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On the status of perturbation theory

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Abstract

Perturbation theory has always been an important part of natural sciences. From celestial mechanics to quantum theory of fields it has always played a central role that this little note would like to analyze briefly. We will show in particular how its epistemological situation has changed, from being a "tool" to being the basis of definition of objects in quantum field theory.

1. Introduction

Perturbation is often considered as something not essential, something sometimes helpful but without any conceptual power. We would like to show in this paper that it played a very important role in the development of the physics and mathematics of the XXth century. After having participated to the birth of the theory of dynamical systems in the pioneering work of H. Poincaré, perturbation theory was essential for the emergence of quantum mechanics, in particular in the works of M. Born and W. Heisenberg. More recently quantum field theory, mostly perturbative, is the theory of elementary particles and its most spectacular success are once again perturbative.

On these three examples one can already see the difference of status that perturbation theory takes: in celestial mechanics (Poincaré) it has the property of suggesting new paradigms INSIDE a given theory (invention of chaos). In the second case perturbation theory causes a fundamental conceptual change: the birth of quantum mechanics. Finally in quantum field theory it takes a more conceptual place: the theory exists essentially through its perturbative aspects and the question of knowing if it is or not an approximation of something else is still open..

We would like to develop here 3 ideas: perturbation theory is at the center of sciences in XXth century, it occupies a more and more important place by its own way of producing negative results, and it *charms* by its ability of dynamically bouncing after meeting a difficulty.

2. What means perturbation?

Let us start with a very simple fact: cases where one can solve explicitly a scientific problem are very rare. At the opposite of this situation, generic systems are very difficult to handle. Nevertheless inside this last group, there exist systems which are not simple, but close to simple ones, that is perturbations of explicit systems. Close means here that the expression of the problem is close to a simple one. And the perturbation machinery will try to build up a solution "close" to the one of the simple problem.

Let us be more synthetic; consider a system Ω that we see as a system Ω_0 perturbed by a "small" term P_0. We will write this (without any precise signification of the sign+)

$$\Omega \;=\; \Omega_0 + P_0.$$

The method will consist in *changing our look* on Ω (in practice this will mean for example performing a change of variable, or conjugating by a unitary operator) in such a way that the system Ω "seen" in this different way will appear under the form

$$\Omega_1 + P_1$$

that is of the same form than Ω, but with a P_1 much smaller than P_0 (for example $P_1 \sim (P_0)^2$). Then we are reduced to a much simpler, in the sense that it is closer to Ω_0. Let us suppose that we play this game another time, producing

$$\Omega_2 + P_2$$

with, e.g., $P_2 \sim (P_1)^2$. Here again we end up with something even closer to Ω. Implementing this series of change of "regard" on our system we get

$$\Omega = \Omega_0 + P_0 \to \Omega_1 + P_1 \to \Omega_2 + P_2 \to \dots (\textit{potentially}) \dots \Omega_\infty + P_\infty = \Omega_\infty$$

since $P_\infty \sim (P_0)^\infty = 0$.

A perturbation theory will produce the Ω_∞ as a series

$$\Omega_\infty = \Omega_0 + \omega_1 + \omega_2 + \dots$$

This series, which might converge or not, is truly what a perturbation theory gives to us: an algorithm of reducing the distance between a given system and an explicitly solvable one.

3. Perturbation in celestial mechanics and the invention of chaos

In 1982 Henri Poincaré publish his "Méthodes nouvelles de la mécanique céleste". What interests Poincaré is the famous 3-body problem: is the system consisting of the sun and two planet integrable?

An integrable system is a dynamical system all trajectories of which are simple in the sense that they consist in linear flow on tori. For an integrable system the phase-space is foliated by (invariant by the flow) tori (see next section for details) and on each of them the flow is linear in time, that is, it looks like a free particle's one. The two bodies system of celestial mechanics is integrable (by reduction to the center of mass) and it has been a long time challenge to know if a perturbation of this would remain integrable. Since the mass of the planet are small compared to the sun's one, and the gravitation law is proportional to the masses, a 3 body celestial system is a perturbation of the 2 body.

