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Reflections on Geometry and Physics

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1 Philosophical reflection. In this lecture I will discuss in general terms what has been happening to the theoretical physics/mathematics frontier over the past 15 years.

Specifically I refer to the geometric and topological aspects of quantum field theory which have now spread in a variety of directions. New terms such as quantum groups, quantum geometry, quantum cohomology are appearing. These indicate the scope and significance of the interaction, but it is premature in my view to try to force everything into a particular mould. Time will tell what the significant aspects really are and then the right title to adopt will be clearer.

However, there has been unease expressed in certain quarters, most recently by Jaffe and Quinn, about the doubtful mixture that is emerging. Not tied closely to experimental physics nor to rigorous mathematics, standards are endangered and warning signs should be erected! Like a ship exploring unchartered seas, with inadequate maps and faulty compasses, catching glimpses of beautiful tropical islands: mirages or reality?

We can distinguish perhaps four different types of reaction by mathematicians towards these developments

(A) Take the heuristic results "discovered" by physicists and try to give rigorous proofs by other methods. Here the emphasis is on ignoring the physics background and only paying attention to mathematical results that emerge from physics. Like Ramanujan who intuited marvelous formulae that defied mathematical proof so physicists are viewed in the same light. The task of the mathematician is to start from scratch and aim to prove these marvelous "intuited truths". This is, of course, the minimalist reaction: the mathematician reacting in his own terms to an externally posed problem. We cannot ignore such challenges and we would all agree that a rigorous proof is a desirable objective.

(B) The second approach is try to understand the physics involved and enter into a dialogue with physicists concerned. This has great potential benefits since we mathematicians can get behind the scenes and see something of the stage machinery. This may provide clues for possible proofs, it may enable us to generalize the story and it may help us to see unexpected links with other areas. We may also be able to assist the physicists in their task, by pointing out relevant bits of mathematics or suggesting new points of view. This dialogue has, in fact, been developing widely in recent years, so that a whole new generation of mathematicians and physicists have begun to speak a common language. The worry of Jaffe and others is that this is a kind of pidgin

English, with little grammar and no literary merit. But in its own terms it has been a remarkable success. The "results" keep growing in scope and depth and inevitably attract the incoming generation. The question is: where is it all leading?

(C) Following on from (B), one natural road for mathematicians to take is to try to develop the physics on a rigorous basis so as to give formal justification to the conclusions. This is the traditional role of the "mathematical physicist", of whom Jaffe is a fine exponent, and who have made rich contributions in the past. While undeniably the "right" approach for a respectable mathematician, it is sometimes too slow to keep up with the action. Depending on the maturity of the physical theory and the technical difficulties involved, the gap between what is mathematically provable and what is of current interest to theoretical physicists can be immense. Moreover, proofs are not always constructed from the bottom up. They may start from the top, or from the side, and only emerge after many hesitant steps and experimentation. Moreover, the right framework has to be established before rigorous work can begin, just as an architectural plan is necessary before the builders move in.

(D) Finally, and most ambitions of all, we may try to understand the deeper meaning of the physics-mathematics connection. Rather than view mathematics as a tool to establish physical theories, or physics as a way of pointing to mathematical truths, we can try to dig more deeply into the relation between them. This may lead us into the perennial problem of deciding whether mathematical results are invented or discovered. This investigation may only have philosophical or theoretical interest but it could lead to better understanding and even to new insights and genuine progress.

These four approaches are not, of course, mutually exclusive but many people will only dip their toes into this whole area and are happy to stick with (A). A sizeable community goes as far as (B), while (C) and (D) are definitely minority tastes. I do not disguise my attraction to (D) and this lecture will try to develop my ideas in that direction.

2 A Survey. Having set the philosophical scene, and raised some questions, I want to spend some time surveying briefly some of the new ideas and results in mathematics that have emerged from the interaction with theoretical physics.

2.1 Index Theory. The index theorem for the Dirac operator on compact Riemannean manifolds has turned out to be of great interest and relevance in gauge theories, since it measures the difference between left-handed and right-handed spinors or other physical entities. Various new proofs emerged naturally from the physicist's viewpoint. In particular, supersymmetry, an algebraic formalism that is increasingly used to bring fermions and bosons onto an equal footing, has led to useful simplifications. Moreover, a whole range of generalizations, including the study of the dependence of the Dirac operator on background gauge potentials, have been suggested by the physics. These have subsequently been given rigorous proofs by Bismut and others.

