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Physical Mathematics and the Future

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Abstract: These are some thoughts meant to accompany one of the summary talks at Strings2014, Princeton, June 27, 2014. This is a snapshot of a personal and perhaps heterodox view of the relation of Physics and Mathematics, together with some guesses about some of the directions forward in the field of Physical Mathematics. At least, this is my view as of July 21, 2014.

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If there is anyone here whom I have not insulted, I beg his pardon. - Johannes Brahms

1. Predicting the future

The organizers of Strings 2014 asked me to give a "vision talk" on the mathematical aspects of string theory. I usually associate visions with Prophets and Saints. Being neither saint nor prophet I have sought out advice - lots of it. Among other things, I consulted some of the great vision talks of the past. Surely one of the greatest was David Hilbert's address to the second International Congress of Mathematicians, in Paris at the turn of the century. He began with the following resounding words - one can almost hear the opening measures of Strauss' *Also Sprach Zarathustra* in the background:

Wer von uns würde nicht gern den Schleier luften, unter dem die Zukunft verborgen liegt, um einen Blick zu werfen auf die bevorstehenden Fortschritte unsrer Wissenschaft und in die Geheimnisse ihrer Entwickelung während der künftigen Jahrhunderte! Welche besonderen Ziele werden es sein, denen die führenden mathematischen Geister der kommenden Geschlechter nachstreben? welche neuen Methoden und neuen Thatsachen werden die neuen Jahrhunderte entdecken - auf dem weiten und reichen Felde mathematischen Denkens?

Who of us would not be glad to lift the veil behind which the future lies hidden; to cast a glance at the next advances of our science and at the secrets of its development during future centuries? What particular goals will there be toward which the leading mathematical spirits of coming generations will strive? What new methods and new facts in the wide and rich field of mathematical thought will the new centuries disclose?

Unlike Strauss, Hilbert then went on, famously, to live up to this magnificent opening with a list of 23 problems for the 20th century. That Problem Set remains incomplete even though it has occupied some of the best talent in Mathematics ever since. Of particular concern to us here is the sixth problem:

Durch die Untersuchungen über die Grundlagen der Geometrie wird uns die Aufgabe nahe gelegt, nach diesem Vorbilde diejenigen physikalischen Disciplinen axiomatisch zu behandeln, in denen schon heute die Mathematik eine hervorragende Rolle spielt ...

The investigations on the foundations of geometry suggest the problem: To treat in the same manner, by means of axioms, those physical sciences in which mathematics plays an important part ....

If we (with some generosity) interpret Hilbert's sixth problem as a search for the ultimate foundations of physics, we can certainly say that one common goal (though not the *only* goal) in Physical Mathematics is to elucidate the solution to that problem. We are all students of Hilbert.

To be sure, Hilbert and his students did not have all the relevant prerequisites! Little could they suspect on that cool and rainy Wednesday, August 8, 1900, in Paris, that in two short months, on a clear and mild Sunday, October 7, in Berlin, Max Planck would guess his famous formula for the energy density of blackbody radiation, and in December derive it using truly revolutionary reasoning. If then, Herr Doktor Hilbert, who possessed such deep mathematical insight, could not read the simplest aspects of the quantum relativistic future in its profounder and more subtle meanings, how may unlettered Ishmael hope to read the awful Chaldee of String Theory's future? Neither saint nor prophet, I will do what little endeavor I can to elucidate some possible future directions in Physical Mathematics.

2. What is Physical Mathematics?

I must preface my remarks by describing what I mean by "Physical Mathematics," a term which seems to confuse many people. Here it is best to take an historical and philosophical approach. I hasten to add that I am neither a historian nor a philosopher of science, as will become immediately obvious to any expert, but my impression is that if we look back to the modern era of science then major figures such as Copernicus, Galileo, Kepler, Leibniz, and Newton were neither physicists nor mathematicans. Rather they were Natural

Philosophers. Even during the 18th century and at the turn of the 19th century the same could still be said of the Bernoullis, Euler, Lagrange, and Hamilton. But a real divide between Mathematics and Physics began to open up in the 19th century. For example in volume 2 of *Nature*, from 1870, we read of the following challenge from the pure mathematician J.J. Sylvester:

What is wanting is (like a fourth sphere resting on three others in contact) to build up the ideal pyramid is a discourse on the relation of the two branches (mathematics and physics) to, and their action and reaction upon, one another - a magnificent theme with which it is to be hoped that some future president of Section A will crown the edifice, and make the tetrology .... complete.

