particle as Wigner taught us, ie as an irreducible representation (of the Poincaré group).

AC said...

Dear Arivero, thanks ! nice that Tao and myself are discussing the same thing. In fact my first compte rendu note (1970) was about selective ultrafilters and minimal nonstandard models constructed using ultraproducts. My belief is that the two points of view on infinitesimals are the reflection of the nuances existing already at the beginning of the invention of the calculus. The non-standard numbers in the sense of logics or ultrafilters are very close to the point of view of Leibniz.

July 17, 2007 at 7 :30 PM

AC said...

Dear Alon Levy

Yes, of course I am willing to put the post about nonstandard analysis on the carnival blog, thanks,

Alain

August 19, 2007 at 1 :57 PM

Friday, August 3, 2007

Paul's Seventies

I am just back from a very nice event around Paul Baum's seventies, which took place in Warsaw last Monday, thanks in particular to Piotr Hajac. I have known Paul since the summer of 1980 when we first met in Kingston. I really had, when I first met him, the impression of meeting l'"Homologie en personne". The more I got to know him through our very long collaboration, the better I enjoyed his clarity of mind and his relentless quest for simplicity and beauty. In many ways he succeeds indoing something very difficult, which Grothendieck advised in *Récoltes et Semailles*, namely to keep "une innocence enfantine" in front of mathematics. The dinner on monday night was comparable in intensity to the memorable one in Martin Walter's place, in Boulder, for Paul's sixties when the team Paul Baum - Raoul Bott forced Martin to search (again and again) his cellar for more bottles of wine to keep up with their drinking ability !.

Raoul Bott died in December 2005. Not long before, Paul went all the way to California to visit him and they talked together for an entire day. This type of faithfulness in friendship and understanding of what really matters, is an attitude towards life which Paul has and which I truly admire.

Monday, August 13, 2007
Harmonic mean
This "post" is mainly an attempt to see if one can manage to use formulas in the blog and discuss some real stuff somehow. The formulas should be really visible, a bit like with transparencies. So as a pretext, I'll start by discussing an issue related to the basic Ansatz :

$$\mathbf{ds} = D^{-1}$$

which gives the operator theoretic line element in terms of the Dirac operator in the general framework of "metric" noncommutative geometry. The kernel of the operator D is finite dimensional and one takes **ds** to vanish on that kernel. As was already discussed here, the knowledge of D gives back the metric. Moreover the noncommutative integral, in the form of the Dixmier trace, gives back the volume form. Thus the integral of a function f in dimension n is simply given by

$$⨍ f|\mathbf{ds}|^n$$

where the "cut" integral is the Dixmier trace ie the functional that assigns to an infinitesimal of order one the coefficient of the logarithmic divergency in the series that gives the sum of its eigenvalues.

I will not try to justify the heuristic definition of the line element any further. It is more interesting to put it to the test, to question it, and I will discuss an example of an issue which left me perplex for quite sometime but has a pretty resolution.

The point is to understand what happens when one takes the product of two noncommutative geometries. One gets the following relation for the squares of the corresponding Dirac operators :

$$D^2 = D_1^2 + D_2^2$$

where we abuse notations by removing the tensor product by the identity operator that normally goes with each of the operators D_j. Now this relation is quite different from the simple Pythagorean relation of the classical line elements whose square simply add up and it thus raises the question of reconciling the above Ansatz with the simple formula of addition of the squares of the Dirac operators. More generally, one can consider a bunch of NC spaces with Dirac operators D_μ and combine them as follows : One starts with a positive matrix of operators in Hilbert space :

$$g = (g^{\mu\nu}) \in M_n(\mathcal{L}(\mathcal{H}))$$

and one extends the above formula giving D^2 for a product of two spaces and forms the following sum :

$$D^2 = \sum_{\mu,\nu} D_\mu g^{\mu\nu} D_\nu^*$$

we make no commutativity hypothesis and even drop the self-adjointness of D_μ which is not needed. We want a formula for the inverse of the square of D ie for :

$$\mathbf{ds}^2 = D^{-2}$$

in terms of the inverse matrix :

$$g^{-1} = (g_{\mu\nu})$$

which plays a role similar to the $g_{\mu\nu}$ of Riemannian geometry, and of the operators

$$dz^\mu = D_\mu^{-1}$$

where the notation with z stresses the fact that we do not even assume self-adjointness of the various D_μ.

It sounds totally hopeless since one needs a formula for the inverse of a sum of noncommuting operators. Fortunately it turns out that there is a beautiful simple formula that does the job in full generality. It is given by :

$$\langle \xi, \mathbf{ds}^2 \xi \rangle = \mathrm{Inf} \sum_{\mu,\nu} \langle dz^\mu \xi^\mu, g_{\mu\nu} dz^\nu \xi^\nu \rangle$$

The infimum is taken over all decompositions of the given vector as a sum :

$$\sum_\mu \xi^\mu = \xi$$

Note that this formula suffices to determine the operator $\mathbf{ds}^2$ completely, since it gives the value of the corresponding positive quadratic form on any vector in Hilbert space. The proof of the formula is not difficult and can be done by applying the technique of Lagrange multipliers to take care of the above constraint on the free vectors ξ^μ.