The change of look that we were mentioning in the previous paragraph consists here in trying to build invariant tori which would be invariant by the flow, modulo a remainder being smaller and smaller. This task is possible (in the good cases) up to any order in the size of the perturbation, but, in general, the obtained series will NOT converge (we will return to this later on). Therefore the system is not integrable. The way Poincaré handled this is remarkable: let us just say that he took this negative statement is a positive way, showing that the reason for which the series diverges has its own interest, and developing the beginning of the theory of dynamical system: if a system close to an integrable one is not integrable, it might be chaotic.

The way Poincaré insists on the negative results is amazing, but the strong reason is that he wants to understand why the series diverges. Looking just at the "table des matières" of the book is illuminating: almost all the presented series are proved to be divergent, and many sections or chapters of the book contain the word "divergence". Let us quote him, beginning of the last section of the book: *225. Nous avons vu au n^o 212 que les séries auxquelles conduit la méthode de M. Bohlin sont généralement divergentes et j'ai cherché à expliquer le mécanisme de cette divergence. Je crois devoir revenir sur ce sujet et étudier avec quelques détails un exemple simple qui fera mieux comprendre ce mécanisme*[†]. That is, not only the divergence is proven, but Poincaré insists on a simple "toy" example of what happens in the mechanism of divergence, feeling that this mechanism is not a failure but a positive sign toward non-Laplacian causality. In another part of "les Méthodes" he treats the case of homoclinic orbits by showing that the perturbation, as small as we want, of an unstable fixed point situation gives rise to the phenomenon of crossing of invariant manifolds, leading to a figure so complicated that "he wouldn't dare to draw it", and to chaotic behavior. Here again small perturbation of (degenerate) integrability gives complex dynamics.

The importance of the result of Poincaré is twofold: first of all it opens a new area of sciences by braking the idea that integrability was stable under perturbation. As soon as this "taboo" felled down the theory of dynamical system started to develop in a fast way. But the second interest of the result of Poincaré is also that it shows the importance of negative results in sciences (Longo 2005): transforming a bad new (the system of 3 body in not integrable) in a rich positive statement (chaos exists) is, for us, a trace of genius. Making sensitivity to initial condition a new paradigm was extremely powerful and the theory of chaotic dynamical systems became an important piece of science in XXth century. In fact what is best understood since Poincaré are the integrable and the chaotic (hyperbolic systems). Intermediate situations are much more difficult to handle. One has to think as integrability as being a unstable fixed point at the top of a hill, and

[†] 225. We saw at number 212 that the series to which we get by the method of M. Bohlin are generally divergent, and I tried to explain the mechanism of this divergence. I think that I have to go back to this subject and study in detail a simple example which let understand better this mechanism

chaotic systems as being the lowest points in the valley. As we know a little kick let a ball at the top going far down, without stopping in-between.

It took a long time to finally solve in a positive way the problem of perturbation of integrable systems. In 1954 A. Kolmogorov, (Kolmogorov 1954), showed that, roughly speaking, the series of perturbation can be made convergent (in fact they're equivalent to a Newton type implementation) for very special values of the parameters contained in a non-open set of maximal measure. Here appears another way of solving the problem: the perturbed system is not integrable in the sense that not all initial condition will remain stable, but with good probability the initial condition will be in the good set. As we see again the view on the problem has changed. Another beautiful perturbative method is the so-called Chirikov criterium, (Connes and Kreimer 1998), a criterion which predicts transition to stochasticity for a large enough value of the perturbation....with a first order calculation.

Of course an (even very small) panorama of perturbation methods in classical dynamics is out of the scope of this paper. Let us just keep in mind this "pathology": whatever perturbative results are positive or negative they have been up to now always a great source of progress. And it would be, according to us, very prejudicial if, because of increase of numerical computing power, approximate analytical results would be depreciated.

