Round Table
PHYSICS AND MATHEMATICS

Organiser : J. Lebowitz

Panelists : M. Atiyah Cambridge England
E. Brézin, Ecole Normale, Paris, France
A. Connes, Collège de France and I.H.E.S., France
J.Fröhlich, ETH, Zurich, Switzerland
D. Gross, Princeton University, U.S.A.
A. Jaffe, Harvard University, U.S.A.
L. Kadanoff, University of Chicago, U.S.A.
D. Ruelle, I.H.E.S., France

Every attempt to employ mathematical methods in the study of chemical questions must be considered profoundly irrational and contrary to the spirit of chemistry. If mathematical analysis should ever hold a prominent place in chemistry – an aberration which is happily almost impossible – it would occasion a rapid and widespread degeneration of that science.

A. Comte

Simple reflection as well as experience agree in telling us that it is hopelessly difficult to hit upon correct conceptions of Nature by mere guess-work. Rather, these always grow slowly out of isolatedf lucky ideas through a process of adaptation. Therefore epistemology rightly opposes the many frivolous instant theoreticians Hypothesenschmiede who hope to find with little effort a hypothesis explaining all of Nature, and it also opposes the metaphysical and dogmatic derivation of atomism.

L. Boltzmann

The miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve.

E. Wigner

Why do you want to know ?

P.A.M. Dirac

Foreword

The subject of our round table was "Physics and Mathematics". There were many proposals to make the title sharper but it seemed better to leave the discussion open. As well known, relations between physics and mathematics have been important for both sides all along centuries and they have again proved to be very fruitful in recent years. We were very pleased to have with us in this Congress many of the great scientists involved in these developments. On the other hand, it is considered by some mathematicians and physicists, in particular today, that these relations are not essential and can even be dangerous: physics would introduce a lack of rigour in mathematics, mathematics would sterilize true research in physics,...

Our friend and great scientist Joel Lebowitz has agreed to organize this round table, has done a very important work for its preparation and has conducted the debate in a very pleasant and efficient way. You will find here his comments and preliminary contributions from some panelists and other scientists he has consulted. His main focus was how, today, mathematicians and/or mathematical physicists can best use their talents for improving the human understanding of nature. The relevance of (theoretical or mathematical) physics to the evolution of mathematics is a second subject of basic interest which he preferred to leave out in view of recent related discussions on the subject, in particular in the paper by A. Jaffe and F. Quinn entitled "Theoretical Mathematics: toward a cultural synthesis of mathematics and theoretical physics", and responses of some scientists in a recent bulletin of the AMS (April 1994). These documents, more oriented towards problems in pure mathematics (nature of mathematical research and proofs, heuristic ideas versus rigour, ...), were made available to our participants during the Congress.

The general organization at the UNESCO made possible the participation of the audience.

Many thanks to Joel Lebowitz, to our panelists and to all our participants,

Daniel Iagolnitzer

P.S.: The contributions presented below by Joel Lebowitz are sometimes a list of open problems, in accordance with one aspect of his initial request.

Presentation : J. Lebowitz

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S.T. Yau, Cambridge, England

Joel Lebowitz, Rutgers University

I present here excerpts from the written material I received from the panelists and others in response to my request which read in part as follows:

I believe that the Round Table should focus on the question of how the participants in this conference and mathematical physicists in general could best use their talents to advance the human understanding of nature. This charge is paraphrased from Thurston's article in the April Bulletin of the AMS. The extensive discussions there are the reasons I am deliberately leaving out contributions of mathematical physics to mathematics.

I then cited some examples, from among those with which I have some familiarity, where mathematical physics did contribute to our understanding of nature: a) The notion of the thermodynamic limit for phase transitions and the related Gibbs formalism, including its applications to dynamical systems (e.g. Markov partitions) and to field theory. b) The mathematical discovery of solitons and the apparently deep relationships between integrable systems, solvable statistical mechanical models, conformal field theory and critical point universality. c) Generic complexity of nonlinear dynamical systems: sensitive dependence on initial conaitions and parameters, strange attractors, period doublings, universal routes to turbulence, etc.

