PERTURBATION THEORY ?

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The seed ye sow, another reaps!

§1 Mathematical Physics, in general.

It is quite difficult to find a subject to discuss as afterthought about a conference on *Mathematical Physics towards the XXI century.* I will describe some personal viewpoints and discuss a recent work (with some variations with respect to the previous versions) to stress some of the points made.

Should I say which is the legacy of Mathematical Physics to the next century I would point to a subject which has been left out of the talks by most of the speakers: the understanding and formalization of perturbation techniques for problems quite different from classical mechanics (and for classical mechanics as well).

It is of course still true that the so called "non perturbative techniques" are more impressive and glamorous; they also look deeper. And there are other aspects of Mathematical Physics that have received a large impulse in this century.

Let me begin by stressing the similarities between the perturbative and non perturbative techniques in Mathematical Physics. What I see as common to the two is what I consider a very basic aspect of Mathematical Physics which, although coming from the XIX century, has become very clear during the XX century: the attempt, sometimes successful, to put on a really rational (*i.e.* mathematically rigorous) basis the fundamental problems of Physics.

It is not an overstatement to claim that many important results came out of attempts to set in a clear form problems which were too empirically treated. For instance the Dirac formulation of quantum mechanics is comparable to the Lagrange and Hamilton formalization of classical mechanics. The latter made available to everybody the possibility of computing in an unambigous way the solution to mechanics problems which were accessible only to few people, who managed to dominate the rather non systematic state of the matter at the time. After Dirac's work "everybody" could work out Atomic Physics computations and predictions.

The attempt to axiomatize quantum field theory and renormalization theory (by Wightman and the Zürich school, [SW]) led first ("as a byproduct") to the clear formulation of the major problems of Statistical Mechanics (Ruelle, Fisher and the Moscow school, [R]): and generated a great number of new results and a deep understanding of the phenomena of phase transition. Thus opening way to the renormalization group theory of the scale invariance and universality (Widom, Kadanoff, Fisher, Wilson, [Ma], [P]) which, eventually, brought a good understanding of renormalization theory and quantum field theory themselves.

Turbulence is a phenomenon which was typically only empirically described and scarsely "objectively analyzed". Attempting at its precise definition produced surpringly new results and points of view (Lorenz, Ruelle, [R2], [ER]): and nowadays everybody knows about "chaos": which was essentially unknown only twenty years ago.

General relativity is even more exceptional as it is an example (the only one I know) of a theory born as a mathematically well formulated theory, [W].

All the above examples, which are not exhaustive and only reflect my personal interests, are remarkable: but I see many colleagues (and I have seen many more of them in the past) saying that all of the above was not so important. The real problems would have been, or had been, indeed solved without need of refined mathematics or of precise formulations. This attitude is quite erroneous in my view at least as a general statement, as the examples of the theory of chaos or of general relativity show, in a clear way, to those who wish to see.

It is nevertheless true that most progress was achieved before a precise mathematical formulation: for instance the Schrödinger equation came before Dirac's work. Schwinger, Feynman and Dyson understood renormalization theory before the work of the mathematical physicists (Hepp, [H], Glimm, Jaffe, Nelson,

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! P. Shelley: *Song to the men of England.*

Guerra, Spencer, [Si], [E]). In the same way as the work of Newton, D'Alembert, Laplace, Gauss, Euler [1] preceded analytical mechanics.

The reason I see the mathematical formulations as important, even in fields in which "progress was already achieved", is that the rigorization attempts had a "democratic nature": they made, and make, accessible to the vast majority ideas and methods that were (obviously) perfectly understood by only a few scientists. And, if science has to advance, it needs "democracy", *i.e.* many scientists who control what they are doing.

Mathematical Physics in this century had mostly the role of making accessible results that were very deep and difficult to understand because the scientists who developed them found no time (or no need) to explain them in a form that could be widely understood. The mathematical language is really universal, as Galileo explicitly noted, and is very well suited for the transmission of knowledge.

§2 Fundamental, exact, non perturbative, and constructive methods.

One can distinguish roughly four aspects of Mathematical Physics. The first I will call "fundamental": it consists in the analysis of the general structure of various problems, and of their proper mathematical formulation.

For instance the formalisms of statistical mechanics (general notion of equilibrium states, of thermodynamic limit, of phase transitions, of ground states, of correlations), field theory (general notion of relativistic quantum field, of interaction) or fluid mechanics (general notion of regular and chaotic motions). In this conference the above aspects have been stressed and discussed quite in detail.

I consider the "fundamental" aspects as essential, in the same sense as hamiltonian mechanics is essential for classical mechanics. It is true that one could just ignore the formalisms: but at least in the case of hamiltonian mechanics hardly anybody would dare saying that it is useless.[2]

But others could still insist that the formalisms are useless (it is not uncommon to meet people who refuse to try to understand the general theory of chaos as emerged from the work of Ruelle on the grounds that it is a formalism while "science deals with concrete problems"). The reason I am not too impressed by such statements is that Mathematical Physics has been able to go far beyond the formalisms in all the examples I quoted. The formalism has been used to set a frame into which to formulate precisely concrete problems to be solved, *and their solutions*. I would, in fact, also agree in considering of little interest a formalism which did not attack (and solve, at least partially) some physical problems.

A way to make use of a formalism is by discussing in its frame the properties of concrete models. As some models can be studied "exactly"; and this brings up a second aspect of Mathematical Physics.

This century will leave the legacy of many exactly soluble models which play, and will continue to play, the role of the basic mechanical models like harmonic oscillators, central forces and two body problems, rigid body, vibrating string, heat equation, linear waves): they clarify the meaning of the formalism and provide examples of physical significance as well as firm comparison terms for checks of approximate theories and methods. I just mention the work of Onsager on the Ising model, or the work of Lieb, Yang and Baxter on the ground state of the Heisenberg model, [B]. Such models, and many others (Schwartzschild and other metrics in general relativity, [W], Thirring model in field theory, Luttinger model in solid state Physics, one dimensional dynamics of hard rods, [ML], Korteveg De Vries equation, Toda Lattice *etc*) are not only non trivial and illustrative of rather complicated behaviour but they have provided important new ideas in probability theory, PDE's and other fields.

Another way of making use of the formalism is via the so called "non perturbative methods": they consist in studies of concrete models for which no exact solution can be provided. One tries to obtain informations about some general properties by using various special features of the models. An example of this is the theory of the thermodynamic limit or the theory of statistical ensembles for systems with short range forces in statistical mechanics (Fisher, Ruelle, [R]). More specific (non exhaustive) examples are the existence of thermodynamics for systems with electromagnetic forces (Dyson, Lenard, Lieb, Thirring, Fefferman, [L], [Fe]), the theory of coexistence in the Ising model (Russo, Aizenman, Higuchi, [Ai],[Hi]), the theory of percolation (Russo, Kesten, [Gr]), the theory of the critical point in ferromagnets (Aizenman, [Ai]), structure of "large atoms" (Lieb, [L2]), existence of phase transitions in some quantum statistical mechanics models (Dyson, Lieb, Simon, [DLS]), theory of "incommensurate crystals" (Aubry, Mather, [Au],[M]).

