Interview of A. Connes for EMS, by G. Skandalis and C. Goldstein

Are there mathematicians of the past you feel close to ?

Close to, I would not say but there is one I admire in particular : Galois. There's a very striking characteristic in his writings, their formulation is amazingly simple ; for instance *Take an equation with n different roots. Then, first statement, there is a rational function of these roots which takes $n!$ different values when you permute the roots ; and, second statement, the roots are rational functions of this function.*
In spite of the deceiving simplicity of their formulation, using these statements Galois succeeds in going extremely far. He writes down the equation whose roots are the $n!$ different values of the rational function, splits it into irreducible factors and chooses one of them, he writes how the roots of the original equation depend on the roots of this factor and he sees a group. And he shows that this group is independent of all the choices made along the way... To achieve this he characterizes the group abstractly by a unique property : *A function of the roots is rationally determined if and only if it is invariant by this group.* It is so simple ; what I find fabulous is this kind of leap using the power of abstraction, this enormous step in conceptualizing things. The power of Galois's intuition is not based on the idea of symmetry, but on a concept of ambiguity. Naively, you might say he studied the invariance group of certain functions. But Galois's first step is just the opposite, he breaks the symmetry as much as possible, by choosing a function which has no invariance at all. The mathematicians before him – Cardano, Lagrange – worked with symmetric functions of roots. Galois, in the foot steps of Abel, does the opposite, he chooses a function with the least symmetry possible. And this is the function he starts with. What strikes me is the fecundity of these ideas, the various formalisms we have developed to catch them do not yet exhaust their power. Galois's ideas have a clarity, a lightness, a thought provoking potential which remains untamed to this day and finds an echo in the minds of Mathematicians till now. They have generated great concepts like Tannakian categories or the Riemann-Hilbert correspondence... These ideas are very pretty but they are often set out with such pedantry that they look like heavy yokes and you don't get the impression they have been freed to the point Galois had freed them. Other avatars of Galois ideas are the differential Galois theory and the theory of motives which can be seen as a higher-dimensional analog of Galois theory. But have we really understood what Galois had in mind when he was writing :

> *"Mes principales méditations depuis quelque temps étaient dirigées sur l'application à l'analyse transcendante de la théorie de l'ambiguïté. Il s'agissait de voir a priori dans une relation entre des quantités ou fonctions transcendantes quels échanges on pouvait faire, quelles quantités on pouvait substituer aux quantités données sans que la relation pût cesser d'avoir lieu. Cela fait reconnaitre tout de suite l'impossibilité de beaucoup d'expressions que l'on pourrait chercher. Mais je n'ai pas le temps et mes idées ne sont pas encore assez développées sur ce terrain qui est immense"*

There are other examples of mathematicians who really helped me at an early stage as a source of inspiration.. It isn't that I feel close to them at all in what I do, but I admire what they do. At first, I was fascinated by Jacobi, because I found his way of computing marvelous. And by von Neumann, the depth of what he had discovered and the way he talked about it... And by Tomita of course. I was fascinated by Tomita's mysterious personality; he's someone who has succeeded in avoiding all the traps that society tends to set for someone extremely original. He became deaf at two; when he started his research, his thesis advisor gave him a huge book telling him "come back and see me once you have read this book". Tomita met accidentally his thesis advisor two years later, and the latter asked him "how is the book going?" to which Tomita replied "Oh, I lost it after one week" ... But I think the freshest, the most limpid source, is Galois. It's very odd, but I have never separated Galois from this powerful mixture of simplicity and fecundity.

Would you like to say something about Choquet?