Other examples abound and each reader will have her or his favorites. Xhat is important, in my opinion, is for mathematical physicists not to get seduced by their facility with manipulation of symbols and, in the words of J.T. Schwartz [1] "to elaborate upon any idea, however absurd; to dress scientific brillancies and scientific absurdities alike in the impressive uniform of formulae and theorems. Unfortunately however, an absurdity in uniform is far more persuasive than an absurdity unclad. The very fact that a theory appears in mathematical form, that, for instance, a theory has provided the occasion for the application of a fixed-point theorem, or of a result about difference equations, somehow makes us more ready to take it seriously. And the mathematical-intellectual effort of applying the theorem fixes in us the particular point of view of the theory with which we deal, making us blind to whatever appears neither as a dependent or as an independent parameter in its mathematical formulation. The result, perhaps most common in the social sciences, is bad theory with a mathematical passport". Schwartz then goes on to quote Keynes' book *General Theory*, "Too large a proportion of recent "mathematical" *economics* are mere concoctions, as imprecise as the initial assumptions they rest on, which allow the author to lose sight of the complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols".

I have italicized the word *economics* in the quote so you, the reader, can substitute your own favorite example. These too abound.

Let me conclude by thanking the panelists, the respondents, and most especially Daniel Iagolnitzer for his very hard work in organizing a very successful meeting. Not only were the official lectures informative and interesting, but, equally important, there was a very good atmosphere in which many private scientific and social interactions took place.

[1] The Pernicious Influence of Mathematics on Science, J.T. Schwartz, Proceeding of the 1960 International Congress on "Logic, Methodology and Philosophy of Science", Stanford University Press, 1962.

E. Brézin, ENS

The world, even the world of science, is full of ayatollahs who promote easily their personal beliefs to the rank of absolute truths; they decide without blinking, what is physics and non-physics, or mathematics and non-mathematics. As far as I am concerned I am happy that the castle of science has many rooms. Clearly I live in a non-mathematical wing of this palace, although no matter where you stand, you are always the mathematical physicist of someone else. This leads me to wonder whether I have been invited to this round table in order to demonstrate *ad absurdum* how physicists may be blind to the achievements of mathematical physics. I am certainly short-sighted but I don't feel blind. Many important and deep physical questions in my field have been settled by mathematical physics. I am sure that you all have zillions of examples that come to your mind. Let me mention simply some ancient facts concerning phase transitions - ancient otherwise I am bound to offend 9/10 of this distinguished audience:

- do the principles of statistical mechanics, set up by Boltzmann and Gibbs, allow for a phase transition ? The question was debated until Peierls proved that the Ising model has a spontaneous magnetization at low enough temperatures.

- the existence of a critical point, non-classical indices in statistical mechanics ? This of course was settled by Onsager's solution.
- the Mermin-Wagner theorem...

The modern history of mathematical physics has been particularly productive in this area, and more is needed (for instance to stop various rear guard skirmishes).

However some of the triumphs of science, of theoretical physics, struck me as being non-mathematical in their intimate structure. One often considers theoretical physics as a conjectural stage waiting for mathematical proofs, very much like conjectures and proofs in mathematics, and I disagree with this view. I would like to illustrate my point on the example of QED, one of the most beautiful and precise constructions ever achieved by human mind.

Dyson was the first to point out that we were lucky that the fine structure constant was so small, not simply because we could then limit ourselves to a few Feynman diagrams, but also because otherwise we would have to supplement the divergent perturbation series with non-perturbative information. One would think that this is where mathematical physics comes into play, but science has chosen a different path. At the time it was believed that a renormalizable theory was required because QED would apply to all distances from the largest down to the smallest in the universe. After Wilson's work it because manifest that renormalizability was simply a property of effective theories, valid at large distances, whatever one dos not know or consider at short distances. Thus QED became an effective theory. Furthermore the renormalization group arguments led to the strong likelihood that non asymptotically free theories were in fact free. Even if one ignores this triviality, 't Hooft's renormalons would probably kill any attempt to make sense of this theory.

QED does not seem to be mathematically sound then. The most precise theory ever invented, presumably does not make sense, although it is as precise as we need ! Is QCD a consistent theory without IR cut-off ? The answer is unknown. May be there is no consistent quantum field theory of anything and still QED and QCD are treasures of mankind. Therefore, I am connecting here with the concepts well argumented in Jack Schwartz article, I regard theoretical physics and mathematical physics as two fields with different goals and perspectives in spite of their strong mutual influence and their innumerable crossed links.