4. The birth of quantum mechanics

After Poincaré published his book, the birth of quantum theory constitutes a new adventure in physics. In 1900 Max Planck publishes his famous paper on radiation of black body, in 1905 Einstein his photo-electric theory and in 1910 Niels Bohr applies them to the atomic structure (generalized a few years later by A. Sommerfeld in the multi-dimensional case, and put in a more synthetic and geometrical way by Einstein in an astonishing paper of 1917).

Giving some details about Bohr theory will show us how the quantum theory (i.e. before quantum mechanics of 1925/1926 (see the article "Discrete-continuous and classical-quantum" in this volume)) was much *phenomenological* and needed a drastic conceptual change (which will be impulsed by the perturbation theory as we are going to see soon).

A classical system is integrable if the phase-space is foliated by tori, invariant by the flow, on which the flow is linear. Another way of saying this is the fact that one can perform a change of variables leading to a system of coordinates "action-angles" (A, ϕ), for which A is constant along the evolution and the angle ϕ evolves linearly with time. Saying this is equivalent to say that, in the new action-angle coordinates the energy reads:

$$h(A),$$

in other words it is independent of the angles.

Bohr (and Sommerfeld) "noticed" that selecting (for the hydrogen atom) those energies corresponding to the discreet values of the action parameter given by

$$A = n\hbar, \; n \in \mathbb{N}$$

gives rise to the experimental values of the energy of the hydrogen atom (more precisely that the differences of "quantized" energies gives the spectral line of hydrogen atom observed experimentally).

This theory, although it gave (and still gives!) wonderful results for more general systems than hydrogen, was bothered by an unsatisfying conceptual setting: no "good" explanation of this golden rule was provided (the analogy with the Kepler's law before the Newtonian theory seems to us relevant). Nevertheless the biggest obstacle to the theory was the fact that one couldn't see how to apply it to general systems, that is to non-integrable ones.

Removing this obstruction took about 15 years. In between Max Born in Göttingen tried to push the Bohr theory into the perturbation setting in a way we are going to see now. In the early 20's Born had a young assistant named Werner Heisenberg and invited him to work on this subject. In 1925 Heisenberg felt the need of "transcending" the commutative convolution algebra often used in classical perturbation theory: the modern quantum mechanics was "born".

What did Born?

Born red Poincaré. And especially the chapter on perturbation theory which gives a natural way of making a system close to integrable in an "action-angles" form, modulo a remainder, non-integrable and as small as we want. More precisely one of the first interest in the book of Poincaré was to provide a systematic way of computing an expansion in series in the size of the perturbation for the change of variable making the initial energy in the form of

$$\sum_{j=0}^{K} h_j(A) + 0(\epsilon^{K+1})$$

where ϵ is the size of the perturbation.

Therefore a *quantization* formula ends up from this sum, in the form of selecting the values of the energies given by

$$\sum_{j=0}^{K} h_j(n\hbar) \ n \in \mathbb{N}$$

by neglecting the remainder.

In his "Vorlesungen" (Born 1925) published in 1924 Max Born explains in great details how this point of view should apply. He treats in detail not only the regular cases, but also the famous degeneracies, sources to the divergence of the series. The vibration situation, the libration ones, the intrinsic degeneracy, the accidental one. One can say that this part is a wonderful exposition of perturbation theory for classical systems. And then he decides to look at phase coherences, that is to the Bohr theory applied to this perturbative situation. He treats the anharmonic oscillator and finish that section by a reference to a work containing applications of this method, in collaboration with Heisenberg. Looking at phase coherence for degenerate system involves more ad more refined calculation taken sometimes from Poincaré, sometimes original.

Finally he arrives to the (excited) helium atom, for which the application of the method fails[‡]. This is his conclusion of the corresponding chapter, and of the whole book. But this conclusion is very interesting as it opens, from a negative result, the necessity of a new theory.