I remember the first years I was doing research, I worked alone, at home, but every Thursday, I attended Choquet's seminar. And he shone by his intelligence, his wit. There were questions bursting out, it was extremely open. This shaped me, in depth. And Choquet had something unique : he had been very close to the Polish school of mathematics before the war. And so he knew a lot of things which do not make up the usual curriculum of mathematicians, but which in fact are quite interesting. It is only with Choquet for instance that I learnt the theory of ordinals. You might think that this theory is useless, but that's absolutely false. For instance, I remember once, the IHES had an open-door day, there was a first-grade class, little kids and among them a girl with shining intelligence, and so after the subject of undecidability had been brought up, I gave them an example from the theory of ordinals, the story of the hare and the tortoise. You take a number N, not too big, they had taken 5 or something like that. They had learnt to write numbers in various bases, 2, 3, etc. I explained to them that one writes the number in base 2, then the hare comes and replaces all the 2's by 3's; thus $5 = 2^2 + 1$ gets replaced by $3^3 + 1 = 28$... and the tortoise just subtracts 1; then one writes the result in base 3, and the hare comes and replaces all the 3's by 4's; and the tortoise subtracts 1 again, etc. Well, the extraordinary phenomenon which comes from the theory of ordinals, is that the tortoise wins : after a finite number of steps and even though you have the impression that the hare makes absolutely gigantic jumps each time, you get 0! And what's hard to believe, it is that this cannot be proved in the framework of Peano arithmetic. The proof uses the theory of ordinals! You can in fact show that the number of steps required before the tortoise wins is growing faster than any function of N you can write explicitely. You can see on the computer how many steps it takes to reach 0. But the proof that the tortoise wins, takes one line with the theory of ordinals. What do you do? You take the first number and instead of replacing the 2's by 3's, then by 4's, etc., you replace them by the ordinal ω. For example 5 is $2^2 + 1$, so you write $\omega^\omega + 1$. This is an ordinal, and an ordinal is a well-ordered set, and every decreasing sequence of ordinals necessarily stops. Now when you do the hare's move, it doesn't change anything, but the tortoise's move subtracts 1 and you obtain in this way a strictly decreasing sequence of ordinals, this has to stop, and you have the proof. And this proof uses ω^{ω^ω}... so it is not so surprising that it goes beyond Peano arithmetic. This is typical of the kind of things we discussed at Choquet's seminar. And this is a partly-forgotten mathematical culture, but which is in fact an extremely rich one. We live in a mathematical world which is more and more monocultural. We proclaim principles to say which mathematics is important and which isn't. I try to defend diversity. I believe it is crucial to let schools blossom. This is very important for the health of Mathematics.

Operator algebras and coincidences : How did it all begin ?

In 1970, I went to the Les Houches summer school [in physics], sent by Choquet ; at that time, I had been working on non-standard analysis, but after a while I had found a catch in the theory....The point is that as soon as you have a non-standard number, you get a non-measurable set. And in Choquet's circle, having well studied the Polish school, we knew that every set you can name is measurable ; so it seemed utterly doomed to failure to try to use non-standard analysis to do physics. But it did offer me as a passport to Les Houches in 1970. And from there, I was taken on as a fellow at the Battelle Institute and I got an invitation for Seattle. I accepted it mostly to visit the States – I did not even look at the program. And the coincidence that occurred is that I stopped in Princeton to visit my brother and I bought a book, at random, at the Princeton bookstore. I hesitated among several books until I came across one which fascinated me, by Takesaki on Tomita's theory. And as I knew I was going to have a long train trip, I bought the book. And I contemplated the book – I can't say I read it, it was really too hard – during the trip through the plains of the Middle West. And the most extraordinary coincidence was that, when I arrived in Seattle, the first day I went and saw the program of the conference and there was Takesaki lecturing on Tomita's theory. From that day, I said to myself : That's it, I don't go at any other lecture, just Takesaki's.

Not a very scientific attitude...