Carlo Cercignani, Milan

In the area of kinetic theory, where my main interests lie I see the following possible developments:

1) Derivation of fluid dynamics from the Newton equations. Although one might expect that Newton dynamics should yield, under the a suitable scaling,

the Euler equations, our ignorance of the long time behavior of the Hamiltonian systems is such that, at the moment, we are quite far from a rigorous derivation of the equations of hydrodinamics from the basic laws of Classical Mechanics. Thanks to the work of Olla, Varadhan and Yau, however, the hydrodynamics of a class of Hamiltonian systems can be derived if we assume that some ergodic properties are satisfied, at least as far as a smooth solution of the Euler equations exists. This is an important connection between ergodic properties and derivation of macroscopic equations that should be pursued.

2) It may be worth to underline how different is the hydrodynamic behavior of a gas obeying the Boltzmann equation and thus the state law of perfect gases, from the behavior arising from a particle system describing a real gas and thus a more complicated state law, including the effects of the interaction potential between molecules. In other words, as a consequence of the Boltzmann-Grad limit, the local equilibrium of a Boltzmann gas is that of a free gas, while, in general, the local equilibrium of a gas is a Gibbs state for an interacting particle system. Although the latter is the local equilibrium taking place in real fluids, the mathematical analysis of the hydrodynamics arising from the Boltzmann equation is technically easier and has produced more results. Even here, however, more work should be done. One can follow two paths: a) use the solutions close to equilibrium for which we have powerful estimates. This is a path followed by De Masi, Esposito and Lebowitz, and Esposito, Lebowitz and Marra: it produces interesting, almost explicit results, but has the disadvantage that the work to be done in order to obtain the necessary estimates becomes cumbersome for problems with nontrivial geometry, because of the difficulty of controlling the solution inside boundary layers; b) try to use the more abstract approach in L^1 by DiPerna and Lions as attempted (with only partial success) by Bardos, Golse and Levermore; here the difficulty lies in the rather modest information on the behavior of the solution.

3) Validity of the Boltzmann equation: Lanford's short time proof uses estimates of the L^∞ type. If one could work in L^1 a global prove could be obtained, but something is missing (the difficulties are of the type of 2b) above).

4) Due to the singularity of the interaction kernel, the validity of the Vlasov-Poisson equation has not been established as yet and the mere existence and uniqueness of smooth solutions in dimension 3 has only recently been achieved.

5) The Enskog equation is known to describe well a dense gas of hard spheres, but it is not exact; can we explain why it is so good? Or can we obtain something better?

Sergio Doplicher, Roma

How can Mathematical Physicists best advance human understanding of Nature? To your big question I propose as a tentative answer to commandments:

1) Though shall be Physically motivated

2) Though shall be Mathematically precise

plus if possible

3) Thy method shall be of interest to Mathematics per se.

Not everybody cares of 2) but most pretend they obey 1). I propose a criterion: The problem should admit a faithful translation in words which do not depend on a specific formalism and yet sounds physically interesting.

I do believe that the Operator Algebraic Approach to Quantum Field Theory and Statistical Mechanics – happily named Local Quantum Physics by Rudolf Haag – is rich of big problems in line with those principles. Each big problem contains a lot of smaller ones. Here are some of the big ones (details may be found in the volume in memory of Hoegh-Krohn, Albeverio et al. eds.; for the general frame see Rudolf Haag's book Local Quantum Physics).

What is the most general notion of statistics compatible with the principle of Locality in presence of massless particles? What is its stability against perturbations of the Dynamics? Is stability expressed by an index theorem relating statistics to deformation invariants?

How is the existence of Topological Charges encoded in the algebraic structure of the local observables?

What distinguishes Gauge Theories at the level of Local Observables? (Rudolf Haag's long standing question).

Is there a strict Quantum Noether Theorem providing conserved current Wightman fields out of the local algebras of observables? Can we characterize theories where the local charge and energy momentum density fields generate the local observables?

These problems, and others in this line, might stimulate physical insight in QFT as well as mathematical research on inclusions of factors, structure of endomorphisms of Operator Algebras, actions of categories on operator Algebras and their deformations, crossed products, etc.

Jim Glimm, Stony Brook

1. Stochastic variability occurs in nature due to causes which are not known, or which while known, may be too detailed in their specification to be useful in a practical sense. Stochastic variability is also used to model incomplete, or missing data.