Very often the above general "non perturbative" methods are based on the use of convexity inequalities or of other classes of inequalities (GKS, FKG, *etc*). Therefore very often (though not always) the results

[1] I stop the list for lack of space.

[2] although the book of Laplace could easily provide arguments in support of such thesis.

are "non constructive": *i.e.* they are intrinsically unable to provide estimates for the speed at which some limits are reached, or for the value of various constants whose existence is proved. They describe general, non trivial, properties which are very useful to know, particularly to check approximate theories results.

The fourth aspect are the fully constructive results for specific models, based on approximations by convergent series expansion to quantities that, by general arguments, can be shown to exist. Such theories, usually called "pertubative theories", are often considered "too technical" and the word "perturbative" has very often a ironic and negative connotation. But they are certainly among the greatest achievements of the subject and essentially they provide the only cases in which the "problem" is completely solved. The latter aspect should not be underestimated: in fact there have been examples of problems "solved" by general theorems of "fundamental nature" but whose solution has been subject to critique. In fact one of the dangers of Mathematical Physics is that, in order to solve a poblem, it has to formulate it in a precise mathematical way: but this can lead to subtle changes of the problem itself or to too strict a formulation of it. So that the solution achieved by one might not be regarded as such by others. This has certainly damaged the image of research in Mathematical Physics in many respects (for instance claims on supposed "theorems" on non existence of crystals only threw discredit on the field).

A very illustrious example of a "theorem" which, although mathematically correct and of great importance, missed completely the point is the generic non integrability of pertubations of integrable hamiltonian systems and in particular of the three body problem, [G2]. This theorem of Poincarè has been poorly interpreted and referred to only by quotations for about half a century: until it was shown that a slight modification of the statement gave rise to a non trivial and very interesting class of results (the KAM theory, see [G2]) which was stating the truth of almost opposite properties.

Another important example is the "triviality theorem" of scalar field theories in space time dimension 4, ([Fr],[Ai]): this theorem is valid under some very strong assumptions, so far, and skeptics may interpret it as saying that the assumptions are too strong or inappropriate. It is nevertheless very often quoted as a theorem established in full generality, see [GR].

Unfortunately the attitude to quote the "theorems" of Mathematical Physics out of context and as absolute truths, proving or disproving some fundamental results, is still quite widespread: mostly because non specialists try to use Mathematical Physics acritically refusing to recall that theorems need assumptions and the assumptions have a physical meaning which is not always entirely conveyed by the words which are used to state them. So that a technical analysis of the assumptions is always necessary and useful. The above attitude is unfortunately taken even inside Mathematical Physics itself: specialists in one area tend to quote acritically results concerning other areas: I am afraid that this will not be cured by us and it will be a problem also for the next century.

In some sense only "positive" results should really matter. But "no go" results are also in some sense positive results. And sometimes positive results might be of little physical interest. Even if they come in a totally constructive form, with all desirable error or remainder estimates as they always come out of perturbation theoretic approaches. Nevertheless perturbation theory is usually non trivial, and I will illustrate some of its achievements which I find remarkable not only for their intrinsic interest but for the surprising unity of methods of solution of different problems, that they make evident.

Another legacy that we shall certainly leave to the next century is our unwillingness of considering seriously the work done in other fields even inside the domain of Mathematical Physics. Hence we shall certainly not be able to find a way to avoid that results found in some area will reappear, derived under different words but in essentially identical manner, or regarded as open problems in other areas (or even in the same area): I am not thinking here of plagiarism (which is of little interest except from a moral point of view, totally ridiculous in our present world except on individual grounds), but just of honest ignorance. I am always surprised when I meet such cases: and I am afraid that I have myself shown this behaviour. But this might in fact be a "good thing": it is more likely that the results will not be forgotten if they are duplicated enough many times: the main works of greek science were lost because there were not enough copies of them.

§3 A review of perturbation theory methods.

Perturbation theory arises when one studies a problem dependent on a parameter ε, and for $\varepsilon = 0$ the problem is exactly soluble (at least in some aspects). Until recently a perturbation treatment meant finding a series in ε convergent or asymptotic for small ε, whose sum was equal or asymptotic to a quantity of predeclared interest, for ε small.

The simplest examples of perturbation theory dealt with the problems of celestial mechanics and in the last century not much attention was devoted to the actual convergence of the series, the main problem being whether they could be defined as formal power series (*i.e.* their coefficients were well defined). In fact Poincarè proved that some of the most used series in celestial mechanics could not possibly be convergent or even defined to all orders and that they were, at best, asymptotic (see [G2]).

The convergence problem never seemed to bother the theoretical physicists: perhaps because good enough practical results seemed to come out of the series, if regarded as asymptotic. Or perhaps because the series met in the theory of atoms were actually convergent, (see [K],[RS],[T]).

The question of convergence of the series started playing a role when it was discovered that the series expansions for quantum field theory were not even well defined, at least not from a naive viewpoint. Renormalization theory provided a way to define them properly, but it also generated the puzzle of how proper the definition really was: whether it was a fundamental definition or just an arbitrary prescription. This was a particularly pertinent question as it was in any event clear that in most cases the series could not be convergent but at best they could be asymptotic.

The first series to be studied was precisely that arising in the theory of perturbations of integrable hamiltonian systems: this led to the theorem of Kolmogorov on quasi periodic hamiltonian motions.

In the same years the virial series for statistical mechanics systems with short range forces was proved to converge for small enough density (Morrey, [Mo], Groeneveld, Penrose, Ruelle, [R]: independently in an arc of a decade).

In my opinion the latter was the key stone for the future developments: it was, I remember, for many a great surprise that such series describing the behaviour of systems with infinitely many degrees of freedom could be "proved" convergent. The technique soon found applications in other fields, for instance to the theory of phase transitions and to the study of the pure phases and of phase coexistence (started by Dobrushin, Minlos, Sinai, [S]).

At the end of the sixties the situation was mature for attempting at the proof of convergence, or asymptoticity, of the formal series generated by renormalization theory in quantum field theory, say for the computation of the Schwinger functions. The work on the foundations had permitted to define precisely the quantities to be studied. The series giving formally their values had been mathematically proven to be well defined, at least in simple cases, (Hepp, [H]): a fact that the discoverers of renormalization theory certainly had clear enough to consider a formal proof unnecessary. The asymptoticity proof came from the work by Glimm, Jaffe, Spencer, [E], and it was achieved by showing the convergence of another series in terms of *functions* of the expansion parameter ε, which were singularly depending on ε, but in a controlled way. The difficult part was to set up the appropriate convergent expansion, whose convergence turned out to be, essentially, a consequence of the same ideas leading to the convergence of the virial series.