James Clerk Maxwell - undoubtedly a physicist - as president of the British Association takes up the challenge in a very interesting address in [292]. He modestly recommends his somewhat-neglected dynamical theory of the electromagnetic field to the mathematical community. According to [136] not many mathematicians paid attention, constituting one of the greatest Missed Opportunities of all time.

That is not to say that first class pure mathematicians of the 19^{th} century were not deeply interested in physics. Riemann and Klein are two outstanding examples. In his address to the very first International Congress of Mathematicians in Zürich in 1897, Henri Poincaré chose as his topic "Sur les rapports de l'analyse pure et de la physique mathématique," ("On the relation of pure analysis to mathematical physics"). He was particularly impressed with Maxwell's achievement: [1]

Comment ce triomphe a-t-il été obtenu?
C'est que Maxwell était profondément imprégné du sentiment de la symétrie mathématique; en aurait-il été de même, si d'autres n'avaient avant lui recherché cette symétrie pour sa beauté propre? [....]
Maxwell en un mot n'était peut-être pas un habile analyste, mais cette habileté n'aurait été pour lui qu'un bagage inutile et gênant. Au contraire il avait au plus haut degré le sens intime des analogies mathématiques. C'est pour cela qu'il a fait de bonne physique mathématique.

How was this triumph attained?
Maxwell succeeded because he had become imbued with the idea of mathematical symmetry. Would he have triumphed so well had others before him not explored this symmetry for its

[1] Poincaré's address is, unfortunately, not available in English. I thank Boris Pioline for his help in understanding the subtleties of Poincaré's prose.

own sake? [...]
Analysis was perhaps not among Maxwell's skills, but to him it would have only been cumbersome and useless baggage. On the contrary, he was gifted with a profound sense of mathematical analogy. This is why he produced good mathematical physics.

These quotations demonstrate that, while the fields of Mathematics and Physics were considered separate, there was still a strong binding between them. Now, the great upheavals in physics in the first quarter of the twentieth century only deepened the relation between Physics and Mathematics. In his stunning 1931 paper (in which he predicted the existence of three new particles - the anti-electron, the anti-proton, and the magnetic monopole) Dirac was both eloquent and exuberant at the very outset [120]:

The steady progress of physics requires for its theoretical formulation a mathematics that gets continually more advanced ... What however was not expected by the scientific workers of the last century was the particular form that the line of advancement of the mathematics would take, namely, it was expected that the mathematics would get more and more complicated, but would rest on a permanent basis of axioms and definitions, while actually the modern physical developments have required a mathematics that continually shifts its foundations and gets more abstract ... It seems likely that this process of increasing abstraction will continue in the future ...

He followed up these prophetic words with great prescience and insight in his 1939 Scott Lecture [121]. He predicted, correctly, that new domains (and even *which* domains) of pure mathematics would need to be incorporated into physics:

Quantum mechanics requires the introduction into physical theory of a vast new domain of pure mathematics - the whole domain connected with non-commutative multiplication. This, coming on top of the introduction of the new geometries by the theory of relativity, indicates a trend which we may expect to continue. We may expect that in the future further big domains of pure mathematics will have to be brought in to deal with the advances in fundamental physics.

Around the same time, Einstein echoed similar sentiments [141]:

Our experience up to date justifies us in feeling sure that in Nature is actualized the ideal of mathematical simplicity. It is my conviction that pure mathematical construction enables us to discover the concepts and the laws connecting them which give us the key to the understanding of the phenomena of Nature. Experience can of course guide us in our choice of serviceable mathematical concepts; it cannot possibly be the source from which

they are derived; experience of course remains the sole criterion of the serviceability of a mathematical construction for physics, but the truly creative principle resides in mathematics. In a certain sense, therefore, I hold it to be true that pure thought is competent to comprehend the real, as the ancients dreamed.

There are many other interesting statements and discussions about my theme, but two in particular will play an important role in the remainder of my tale. In 1960 Wigner waxed philosophical in his famous essay "On the Unreasonable Effectiveness of Mathematics in the Physical Sciences" [395]. In 1972 Freeman Dyson wrote a beautiful essay on the subject, "Missed Opportunities," [136].

In our own time the relation of physics and mathematics is much discussed and debated (and worse) on various blogs and internet sites. One can't help but think of Ovid's Four Ages of Man.