No, and moreover at this time I was fascinated by everything Japanese ; it was more at the level of a sensibility to something totally different, that I didn't know at all...If there is a lesson to be drawn, it is that this pulled me completely out of the circle of ideas I was engrossed in at the time. And just then, there was another coincidence, so that when I came back, I had one more incredible stroke of luck. I had understood a bit of Tomita's theory, a small bit ; I wasn't able to do research. But when I came back, I told myself I would go to the seminar in Paris which deals with operator algebras. So I went to the Dixmier seminar and the first time I went there, it was the organizational meeting, the main theme for the year was to be the Araki-Woods work on infinite tensor products. Dixmier was distributing the papers among the participants, a bit at random ; there was just one left, I raised my hand. Riding home on the RER [suburban train], I was bored, I looked a bit at the paper I had been given, and then I was really knocked over backwards. I realized that in this paper there were formulas that I would have had to be a complete idiot not to see were identical, exactly matching those in Tomita's theory. And these formulas said that a certain vector was an eigenvector for the operator defined by Tomita. An hour after I arrived at home, I wrote a letter to Dixmier saying : here are the Araki-Woods invariants and here is Tomita's theory, you can see that one can get the first invariants from the intersection of the spectra of Tomita's operators, and I gave him the formulas. And since I had been raised by Choquet, I wrote all this in half a page. Dixmier immediately wrote back : *What you write is totally incomprehensible, I need details.* And so I wrote back three pages of details, which wasn't difficult, explaining that one could define an invariant, that I called S [1]. Dixmier fixed an appointment with me for after his next seminar. I went to see him and all he said then was : "Foncez" which in French is a strong form of "Go ahead !" That was the point of departure.

[1]the intersection of the spectra of all modular operators

It was really an incredible piece of luck : it wasn't really difficult. Though not exactly written black on white, it was there in the formulas. It is sure that if I had remained in Paris, if I hadn't moved outside of my circle, I would have continued to work in a narrow direction, and I wouldn't have opened up to totally different horizons. I really had this impression at that moment that I got a breath of completely fresh air which allowed me to access to a more central part of mathematics. I have often had the impression that there are concentric circles in the mathematical world ; that one begins to work in a totally eccentric part and one tries to get gradually closer to the heart.

What is this heart ? Is it subjective ?

What I mean by the heart of mathematics is that part which is interconnected to essentially all others. A bit like all roads lead to Rome, what I mean is that, when the mental picture you get of a mathematical subject becomes more and more precise, you realize in fact that, whatever the topic you begin with, if you look at it sufficiently precisely, after a while, it converges toward this heart : modular forms, L functions, arithmetic, prime numbers, all sorts of things linked to that. It is not that these things are more difficult and I would hate to follow the wrong example I was discussing before of putting down eccentric topics. What I mean is that if you walk long enough, you are obliged to go towards these domains, you cannot remain outside. If you do, it is a bit out of fear. You can succeed in doing a lot of things by refining techniques in a given topic, but unless you keep moving towards this heart you feel you are left outside. It is very strange and surely subjective.

In your research, you have got lightning-like results - you mentioned earlier the discovery of the invariant S, there is also the case of the 2×2 cocycles – and others which cost you considerable efforts.

Of course. This 2 by 2 matrix trick which is of a utter simplicity[2] came indeed to me all of a sudden, but only after I had spent three months doing horrible computations - I was doing concrete computations of modular automorphisms with almost-periodic states, etc, In fact, before discovering this cocycle property, I had come across it by experience. The 2 by 2 matrix trick came to me by chance in a flash, but because the ground had been prepared by tons and tons of examples, tons and tons of computations. My impression is that I have never obtained anything at low cost. All my results have been preceded by preparatory ones, setting up work, a very long experimentation - hoping that at the end of this experimentation, an incredibly simple idea occurs which comes and solves the problem. And then you need to go through the checking period, almost intolerable because of the fear you have of being mistaken. I will never let anyone believe that you can wait just like this until results come all by themselves. I spent the whole summer [of 2006] checking a formula which gives the standard model coupled with gravitation in our joint work with Chamseddine and Marcolli. The computation is monumental : in the standard model, there are four pages of terms with coefficients $\frac{1}{8}$, $\frac{1}{4}$, of sine of cosine of the Weinberg angle... and if you have not checked everything with all the coefficients, you can't claim that the computation gives the right result. I found different coefficients than those in Veltman's book, which obliged me to do again and again these computations until Matilde Marcolli [with whom I am writing a book]

[2] Groupe modulaire d'une algèbre de von Neumann C. R. Acad. Sci. Paris, Sér. A-B, 274, 1972.

realized that the coefficients we had were the right ones and had already been corrected by Veltman in his second edition! There is always this permanent fear of error which doesn't improve over the years... And there is this part of the brain which is permanently checking, and emitting warning signals. I have had haunting fears about this. For example, some years ago, I visited Joachim Cuntz in Germany, and on the return train I looked at a somewhat bizarre example of my work with Henri Moscovici on the local index theorem. I had taken a particular value of the parameter and I convinced myself on the train that the theorem didn't work. I became a wreck – I saw that in the eyes of the people I crossed on the suburban train to go back home. I had the impression that they read such a despair in me, they wanted to help... Back home, I tried to eat, but I couldn't. At last, taking my courage in both hands, I went to my office and I redid the verifications. And there was a miracle which made the theorem work out in this case... I have had several very distressing episodes like this.