I will never understand why such result, which among other things solved the question of whether a relativistic quantum theory with non trivial S–matrix was possible, i.e. showed the compatibility of non trivial quantum mechanics and special relativity (at least for some models and in "low" space time dimension, (i.e. 3)), has been very often ignored and sometimes considered irrelevant: the introduction of an important later paper provides us with a rather typical example, see [PW].

The above work was the last major one before the new ideas generated by the renormalization group started playing a role, [Po],[G3]; but a typical tool of the renormalization group is already present: *i.e.* the proof of asymptoticity (or convergence) of a perturbation series by checking convergence of series in other quantities with controlled (albeit singular) analyticity properties in terms of the expansion parameter ε.

This method can be called "proof of asymptoticity by resummations" and is a classic mathematical method. What is not classic and non trivial is its combination with the theory of convergence of the virial series, known nowadays as *cluster expansion*, [E],[G2].

The cluster expansion led also to the solution of some old problems, like the proof of existence of the Debye screening in classical systems interacting at large distance by an electrostatic Coulomb potential (Brydges, [Br]).

Thus it is interesting to see that the resummation techniques could all be considered part of the same basic idea rooted in the *renormalization group* approach to the critical point in statistical mechanics. The approach is particularly interesting as it shows that the really basic idea of *asymptotic freedom* which is one of the really new ingredients in the theory of renormalization and which had escaped to the founding fathers, is the idea which allows us to build the rigorous proofs of asymptoticity of the perturbation expansion of various field theories.

In fact the renormalization group can be viewed as a method for resumming formal series: this was realized through various works on the rigorous mathematical meaning of the renormalization group "calculations" (see the review papers [CE], [Po], [G3], [GK1]) in which the "old" results were rederived and a host of new ones (among them see [FS]: for the Anderson localization problem, [3] [GK2] for the theory of the Gross–Neveu model, [BGPS] for the theory of the ground state of spin 0 one dimensional Fermi gases; just to mention a few among them). Other applications are promising to come, see [A],[Sh],[Po2],[W2]. All the above problems have one feature in common: namely they have some scaling property playing a key role. However a fully unified treatment is not yet available, although it seems that we possess all the necessary knowledge.

§4 An example of modern perturbation theory: twistless invariant tori in hamiltonian mechanics.

The resummation is not always possible, or not always known: in particular the asymptoticity of the (well defined) perturbation expansions cannot be treated rigorusly in important cases such as 4 dimensional field theories like quantum electrodynamics or the standard model of elementary particles.

More or less hidden scaling properties are the origin of similarities of problems of perturbation theory in statistical mechanics and field theory and other apparently unrelated problems like the Feigenbaum cascades in the theory of maps of the interval (see [F], [CE2], [ER]), or the KAM theory or the invariant tori breakdown theory of Kadanoff, Mc Kay, Aubry, Mather, ([Mc],[AL],[M]). Or the theory of regularity of the solutions of the Navier Stokes equations in 3 dimensions, see the review [G4].

I will describe here the connection between the Kolmogorov theorem on the existence of quasi periodic motions and the resummations of perturbation expansions used in quantum field theory. The basic idea goes back to Eliasson: the idea has a particularly simple application as the series studied turns out to be a convergent series because of some remarkable cancellations. Although a part of the problem is therefore trivial (the series being convergent rather than asymptotic) what is left in non trivial enough to be interesting and to show the relation with the renormalization group methods in quantum field theory.

The connection might have been suspected already from the relation between the KAM theory and the renormalization group methods, pointed out in various independent works ([G2], [Mc], [ED]). I find it nevertheless quite surprising that the KAM problem can in fact be formulated as a field theory problem, in which interesting scaling properties appear, and treated as such.

The explanation of the field theory model given below requires, to be understood, some familiarity with the theory of the dipole gas and of the sine gordon model. But the analysis following it is an independent and self contained proof of the KAM theorem, not requiring any knowledge of field theory or statistical mechanics. The field theory model linked to the KAM theorem was pointed out to me by G. Parisi, who showed me what I should have done in my attempts to interpret field theoretically my version of Eliasson's ideas.

To establish a connection with field theory consider first a vector field $\vec F_{\vec\psi}$, with zero average, defined on the torus T^l with free propagator given by the operator $(\vec\omega_0\cdot\vec\partial)^2$, instead of the usual laplacian, with $\vec\omega_0\in R^l$ being a vector verifying a strong diophantine condition, see below.

Consider the function of the field corresponding to the "action potential" $V(\vec F)=\varepsilon\int f(\vec\psi+\vec F_{\vec\psi})\,d\vec\psi$, where the f is an even trigonometric polynomial.

The partition function Z is then formally expressed in terms of the gaussian process $P(d\vec F)$ generated by the differential operator $(\vec\omega_0\cdot\vec\partial)^2$ on the functions $\vec F_{\vec\psi}$ with zero average: it is the functional integral of the functional $\exp V(\vec F)$. And the Schwinger function $\vec h(\vec\psi)=\langle\vec F_{\vec\psi}\rangle$ is the formal average of $\vec F_{\vec\psi}$ with respect to the measure $Z^{-1}P(d\vec F)e^{V(\vec F)}$.

I shall show below that the evaluation of $\vec h(\vec\psi)$ via perturbation theory, if performed *neglecting all Feynman diagrams which are nor tree diagrams*, gives exactly the perturbation series solution to the problem of finding an invariant KAM torus for the model considered below, (that I call Thirring model, or rotators model, see (2) below).

It was pointed out to me by G. Parisi that the approximation to $\vec h(\vec\psi)$ obtained by evaluating $\vec h(\vec\psi)$ via a perturbation expansion in ε, in which only the Feynman diagrams with tree structure (*i.e.* no loops) are retained, yields the solution to the equation: $(\vec\omega_0\cdot\vec\partial)^2\vec h=-\varepsilon\vec f'(\vec\psi+\vec h(\vec\psi))$, where $\vec f'$ denotes the gradient of the function f. Below I show that the latter equation is the equation that has to be solved to determine the KAM tori for the model introduced in the following equation (1). Hence a field theoretic interpretation of the KAM theorem will easily follow. A further connection with field theory was pointed out to me by A. Berretti who remarked that the tree approximation is exact in mean field theory: therefore the model

[3] a remarkable new method, non perturbative, for this theory has been presented at this conference, see [AM].

provided by Parisi solved in the mean field approximation gives rise to a non trivial equation whose solution is equivalent to the Thirring model.

The above formal field theoretic interpretation is interesting, but it has the drawback of being "an approximation" and one can ask whether one can find a field theory whose one point Schwinger function is exactly, at least formally, the function $\vec{h}(\vec{\psi}\,)$ describing the invariant tori of the Thirring model.