AC said...
Dear Christophe
What I did was to embed small images inside the text, first I wrote a pdf file and then I extracted small portions of the pdf using adobe professional and saving them as jpeg.
September 7, 2007 at 4 :51 PM

Wednesday, September 5, 2007
News on K-front
Today the editorial board of the new Journal of K-theory put out a public statement, which we reproduce below : STATEMENT OF THE EDITORS OF THE “JOURNAL OF K-THEORY”

After several public statements and news articles regarding the Springer journal “K-theory” (KT), and the new “Journal of K-Theory” (JKT) to be distributed by Cambridge University Press (CUP), the mathematical community has become aware of ongoing changes. On behalf of the entire Editorial Board of the new JKT, we want to give as precise a picture of the situation as we can at the moment, especially to the authors. It is very important to us that the authors should not suffer as a result of the transition. Those authors who submitted papers to KT before August 2007, regardless of whether the paper has already been accepted or is just awaiting review, have three choices : 1) Choose another journal. 2) Maintain submission with KT for final review if necessary and publication if accepted. 3) Transfer their article to the new JKT. All authors who have not yet done so should please notify Professor Bak on the one hand, Professors Lueck and Ranicki on the other hand, about their choice, as soon as possible. For those who opt for choice 2, Professors Lueck and Ranicki have promised to take over the remaining editorial duties. We can guarantee that the authors who choose option (3) will have a smooth transition, with their articles progressing as if there has been no change. We will also do everything we can to help those who choose options (1) and (2). In particular, if the authors instruct us, we will be happy to forward to the journals of their choice the full information regarding the status of their articles. In 2004, because of growing dissatisfaction with Springer, the editorial board of KT authorized Prof. Anthony Bak, the Editor in Chief, to begin negotiations with other publishers. The editorial board was unhappy with the poor quality of the work done by Springer, for example the huge number of misprints in the published version of the articles, the long delay in publication and the high prices Springer was charging. The negotiations came to a conclusion in 2007. A new journal, entitled “Journal of K-theory” (JKT) will commence publication in late 2007. It will be printed by Cambridge University Press. Papers will appear earlier online, as “forthcoming articles”. The title of JKT is currently owned by a private company. This situation is only meant as a temporary solution to restart publication of K-theory articles as soon as possible. It is the Board’s intention to create a non-profit academic foundation and to transfer ownership of JKT to this foundation, as soon as possible, but no later than by the end of 2009, a delay justified by many practical considerations. This shift towards more academic control of journals is not new. We follow here a path opened by Compositio Mathematica, Commentarii Mathematici Helvetici, and others (see for instance the interesting paper of Gerard van der Geer which appeared in the Notices of the AMS in May 2004). We believe that such changes can help keep prices low. We trust in Prof. Bak’s leadership for the launching of JKT and forming, together with the editorial board, the foundation to house the Journal. The statutes of the foundation will provide democratic rules governing the future course and development of the journal, including the election of the managing team. We hope to have provided a fair picture of the current situation, and we plan to issue another public statement when new developments come up. In case of further questions, please contact any of the signatories. Let us conclude from a broader perspective : The editorial board is committed to secure the journal’s quality and long-term sustainability. Signatures : A. Bak, P. Balmer, S. Bloch, G. Carlsson, A. Connes, E. Friedlander, M. Hopkins, B. Kahn, M. Karoubi, G. Kasparov, A. Merkurjev, A. Neeman, T. Porter, J. Rosenberg, A. Suslin, Guoping Tang, B. Totaro, V. Voevodsky, C. Weibel, Guoliang Yu.

AC said...
Dear Peter
First I (ac) heard about this episode and checked its accuracy only now. If I had known earlier I would of course have done something.

What needs to be done for sure is to remove from all librairies the copy of the Journal with that terrible "data corruption". As you say it is the responsibility of Springer to do that. They have been able to make the author sign an "excuse" as if any sensible person could believe that it was the intention of the author to have his paper appearing in that corrupted form ! ! !

Only Springer has the means to recall the volume and to replace it, and I am writing to them to ask that they do it. I don't see what Kreck has to do with that, except if you consider that he represents Springer. The present crisis has its origin in innumerable printing problems since 2003-2004 (less serious than the above one fortunately), but just to quote examples nilpotent $\rightarrow$ impotent everywhere in a paper, 4 pages missing in the middle of a paper, the author of a ten pages paper spending more than a year of back and forth proofs with the publisher before getting a sensible version etc etc...

This resulted in permanent complaints of authors to Bak and no solution was found except to move to another publisher. The only sensible thing to do at this point is to move ahead, get the new JKT on the rails making sure that it will be ran in a democratic manner and stop all this sterile quarelling.
September 12, 2007 at 4 :31 PM

AC said... Good news !
I just got a positive answer from Catriona Byrne from Springer concerning the corrupted issue of K-theory which was discussed in this blog. I had sent an email asking if the corrupted volume could be destroyed and fortunately the answer is "yes" : "We agree. This is actually in the works right now. The corrected issue will have a covering note asking librarians to destroy the original issue, and pointing out that the online version is correct."
September 13, 2007 at 7 :10 PM

AC said...
Update
I am really grateful to Catriona Byrne for her understanding of the situation and her help in removing the corrupted copy of K. I realise that the phrase :
"They have been able to make the author sign an "excuse" as if any sensible person could believe that it was the intention of the author to have his paper appearing in that corrupted form ! ! !" could be misunderstood and I would rather say "They have been able to publish an "excuse" of the author, as if any sensible person could believe that it was the intention of the author to have his paper appearing in that corrupted form ! ! !"

What I meant of course is that in my opinion the responsability for missing the "typos", and in particular the disordered alphabetic listing of references, should be shared and not entirely endorsed by the author. What happened to the author in that case is something that could happen to any of us : some "data corruption" occured at some point in the publication process and he missed the typo in the proofreading process. Good typesetters ask us to update old bibliographical references and of course they double check things like the alphabetic ordering.
September 16, 2007 at 9 :12 AM

AC said...
A Scholarly Society Makes a Logical - and Symbolic - Move to Cambridge U. Press
By JENNIFER HOWARD
In scholarly-journal publishing, as in marriage, love can have very little to do with one's decision to stay committed to a partner.