2.2 Elliptic Genera. Quantum field theory (for one space dimension) led Witten to introduce an appropriate Dirac operator on loop spaces. This has shed light on elliptic genera: these are generating functions for an appropriate sequence of Dirac operators coupled to bundles associated to the tangent bundle. It turns out that they are modular forms and the physics gives a natural interpretation of modularity as a consequence of (2-dimensional) relativistic invariance. Moreover a conjectured rigidity theorem (for compact group actions) also followed naturally from the physics and was eventually given rigorous proofs by Bott and Taubes.

2.3 Topological Quantum Field Theories. A number of extremely interesting topological theories, including Jones' work on knot invariants and Donaldson's work on 4-manifolds, have been given quantum field theory formulations by Witten. This has provided a unifying framework and has also led to generalizations of the original work. Thus the Jones invariants of knots in S^3 have now been extended to knots in general closed 3-manifolds.

Theories of this type, in dimension 2, have led to very precise and new information about the moduli space of flat unitary bundles on Riemann surfaces.

2.4 Conformal Field Theory. The representation theory of certain infinite-dimensional algebras, related to the circle, has a globalization over Riemann surfaces. The original circle here appears as the boundary of a "puncture" on the surface. Such "conformal field theories" are reasonably precise algebraic objects which connect representation theory to topology, via the topological Jones theory of (2.3).

2.5. Quantum Cohomology. Quantum field theory leads to a natural deformation of the ordinary cohomology ring of a manifold. This may briefly be referred to as "quantum cohomology". For example, for the complex projective line P_1 the ordinary cohomology is generated by $x \in H^2(P_1)$ with $x^2 = 0$, while the quantum cohomology has $x^2 = \beta$, where β is a real number (a parameter of the theory so that $\beta \to 0$ is the classical limit).

These "quantum cohomologies" are of considerable mathematical interest. For projective spaces and more generally Grassmannians they are related to the "Verlinde algebra" which plays a key role in conformal field theory and related topics. For 3-dimensional Calabi-Yau manifolds it contains information about the numbers of rational curves. This information is consistent with known results but does not yet have a rigorous mathematical proof. The physicist's "proof" involves the intriguing notion of dual or mirror manifolds, a pair of Calabi-Yau manifolds which are supposed to yield the same quantum field theory, but in dual ways.

2.6 2-dimensional gravity. The examples so far all fall within the class of gauge theories for forces other than gravity. However, there have been interesting developments related to gravity in 2-dimensions. These are closely involved with the moduli spaces of Riemann surfaces. In particular, triangulations of these moduli spaces link up with the combinatorial techniques of Feynman diagrams. The most exciting developments in this direction are due to Kontsevich and they also link up with the topology of 3-manifolds.

2.7 "Twisted" theories. Witten has shown how many physical quantum field theories can be "twisted" to yield topological theories. The twisting involves changing the spin of various fields. Certain correlation functions of the physical theory can be identified with some of the correlation functions of the twisted topological theory. This link has potentially important consequences. For example, Witten has suggested that the presence of a mass gap for $N = 2$ supersymmetric Yang-Mills theory in 4-dimensions may be related to conjectural properties of the Donaldson polynomials (which are derived from the topological Yang-Mills theory).

3 Interpretation. All these examples of fruitful interaction between quantum field theory and topology indicate that something substantial and widespread is involved. How should we interpret all this, what does it imply for "real" physics, and how are we to deal with its mathematical aspects?

Perhaps it is helpful if we recall the role of symmetry (and group theory) in physics. Over the years symmetry has come to be recognized as a crucial guiding principle in large parts of fundamental physics. Starting with finite symmetries (as in crystals) and then moving on to continuous symmetries of compact groups, quantum physicists eventually introduced Hilbert space representations of non-compact Lie groups. This introduces extra analytical difficulties and, at first, there was no systematic mathematical theory to build on. However, mathematicians such as Gelfand and Harish-Chandra soon moved in to establish a base and develop an elaborate theory. Infinite-dimensional representations are now regarded as a vital part of many branches of mathematics including those like Number Theory, which are far removed from Physics.