This can be done as follows (a procedure probably known in field theory): let $\vec{F}^{\pm}_{\vec{\psi}}$ be two complex vector fields, with zero average on the torus T^l, and suppose that their free propagator is $\langle F^{+}_{i,\vec{\psi}}F^{+}_{j,\vec{\psi}\,'}\rangle = 0 = \langle F^{-}_{i,\vec{\psi}}F^{-}_{j,\vec{\psi}\,'}\rangle$ while $\langle F^{+}_{i,\vec{\psi}}F^{-}_{j,\vec{\psi}\,'}\rangle = \delta_{ij}S(\vec{\psi}-\vec{\psi}\,')$ where S has Fourier transform equal to $(\vec{\omega}_0\cdot\vec{\nu})^{-2}$. The fields $\vec{F}^{\pm}$ can be realized by considering two independent real vector fields $\vec{\Phi}^q$, $q=1,2$, with free propagator $\delta_{ij}S(\vec{\psi}-\vec{\psi}\,')$. One simply sets: $\vec{F}^{\pm}=\vec{\Phi}^1\pm i\vec{\Phi}^2$.

Then consider the Schwinger function:

$$\vec{h}(\vec{\psi}\,) = \frac{\int P(d\vec{F})\,\vec{F}^{+}_{\vec{\psi}}\,e^{\varepsilon\int \vec{F}^{-}_{\vec{\psi}\,'}\cdot\vec{\partial}f(\vec{\psi}\,'+\vec{F}^{+}_{\vec{\psi}\,'})\,d\vec{\psi}\,'}}{\int P(d\vec{F})\,e^{\varepsilon\int \vec{F}^{-}_{\vec{\psi}\,'}\cdot\vec{\partial}f(\vec{\psi}\,'+\vec{F}^{+}_{\vec{\psi}\,'})\,d\vec{\psi}\,'}} \tag{1}$$

and it is easy to check that the linearity in $\vec{F}^-$ of the potential implies that only the tree diagrams of the perturbation expansion of (1) do not vanish. And the tree diagrams *have the same value* as the ones of the previously considered, single field $\vec{F}$, field theory. Hence $\vec{h}(\vec{\psi}\,)$ is a (formal) sum of the tree diagrams for (1).

One should not think that the functional integrals in (1) are easy to define rigorously. In fact the fields $\vec{F}^{\pm}$ are complex valued and therefore the argument of the exponentials is unbounded: so that summability is by no means obvious. The KAM theorem discussed below can be interpreted as a theory of the above functional integral and as a proof of the convergence of the perturbation expansion for it.

To avoid references to field theory I begin by formulating from scratch the problem of the KAM theorem version that I will discuss. It will be a particularization of the Eliasson method, [El], for KAM tori to a special model: the Thirring model. This is a system of rotators interacting via a potential. It is described by the hamiltonian:

$$\frac{1}{2}J^{-1}\vec{A}\cdot\vec{A}\,+\,\varepsilon f(\vec{\alpha}) \tag{2}$$

where J is the (diagonal) matrix of the inertia moments, $\vec{A}=(A_1,\ldots,A_l)\in R^l$ are their angular momenta and $\vec{\alpha}=(\alpha_1,\ldots,\alpha_l)\in T^l$ are the angles describing their positions: the matrix J will be supposed non singular; but we only suppose that $\min_{j=1,\ldots,l}J_j=J_0>0$, and *no assumption* is made on the size of the *twist rate* $T=\min J_j^{-1}$: the results will be uniform in T (hence the name "twistless" that can be given to the above tori). We suppose f to be an even trigonometric polynomial of degree N:

$$f(\vec{\alpha}) = \sum_{0<|\vec{\nu}|\le N} f_{\vec{\nu}}\,\cos\vec{\nu}\cdot\vec{\alpha},\qquad f_{\vec{\nu}}=f_{-\vec{\nu}} \tag{3}$$

We shall consider a "rotation vector" $\vec{\omega}_0=(\omega_1,\ldots,\omega_l)\in R^l$ verifying a *strong diophantine property* with dophantine constants C_0,τ,γ,c; this means that:

$$\begin{array}{lll} 1) & C_0|\vec{\omega}_0\cdot\vec{\nu}|\ge|\vec{\nu}|^{-\tau}, & \vec{0}\ne\vec{\nu}\in Z^l \\ 2) & \min\limits_{0\ge p\ge n}\big|C_0|\vec{\omega}_0\cdot\vec{\nu}|-\gamma^p\big|>\gamma^{n+1} & \text{if } n\le 0,\ 0<|\vec{\nu}|\le(\gamma^{n+c})^{-\tau^{-1}} \end{array} \tag{4}$$

and it is easy to see that the *strongly diophantine vectors* have full measure in R^l if $\gamma>1$ and c are fixed and if τ is fixed $\tau>l-1$: we take $\gamma=2,c=3$ for simplicity; note that 2) is empty if $n>-3$ or $p<n+3$. We shall set $\vec{A}_0=J\vec{\omega}_0$. And a special example can be the model $f_0(\vec{\alpha})=J_0\vec{\omega}_0^2(\cos\alpha_1+\cos(\alpha_1+\alpha_2))$.

We look for an ε–analytic family of motions starting at $\vec{\alpha}=\vec{0}$ and having the form:

$$\vec{A}=\vec{A}_0+\vec{H}(\vec{\omega}_0t;\varepsilon),\qquad \vec{\alpha}=\vec{\omega}_0t+\vec{h}(\vec{\omega}_0t;\varepsilon) \tag{5}$$

with $\vec{H}(\vec{\psi};\varepsilon),\vec{h}(\vec{\psi};\varepsilon)$ analytic in $\vec{\psi}\in T^l$ and in ε close to 0. We shall prove that such functions exist and are analytic for $|\operatorname{Im}\psi_j|<\xi$ for $|\varepsilon|<\varepsilon_0$ with:

$$\varepsilon_0^{-1}=bJ_0^{-1}C_0^2f_0N^{2+l}e^{cN}e^{\xi N} \tag{6}$$

where b, c are l-dependent positive constants, $f_0 = \max_{\vec\nu} |f_{\vec\nu}|$. This means that the set $\vec A = \vec A_0 + \vec H(\vec\psi\,;\varepsilon)$, $\vec\alpha = \vec\psi + \vec h(\vec\psi\,;\varepsilon)$ described as $\vec\psi$ varies in T^l is, for ε small enough, an invariant torus for (2), which is run quasi periodically with angular velocity vector $\vec\omega_0$. It is a family of invariant tori coinciding, for $\varepsilon = 0$, with the torus $\vec A = \vec A_0$, $\vec\alpha = \vec\psi \in T^l$. One recognizes a version of the KAM theorem. The proof that follows simplifies the one reported in [G1].

Supposing $J_0 \equiv J_1 < J_2$ the uniformity in J_2 (*i.e.* what we call the *twistless* property) implies that the same ε_0 can be used as an estimate of the radius of convergence in ε of the power series describing the KAM tori with rotation vector $\vec\omega_0 = (\omega_1, \omega_2)$ in the system $(2J_1^{-1}A_1^2 + \omega_2 A_2 + \varepsilon f_0(\alpha_1, \alpha_2)$, which is one of the most studied hamiltonian systems. The estimate can be improved. Note that a careful analysis of the proof of the KAM theorem also shows the uniformity in the twist rate in the case (2).