Lately, scholarly societies have been tempted to make alliances with well-heeled suitors. A commercial outfit like Springer or Wiley-Blackwell commands vast global marketing and distribution networks ; a specialized nonprofit publisher can offer publishing platforms and services that university presses may find hard to match. And such assets often help seal the deal.

The American Anthropological Association, for instance, announced in September that it would leave the

University of California Press for Wiley-Blackwell (The Chronicle, September 19).

And this year the American Astronomical Society abandoned the University of Chicago Press for IOP Publishing, part of the nonprofit Institute of Physics (The Chronicle, May 18).

But another society, the Association for Symbolic Logic, has reversed the trend and decided to ditch a commercial publisher for a university press. It has severed its ties to Springer, which owns and publishes the Journal of Philosophical Logic, a journal edited by the association, and formed an alliance with Cambridge University Press.

Together the association, which is known as ASL, and the press will start the Review of Symbolic Logic as a successor to the Springer-owned journal. Revenue from the new journal will be shared between the parties, while the association will retain editorial control.

All of the ASL editors of the Springer journal are switching over to the new journal, which will make its debut in June 2008, taking its place alongside the association's two other publications, The Journal of Symbolic Logic and The Bulletin of Symbolic Logic. (The group typesets those publications itself, and the American Mathematical Society handles printing and mailing.) Dues-paying members of the symbolic-logic group will receive the journal as a benefit of membership.

Those involved with the new Review say it will be broader in scope than its predecessor. The association brings together logicians who work in mathematics, philosophy, linguistics, computer science, cognitive science, and other fields. It envisions its new journal as a meeting place for work in several complementary areas, with an emphasis on philosophical logic and its applications, the history of philosophy of logic, and the philosophy and methodology of mathematics. Penelope Maddy, president of the association and a professor of logic and the philosophy of science at the University of California at Irvine, points to a number of "growth industries" that the Review will spotlight – for instance, how scholars in computational linguistics, game theory and decision theory, and cognitive science apply the tools and methods of philosophical logic. "It's all about logic," she says, "but you can come at logic from very different disciplinary perspectives."

"Different perspectives" would be a kind way to sum up the association's relationship with Springer. Thanks to the vicissitudes of corporate mergers, Springer is the latest in a line of commercial publishers to own the Journal of Philosophical Logic, which was founded in 1972. The association has edited it since 1987.

Working with Springer was a headache almost from the start, says G. Aldo Antonelli, coordinating editor of the journal and chairman of the department of logic and the philosophy of science at Irvine. Papers went missing, typesetting went awry. "Authors were up in arms," he says. The editors would submit clean manuscripts and "get page proofs back that were full of typos and errors."

The association even tried to buy the journal from Springer, but its offer was rebuffed. So in 2006, when Cambridge signed on to handle book projects for the group, talk quickly turned to a new journal as well.

Charles Erkelens, editorial director for the humanities at Springer, plays down the troubles in the relationship. "There has been an occasional article where things have gone wrong and we've fixed them again, but I have no bad relations with ASL in any way," he says.

The Journal of Philosophical Logic has done well for Springer, and the company will continue to publish it, with a new editorial board. "It's fine for philosophical logic to have more outlets for people to publish in," Mr. Erkelens says. "I still think the Journal of Philosophical Logic will remain the most important of those."

David Tranah, editorial director of mathematical sciences at Cambridge University Press, was matchmaker for both the books program and the new journal. Commercial publishers like Springer "have been vigorously courting learned societies," he says, but often "what they require is more than they can offer." Cambridge has vowed not to be so demanding. "We do not insist on ownership, we do not insist on retaining copyright," he says. "We want to explore possibilities for them. It's a different sort of partnership."

The union may be a meeting of minds, but both partners stand to gain in financial terms as well. Previously "we were putting in all this work and Springer was making pots of money," says Charles Steinhorn, the association's secretary-treasurer, who is a professor of mathematics at Vassar College. If Cambridge's calculations are correct, he says, "we should be able to support new scholarly activities" with the extra income - a graduate fellowship, perhaps, or research support. Meanwhile Cambridge has an incentive to be active on the journal's behalf, spreading the word through its networks of editors and marketers. The association's officers say they're over the moon. "We were nowhere near this with Springer," Ms. Maddy says. "Assuming the Review does as well as we think it will do, this is a great boon to the organization."

September 29, 2007 at 5 :01 PM
Anonymous said...
The reference for the story above is Chronicle of Higher Education, September 27, 2007.
October 5, 2007 at 10 :18 AM

Tuesday, September 18, 2007
Les motifs - ou le coeur dans le coeur
It is with this fascinating title that A. Grothendieck presents in *Récoltes et Semailles* (cfr. Promenade à travers une oeuvre ou l'Enfant et la Mère) the subject of motives : the deepest of the twelve research themes around which he developed his "long-run" research program that literally revolutionized the field of algebraic geometry in the decade 1958-68. Motives were envisaged as the "heart of the heart" of the new geometry (arithmetic geometry) that Grothendieck invented following a scientific strategy based on the introduction of a series of new concepts organized on a progressive level of generality : starting with schemes, topos and sites then continuing with the yoga of motives and motivic Galois groups and finally introducing anabelian algebraic geometry and Galois-Teichmuller theory.

If the notions of scheme and topos were the two crucial ideas which constituted the original driving force in the development of this new geometry – Grothendieck was evidently fascinated by the concepts of geometric point, space and symmetry – it is only with the notion of a motive that one eventually captures the deepest structure, the heart of the profound identity between geometry and arithmetic.