This is not surprising[that the quantization of the perturbation series fails], for the principles used are not really consistent (...) A complete systematic transformation of the classical mechanics into a discontinuous mechanics of the atom is the goal toward which the quantum theory strives[§]

The foreword of this book is dated from November 1924. In the preface of the English translation he says: *Since the original appearance of this book in German, the mechanics of the atom has developed with a vehemence that could scarcely be forseen. The new type of theory which I was looking for as the subject-matter of the projected second volume has already appeared in the new quantum mechanics, which has been developed from two different points of view*[¶]. The second volume appeared (Born and Jordan 1930) in 1930 and this theory of semiclassical perturbations has totally disappeared, replaced by the modern quantum perturbation theory.

This is precisely by working in the Born's group that Heisenberg realized the extension he needed. A general method in classical perturbation theory consists in using Fourier series in order to invert periodic functions. And therefore to deal with the algebra (commutative) of convolution.

$$(a * b)_k = \sum_l a_{k-l} b_l$$

At one point in his (tedious) computations, Heisenberg (in order to fit different formulæwhich didn't) had the idea of replacing the dependence in $k - l$ of a_{k-l} by a full dependence (of a) in both k and l. That is to consider "multiplication" of the form:

$$(a * b)_{k,j} = \sum_l a_{k,l} b_{l,j}.$$

This multiplication law is the law of multiplication of matrices, and the quantum mechanics was born.

What seems to us interesting here is the fact that, once more, perturbation theory created, through its own difficulties, a way of solving the problem by enlarging the paradigmatic structure: once Heisenberg noticed that his little transformation was helpful for his (perturbative) computation, he realized that these ideas extend immediately to the non perturbative situation by giving a new setting for the theory.

But in his paper (Heisenberg 1925) he notes: *One should be able to prove, for example, that the introduction of a small perturbation in the dynamical problem leads to additional terms for the energy (...) of the type of the ones found by Kramers and Born....* And he treats the example of the anharmonic oscillator with small value of the parameter by

‡ let us remind that the helium atom is not perturbative, at the contrary of the celestial 3-body problem

§ Dies ist nicht verwunderlich. Denn im Grunde sind die benutzten Prinzipien keineswegs in sich konsequent (...)Die systematische Verwandlung der klassicher Mechanik in eine diskontinuierliche Atommechanik ist das Ziel, dem die Quantentheorie zustrecht

¶ Heisenberg's and Schrödinger's ones

expansion in powers of it. Let us remark also that perturbations methods were going to remain a very big success in the Schrödinger picture (in which it is much simpler than in classical mechanics) and was part of the success of the new theory ‖. To be convinced of this it is enough to quote M Brillouin, (Brioulouin 1988), in his preface of the French translation of the collection of reprints of the memoirs by Schrödinger: *Tous les phénomènes de perturbations ont étés résolus par M. Schrödinger sans avoir besoin d'autre chose que de renseignements très généraux sur la strucrure de la fonction d'onde non perturbée (...)Effet Zeeman (,,,)- et même effet Raman avant la lettre- ont étés coordonnés de la manière la plus heureuse*[††].

5. Quantum theory of fields

Almost immediately after the birth of quantum mechanics arose the problem of quantizing the electromagnetic field. The reader is refereed to the beautiful historical Introduction in the book by Weinberg (Weinberg 2002). To show the importance of the perturbative setting in quantum fields theory, let us quote Enrico Fermi (Fermi 1930) in 1931:

au lieu de considerer l'atome et la radiation comme deux systèmes. il [Dirac] les considère comme un système unique dont l'énergie est la somme de l'énergie de l'atome, de l'énergie du champ et d'un troisième terme **petit** *vis-à-vis des deux autres et représentant l'énergie due au couplage de l'atome et du champ électromagnétique. Si on négligeait ce dernier terme cela reviendrait à supposer l'atome et le champ complètement indépendants...* [‡‡]*....*

One see here the key role played by the perturbative aspect. The starting of quantum field theory was long and painful, because of the famous infinities which appeared in the computations: not only the convergence of series like $\sum_{j=0} h_j(A)$ were not insured (and still are not), but the coefficients themselves were given by diverging integrals, that is the series itself was not defined. In other words the perturbation technique itself was facing a problem of existence, not convergence. The problem was solved in the late 40s by the so-called renormalization theory and one can say that things went quite fast as soon as one had the idea of imposing to the perturbative calculation the same symmetry invariance than to the general theory, that is to compute perturbations series by keeping relativistic invariance (which was not present in the computations of the 30s).