But something happened between the 1930's, the time of the confident statements of Dirac and Einstein, and the time of Dyson's 1972 essay. For in the latter he famously proclaimed:

As a working physicist, I am acutely aware of the fact that the marriage between mathematics and physics, which was so enormously fruitful in past centuries, has recently ended in divorce.

Well, I am happy to report that Mathematics and Physics have remarried, but the new relationship has altered somewhat.

Indeed, at the very time when Dyson was writing his dire announcement, a sea change in our field had begun. Major mathematicians such as Michael Atiyah, Raoul Bott, Graeme Segal, Isidore Singer, and many others, [2] began to take a more serious interest in physics, especially the physics of gauge theories and string theories. At about the same time, major physicists such as Sidney Coleman, David Gross, Edward Witten, and again, many others, [3] started to produce results that called for much greater mathematical sophistication than was needed in the 1940's through the 1960's. It gradually became clear that geometers and mathematically-oriented particle physicists had much to say to one another. The following quote from Raoul Bott's Plenary Lecture at the AAAS meeting in Boston, 1988 captures the spirit of the time [57]:

Although we still often do not understand each other, the push and pull relationship of our two points of view has never been stronger and has invigorated both of us. Certainly in mathematics the physically inspired aspects of the Yang-Mills theory has had a profound effect on our understanding of the structure of 4-manifolds, and I also think we mathematicians are only now learning to appreciate the rich mathematical structure of the Dirac

[2] I have just mentioned the four mathematicians who had the greatest influence on my own development.

[3] Again, I have just mentioned the three physicists who were my principle teachers.

sea – and indeed of the whole Fermion-inspired world of the physicists, as well as their mystical belief in supersymmetry. And on the other hand the most modern achievements of mathematics - from cobordism to index theory and K theory - have by now made their way into some aspects of present day physics - I think to stay.

One thing led to another and, with a great boost from the resurgent interest in string theory, after 40-odd years of a flowering of intellectual endeavor a new field has emerged with its own distinctive character, its own aims and values, its own standards of proof. I like to refer to the subject as "Physical Mathematics."

The use of the term "Physical Mathematics" in contrast to the more traditional "Mathematical Physics" by myself and others is not meant to detract from the venerable subject of Mathematical Physics but rather to delineate a smaller subfield characterized by questions and goals that are often motivated, on the physics side, by quantum gravity, string theory, and supersymmetry, (and more recently by the notion of topological phases in condensed matter physics), and, on the mathematics side, often involve deep relations to infinite-dimensional Lie algebras (and groups), topology, geometry, and even analytic number theory, in addition to the more traditional relations of physics to algebra, group theory, and analysis. To repeat, one of the guiding principles is the goal of understanding the ultimate foundations of physics. Following the lessons of history, as so beautifully stressed by Dirac, we may reasonably expect this to lead to important new insights in mathematics. But - and here is the central point of this essay - it is also true that getting there is more than half the fun: If a physical insight leads to a significant new result in mathematics, that is considered a success. It is a success just as profound and notable as an experimental confirmation from a laboratory of a theoretical prediction of a peak or trough. For example, the discovery of a new and powerful invariant of four-dimensional manifolds is a vindication just as satisfying as the discovery of a new particle. I do not pretend to know the true locus of mathematical reality, but to me such a discovery uncovers an element of truth about our "real world." But this is as far as I dare venture into the treacherous domain of epistomology.

3. String-Math vs. Strings Meetings

The previous section concluded with what might be considered as a creed for Physical Mathematics. I realize that many physicists will regard the above creed as anathema. This manifesto would have been more enthusiastically embraced as a summary talk in Edmonton, rather than in Princeton, and this highlights a problem.

The annual Strings meeting has evolved from an annual meeting on GUTs to Strings 1995, and ever since they have been the premier annual meeting on string theory. But recently, many leading practitioners of Physical Mathematics have seen the need for a separate, more mathematically-oriented conference [373]. The resulting String-Math conferences beginning at Penn in 2011 have been a roaring success. Still the creation of an annual String-Math alternative to the Strings conferences should give us pause. Is this a

sign of another impending divorce? I hope not. I hope it is a sign of progress and ever-burgeoning diversity, rather than divorce. Still, we might worry that a parting of the ways inevitable? To quote Robert Oppenheimer [332]:

This is a world in which each of us, knowing his limitations, knowing the evils of superficiality and the terrors of fatigue, will have to cling to what is close to him, to what he knows, to what he can do, to his friends and his tradition and his love, lest he be dissolved in a universal confusion and know nothing and love nothing.