Grothendienck wrote very little about motives. The foundations are documented in his unpublished manuscript "Motifs" and were discussed on a seminar at the Institut des Hautes Études Scientifiques, in 1967. We know, by reading *Récoltes et Semailles*, that he started thinking about motives in 1963-64. J.P. Serre has included in his paper "Motifs" an extract from a letter that Grothendieck wrote to him in August 1964 in which he talks (rather vaguely, in fact) of the notions of motive, fiber functor, motivic Galois group and weights.

Motives were introduced with the ultimate goal to supply an intrinsic explanation for the analogies occurring among the various cohomological theories for algebraic varieties : they were expected to play the role of a universal cohomological theory (the motivic cohomology) and also to furnish a linearization of the theory of algebraic varieties, by eventually providing (this was Grothendieck's viewpoint) the correct framework for a successful attack to the Weil's Conjectures on the zeta function of an algebraic variety over a finite field.

Unlike in the framework of algebraic topology where the standard cohomological functor is uniquely characterized by the Eilenberg-Steenrod axioms in terms of the normalization associated to the value of the functor on a point, in algebraic geometry there is no suitable cohomological theory with integers coefficients, for varieties defined over a field k, unless one provides an embedding of k into the complex numbers. In fact, by means of such mapping one can form the topological space of the complex points of the original algebraic variety and finally compute the Betti (singular) cohomology. This construction however, does in general depend upon the choice of the embedding of k in the field of complex numbers. Moreover, Hodge cohomology, algebraic de-Rham cohomology, etale ℓ-adic cohomology furnish several examples of different cohomology functors which can be simultaneously associated to a given algebraic variety, each of which supplying a relevant information on the topological space.

Grothendieck theorized that this plethora of different cohomological data should be somewhat encoded systematically within a unique and more general theory of cohomological nature that acts as an internal "liaison" between algebraic geometry and the collection of available cohomological theories. This is the idea of the "motif", namely the common reason behind this multitude of cohomological invariants which governs and controls systematically all the cohomological apparatus pertaining to an algebraic variety or more in general to a scheme.

The original construction of a category M of (pure) motives over a field k starts with two preliminary considerations. The first consideration is that M should be the target of a natural contravariant functor connecting the category C of smooth, projective algebraic varieties over a field k to M. Such functor should map an object X in C to its associated motive $M(X)$. The second consideration is that this functor should, by construction, factor through any particular cohomological theory.

Now, keeping in mind this goal, one thinks about the axiomatizing process of a cohomological theory in algebraic geometry. This is done by introducing a contravariant functor $X \to H(X)$ from C to a graded abelian category, where the sets of morphisms between its objects form K-vector spaces (K is a field of characteristic zero, that for simplicity, I fix here equal to the rationals). One also would like that any correspondence $V \to W$ (an algebraic cycle in the cartesian product $V \times W$ that can be view as the graph of a multi-valued algebraic mapping) induces contravariantly, a mapping on cohomology and that the target category is suitably defined so that it contains among its objects any "Weil cohomological theory", namely a cohomology which satisfies among other axioms Poincaré duality and Künneth formula.

This preliminary disquisition helps one in formalizing the construction of the category of motives by following a three-steps procedure. One wishes to enlarges the category C in a precise way with the hope to produce also an abelian category. The three steps are shortly resumed as follows.

(1) One moves from C to a category with the same objects but where the sets of morphisms are the equivalence classes of rational correspondences. Here, the natural choice of the equivalence relation is the numerical equivalence relation as it is the coarsest one among the possible relations between algebraic cycles which can be seen to induce, via the cohomological axioms of any Weil cohomological theory, well-defined homomorphisms in cohomology.

(2) One enlarges the collection of objects of the category defined in (1), by formally adding kernels and images of projectors. This step is technically referred to as the "pseudo-abelian envelope" of the category defined in (1) and it is motivated by the expectation to define an abelian category of motives in which for instance, the Künneth formula can be applied.

(3) Finally, one considers the opposite of the category defined in (2).

Now, after having diligently applied all this abstract machinery, one would like to see a fruitful application of these ideas, in the form, for instance, of the proof of a major conjecture. However, one also perceives quite soon that a successful application of the yoga of motives is subordinated to a thorough knowledge of the theory of algebraic cycles, since the construction of the category M is centered on the idea of enlarging the sets of morphisms by implementing the notion of correspondence. It is for this reason that the Standard Conjectures (cohomological criteria for the existence of interesting algebraic cycles) were associated, since the beginning, to the theory of motives as they seem to play the "conditio sine qua non" a theory of motives has a concrete and successful application.

However, in order to put the Standard Conjectures in the right perspective and to avoid perhaps, an over-estimation of their importance, one should also record that Y. Manin gave in 1968, the first interesting application of these ideas on motives by producing an elegant proof of the Riemann-Weil hypothesis for non-singular three-dimensional projective unirational varieties over a finite field, without appealing to the Standard Conjectures. Moreover, we also know that the Weil's Conjectures have been proved by P. Deligne in 1974 without using neither the theory of motives nor the Standard Conjectures.

Almost forty years have passed since these ideas were informally discussed in the "Grothendieck's circle". An enlarged and in part still conjectural theory of mixed motives has in the meanwhile proved its usefulness in explaining conceptually, some intriguing phenomena arising in several areas of pure mathematics, such as Hodge theory, K-theory, algebraic cycles, polylogarithms, L-functions, Galois representations etc. Very recently, some new applications of the theory of motives to number-theory and quantum field theory have been found or are about to be developed, with the support of techniques supplied by noncommutative geometry and the theory of operator algebras.