Processes occuring in nature are usually spatially dependent; the relevant stochastic processes are thus random fields. From this point of view, spatial stochastic processes are nearly as ubiquitous as are partial differential equations as a model of nature. The normal model should be the stochastic partial differential equation. Here I do not refer to the somewhat well developed theory of spatially correlated forcing terms added to partial differential equations, giving a coupling to a Wiener process or the like, but coupling, often in the coefficiens of the derivatives, to a fully spatially dependent random field. The single major factor most strongly limiting the use of this approach is a lack of understanding of the relevant theory (beyond the level of gaussian processes and linear partial differential equations), and a lack of methods for the effective approximate solution of these equations.

2. Rate dependent, metastable, and nonequilibrium processes are not understood at a level adequate for applications.

3. There are many important problems, both large and small, concerning fluid instabilities.

4. Turbulence presents many open problems, and is one of the major challenges to physics of this century. Practical computations will be possible within a few years, and efforts to use them, especially in cases which are only marginally computable, will create both a need and an opportunity for an increased theoretical effort.

5. Multi-length scale theories, or the effective coupling of theories on distinct length scales are usually very challenging. Often the length scales, while separated, are not infinitely separated, as is required in theories of homogenization. As an example problem, what is the influence of impurities, dislocations and vacancies on continuum level properties such as material strength?

Jim Hartle, Santa Barbara

The outstanding mathematical physics problem in the classical theory of relativity is cosmic censorship. We know from the work of Penrose, Hawking and Geroch that smooth initial data for the Eintstein equation often evolve singularities. Cosmic censorship is the idea that all such singularities that arise in gravitational collapse are hidden in black holes. There are various mathematical formulations (one problem is to get a sufficiently precice formulation of the conjecture) which boil down to understanding the global evolution of this nonlinear set of differential equations. It is an important problem because one can't make predictions past singularities, but if hidden inside black holes we would never encounter this problem classically. Put differently if only black holes can evolve in a gravitational collapse that passes a certain stage, the black hole endstate of stellar evolution is one of a limited class of geometries independent of how the collapse started. and this leads to definite predictions in astrophysics. The cosmic censorship conjecture is an outstanding but tough problem. Someone in mathematical physics should solve it.

Arhur Jaffe, Harvard

I personally take a broad and inclusive view of mathematical physics. In fact for some twenty years, I have advocated (perhaps somewhat in jest) that we use the inclusive symbol

$M \cup \Phi$ in place of the exclusive symbol $M \cap \Phi$

as the trademark of our Association. This has never proved popular. But as I see it, there have evolved at least four components to mathematical physics which illustrate the development of this synergy:

- 1. The use of ideas from mathematics in shedding new light on the existing principles of physics, either from a conceptual or from a quantitative point of view.
- 2. The use of ideas from mathematics in discovering new "laws of physics."
- 3. The use of ideas from physics in shedding new light on existing mathematical structures.
- 4. The use of ideas from physics in discovering new domains in mathematics.

Each of these topics plays some role in our Congress. However, our success in directions 2 and 4 is certainly more modest than our success in directions 1 and 3. In some cases it is difficult to draw a clear-cut distinction between these two sets of components. In fact, we are lucky when it is possible to progress in directions 2 and 4, and when we make major progress there, historians like to speak of a revolution. In any case, many of us strive to understand these deep and lofty goals.

In my own country, the United States, and in many other countries, our governments would like to direct us toward another, more mundane component:

- 5. The use of ideas from physics or from mathematics to benefit "economic competitiveness."

Here too, one might subdivide this component into conceptual understanding on the one hand (such as the mathematical model of Black and Sholes for pricing of derivative securities in financial markets) or invention on the other: the formulation of new algorithms or materials (e.g. personal computers) which might revolutionize technology or change our way of life. As above, the boundary between these domains is not sharp, and it remains open to opinion and interpretation. I will not pursue this strand, which we might characterize as "applied" mathematical physics. Rather I will restrict the remainder of my comments to the first four stands characterizing "fundamental" mathematical physics. In fact, I believe that a case can be made that most of the profound applied directions arise after earlier fundamental progress[1].

We have lived through an extraordinary 20-year period of development of fundamental mathematics and physics. Much of this development has drawn from one subject to understand the other. (Parenthetically this time-scale also coincides both with the existence of the Congresses on Mathematical Physics, and also the time-scale for the original planning of the existence of our Association. I attribute this to the perception among the leaders of the subject twenty years ago that something exciting was taking place.)