I suggest that we are now seeing a similar, but more elaborate story involving the impact of Topology on Quantum Theory. Early topological ideas go back to Dirac (and even to Maxwell) but have only played a major part in the past decade or two. Again we are essentially dealing with infinite-dimensional phenomena (quantum fields) and it is the topology of these infinite-dimensions that is making itself felt. Topology and Symmetry have close analogies and relations, but Topology is inherently broader and more complex. For this reason we should not be surprised if Quantum Topology is a difficult subject which will take many years to mature.

Topology and Group Theory have something in common in their relation to Physics. Both interact, in principle, via Analysis but for many purposes the Analysis can be by-passed and replaced by Algebra. This is why so much of the Physics literature is filled with formulae. In the absence of a fully-fledged theory able to handle all the difficult analysis, physicists work formally and heuristically with algebraic formulae.

It is clear that the presence of symmetry in a physical situation imposes strong constraints and these can be exploited algebraically. What is the corresponding impact of topology? As Witten has explained, topology tends to provide information about low-energy states. For example, Hodge's theory of harmonic forms shows that the zero-energy states (for differential forms) correspond to the cohomology. It is worth noting that no significant topology enters for scalar fields, but in the super-symmetric version, when differential forms are brought in, the topological consequences become very significant. Corresponding statements can be made when we pass from quantum mechanics to quantum field theory. Interesting topology usually requires many nonscalar fields and frequently involves super-symmetry.

Symmetry and topology can play complementary roles with topology helping to determine the ground-states and symmetry then telling us how to build up higher states.

Now let me return to the general question of the "meaning" of all this "quantum topology". It would be hard to deny, in the face of all accumulated evidence, that the physicists who dabble with topology and quantum field theory are really on to something. How should we mathematicians respond, giving that a great deal rests on heuristic calculations and physical insight? Physicists will say that they are trying to develop quantum field theories which will explain all elementary particles and, if they are ambitious, also gravity. They are experimenting with a wide variety of models, many of which are "toy" models in the sense that they are grossly over-simplified in order to make them tractable. Given the extreme difficulty of the "real" physical theories, it is not unreasonable to focus on easier ones where one can make progress and gain insight - science always progresses in this way, although the mark of good scientists is to play with the right toys. A simplified model may be one in lower dimensions or having additional symmetries which lead perhaps to exact solutions. What the physicists typically extract is a lot of algebraic information (they like formulae!) and a toy model usually bristles with formal algebra.

This is the conventional, and acceptable, explanation of the physicists. What should be the reaction of the mathematician? Here we find a marked contrast, depending on the mathematician's background. Analysts, particularly those who have been trying to provide a rigorous basis for quantum field theory, dislike the algebraic superstructure which they think skirts the issue and hides the analytical difficulties. They would prefer to concentrate on the simplest possible theory algebraically so as to face up to analytical difficulties. Other mathematicians, coming from algebra, geometry or topology, are attracted by the superstructure and recognize there numerous features linked to their own experience. They are more than happy to follow the physicists in postponing any consideration of real analysis and concentrating on the formal structure. The hope, and ultimate justification, is that the formal apparatus may in the end lead the way to producing a rigorous theory. Perhaps the analysis will prove more tractable when approached the right way. It is already clear that a more formally complicated theory may turn out to be essentially simpler and better behaved than an apparently elementary theory, For example, in 4-dimensions, it is now recognized that Yang-Mills theory is better behaved than a scalar ϕ^4 theory.

Now let me turn to a more difficult question. What are to make of the striking results in 3 and 4-dimensional geometry that have emerged from field theory ideas? Specifically I have in mind the Jones invariants of knots and Donaldson's profound results on 4-manifolds. Is the relation with physics an accident which will in due course be eliminated, and replaced by more conventional mathematical techniques, or is the physics here to stay?

My own view is that the quantum standpoint is essential and that we are dealing with aspects of geometry or topology which are best understood in terms of quantum physics. For example, the fact that the 4-dimensional phenomena unearthed by Donaldson do not occur in other dimensions is surely an indication of their depth, and an indication that other conventional mathematical techniques will be inadequate to explain them. In 3-dimensions the work of Vassiliev, based on conventional homology (of a function space), has cast new light on the Jones invariants but it has not yet displaced the quantum approach. It provides an alternative avenue with different merits.

My conclusion is that, as in earlier episodes, mathematicians will absorb and abstract the essential quantum theory ideas, and develop an appropriate branch of mathematics. Because of the complexity and depth of the theories, especially if gravity is to be included, this may take time and may develop into an imposing edifice.

References

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