In number-theory, a conceptual understanding of the interpretation proposed by A. Connes of the Weil explicit formulae as a Lefschetz trace formula over the noncommutative space of adèle classes, requires the introduction of a generalized category of motives which is inclusive of spaces which are highly singular from a classical viewpoint. Several questions arise already when one considers special types of zero-dimensional noncommutative spaces, such as the space underlying the quantum statistical dynamical system defined by J.B. Bost and Connes in their paper "Hecke algebras, type III factors and phase transitions with spontaneous symmetry breaking" (Selecta Math. (3) 1995). This space is a simplified version of the adèle classes and it encodes in its group of symmetries, the arithmetic of the maximal abelian extension of the rationals. A new theory of endomotives (algebraic and analytic) has been recently developed in "Noncommutative geometry and motives : the thermodynamics of endomotives" (to appear in Advances in Mathematics). The objects of the category of endomotives are noncommutative spaces described by semigroup actions on projective limits of Artin motives (these are among the easiest examples of pure motives, as they are associated to zero-dimensional algebraic varieties). The morphisms in this new category generalize the notion of (algebraic) correspondences and are defined by means of etale groupoids to account for the presence of the semigroup actions.

An open and interesting problem is connected to the definition of a higher dimensional theory of noncommutative motives and in particular the set-up of a theory of noncommutative elliptic motives and modular forms. A suitable generalization of the yoga of motives to noncommutative geometry has already produced some interesting results in the form, for example, of an analog in characteristic zero of the action of the Weil group on the etale cohomology of an algebraic variety. It seems quite exciting to pursue these ideas further : the hope is that the motivic techniques, once suitably transferred in the framework of noncommutative geometry may supply useful tools and produce even more substantial applications than those obtained in the classical commutative context.

1 comment :
AC said...
Dear Katia
Thanks for this beautiful post. Your question was left unanswered for sufficiently long now, and I'll try (why not) to give some answer in a coming post. Of course it will be some (partial) answer from my own point of view and as such it will have zero pretence to being "the" answer.
October 4, 2007 at 1 :21 PM

Wednesday, October 31, 2007
HEART BIT 1
Katia's last post ended with a provocative question motivated by Grothendieck's description in *Récoltes et Semailles* of the "heart of the heart" of arithmetic geometry, namely the theory of motives. Her question was formulated like this :
——— What is the "heart of the heart" of noncommutative geometry ? ———
I'll try to explain here that there is a definite "supplément d'âme" obtained in the transition from classical (commutative) spaces to the noncommutative ones. The main new feature is that "noncommutative spaces generate their own time" and moreover can undergo thermodynamical operations such as cooling, distillation etc.

This opens up completely new ways of handling geometric spaces and our work with Matilde Marcolli and Katia Consani is just one example of potential applications to number theory. It is closely related to the Riemann zeta function and is very close in spirit to Grothendieck's ideas on motives so that it is not out of place in the present discussion of Katia's question. The story starts by a qualitative distinction between spaces which comes from the classification (by von Neumann) of noncommutative algebras in types I, II and III. The commutative spaces are all of type I. When encoding a space X by an algebra A of (complex valued) functions on X one uses some structure on X to restrict the class of functions (e.g. to smooth functions on a smooth space) and the above distinction between types uses the coarsest possible structure which is the measure theory. The corresponding algebras (called von Neumann algebras) are quite simple to characterize abstractly : they are commutants in Hilbert space of some unitary representation. Since one can take the direct sum of algebras A and B, one can mix algebras of different types.

More precisely any von Neumann algebra decomposes uniquely as an integral of algebras which cannot be decomposed further and are called factors. A factor is a von Neumann algebra whose center is as small as it can be, namely is reduced to the complex numbers. The factors of type I are Morita equivalent to the complex numbers, and thus a type I factor really corresponds to the classical notion of "point" in a space X.

To understand geometrically what factors of type II and III look like, it is useful to describe the (von

Neumann) algebra A associated to the leaf space of a foliated manifold : (V, F). An element T of A assigns to each leaf an operator in the Hilbert space of square integrable functions on the leaf, and it makes sense to say that T is bounded, measurable, or zero almost everywhere. The algebraic operations are done leaf per leaf, and the algebra of bounded measurable elements modulo the negligible ones is a von Neumann algebra. The simplest example corresponds to the foliation whose leaf space is the noncommutative torus. It is the foliation of the two torus by the equation "$dy = a\ dx$" in flat coordinates. The corresponding von Neumann algebra is a factor when "a" is irrational and this factor is not of type I but of type II. To obtain type III examples one can take any codimension one foliation whose Godbillon-Vey invariant does not vanish. The integrable subbundle F defining a codimension one foliation is the orthogonal of a one form v and integrability gives dv as the wedge product of v by a one form w. The Godbillon-Vey invariant is the integral over V of the wedge product of w by dw when V is compact oriented of dimension three. In essence the form w is the logarithmic derivative of a transverse volume element and the GV invariant is an obstruction to finding a holonomy invariant tranverse volume element ie one which does not change when one moves along a leaf keeping track of the way the nearby leaves are developing.

More generally the factors of type II are those which possess a trace and those of type III are those which are neither of type I nor of type II. In the foliation context, a holonomy invariant tranverse volume element allows one to integrate the ordinary trace of operators and this yields a trace on the von Neumann algebra of the foliation.