Calling $\vec H^{(k)}(\vec\psi), \vec h^{(k)}(\vec\psi)$ the k-th order coefficients of the Taylor expansion of $\vec H, \vec h$ in powers of ε and writing the equation of motion as $\dot{\vec\alpha} = J^{-1}\vec A$ and $\dot{\vec A} = -\varepsilon\partial_{\vec\alpha} f(\vec\alpha)$ we get immediately the equation (mentioned above and having a field theory interpretation) $(\vec\omega_0 \cdot \vec\partial)^2 \vec h(\vec\psi) = -\varepsilon\vec\partial_{\vec\alpha} f(\vec\psi + \vec h(\vec\psi))$, or the (equivalent) recursion relations for $\vec H^{(k)}, \vec h^{(k)}$. Namely $\vec\omega_0 \cdot \vec\partial\, h_j^{(k)} = J_j^{-1} H_j^{(k)}$ and, for $k > 1$:

$$\vec\omega_0 \cdot \vec\partial\, H_j^{(k)} = -\sum_{\substack{m_1,\ldots,m_l \\ |\vec\mu|>0}} \frac{1}{\prod_{s=1}^l m_s!} \partial_{\alpha_j}\, \partial^{m_1+\ldots+m_l}_{\alpha_1^{m_1}\ldots\alpha_l^{m_l}} f(\vec\omega_0 t) \cdot {\sum}^* \prod_{s=1}^{l} \prod_{j=1}^{m_s} h_s^{(k_j^s)}(\vec\omega_0 t) \tag{7}$$

where the $\sum^*$ denotes summation over the integers $k_j^s \ge 1$ with: $\sum_{s=1}^l \sum_{j=1}^{m_s} k_j^s = k - 1$.

The trigonometric polynomial $\vec h^{(k)}(\vec\psi)$ will be completely determined (if possible at all) by requiring it to have $\vec 0$ average over $\vec\psi$, (note that $\vec H^{(k)}$ has to have zero average over $\vec\psi$). For $k = 1$ one easily finds:

$$\vec h^{(1)}(\vec\psi) = -\sum_{\vec\nu\ne\vec 0} \frac{iJ^{-1}\vec\nu}{(i\vec\omega_0\cdot\vec\nu)^2} f_{\vec\nu}\, e^{i\vec\nu\cdot\vec\psi} \tag{8}$$

Suppose that $\vec h^{(k)}(\vec\psi)$ is a trigonometric polynomial of degree $\le kN$, odd in t, for $1 \le k < k_0$. Then we see immediately that the r.h.s. of (7) is odd in t. This means that the r.h.s. of (7) has zero average in t, hence in $\vec\psi$, and the second of (7) can be solved for $k = k_0$. It yields an even function $\vec H^{(k_0)}(\vec\psi)$ which is defined up to a constant which, however, must be taken such that $\vec H^{(k_0)}(\vec\psi)$ has zero average, to make $\vec\omega \cdot \vec\partial h_j^{(k)} = J_j^{-1} H_j^{(k)}$ soluble. Hence the equation for $\vec h^{(k)}$ can be solved (because the r.h.s. has zero average) and its solution is a trigonometric polynomial in $\vec\psi$, odd if $\vec h^{(k)}$ is determined by imposing that its average over $\vec\psi$ vanishes.

Hence the (8) provide an algorithm to evaluate a formal power series solution to our problem. It has been remarked, [El], see also [G1], that (7) yields a *diagrammatic expansion* of $\vec h^{(k)}$. We simply "iterate" it until only $h^{(1)}$, given by (8), appears.

Let ϑ be a tree diagram: it will consist of a family of "lines" (*i.e.* segments) numbered from 1 to k taken from a "box" containing k lines with a label, *number label*, going from 1 to k (*i.e.* distinguishable), arranged to form a (rooted) tree diagram as in the figure:

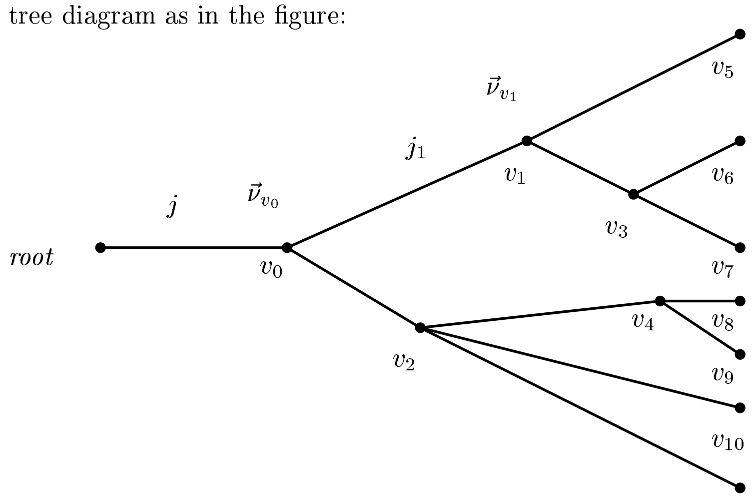

fig. 1: A tree diagram ϑ with $m_{v_0} = 2, m_{v_1} = 2, m_{v_2} = 3, m_{v_3} = 2, m_{v_4} = 2$ and $m = 12$, $\prod m_v! = 2^4 \cdot 6$, and some labels. The line numbers, distinguishing the lines, are not shown.

To each vertex v we attach a "mode label" $\vec{\nu}_v \in Z^l$, $|\vec{\nu}_v| \le N$ and to each branch leading to v we attach a "branch label" $j_v = 1, \ldots, l$. The order of the diagram will be k = number of vertices = number of branches (the tree root will not be regarded as a vertex).

We imagine that all the diagram lines have the same length (even though they are drawn with arbitrary length in fig. 1). A group acts on the set of diagrams, generated by the permutations of the subdiagrams having the same vertex as root. Two diagrams that can be superposed by the action of a transformation of the group will be regarded as identical (recall however that the diagram lines are numbered, *i.e.* are regarded as distinct, and the superpositon has to be such that all the labels of the diagram match: *i.e.* the branch label, the mode label *and* the number label). Trees diagrams are regarded as partially ordered sets of vertices (or lines) with a minimal element given by the root (or the root line): as usual the order relation will be denoted $\le$ and, in general, not all pairs of vertices will be comparable. We shall imagine that each branch carries also an arrow pointing to the root ("gravity" direction, opposite to the order).