There is room for hope: What Oppenheimer does not take into account here are the great syntheses of currents of thoughts into more concise and powerful unifying principles. We need not understand all the intricate details of Leydan jars and the manifold instruments in Faraday's laboratory, thanks to Maxwell's equations. We must strive for these syntheses in the ever-broadening subject of physics related to string theory and quantum field theory. But this cannot happen unless we keep the lines of communication between the two disciplines open. Let me play the role of marriage counselor here: Each field should see something to love and respect in the other! I hope that a substantial number of people will continue to go to *both* Strings and String-Math conferences.

I turn now to describing what I believe will be some of the future adventures in Physical Mathematics. [4] In preparing the following list of problems I have consulted many people for their opinions and received some very useful suggestions. I have also been quite struck by the episodic and fashion-driven nature of our field. It is good to bear in mind Dyson's "Unfashionable Pursuits" [137], and not lose sight of important problems that have been set aside, temporarily.

4. Geometry, Symmetry, and Physics

In the 1980's H. Garland, I. Frenkel, and G. Zuckerman established a wonderful weekly seminar series at Yale that they aptly called the "Geometry, Symmetry, Physics Seminar." If we broadly interpret "geometry" to encompass both geometry and topology (especially low-dimensional topology, where "low" means dimension less than 12, or so), and we likewise broadly interpret "symmetry" to encompass algebraic and categorical structures, in addition to group theory and Lie algebra theory, then their title beautifully encompasses some of the deepest themes in the Math-Physics dialogue. Of course, these seminars were not concerned with events only in New Haven but represented fundamental changes occurring in Austin, Berkeley, Boston, Cambridge, Chicago, Leningrad, Los Angeles, Moscow, Paris, Oxford, Princeton, Santa Barbara, Utrecht, and many other places.

[4] In a parallel "vision talk" at Strings 2014 Andy Strominger presented a list of open problems that he had solicited from a collection of string theorists. The resulting list can be found at http://physics.princeton.edu/strings2014/slides/Strominger.pdf and presents a very interesting cross-section of current (as of June 27, 2014) opinions in the string theory community of what are the most important and pressing questions facing the field. The list has some overlap with the problems I will discuss, but has a distinctly different orientation.

The most spectacular successes and interactions with math have been in the areas of algebra, geometry, and topology. A hallmark of the subject is the astonishing converse of Wigner's title: The unreasonable effectiveness of physics in the mathematical sciences [30, 306]. The subject is dauntingly vast. I will limit my comments to just a few possible future paths.

4.1 Duality Symmetries and BPS states

The dualities such as homological mirror symmetry and strong weak electric-magnetic dualities have made spectacular predictions in the interaction between physics and geometry. See, for examples, [29, 30]. I expect this to continue into the future.

4.1.1 BPS states

The problem of finding the BPS spectrum of a field theory or string theory is a good touchstone of our understanding of the subject. While the particular number of BPS states of a particular charge is not of special interest, the *techniques* that must be developed to find the spectrum, the generating functions for the spectrum, and the way the spectrum changes, are all of great interest.

There has been a lot of progress on this problem for d=4 N=2 *field theories*. We now have many powerful techniques for finding the spectrum in a large class of examples [18, 19, 86, 185, 187, 188, 191]. Nevertheless, we should bear in mind that these techniques fall far short of giving a general prescription for finding the BPS spectrum of an arbitrary d=4 N=2 theory (in fact, we don't even know the Seiberg-Witten curve for the general d=4 N=2 theory!). Even for class S, where there is a simple way of writing the SW curve, the method of spectral networks has yet to be generalized to all ADE (2,0) theories and general surface defects. Similarly, much progress has also been made for the BPS spectrum in "compactification" of type II theories on toric Calabi-Yau manifolds. (For a recent sample reference see [86].) Nevertheless, in spite of a great deal of effort there is no algorithmic tool available finding the BPS spectrum of a single compactification of type II string theory on any compact Calabi-Yau, and hence the enduring and difficult open problem remains:

Develop tools to compute the BPS spectrum of d=4 N=2 compactifications of type II string theory on compact Calabi-Yau manifolds.