In other words taking care of the same demand for the perurbative theory than for the

‖ the problem of recovering the quantized classical perturbation series of Born was mathematical solved years after (Graffi and Paul 1987) although it was used in physics since the early days of quantum mechanics

†† All perturbative phenomena have been solved without need of anythink else than very general information on the structure of the unperturbed wave function (...) Zeeman effect (...)- and even Raman effect before its discovery- were coordinated on the happiest manner

‡‡ instead of considering the atom and the radiation as two systems, he [Dirac] consider them as a unique system whose energy is the sum of the energy of the atom, of the energy of the field and of a third term **small** compared to the two other and representing the energy due to the coupling of the atom and the radiation field. Neglecting this last term would lead to suppose the atom and the field totally independent....

general theory was extremely powerful, and created a new starting point of the theory. One can also say that the "mistake" of the early founder of quantum field theory was to think that, each time an infinite quantity appeared, it was the result of a bad understanding of the model, of the theory, and not of the perturbative method. Trying to cure the problem at the perturbative level led to the modern quantum field theory, and to one of the most exciting pieces of sciences of XXth century.

It seems to us that, with renormalization theory, a new status of perturbation theory was born: perturbation theory was not anymore a tool, but something really crucial, the understanding of which is central in the theory.

To insist on the fact that perturbation theory *deserves* a conceptual treatment let us note, after Weinberg, that the crucial computation of the Lamb shift, wihich was an extreme success thanks to the relativistic invariant approach of renormalization, could have been done in the early 30th. Weinberg notes even that the very first complete computation was done in the old, not invariant, formalism in 1949, but what was missing before was the confidence in the computation, confidence given later on by the symmetry invariant approach.

Up to now rigorous attempts to define consistently and rigorously quantum field theory without referring to perturbation "failed", although developing very beautiful mathematical physics. Axiomatic quantum field theory (Streater and Wightman 1964) gave its "lettres de noblesse" to mathematical physics, and constructive quantum field theory (Glimm and Jaffe 1987) produced very important results, going beyond the original scope. But still QED and Φ^4_4 [§§] resist. The general claim is that quantum field theory should be just an approximation of a more general theory which would be well defined and non-perturbative [¶¶] But the richness of concepts involved in quantum field theory was recently pointed out in a very powerful mathematical way (Connes and Kreimer 1998): it appears that understanding the cancellations, key stone of the renormalization approach, in a global way, at any order, involve mathematics related to non-commutative geometry. This is certainly one step more in considering perturbation theory at a true conceptual level.

6. Perturbation theory in quantum mechanics nowadays

As we said before, perturbation theory in the twenties was a definitive key for the confirmation of quantum mechanics. Nowadays most of the heavy computations are done by computers, but the revival of interest in simple quantum systems, due in particular by problems inherited by quantum computations and quantum information provides new situations where the perturbation theory, as being analytical, presents new challenges.

Quantum information, both theoretical and experimental, involves systems of few atoms

§§ Quantum electrodynamics and quantum field theory with quartic interaction in dimension 4

¶¶ some physicist think that, if "resummed" the perturbation series might give a result.....null. This would mean that the interaction is not big enough to give something, in other words the theory would be free (Gallavotti). This would mean that, since the first correction term give excellent agreement with experience, the theory is not correct, except at very small value of the perturbation. In a certain sense determining this would play the role of finding out if the Bell's inequalities are or are not violated at low energy. Therefore nonrelativistic quantum theory is the underlying theoretical setup. These systems being small, they are at the same time difficult to be realized experimentally, and easy to carry out at theoretical, and analytical level. The models are simple and precise results are expected, given by analytical expressions. Not only can perturbation theory provide such precise formulæ, but it gives also a lot of information concerning scaling properties, asymptotic formulæand a lot of other pieces of information that numerical computation are unable to provide.