We define the "momentum" entering v as $\vec{\nu}(v) = \sum_{w \ge v} \vec{\nu}_w$. If from a vertex v emerge m_1 lines carrying a label $j = 1$, m_2 lines carrying $j = 2$, …, it follows that (7) can be rewritten:

$$\vec{h}^{(k)}_{\vec{\nu}j} = \frac{1}{k!}\sum{}^{*} \prod_{v\in\vartheta} \frac{(-iJ^{-1}\vec{\nu}_v)_{j_v}\, f_{\vec{\nu}_v} \prod_{s=1}^{l} (i\vec{\nu}_v)_s^{m_s}}{(i\vec{\omega}_0 \cdot \vec{\nu}(v))^2} \tag{9}$$

with the sum running over the diagrams ϑ of order k and with $\vec{\nu}(v_0) = \vec{\nu}$; and the combinatorics can be checked from (7), by taking into account that we regard the diagram lines as all different (to fix the factorials). Basically the introduction of the number labels increases the number of trees, but at the same time they acquire all the same combinatorial weight $k!^{-1}$, instead of the more complicated $\prod m_v!^{-1}$ that one would expect from (7).

The * recalls that the diagram ϑ can and will be supposed such that $\vec{\nu}(v) \ne \vec{0}$ for all $v \in \vartheta$ (by the above remarked parity properties). Note that (9) is implied by the corresponding (6.23) of [G1]: one can check that the two formulae coincide (by summing over what in [G1] are called the "fruit values"). The theory in [G1] is in fact a little more general, although it is really applied to the same Thirring model.

There are other diagrams, however, which we would like to eliminate. They are the diagrams with nodes v', v, with $v' < v$, not necessarily nearest neighbours, such that $\vec{\nu}(v) = \vec{\nu}(v')$. Such diagrams are, according to Eliasson's terminology, *in resonance.*

The first remark is that if the sum in (9) was restricted to a sum over diagrams such that:

I) $\vec{\nu}(v) \ne \vec{0}$ if v is any vertex.

II) $\vec{\nu}(v) \ne \vec{\nu}(v')$ for all pairs of comparable vertices v', v, (not necessarily next to each other in the diagram order, however), with $v \ge v_0$.

then the result would be easily reduced to known convergence theorems, like the basic Brjuno's lemma, (see [G1]).

However *there are* resonant diagrams. The key remark is that they cancel *almost exactly.* The reason is very simple: imagine to detach from a diagram ϑ the subdiagram ϑ_2 with first node v. Then attach it to all the *remaining* vertices $w \ge v', w \in \vartheta/\vartheta_2$. We obtain a family of diagrams whose contributions to $h^{(k)}$ differ because:

1) some of the branches above v' have changed total momentum by the amount $\vec{\nu}(v)$: this means that some of the denominators $(\vec{\omega} \cdot \vec{\nu}(w))^{-2}$ have become $(\vec{\omega} \cdot \vec{\nu}(w) \pm \varepsilon)^{-2}$ if $\varepsilon \equiv \vec{\omega}_0 \cdot \vec{\nu}(v)$; and:
2) because there is one of the vertex factors which changes by taking successively the values ν_{wj}, j being the line label of the line leading to v, and $w \in \vartheta/\vartheta_2$ is the vertex to which such line is reattached.

Hence if $\vec{\omega} \cdot \vec{\nu} = \varepsilon = 0$ we would build in this *resummation* a quantity proportional to: $\sum \vec{\nu}_w = \vec{\nu}(v') - \vec{\nu}(v)$ which is zero, because $\vec{\nu}(v) = \vec{\nu}(v')$ means that the sum of the $\vec{\nu}_w$'s vanishes. Since $\vec{\omega} \cdot \vec{\nu} = \varepsilon \ne 0$ we can expect to see a sum of order ε^2, if we sum as well on a overall change of sign of the $\vec{\nu}_w$ values (which sum up to $\vec{0}$).

But this can be true only if $\varepsilon \ll \vec{\omega} \cdot \vec{\nu}'$, for any line momentum $\vec{\nu}'$ of a line in ϑ/ϑ_2. If the latter property is not true this means that $\vec{\omega} \cdot \vec{\nu}'$ is small and that there are many vertices in ϑ/ϑ_2 of order of the amount needed to create a momentum with small divisors of order ε.

Examining carefully the proof of Brjuno's lemma one sees that such extreme case would be essentially also treatable. Therefore the problem is to show that the two regimes just envisaged (and their "combinations") do exhaust all possibilities.

Such problems are very common in renormalization theory and are called "overlapping divergences". Their systematic analysis is made through the renormalization group methods. We argue here that Elliasson's method can be interpreted in the same way.
The above introduced diagrams will play the role of *Feynman diagrams*; and they will be plagued by overlapping divergences. They will therefore be collected into another family of graphs, that we shall call *trees*, on which the bounds are easy. The $(\vec{\omega}\cdot\vec{\nu})^{-2}$ are the *propagators*, in our analogy.
We fix a *scaling* parameter γ, which we take $\gamma=2$ for consistency with (4), and we also define $\vec{\omega}\equiv C_0\vec{\omega}_0$: it is an adimensional frequency. Then we say that a propagator $(\vec{\omega}\cdot\vec{\nu})^{-2}$ is *on scale* n if $2^{n-1}<|\vec{\omega}\cdot\vec{\nu}|\le 2^n$, for $n\le 0$, and we set $n=1$ if $1<|\vec{\omega}\cdot\vec{\nu}|$.
Proceeding as in quantum field theory, see [G3], given a diagram ϑ we can attach a *scale label* to each line $v'v$ in (9) (with v' being the vertex preceding v): it is equal to n if n is the scale of the line propagator. Note that the labels thus attached to a diagram are uniquely determined by the diagram: they will have only the function of helping to visualize the orders of magnitude of the various diagram lines.
Looking at such labels we identify the connected clusters T of vertices that are linked by a continuous path of lines with the same scale label n_T or a higher one. We shall say that *the cluster T has scale n_T*.
Among the clusters we consider the ones with the property that there is only one diagram line entering them and only one exiting and both carry the same momentum. Here we use that the diagram lines carry an arrow pointing to the root: this gives a meaning to the words "incoming" and "outgoing".
If V is one such cluster we denote λ_V the incoming line: the line scale $n=n_{\lambda_V}$ is smaller than the smallest scale $n'=n_V$ of the lines inside V. We call w_1 the vertex into which the line λ_V ends, inside V. *We say that such a V is a resonance if the number of lines contained in V is $\le E\,2^{-n\varepsilon}$*, where $n=n_{\lambda_V}$, and E,ε are defined by: $E\equiv 2^{-3\varepsilon}N^{-1}$, $\varepsilon=\tau^{-1}$. We call n_{λ_V} *the resonance scale*, and λ_V a resonant line.
Let us consider a diagram ϑ and its clusters. We wish to estimate the number N_n of lines with scale $n\le 0$ in it, assuming $N_n>0$.
Denoting T a cluster of scale n let m_T be the number of resonances of scale n contained in T (*i.e.* with incoming lines of scale n), we have the following inequality, valid for any diagram ϑ:

$$N_n\le\frac{4k}{E\,2^{-\varepsilon n}}+\sum_{T,\,n_T=n}(-1+m_T) \tag{10}$$

with $E=N^{-1}2^{-3\varepsilon},\varepsilon=\tau^{-1}$. This "combinatorial" inequality is a version of Brjuno's lemma: a proof can be found in [G1].
Consider a diagram ϑ^1 we define the family $\mathcal{F}(\vartheta^1)$ generated by ϑ^1 as follows. Given a resonance V of ϑ^1 we detach the part of ϑ^1 above λ_V and attach it successively to the points $w\in\tilde V$, where $\tilde V$ is the set of vertices of V (including the endpoint w_1 of λ_V contained in V) outside the resonances contained in V. Note that all the lines λ in $\tilde V$ (*i.e.* contained in V and with at least one point in $\tilde V$) have a scale $n_\lambda\ge n_V$.
For each resonance V of ϑ^1 we shall call M_V the number of vertices in $\tilde V$. To the just defined set of diagrams we add the diagrams obtained by reversing simoultaneously the signs of the vertex modes $\vec{\nu}_w$, for $w\in\tilde V$: the change of sign is performed independently for the various resonant clusters. This defines a family of $\prod 2M_V$ diagrams that we call $\mathcal{F}(\vartheta_1)$. The number $\prod 2M_V$ will be bounded by $\exp\sum 2M_V\le e^{2k}$.
It is important to note that the definition of resonance is such that the above operation (of shift of the vertex to which the line entering V is attached) does not change too much the scales of the diagram lines inside the resonances: the reason is simply that inside a resonance of scale n the number of lines is not very large being $\le\overline{N}_n\equiv E\,2^{-n\varepsilon}$.
Let λ be a line, in a cluster T, contained inside the resonances $V=V_1\subset V_2\subset\ldots$ of scales $n=n_1>n_2>\ldots$; then the shifting of the lines λ_{V_i} can cause at most a change in the size of the propagator of λ by at most $2^{n_1}+2^{n_2}+\ldots<2^{n+1}$.
Since the number of lines inside V is smaller than $\overline{N}_n$ the quantity $\vec{\omega}\cdot\vec{\nu}_\lambda$ of λ has the form $\vec{\omega}\cdot\vec{\nu}^0_\lambda+\sigma_\lambda\vec{\omega}\cdot\vec{\nu}_{\lambda_V}$ if $\vec{\nu}^0_\lambda$ is the momentum of the line λ "inside the resonance V", *i.e.* it is the sum of all the vertex modes of the vertices preceding λ in the sense of the line arrows, but contained in V; and $\sigma_\lambda=0,\pm1$.
Therefore not only $|\vec{\omega}\cdot\vec{\nu}^0_\lambda|\ge 2^{n+3}$ (because $\vec{\nu}^0_\lambda$ is a sum of $\le\overline{N}_n$ vertex modes, so that $|\vec{\nu}^0_\lambda|\le N\overline{N}_n$) but $\vec{\omega}\cdot\vec{\nu}^0_\lambda$ is "in the middle" of the diadic interval containing it and by (4) does not get out of it if we add a quantity bounded by 2^{n+1} (like $\sigma_\lambda\vec{\omega}\cdot\vec{\nu}_{\lambda_V}$). Hence no line changes scale as ϑ varies in $\mathcal{F}(\vartheta^1)$, if $\vec{\omega}_0$ verifies (4).
This implies, by the strong diophantine hypothesis on $\vec{\omega}_0$, (4), that the resonant clusters of the diagrams in $\mathcal{F}(\vartheta^1)$ all contain the same sets of lines, and the same lines go in or out of each resonance (although they are attached to generally distinct vertices inside the resonances: the identity of the lines is here defined by

the number label that each of them carries in ϑ^1). Furthermore the resonance scales and the scales of the resonant clusters, and of all the lines, do not change.

Let ϑ^2 be a diagram not in $\mathcal{F}(\vartheta^1)$ and construct $\mathcal{F}(\vartheta^2)$, *etc.* We define a collection $\{\mathcal{F}(\vartheta^i)\}_{i=1,2,\ldots}$ of pairwise disjoint families of diagrams. We shall sum all the contributions to $\vec{h}^{(k)}$ coming from the individual members of each family. This is the *Eliasson's resummation.*

We call ε_V the quantity $\vec{\omega}\cdot\vec{\nu}_{\lambda_V}$ associated with the resonance V. If λ is a line in $\tilde{V}$, see above, we can imagine to write the quantity $\vec{\omega}\cdot\vec{\nu}_\lambda$ as $\vec{\omega}\cdot\vec{\nu}^0_\lambda+\sigma_\lambda\varepsilon_V$, with $\sigma_\lambda=0,\pm1$. Since $|\vec{\omega}\cdot\vec{\nu}_\lambda|>2^{n_V-1}$ we see that the product of the propagators is holomorphic in ε_V for $|\varepsilon_V|<2^{n_V-3}$. [2] While ε_V varies in such complex disk the quantity $|\vec{\omega}\cdot\vec{\nu}_\lambda|$ does not become smaller than $2^{n_V-1}-2\,2^{n_V-3}\ge 2^{n_V-2}$. Note the main point here: the quantity 2^{n_V-3} will usually be $\gg 2^{n_{\lambda_V}}$ which is the value ε_V actually can reach in every diagram in $\mathcal{F}(\vartheta^1)$; this can be exploited in applying the maximum priciple, as done below.

It follows that, calling n_λ the scale of the line λ in ϑ^1, each of the $\prod 2M_V\le e^{2k}$ products of propagators of the members of the family $\mathcal{F}(\vartheta^1)$ can be bounded above by $\prod_\lambda 2^{-2(n_\lambda-2)}=2^{4k}\prod_\lambda 2^{-2n_\lambda}$, if regarded as a function of the quantities $\varepsilon_V=\vec{\omega}\cdot\vec{\nu}_{\lambda_V}$, for $|\varepsilon_V|\le 2^{n_V}$, associated with the resonant clusters V. This even holds if the ε_V are regarded as independent complex parameters.

By construction it is clear that the sum of the $\prod 2M_V\le e^{2k}$ terms, giving the contribution to $\vec{h}^{(k)}$ from the trees in $\mathcal{F}(\vartheta^1)$, vanishes to second order in the ε_V parameters (by the approximate cancellation discussed above). Hence by the maximum principle, and recalling that each of the scalar products in (9) can be bounded by N^2, we can bound the contribution from the family $\mathcal{F}(\vartheta^1)$ by:

$$\left[\frac{1}{k!}\Big(\frac{f_0C_0^2N^2}{J_0}\Big)^k 2^{4k}e^{2k}\prod_{n\le0}2^{-2nN_n}\right]\left[\prod_{n\le0}\prod_{T,\,n_T=n}\prod_{i=1}^{m_T}2^{2(n-n_i+3)}\right] \tag{11}$$

where:
1) N_n is the number of propagators of scale n in ϑ^1 ($n=1$ does not appear as $|\vec{\omega}\cdot\vec{\nu}|\ge1$ in such cases),
2) the first square bracket is the bound on the product of individual elements in the family $\mathcal{F}(\vartheta^1)$ times the bound e^{2k} on their number,
3) The second term is the part coming from the maximum principle, applied to bound the resummations, and is explained as follows.
i) the dependence on the variables $\varepsilon_{V_i}\equiv\varepsilon_i$ relative to resonances $V_i\subset T$ with scale $n_{\lambda_{V_i}}=n$ is holomorphic for $|\varepsilon_i|<2^{n_i-3}$ if $n_i\equiv n_{V_i}$, provided $n_i>n+3$ (see above).
ii) the resummation says that the dependence on the ε_i's has a second order zero in each. Hence the maximum principle tells us that we can improve the bound given by the first factor in (11) by the product of factors $(|\varepsilon_i|\,2^{-n_i+3})^2$ if $n_i>n+3$. If $n_i\le n+3$ we cannot gain anything: but since the contribution to the bound from such terms in (11) is >1 we can leave them in it to simplify the notation, (of course this means that the gain factor can be important only when $\ll1$).