Until the work of the Japanese mathematician Minoru Tomita, very few positive results existed on type III factors. The key result of Tomita is that a cyclic and separating vector v for a factor A in a Hilbert space H generates a one parameter group of automorphisms of A by the following recipee :

one considers the modulus square $S * S$ of the closable operator S which sends xv to $S(xv) = x * v$ for any x in A, and then raises it to the purely imaginary power "it". Tomita showed that the resulting unitary operator normalizes A and hence defines an automorphism of A. One obtains in this way a one parameter group of automorphisms of A associated to the choice of a cyclic and separating vector v. He also showed that the phase J of the above closable operator S yields an antiisomorphism of A with its commutant A' which coincides with JAJ. In his account of Tomita's work, Takesaki characterized the relation between the state defined by the cyclic and separating vector v and the one parameter group of automorphisms of Tomita as the Kubo-Martin-Schwinger (KMS) condition, which had been formulated in C^*-algebraic terms by the physicists Haag, Hugenholtz and Winnink.

The key result of my thesis (in 1972) is that the class modulo inner automorphisms of the Tomita automorphism group is in fact independent of the choice of the (faithful normal) state that is used in its construction. Needless to say it is this uniqueness that allows to define invariants of factors. The simplest is the subgroup $T(A)$ of R which is formed of the periods, namely the set of times t for which the corresponding automorphism is inner. This, together with the spectral invariant $S(A)$, led me to the classification of type III factors into subtypes III_s for s in $[0, 1]$ and the reduction from type III to type II and automorphisms done in my thesis except for the case III_1 which was later completed by Takesaki. All of this goes back to the beginning of the seventies and will suffice for this first heart beat. It is only the beginning of a long saga which is far from over hopefully, and whose main theme is this mysterious generation of an intrinsic "time" that emerges from the noncommutativity of a von Neumann algebra. Exactly as manifolds come with a natural "smooth" measure class, a noncommutative space X generally gives rise to a von Neumann algebra A which encodes the natural measure class on X. It is thus a totally new feature of the noncommutative world that the corresponding time evolution is well defined and gives a canonical homomorphism :

$$\delta : \mathbb{R} \to \mathrm{Out}(A)$$
$$1 \to \mathrm{Int}(A) \to \mathrm{Aut}(A) \to \mathrm{Out}(A) \to 1$$

where the second line gives the definition of the group of outer automorphisms $\mathrm{Out}(A)$ of A as the quotient of the group $\mathrm{Aut}(A)$ of automorphisms by the normal subgroup $\mathrm{Int}(A)$ of inner automorphisms (which are obtained by conjugating by a unitary element of the algebra A).

AC said...
Dear Urs
What you need to understand is that all the interesting stuff here occurs when the number of degrees of freedom involved is infinite. A typical example is quantum statistical mechanics (such as a spin system

on a lattice). Systems occuring in quantum field theory, in the examples related to prime numbers are all involving infinitely many degrees of freedom and are most often of type III. Very simple quantum mechanical systems are of type I, of course and the deeper structure does not appear there. It has nothing to do with anomalies.

AC said...
Dear Urs
The time evolution is as "canonical" as it can be since any noncommutative algebra has inner automorphisms. Moreover one can show that the time evolution belongs to the center of $\mathrm{Out}(A)$!

If you take very simple examples as the lattice case you will find that an inner automorphism essentially ony affects what happens on finitely many lattice sites. In a simple translation invariant product situation, the hamiltonian (which generates the time evolution we are talking about) is an infinite sum of contributions of lattice sites and its essence is unaltered by a perturbation coming from finitely many terms in the sum. It is the fact that the sum is infinite and does not belong to the algebra of observables that creates the type III behavior.

You can slightly perturb this time evolution by an inner automorphism but its overall global action on the algebra of observables will remain essentially unaltered, since it will only be changed on finitely many of the degrees of freedom. Put in other words this "time evolution" of the algebra is taking place overall, on all degrees of freedom, whereas inner automorphisms only control a total of finitely many such degrees of freedom !

You need to carefully study various examples, including foliations, the set of primes, or the case of QFT to appreciate what is going on... (and I need to get some sleep at this point)...
October 31, 2007 at 10 :16 PM

AC said...
Dear Urs
It is not really nicely spelled out anywhere, so the best is to understand the basic idea in an example without entering in the technicalities. Consider the spin system on an infinite lattice.
The algebra of observables is the inductive limit of the finite dimensional algebras that come from tensor products of matrix algebras over finite subsets of the lattice. By construction these only involve finitely many lattice sites at a time. Thus an inner automorphism - since it is implemented by a unitary element of the algebra - really only "sees" finitely many degrees of freedom.
November 1, 2007 at 5 :49 PM

AC said...
Dear Anonymous
The space-time which allows to recover the Standard Model coupled to gravity is of type I, since it is the product of a manifold M by a finite space F ie a space whose algebra of coordinates is finite dimensional. It is not at this level that we expect to get "emergent time" but rather at the level of the algebra of observables in QG. The origin of this idea comes from Carlo Rovelli who - completely independently from the KMS story - had found by reflecting about basic philosophical issues in QG that the "time we feel" (as opposed to a time coordinate in space-time) should be of thermodynamical nature and should be tied up to a thermal state : the heat bath of the relic photon radiation which breaks naturally Lorentz invariance. The real thing now is to put one's hands on a good model for an algebra of spectral observables in QG. Some ingredients towards this are explained at the end of our forthcoming book with Matilde Marcolli. But I'd rather tell the story in one of the coming "heart beats" rather than explain it in a comment...
November 2, 2007 at 2 :42 PM

AC said...
Dear Anonymous
I don't like to be too negative in my comments. Li's paper is an attempt to prove a variant of the global trace formula of my paper in Selecta. The "proof" is that of Theorem 7.3 page 29 in Li's paper, but I stopped reading it when I saw that he is extending the test function h from ideles to adeles by 0 outside ideles and then using Fourier transform (see page 31). This cannot work and ideles form a set of measure 0 inside adeles (unlike what happens when one only deals with finitely many places).
July 3, 2008 at 7 :50 AM

AC said... 1/8/8
As an epilogue of this long-overview articles on the Workshop at Vanderbilt University we would like to thank all the speakers for their spontaneous and generous participation and for sharing their ideas with us about the field with one element and the new connection with NCG. We also would like to thank all the participants for coming to the talks and patiently listening to the discussions which were at times intense and certainly "very alive" and stimulating...