7. Conclusion

We have concentrated in this paper on three moments reflecting the importance of perturbation theory in modern science. First of all we have shown how Poincaré in his rereading (and probably partial redoing) of calculations of astronomers changed the paradigm of dynamics by introducing the concept of sensitivity to initial conditions. Emergence of modern dynamical systems is certainly a big turn in our vision of the word: small changes can produce big effects. And Poincaré understood immediately how a perturbative expansion can give information, even in absence of convergence. Let us quote here his reflection on the notion of asymptotic series (Poincaré 1987):

Ainsi pour prendre un example simple, considérons les deux séries qui ont pour terme général:

$$\frac{1000^n}{1.2.3...n} \ et \ \frac{1.2.3...n}{1000^n}.$$

Les géomètres [mathématiciens] diront que la première série converge, et même qu'elle conbèrge rapidement, parce que le miliionième terme est beaucoup plus petit que le 999999e ; mais regarderons la seconde comme divergente, parce que le terme général peut croître au delà de toute limite.

Les astronomes[physicie ns], au contraire, regarderons la première série comme divergente, parce que les 1000premiérs termes vont en croissant, et la seconde comme comvergente, parec que les 1000 premiers termes vont en décroissant et que cette décroissance est d'abord très rapide[‖‖].

In short one can say that Poincaré took at positive a negative result (non-convergence of perturbation series) and showed that this divergence create new phenomena.

In the second example we treated (birth of quantum mechanics), the situation is a little different: from non agreement with experimental data of the first terms of perturbation series, Heisenberg concluded (positively) that the paradigm of the theory itself had to

[‖‖] So to take a simple example, let us consider the two series whose general terms are:

$$\frac{1000^n}{1.2.3...n} \ et \ \frac{1.2.3...n}{1000^n}.$$

Géometer will say that the firstseries converges, and event that it converges fast, as the 1000000th term is much smaller than the 999999th; but will look at the second series as divergent, as the general term can increase beyond any limit. Astronomers will look at the first series as divergent, as the 1000 first terms go increasing, et the second as convergent as the 1000th term go decreasing and the decreasing is very fast.

be changed, not only its effects. Building a brand new theory in physics in certainly more stringent than changing of paradigm *inside* the theory. In this case it is not that a property of perturbation theory (non-convergence) is involved, but the whole theory itself, and the result is the invention of a new theory. One understands clearly the epistemological difference: this time perturbation theory creates something totally new.

But in these two cases perturbation theory remains a tool, a tool that can be avoided, after all. Chaotic dynamical system exist outside perturbation, and quantum mechanics found its own definition outside the perturbative regime.

For quantum field theory (our third example) the situation changes drastically: perturbation is not a tool anymore (at least in the current situation), it occupies this time the central place, play the central role, and in a certain sense its own structure IS the conceptual kernel of the theory. Representing the different terms of the expansion by (Feynman) diagrams is not only useful, it reflects (for us) perfectly the situation: the theory is the addition of phenomena that can be expressed simply, but whose "sum" cannot be handled rigorously. From being existential (as leading to computations) the series become essential, developing for itself rich enough conceptual objects that it proposes to us a certain kind of realism, maybe the only realism available.

Most physicists don't have the same opinion: the theory must exist by itself, and the perturbation theory must be only a way of computing. But after so many years, so many success we would like to defend the idea that, in a period where e.g. biology has difficulty in finding a conceptual setting "close" to the one that classical sciences gave us, a physical theory which would live only through its emergence concept, its tangential appearance, its asymptotic effect could have a chance to be *aesthetic* enough to be accepted as a new paradigm for reality. Maybe a free way of considering the world.