Hence substituting (10) into (11) we see that the m_T is taken away by the first factor in $2^{2n}2^{-2n_i}$, while the remaining 2^{-2n_i} are compensated by the -1 before the $+m_T$ in (10), taken from the factors with $T=V_i$, (note that there are always enough -1's).

So that the product (11) is bounded by:

$$\frac{1}{k!}(C_0^2J_0^{-1}f_0N^2)^k e^{2k}2^{4k}2^{6k}\prod_{n=-\infty}^{0}2^{-4nkE^{-1}2^{\varepsilon n}}\le\frac{1}{k!}B_0^k \tag{12}$$

with B_0 suitably chosen.

To sum over the trees we note that, fixed ϑ the collection of clusters is fixed. Therefore we only have to multiply (12) by the number of diagram shapes for ϑ, ($\le 2^{2k}k!$), by the number of ways of attaching mode labels, ($\le(3N)^{lk}$), so that we can bound $|h^{(k)}_{\vec{\nu}j}|$ by (6).

Remark:

It is interesting to remark that we know, from the proof of [Th] of the KAM theorem for the model (2) (for instance, and most directly, see [Th]), that there is a canonical transformation with generating function

[2] In fact $|\vec{\omega}\cdot\vec{\nu}^0_\lambda|\ge2^{n+3}$ because V is a resonance; therefore $|\vec{\omega}\cdot\vec{\nu}_\lambda|\ge2^{n+3}-2^{n+1}\ge2^{n+2}$ so that $n_V\ge n+3$. On the other hand note that $|\vec{\omega}\cdot\vec{\nu}^0_\lambda|>2^{n_V-1}-2^n$ so that $|\vec{\omega}\cdot\vec{\nu}^0_\lambda+\sigma_\lambda\varepsilon_V|\ge2^{n_V-1}-2^n-2^{n_V-3}\ge2^{n_V-1}-2^{n_V-3}\ge2^{n_V-2}$, for $|\varepsilon_V|<2^{n_V-3}$.

having the form $\Phi(\vec{A}',\vec{\alpha}) = (\vec{A}'-\vec{A}_0)\cdot\vec{\alpha} + (\vec{A}'-\vec{A}_0)\cdot\vec{g}(\vec{\alpha}) + \gamma(\vec{\alpha})$ with suitable analytic functions $\vec{g}(\vec{\alpha})$ and $\gamma(\vec{\alpha})$, changing coordinates from $(\vec{A},\vec{\alpha})$ to $(\vec{A}',\vec{\psi})$, and giving the invariant torus, (5), the form:

$$\vec{A} = \vec{A}_0 + \vec{\partial}_{\vec{\alpha}}\gamma(\vec{\alpha}), \qquad\qquad \vec{\psi} = \vec{\alpha} + \vec{g}(\vec{\alpha}) \tag{13}$$

Comparing with (5), and with the equation $(\vec{\omega}_0\cdot\vec{\partial}_{\vec{\psi}})^2\vec{h}(\vec{\psi}) = -\varepsilon\vec{\partial}_{\vec{\alpha}}f(\vec{\psi}+\vec{h}(\vec{\psi}))$ (meaning that (5) is an invariant torus and generating (7)) we see that $\vec{g}(\vec{\alpha}) = -\vec{h}(\vec{\psi})$ and $\vec{\partial}_{\vec{\alpha}}\gamma(\vec{\alpha}) = \vec{H}(\vec{\psi}) = (\vec{\omega}_0\cdot\vec{\partial}_{\vec{\psi}})\,J\vec{h}(\vec{\psi})$ if $\vec{\alpha} = \vec{\psi}+\vec{h}(\vec{\psi})$.

This implies that $\vec{\partial}_{\vec{\alpha}}\gamma(\vec{\alpha}) = -\varepsilon(\vec{\omega}_0\cdot\vec{\partial}_{\vec{\psi}})^{-1}\vec{\partial}_{\vec{\alpha}}f(\vec{\alpha})$, if $\vec{\alpha} = \vec{\psi}+\vec{h}(\vec{\psi})$, and it would be nice to see which identities among tree values are equivalent to the last relation (*i.e.* to the identity of the second order cross derivatives of $\gamma(\vec{\alpha})$).

§5 Some problems that will be left over.

The following are a few problems that have interested me and many of us which I believe (having no reason to believe the contrary) will be left over to the next century:

1) develop a perturbation theory that applies to the simplest dynamical problems in statistical mechanics: like a perturbation expansion in the density of the transport coefficients. Only "recently" it has been realized that the classical expansions were simply not well defined, (*i.e.* had infinite coefficients), if the computation was attempted and resummation schemes were proposed, [CD].

2) develop a theory of the (incompressible) Navier Stokes equation, like some constructive theorem for its solution in $d=3$ dimensions, see [G4]. This seems to be the same as understanding a way to extend the theory of the $d=2$ case, based on the vorticity conservation. No mathematical theory of the $d=3$ Navier Stokes equation incorporates the fact that, for 0 viscosity, there should be vorticity conservation in the form of Thomson's theorem. The best known results, [CKN], from the point of view of existence and regularity are based only on the energy conservation: or more mathematically on the equation obtained by taking the scalar product of the velocity field with the difference of the two sides of the equation, and setting this equal to 0 (with the pressure being given by the well known formula): *i.e.* on just too little information, see also [G4].

3) is it possible to construct (*i.e.* prove existence of) a relativistic scalar field theory in dimension 4 which has an asymptotic series for the Schwinger functions given by the renormalized series for the $\lambda\varphi^4$ model? see [GR].

4) can one understand the Bose condensation? definition, existence and basic properties, see [ADG].

5) can a Fermi liquid be normal at $T=0$, in 2 or more dimensions? see [A]. In fact definition, existence, and basic properties of the ground state of Fermi systems are poorly understood, particularly if $d\ge 2$.

6) can one show the existence of a liquid solid phase transition in statistical mechanics one component systems interacting via short range translation and rotation invariant interactions?

7) can developed, stationary, turbulence be understood more quantitatively in terms of the observed scaling properties and in conformity with the phenomenological theory of Kolmogorov?

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