Concerning heuristics : you have written several times that geometry is on the side of intuition. On the other hand, formulas seem to play a leading role in the way you work.

Ah, yes, absolutely. I can think much better about a formula than about a geometrical object because I never trust that a geometric picture, a drawing, is sufficiently generic. I don't really have a geometrical mind. When there is some geometry problem, and I succeed in translating it into algebra, then it's fine. There are several steps, first the translation, then the purely algebraic thinking. I always try to distinguish between the intuitive side, the geometrical one, and the linguistic one, the algebraic one, in which one manipulates formulas, and I think much better on that side. For me, algebra unfolds in time : I can see a formula live and turn and exist in time, whereas geometry has something instantaneous about it and I have much more difficulty with it. As far as I go, formulas create mental pictures.

You often give the impression you love computations.

Absolutely. My mathematical thinking is heavily dependent on computations. But, of course computing does not suffice, then, one has to interpret things at the conceptual level. Galois was one of the first to understand that one can deal with a computation even if the latter is not practically feasible. For instance, take an equation of degree 7, the polynomial that Galois associates has degree 7! ; and one has to factorize it. What Galois says,

> *"Sauter à pieds joints sur ces calculs ; grouper les opérations, les classer suivant leurs difficultés et non suivant leurs formes ; telle est selon moi, la mission des géomètres futurs "*

is that one should jump *above* the computations, organize them according to their difficulty. One should do them, but only like a thought experiment in one's mind, not in a concrete manner. In Galois' example, you can give an explicit function f of the roots of an equation $E = 0$ which takes the $n!$ different values when you permute the roots, you just take a linear form with generic rational coefficients. You can then go ahead and express the roots of $E = 0$ as rational functions of f, this can be done by Euclide's algorithm and by elimination. One can use the computer, and the expression one gets is awfully complicated, even when the starting equation $E = 0$ has degree 4 or 5. If you tried and implement concretely the computations, you would quickly get lost in the complexity of the results. On the contrary, what you have to be able to do, is to perform them abstractly, and to build mental objects which represent the intermediate steps and results at an idealized level. I always proceed in the following way : whatever the complexity of the problem, instead of trying it first on a piece of paper, with a pencil, I just go out for a walk, and try to have all the ingredients present in my mind, in order to start manipulating them mentally. Only after this exercise, am I able to see clearly, think about the various steps and begin to get a mental picture. This is a painful process which consists in gathering in your mind, in your memory, all the elements of the problem, in order to begin manipulating them. It is an exercise that I recommend - well, of course, different people function differently- if one wants to be able not to depend upon paper and pencil. Because with paper and pencil, you get tempted to start writing immediately, and if you haven't thought long enough before, you will get nowhere. You will get discouraged before having had enough time to create in the linguistic part of the brain specific mental pictures that you can then manipulate, as usual, by zipping them, transforming them into something smaller, and then moving them around.

If you make computations, it is crucial to avoid mistakes... There are ways to check, for instance using different paths to the same result. Also one can see if the result of a computation looks right or not. I remember when I worked with Michel Dubois-Violette, we had a sum of 1440 integrals, each of which was an integral over a period of a rational function of theta functions and their derivatives. We expected the sum to have a simple factorization. Indeed, we found a simple result which was a product of modular forms, elliptic functions, *etc.* When you find that a huge sum like that gives a product, you feel rather confident that no mistake was made along the way...

Non commutative geometry

What is non-commutative geometry ? In your opinion, is 'non commutative geometry' simply a better name for operator algebras or is it a close but distinct field ?