Appendix A.

In this appendix we would like to address the question of the pertinence of the perturbative concept outside the scientific activity. In sciences the concept of perturbation has been since a long time a motor, not only for practical computations, but also for conceptual approach, as we tried to prove. It has been so since the physics got to a point of " mathematization" strong enough to support the idea of variation inside the continuum (that is after the Galilean revolution, and effectively after Newton). That knowing something should help knowing something close is very familiar in sciences, but, expressed as we did in section 2, it has nothing to do with mathematics and is perfectly conceivable in other domains. Nevertheless it seems to be almost absent to human sciences. And this suggests to wonder if there is a concept of approximation/perturbation in philosophy.

In fact it is already inside sciences that the status of the perturbation theory is underestimate. Considered for a long time as a tool, it might be though to be replaceable by computer and numerical computations. But what is behind this seems to be, according to us, a natural tendency to something that could be called a "naive realism" (but a natural one) in the set of scientific theories. For most of the physicists perturbation theory is something behind which a "true" theory exists. And this is certainly inherited from the history of modern physics, always showed up as built form well establish concepts, although constructed from painful vague considerations. Bachelard points out (Bachelard 1969) how a given physical theory has always a part of approximation, a "shadow" part, which is absolutely necessary. One example is the kinetics theory of gas, for which nothing is really said about the phenomenon of shocks between molecules, and even the theory needs this Indetermination of "realism" at the microscopic scale. In fact it is our position in front of fundamental concept which is in question. And this is particularly obvious in biology where it seems that the equivalent of Galilean revolution didn't happen yet. Let us remember that in (Graffi and Paul 1987) Bachelard wants to construct "une philosophie de l'inexact".

Vladimir Jankélévitch uses a lot (maybe abuses) approximation concept in philosophy. Presque-rien, je-ne-sais-quoi are stones of a philosophy considered as being far from epistemology, but which could very well get in resonance with this "presque-rien" perturbative taking the place (e.g. in quantum field theory) of the je-ne-sais-quoi behind the perturbative aspects. And the second part of the title of (Jankelevitch 1950), "introduction à une philosophie du presque" leads, maybe, to thing that a philosophy "première" is linked with the concept of perturbation.

In fact, to continue a bit with Jankélévitch, let us say that perturbation approach, considered at first glance as being a appetizer, a prelude to THE THEORY, might be precisely like the *prélude* in music, a prelude preluding a fugue in 18th century, but preluding nothing (maybe itself) after the Opus 28 by Chopin.

In sciences these questions are also related to methodological aspects. This is reflected in this kind of duality between extremely formalized questions versus modest answers. the case of quantum field theory shows this perfectly: to the very ambitious question of the quantization of fields, modest perturbative answers were proposed, and when they failed people started always by trying to improve the question. After people took seriously the perturbative answer, e.g. by forcing it to have the same symmetries that the question, things went much faster, and deeper. The duality might be interesting to transpose to philosophy, where formalization of questions are so important.

At the end can we ask the question of a perturbative ontology? approximate ontology? almost ontology? Let us say that maybe the answer is a non-trivial question of formalism: the "+", the sign +, which is present in the perturbative expansions is certainly more that the one of the addition of numbers. When one represents in quantum theory of fields a phenomenon as being the addition of Feynman diagrams, one represents an unknown reality as being decomposable in very realistic simple phenomena. And each "shock" (i.e. vertex) is always (like the shocks in kinetic theory mentioned by Bachelard) a black box, till we get to the next order which will decompose in simple forms. The final result is a number (probability of transition), but the "+", in the sum in powers of the size of the perturbation, is obtained through decomposition of operators and quantum states, that is, it is in fact the "+" of the linear structure of quantum theory. It is the 'principle of superposition", so orthogonal to our classical way of thinking. Maybe, when the formalism of quantum mechanics will be introduced in philosophy, a philosophy of approximation/perturbation will be possible.

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