Not only have concepts from diverse fields been united: statistical physics, quantum field theory, and functional integration; gauge theory and geometry; index theory and knot invariants, etc. But also new phenomena have been recognized and new areas have emerged whose signifigance we only partially understand — both for mathematics and for physics: such as non-commutative geometry; mirror symmetry; new invariants of manifolds; and the general notion of deformation quantization.

Over the past twenty years, there is no question that the ideas from physics have led to far greater invention of new mathematics, than the ideas from mathematics have been in discovering new laws of physics. But this just underscores the opportunities for future progress in the other direction! We await a new understanding of the quantum nature of the world.

There has been great publicity and recognition attached to the progress made in geometry, in representation theory, and in deformation theory due to this interaction. But one should not ignore the substantial deep progress in analysis and in probability theory, which unfortunately is more difficult to understand because of its delicate dependence on subtle notions of continuity.

On the other hand, I do not claim that physics and mathematics are the same. Quite to the contrary, they have evolved from different cultures and they each have a distinctive set of values of their own, suited for their different realms of universality. Both subjects are based on **intuition**, some natural and some acquired, which form our understanding. On the one hand, physics describes the natural world. Hence physicists appeal to observation in order to verify the validity of a physical theory. On the other hand, although much of mathematics arises from the natural world, mathematics has no analogous testing ground. Mathematicians appeal to their own set of values, namely mathematical proof, to justify the validity of a mathematical theory.

In the past I have written on this at length from several points of view: for example my essay from the 1984 David Report[1] and a recent paper in collaboration with Frank Quinn[2]. The latter essay is directed especially toward mathematical community, and was published in a mathematics journal. In fact this paper evoked extended discussion[3]. I take this opportunity to point out that much of the discussion of our paper eluded the main point, which is our plea for "Truth in Advertising." When announcing results of a mathematical nature, claim a theorem when your proof meets the mathematics community standards

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[1] See Notices Amer. Math. Soc., (1984) Vol. 31, 589–608; or SIAM Review, (1984) Vol. 26, 473–500.

[2] Theoretical Mathematics: Toward a Cultural Synthesis of Matheamtics and Theoretical Physics, Bull. Amer. Math. Soc., (1993) Vol. 29, 1–13.

[3] Scientific American, Culture Clash, August 1993, page 14; Bull. Amer. Math. Soc., (1994) Vol. 30, 159–211.

for a proof. Otherwise make a conjecture with a detailed outline for support. Physics, on the other hand, has completely different standards.

There is no question that the interaction between mathematics and physics will change radically over the next 20 years too. In fact, I believe that the mode of electronic communication of science will change the way we work, the way we interact, and the way we view things. They will change to such an extent that predictions we make today will inevitably be wrong twenty years from now. Thus I will not try that. But I do hope that this evolution will preserve the positive experiences of being a mathematician, of being a physicist, or of being a mathematical physicist, so that it remains attractive to the brightest and most capable students tomorrow.

R. Newton, Indiana

I think the round table should address itself not only to such down-to-earth questions as "what problems ought to be attacked with high priority", but also to more general philosophical questions: Why is mathematics so "unreasonably effective" in the natural sciences (as Wigner put it)? Is Poincare's analogy correct, according to which science is a library, in which the experiments write and contribute the books, and the mathematical physicists furnish the index? Why is it that almost all mathematical structures, invented for their own sake, eventually turn out to be of use in physics? Can mathematics be expected to tell us why "God had no choice in the way He constructed the world"?

Adrian Patrascioiu, Arizona, Erhard Seiler, Munich

In our opinion the main benefit Physics can derive from Mathematical Physics is that the latter can separate fact from fiction. There is one important area where this would be sorely needed, that is the need to prove or disprove the dogma in particle and condensed matter Physics that there is a fundamental difference between Abelian and non-Abelian models, both in the case of two-dimensional ($2D$) spin models and four-dimensional ($4D$) Yang-Mills theories. The dogma states that the non-Abelian versions of those models enjoy the properties called Asymptotic freedom and dynamical mass generation. This dogma is rooted in the perturbative computation of the Callan-Symanzik β-function and has been challenged by us from two sides:

1. We have produced an argument based on percolation theoretic considerations that leads to the conclusion that all $2D$ $O(N)$ models have a soft low

temperature phase contrary to the dogma. Our arguments admittedly are not mathematically rigorous, but it is very hard to see how they could fail and they have never been challenged. Moreover published numerical studies lend support to the existence of such a soft phase. It seems to us that Mathematical Physics should recognize this as an important open problem and try to settle the question once and for all.