Monday, August 4, 2008
IRONY
In a rather ironical manner the first Higgs mass that is now excluded by the Tevatron latest results is precisely 170 GeV, namely the one that was favored in the NCG interpretation of the Standard Model, from the unification of the quartic Higgs self-coupling with the other gauge couplings and making the "big desert" hypothesis, which assumes that there is no new physics (besides the neutrino mixing) up to the unification scale. My first reaction is of course a profound discouragement, mixed with an enhanced curiosity about what new physics will be discovered at the LHC.
I'll end with these verses of Lucretius :
Suave, mari magno turbantibus aequora ventis,
e terra magnum alterius spectare laborem ;
non quia vexari quemquamst jucunda voluptas,
sed quibus ipse malis careas quia cernere suave est.

[Pleasant it is, when over a great sea the winds trouble the waters,
to gaze from shore upon another's tribulation :
not because any man's troubles are a delectable joy,
but because to perceive from what ills you are free yourself is pleasant.]

Tuesday, February 21, 2012
Galois
This is just a very short post for those interested in a basic talk about Galois, his relations with the French mathematicians of his time, and a general introduction to the "theory of ambiguity". The talk is in French, available at http://www.alainconnes.org/fr/videos.php
Do not forget to click on the "HD" symbol on the screen to get a better quality of video..

Saturday, June 9, 2012
SAD NEWS
It is with profound sadness that we learn about the sudden death of Jean Louis Loday who fell by accident off his sailing boat on June 6th. We loose an outstanding mathematician with so many great achievements and a wonderful friend of many years.

Friday, August 10, 2012
A DRESS FOR THE BEGGAR ?

Since 4 years ago I thought that there was an unavoidable incompatibility between the spectral model and experiment. I wrote a post in this blog to explain the problem, on August 4 of 2008, as soon as the Higgs mass of around 170 GeV was excluded by the Tevatron. Now 4 years have passed and we finally know that the Brout-Englert-Higgs particle exists and has a mass of around 125 Gev.
In the meantime the problem of this discrepancy in the Higgs mass seemed very hard to resolve and this certainly slowed down quite a bit the interest in the spectral model since there seemed to be no easy way out and whatever one would try would not succeed in lowering the Higgs mass. The reason for this post today is that this incompatibility has now finally been resolved in a fully satisfactory manner in a joint work with my collaborator Ali Chamseddine, the paper is now on arXiv at http://fr.arxiv.org/pdf/1208.1030
What is truly remarkable is that there is no need to modify the spectral model in any way, it had already the correct ingredients and our mistake was to have neglected the role of a real scalar field which was already present and whose couplings (with the Higgs field in particular) were already computed in 2010 as one can see in http://fr.arxiv.org/pdf/1004.0464
This completely changes the perspective on the spectral model, all the more because the above scalar field has been independently suggested by several groups as a way for stabilizing the Standard Model in spite of the low experimental Higgs mass. So, after this fruitful interaction with experimental results, it is fair to conclude that there is a real chance that the spectral approach to high energy physics is on the right track for a geometric unification of all known forces including gravity.

A few words about the picture, the metaphor of the Standard Model as a beggar with a diamond inits pocket was suggested by Daniel Kastler a long time ago, so this explains the character on the right. The character on the left wears the symbols of NCG, ingredients of spectral nature which allow one to reconstruct the geometry from gravitational observables such as the spectrum of the Dirac operator, and to write down the action of the Standard Model coupled to gravity.

Tuesday, October 30, 2012
THE MUSIC OF SPHERES
The title of this post, the music of spheres, refers to a talk *The music of shapes* https://www.dailymotion.com/video/xuiyfo which I gave in Lille, on the 26th of September, on the occasion of a joint meeting with the Fields Institute. The talk is an introduction to the spectral aspect of noncommutative geometry and its implications in physics.

The starting point is the naive question "Where are we ?", or how is it possible to communicate to aliens our position in the Universe. This question leads, in the Riemannian framework of geometry, to that of determining a complete set of geometric invariants, both for a space and for a point in a space. The theme of Mark Kac, "Can one hear the shape of a drum ?" associates to a shape its musical scale which is the spectrum of the square root of the Laplacian, or better of the Dirac operator. After illustrating this familiar theme by many concrete examples we give a hint of the additional invariant which allows one to recover the geometric picture, namely the CKM invariant, and illustrate it, in a simplified form, in the simplest possible example of isospectral but non congruent shapes.

What about the relation with music ? One finds quickly that music is best based on the scale (spectrum) which consists of all positive integer powers q^n for the real number $q = 2^{1/12} \sim 3^{1/19}$. Due to the exponential growth of this spectrum, it cannot correspond to a familiar shape but to an object of dimension less than any strictly positive number. As explained in the talk, there is a beautiful space which has the correct spectrum : the quantum sphere of Poddles, Dabrowski, Sitarz, Brain, Landi et all. Its spectrum consists of a slight variant of the q^j where each appears with multiplicity $O(j)$. (See the original paper of Dabrowski and Sitarz
arXiv:math/0209048 (Banach Center Publications, 61, 49-58, 2003)
for the precise formula, and the paper of Brain and Landi
arXiv:math/1003.2150 for a variant and the many references to the mathematicians involved, my apologies to each of them for not puting the list here.)