Yes, it's important to be more precise. First, non-commutative geometry for me is this duality between geometry and algebra, with a striking coincidence between the algebraic rules and the linguistic ones. Ordinary language never uses parentheses in writing words. This means that associativity is taken into account, but not commutativity, which would permit permuting the letters freely. With the commutative rules my name appears 4 times in the cryptic message a friend sent me recently :

> *"Je suis alenconnais, et non alsacien. Si t'as besoin d'un conseil nana, je t'attends au coin annales. Qui suis-je ?"*

So somehow commutativity blurs things. In the non-commutative world, which shows up in physics at the level of microscopic systems, the simplifications coming from commutativity are no longer allowed. This is the difference between non-commutative geometry and ordinary geometry, in which coordinates commute. There is something intriguing in the fact that the rules for writing words coincide with the natural rules of algebraic manipulation, namely associativity, but not commutativity.

Secondly, for me, the passage to non-commutative is exactly the passage from a completely static space in which points do not talk to each other to a non-commutative space, in which points start being related to each other, as isomorphic objects of a category. When some points are related to each other, they will be represented by matrices on the algebraic side, exactly in the same way as Heisenberg discovered the matrix mechanics of microscopic systems.

One does not go very far if one remains at this strictly algebraic level, with letter manipulations... and the real point of departure of non-commutative geometry is von Neumann algebras. What really convinced me that operator algebras is a very fertile field is when I realized - because of this 2 by 2 matrix trick - that a non-commutative operator algebra evolves with time! It admits a *canonical* flow of outer automorphisms, and in particular it has "periods"! Once you understand this, you realize that the non-commutative world instead of being only a pale reflection, a meaningless generalization of the commutative case, admits totally new and unexpected features, such as this generation of the flow of time from non-commutativity.

However, I don't identify non-commutative geometry with operator algebras, this field has a life of its own, new phenomena are discovered and it is very important to study operator algebras per se - I have spent a large part of my life doing that. But on the other hand, operator algebras only capture certain aspects of a non-commutative space, and the "only" commutative von Neumann algebra is $L^\infty[0,1]$! To be more specific von Neumann algebras only capture the measure theory, and Gelfand's C^*-algebras the topology. And there are many more aspects in a geometric space : the differential structure, and crucially the metric. Non-commutative geometry can be organized according to what qualitative feature you look at when you analyze a space. But, of course, as a living body you cannot isolate any of these aspects from the others without destroying its integrity. One aspect on which I worked with greatest intensity in recent times is a shift of paradigm which is almost forced on you by non-commutativity : it bears on the metric aspect, the measurement of distances. This is where the Dirac operator plays a key role. Instead of measuring distances effectively by taking the shortest path from one point to another, you are lead to a dual point of view, forced upon you when you are doing non-commutative geometry : the only way of measuring distances in the non-commutative world is spectral. It simply consists in sending a wave from a point a to a point b and then measuring the phase shift of the wave. Amusingly this shift of paradigm already took place in the metric system, when in the sixties the definition of the unit of length which used to be a concrete metal bar, was replaced by the wave length of an atomic spectral line. So the shift which is forced upon you by non-commutative geometry already happened in physics. This is a typical example where the non-commutative generalization generates an abrupt change even in the commutative case. I realized recently that the only information we have on the very distant universe is spectral. I hadn't understood that the 'red shift' is not a frequency shift but a scaling of frequencies. If you look far enough back in the universe, frequencies are divided by a factor of up to a 1000. This is amazing. And you see it purely in a spectral way. This spectral point of view is the one which appears from experiments, when you study the universe, this is no fantasy. And this is a compulsory point of view when you look at a geometrical space from the perspective of non-commutative geometry. From this point of view one is lead very naturally to the spectral action principle which allows one to encode geometrically in a nut-shell the tremendous complexity of the standard model coupled with gravity. What happens is simply that space-time admits a fine structure, a bit like atomic spectra, and is neither a continuum nor a discrete space but a subtle mixture of the two.