2. It is obvious that our conclusions can only hold if perturbation theory (PT) is misleading and making the wrong prediction for the Callan-Symanzik β-function. Recent computations by us show that there is indeed something wrong with the perturbative method applied to those models: It produces answers that depend on the boundary conditions chosen even in the (termwise) thermodynamic limit. This happens in $2D$ spin models with non-Abelian symmetry as well as in lattice Yang-Mills theory in any number of dimensions. The challenge to Mathematical Physics would be to find out which, if any, of those PT answers constitute the correct asymptotic expansion for those models in the (presumably unique) thermodynamic limit.

We have heard theoretical physicists express the opinion that mathematical physicists have actually proven the correctness of the dogma. In fact the rigorous results obtained so far can control the ultraviolet limit only at the price of introducing some mass or infrared cutoff, whereas in our opinion it is control of the infrared behavior that is the crucial point. It has been claimed that this limitation is due only to technical difficulties, but we believe that these difficulties are reflections of the troubles we have been pointing out, namely the unavoidable large fluctuations in the infrared present in these models. Thus it would be most desirable if Mathematical Physics could settle these issues one way or another.

David Ruelle, IHES

A discussion on physics and mathematics is likely to reflect contemporary pressures on science. Let me mention some of those :

- pressure to do application-oriented research (US)
- pressure to use the French language in research (France)
- pressure to publish more and more at a time when one reads and less
- development of administrative power structures in mathematics (NSF)
- growing role of the media and scientific prizes (everywhere)

My contention is that such pressures are counterproductive in mathematical physics (and in other fields where heavy equipment is not needed).

Let me discuss briefly the example of chaos. Now that the dust has settled a bit, one must admit that the ideas of chaos have led to definite improvement of our understanding of nature in several domains: meteorology, hydrodynamics, astronomy of the solar systems, and others. If one looks at the papers that started this field, it is clear that they were not motivated by the pressures mentioned above. They were motivated by irritation of the authors who did not understand some fact, and worked until they had an explanation.

I have mentioned chaos because I know the subject, but I am not pushing people to go into chaos *now*. I rather think that a scientist who wants to do a research carreer in mathematical physics should start thinking about one of the many irritating problems that we do not understand: developping a theory of phase transitions, a theory of crystals, a theory of approach to equilibrium, a theory of fully developed turbulence, and so on. I said to start thinking about those topics, not to start writing papers.

Of course, leisurely thinking about science is definitely not a politically correct suggestion at this time in the US, but probably it remains the most efficient way of hitting useful new ideas. I would find it quite natural that those people who never find anything be discouraged to continue "doing research" all their lives (but this idea is definitely not politically correct in France).

In conclusion, I suspect that politically correct research, and good research have little relation with each other.

B. Schroer, Berlin

In many plenary talks and through the panel discussion, the crisis in theoretical and mathematical physics became obvious. It originates in the area of what used to be quandum field theory and particle physics and by now pervades large areas of mathematical physics. In this "new age" type of mathematical physics (for which even a starting date was fixed in one of the plenary talks), the main theme is mathematically oriented geometric inventions, disregarding history and conceptual ideas (physical principles) to such a degree that one gets the impression that the authors never even knew that there are such things. Whereas inventions are very good for physicists (and some mathematicians), they have a devastating influence on theoretical physics.

At the panel discussion we were given the choice between the Scylla of Lagrangian field theory with cutoff (to console us with the fact that there may be no local theory behind perturbation theory as e.g. in QED, sigma-models etc.) and the Cariddy of the new age mathematical physics.

Paradoxa, contradictions (i.e. the motor of theoretical physics after the Bohr atomic model) and their enigmatic power are not en vogue any more.

Must this century, which started with great conceptual discoveries (Einstein causality, the principles of quantum physics) end in conceptual poverty drowned by sophisticated new age mathematical physics journalism? Is a fin de siecle crisis a recurrent phenomenon?