We experiment in the talk with this spectrum and show how well suited it is for playing music. The new geometry which encodes such new spaces, is then introduced in its spectral form, it is noncommutative geometry, which is then confronted with physics. There the core is the spectral Standard Model of A. Chamseddine and the author which goes back to 1996. We tell the tale of the resilience of this model in its successive confrontations with experiments. Both the start and the end part of the talk are unusual. The previous talk was a talk by Alain Aspect on his recent experiments, with his collaborators, confirming experimentally the "delayed choice" Gedankenexperiment of John Wheeler. So the very beginning of my talk refers to Aspect's point about the subtelty of the concept of "reality" implied by the quantum. The thesis which I defend briefly is that the total lack of control that we have on the outcome of a quantum experiment (we control only the probabilities), is a "variability" which is more primordial than the classicalvariability governed by the passing of time (on which we have no control either). I also explain briefly why time will emerge from the quantum variability. The end part, in the question session, is also unusual, it is a long answer to a question which was posed by Alain Aspect.

The three speakers, Lille 9/26 : E. Ghys, A.Aspect, A. Connes
Update : The talk of Alain Aspect is now also available at the conference website
https://www.youtube.com/watch?v=vqEg4VnoCmc.

Sunday, November 9, 2014
PARTICLES IN QUANTUM GRAVITY
The purpose of this post is to explain a recent discovery that we did with my two physicists collaborators Ali Chamseddine and Slava Mukhanov. We wrote a long paper *Geometry and the Quantum : Basics* https://arxiv.org/abs/1411.0977 which we put on the arXiv, but somehow I feel the urge to explain the result in non-technical terms. The subject is the notion of particle in Quantum Gravity. In particle physics there is a well accepted notion of particle which is the same as that of irreducible representation of the Poincaré group. It is thus natural to expect that the notion of particle in Quantum Gravity will involve irreducible representations in Hilbert space, and the question is "of what ?".

What we have found is a candidate answer which is a degree 4 analogue of the Heisenberg canonical commutation relation $[p, q] = ih$. The degree 4 is related to the dimension of space-time. The role of the operator p is now played by the Dirac operator D. The role of q is played by the Feynman slash of real fields, so that one applies the same recipe to spatial variables as one does to momentum variables. The equation is then of the form $E(Z[D, Z]^4) = \gamma$ where γ is the chirality and where the E of an operator is its projection on the commutant of the gamma matrices used to define the Feynman slash.

Our main results then are that :

1) Every spin 4-manifold M (smooth compact connected) appears as an irreducible representation of our two-sided equation.

2) The algebra generated by the slashed fields is the algebra of functions on M with values in $A = M_2(H) \oplus M_4(C)$, which is exactly the slightly noncommutative algebra needed to produce gravity coupled to the Standard Model minimally extended to an asymptotically free theory.

3) The only constraint on the Riemannian metric of the 4-manifold is that its volume is quantized, which means that it is an integer (larger than 4) in Planck units.

The result 1) is a consequence of deep results in immersion theory going back to the work of Smale, and also to geometric results on the construction of 4-manifolds as ramified covers of the 4-sphere, where the optimal result is a result of Iori and Piergallini asserting that one can always assume that the ramification occurs over smooth surfaces and with 5 layers in the ramified cover. The dimension 4 appears as the critical dimension because finding a given manifold as an irreducible representation requires finding two maps to the sphere such that their singular sets do not intersect. In dimension n the singular sets can have (as a virtue of complex analysis) dimension as low as $n-2$ (but no less) and thus a general position argument works if $(n-2)+(n-2)$ is less than n, while $n = 4$ is the critical value.

The result 2) is a consequence of the classification of Clifford algebras. When working in dimension 4, the sphere lives in five dimensional Euclidean space and to write its equation as the sum of squares of the five coordinates one needs 5 gamma matrices. The two Clifford algebras $Cliff(+,+,+,+,+)$ and $Cliff(-,-,-,-,-)$ are respectively $M_2(H) + M_2(H)$ and $M_4(C)$. Thus taking an irreducible representation of each of them yields respectively $M_2(H)$ and $M_4(C)$.

The result 3) comes from the index formula in noncommutative geometry. One shows that the degree 4 equation implies that the volume of the manifold (which is defined as the leading term of the Weyl asymptotics of the eigenvalues of the Dirac operator) is the sum of two Fredholm indices and is thus an integer. It relies heavily on the cyclic cohomology index formula and the determination of the Hochschild class of the Chern character. The great advantage of 3) is that, since the volume is quantized, the huge cosmological term which dominates the spectral action is now quantized and no longer interferes with the equations of motion which as a result of our many years collaboration with Ali Chamseddine gives back the Einstein equations coupled with the Standard Model.

The big plus of 2) is that we finally understand the meaning of the strange choice of algebras that seems to be privileged by nature : it is the simplest way of replacing a number of coordinates by a single operator. Moreover as the result of our collaboration with Walter van Suijlekom, we found that the slight extension of the SM to a Pati-Salam model given by the algebra $M_2(H) \oplus M_4(C)$ greatly improves things from the mathematical standpoint while moreover making the model asymptotically free ! (see Beyond the spectral standard model, emergence of Pati-Salam unification.) To get a mental picture of the meaning of 1), I will try an image which came gradually while we were working on the problem of realizing all spin 4-manifolds with arbitrarily large quantized volume as a solution to the equation.
“The Euclidean space-time history unfolds to macroscopic dimension from the product of two 4-spheres of Planckian volume as a butterfly unfolds from its chrysalis.”