In the book I am writing with Matilde Marcolli, we develop the first three hundred pages on physics : the standard model and renormalization - linked to motives and Galois groups- and the last three hundred pages on the zeta function, its spectral realization and the spontaneous symmetry breaking of arithmetic systems. We are reaching the end of the write-up of the book and we are finding out that quite surprisingly there is a deep relation between the two a-priori disconnected pieces of the book. In fact there is an analogy, a conversion table, between the formalism of spontaneous symmetry breaking which is used for arithmetic systems, zeta functions, dual systems *etc.* and a formalism which seems extremely tempting to people who are trying to quantize gravity. While establishing this dictionary, we found out in the literature that the notion of KMS state, which plays a fundamental role in our work on symmetry breaking for arithmetic systems, also plays a role in the electroweak symmetry breaking which gives masses to particles in the standard model. This allows us to go further in the analogy and it suggests that the people who are trying to develop quantum gravity in a fixed space are on the wrong track. We know that the universe has cooled down, well, it suggests that when the universe was hotter than, say, at the Planck's temperature, there was no geometry at all, and that only after the phase transition was there a spontaneous symmetry breaking which selected a particular geometry and therefore the particular universe in which we are. This is something we would never have thought of - we would never have had this idea - if our book was not written with the two parallel texts - of course there is no point where one part really uses, or relies, on the other - but you can see an analogy emerging between the two parts.

As André Weil pointed out, this type of mysterious similarity is one of the most fertile things in mathematics. The human mind is still ahead of the computer, for the moment and for a long time to come I hope, to detect the structural analogies between theories which look quite different in content, but in which the same kind of phenomena appear. Translation will never be a literal one, and there will always be two texts written in two different languages and there will never be a one to one correspondence between the words of one language and the words of the other. But there will be these strange hints which may well evaporate if you try to rush and write them down too precisely. There are boxes that are very well understood on one side - and not understood at all on the other. Even if it doesn't provides a key to open something, it binds us, it forces us to think from the other side.

It's true that the name 'non-commutative geometry' is a bit unfortunate, because there is this 'non', the negation. What is important is to think of it as 'non necessarily commutative', so that it includes the commutative part. We could have given it 36 other names. A name that would have been better in the Riemannian part is 'spectral geometry'. What this geometry shows so well is that all the things we perceive are spectral. That seeing them from the set-theoretic point of view, is not the right stand-point. We could have used different names ; though certainly not 'quantum'.

Why ?

Because in the use of the word 'quantum' there is a perversion, *i.e.* people don't understand that the word 'quantum', from the beginning, is not so much 'non-commutative' but rather 'integer'. In the word 'quantum' there is really this discovery by Planck, of the formula for blackbody radiation, from which he understood that energy had to be quantized in quanta of $\hbar\nu$. There is a terrible confusion, created by people doing deformation theory who let one believe that quantizing an algebra just means deforming it to a non-commutative one. They take a commutative space and since they deform the product into a non-commutative algebra, they believe they are quantizing. But this is completely wrong : you succeed in quantizing a space only if you give a deformation into a very specific algebra : the algebra of compact operators. And then, there is an integrality, the integrality of the Fredholm index. The use of the wrong vocabulary, creates confusion and does not help at all to understand. That's why I am so reluctant to use the word 'quantum' - this looks more flashy, perhaps, but the truth is that you are doing something quantum only in very particular cases, otherwise you are doing something non-commutative, that's all. Then this may be less fashionable at the linguistic level, but never mind : it is much closer to reality.

What is more important for you in your mathematical work : unity or evolution ?

It's difficult to decide. Every mathematician has a kind of Ariadne's thread which he follows from his starting point, and that he should absolutely try not to break. So, there is a unity, a kind of trajectory, which makes you start from a place, and because you have started there, in a slightly bizarre and special place, you have a certain originality, a certain perspective, different from that of the others. And this is essential, otherwise, you put everybody in the same mould, everybody would have the same reactions to the same questions. This is not what we want, we want different people who have their own approaches, their own methods. So there is a unity in the trajectory, which is not at all the unity of mathematics. The unity of mathematics, you discover bit by bit, when you realize that extremely different trajectories, of extremely different people, get closer to the same vibrant heart of mathematics. But what I have felt above all is the unity, the fidelity to a trajectory.