If one has ears and eyes for new discoveries which ripen slowly and need to avoid the limelight for the present time, one was not totally disappointed. A fundamental understanding of "new degrees of freedom" in low dimensional QFT and Statistical mechanics with potential rich physical harvest (in condensed matter physics) is one of the more interesting developments. These ideas seriously challenges our framework of "quantization" and functional integrals (Feynman-Kac formulas). There is even the hope that their understanding from first principles (i.e. outside the context of specific conformal algebras) may sharpen our conceptual senses in order to understand new degrees of freedom (e.g. confined quarks) in 4-D theories beyond the quasi-classical straight jackets of present-day quantized gauge theories.

Edward Witten, IAS, Princeton

I am replying to your requests for input to the roundtable discussion.

First I'd like to say that I am not happy with your proposing to exclude applications to mathematics from the discussion. I realize that things may be different in different areas of mathematical physics, but in the sort of things I know best, when a mathematical result is really relevant to a physics problem it often happens that, turning things around, the result can be deduced from the behavior of the physics problem. Ignoring that is artificial and amounts to doing the same thing in a less comprehensive way.

One of the bonuses of making explicit the mathematical implications of the physics problem is that it attracts interest of people who might otherwise not be interested in the physics and results in new points of view being brought to bear. This has proved its worth.

It may well happen that some of the contributions that will advance "understanding of nature" the most over time will appear to some observers to be "mathematics." In general to try to discuss mathematical physics while avoiding mathematics seems to me to be a slippery slope, best avoided.

At any rate, to respond to other points in your letter, I do think that, looking at things in the long run, some of the most exciting mysteries we know about in trying to expand our knowledge of fundamental natural law are "mathematical." Certainly what fascinates me the most is the question of what really is the underlying "geometrical" structure that has the amazing manifestations we see in string theory. I think that question is going to be "pulling the strings" behind the scenes until it is dealt with successfully – which may be a while. Until then, working on some of the manifestations can be a lot of fun and I suspect very influential in the long run.

Anyway, the last paragraph was meant as a very brief statement of how I see the role of what I do. (Though you invited us to make more specific suggestions of small and midsize as well as big problems, I won't do that here concerning the things I work on since some of that will be implicit in the talk I'll be giving at the same conference – the day after the roundtable.)

I do want to comment on one other area, however. I think that constructive field theorists have possibly, in the last few years, been overlooking the opportunity to expand their efforts into areas that could be influential in new ways. The traditional goal in constructive field theory is

to construct more realistic physical models. This is an important and rightly chosen goal. However, it is a very difficult goal and the best route to a satisfying success might not be frontal assault. Raising, as I said above, new issues, attracting new interest and new points of view might be more effective. Superrenormalizable theories in two dimensions are considered relatively trivial in constructive field theory and people don't work on them so much any more. However, people in this area should take note of very rich and fascinating geometrical properties of such superrenormalizable 2d theories (mostly in the supersymmetric case) that have been discovered in the last few years at the heuristic level by string theorists. I do believe that developing some of this material rigorously (I have a hunch that while a hard project it might be doable) would attract a lot of new interest and new points of view from the mathematical world, possibly giving the field a big boost. When I think about what opportunities are being missed right now, this is one that comes to mind. (Note that my advice is in keeping with the comments I stated in the first couple of paragraphs of this letter.)

S.T. Yau, Cambridge

Mathematical Problems in Velocity

- 1. Classify compact four dimensional Einstein manifolds with nonegative scalar curvature. In particular, does S^4 admit any exotic Einstein metric. After Wick rotation, Schwarzchild metric becomes euclidean which has an asymptotic end equal to $S^1 \times S^2$. Is it the only Ricci flat manifold with this property.
- 2. Find a right definition of quasilocal mass in general relativity. Use it to formulate how black hole forms. A proposal was K. Thorne's hoop conjecture that if the mass is greater than the circumference of a region, black hole forms.
- 3. Formulate and prove the cosmic censorship of Penrose. In particular, prove that the total mass is greater than $\frac{\sqrt{A}}{16\pi}$ when A is the area of the outermost black hole.
- 4. Prove the nonlinear stability of the Schwarzchild or Kerr solution.
- 5. Given a generic asymptotic flat nonsingular initial data set on R^3, what is the structure of the null infinity of the space time.
- 6.Find (and prove) an approximate solution of the two body problem in general relativity.