What is often meant by the BPS "spectrum" is a list of indices, such as the second helicity supertrace $\Omega(\gamma)$ in physics or the Donaldson-Thomas invariants in math. However, for many purposes, we would like to know more about the actual Hilbert spaces of BPS states. We might wish to formulate these as cohomologies of chain complexes. Understanding the BPS spectrum at this deeper level is sometimes referred to as "categorification" of the spectrum of indices. Some mathematical work on this has been done recently in [62, 254]. It would be good to have a physical translation of these highly mathematical works. [5] One natural question to ask in this program is:

[5] Perhaps a good place to start would be to answer a question posed to me by D. Joyce: *What is the physical interpretation of the orientation data of Kontsevich and Soibelman [262]?*

Categorify the Kontsevich-Soibelman wall-crossing formula [262], together with its 2d4d extension [185] for d=4 N=2 field theories, using the physics of BPS states, line defects, and surface defects.

With the recent successful categorification [190] of the 2d Cecotti-Vafa wall-crossing formula [77] I believe this problem could be solved in the near future. [6] Nevertheless, the solution itself opens up new questions. Interactions of BPS states in d=2 N=(2,2) massive theories turn out to have a rich algebraic structure encoded in L_∞ and A_∞ algebras and so we must ask:

Are there analogous algebraic structures on the spaces of BPS states in higher dimensions, in particular in d=4 N=2 theories ?

The idea that there are interesting algebraic structures on the set of BPS states is an old dream, which has never been concretely realized except in some special cases [221]. This was motivated by the still-mysterious fact that certain counting functions of BPS states are closely related to denominator products of Borcherds algebras [113, 220]. One recent concrete proposal has been investigated in detail by Kontsevich and Soibelman [264], but their methods and results have not been carefully related to the actual properties BPS wavefunctions of quiver quantum mechanics. Thus, a relatively straightforward and concrete problem, which should not take too long to solve, would be:

Explain in detail the relation of the cohomological Hall algebras of Kontsevich and Soibelman to the BPS states of quiver quantum mechanics. Does it define an algebraic structure on the space of half-BPS states of d=4 N=2 theories ?

4.1.2 Topological string theory

A source of an enormous amount of work in Physical Mathematics is topological string theory, first defined in [403, 406]. Topological string theory is often cited as an extremely simplified version of string theory. If we are ever to have any success with the question: "What is string theory" we should first be able to answer the analogous question for topological string theory. This means we should understand the perturbative theory completely, as well as its nonperturbative completions and the spacetime physics it describes.

In the effort to understand topological string theory much attention has focused on the topological string partition function. In the A-model this is defined, as a formal (possibly asymptotic) series by a collection of functions F_g on Calabi-Yau moduli space, which are themselves generating functions for Gromov-Witten invariants of genus g curves. This topological string partition function has been the focus of intense research. The famous work of Bershadsky, Cecotti, Ooguri, and Vafa [49] almost provides a recursive definition but there is a crucial "holomorphic ambiguity," which has only been fixed up to $g \leq 51$ in some cases [240]. Thus, another enduring problem is:

[6] Full disclosure: I am discussing this problem with T. Dimofte and D. Gaiotto and have a related project with D. Galakhov and P. Longhi. But the full solution is not yet known.

Define topological string theory on a compact Calabi-Yau manifold nonperturbatively, and use it to compute the higher genus Gromov-Witten invariants for all genera.

The computation of F_g for all g has essentially been solved by using techniques adapted from the theory of matrix models for the B-model [60] and by using the topological vertex for the A-model in the case of non-compact Calabi-Yau manifolds. In the case of some toric Calabi-Yau manifolds a nonperturbative definition of topological string theory can in fact be given using matrix models [5], but even in these cases the uniqueness of the nonperturbative extension is not clear. See [288] for a review of the relevant issues. The case of compact Calabi-Yau threefolds seems much more difficult and there are no examples where we can compute all the F_g, let alone give a nonperturbative definition.

One approach to the above problems, trying to quantize carefully the Kodaira-Spencer theory of gravity of BCOV has been pursued by Costello, and this might well lead to interesting progress [93]. Another extremely interesting attempt to define nonperturbative topological string theory uses the physics of supersymmetric black holes [331]. The most thorough attempt to formulate and prove a sharp version of the OSV conjecture was made in [104], but a completion of the project requires the solution of a number of unsolved problems addressed in the conclusion of [104]. One very interesting mathematical prediction that emerged from this and closely related papers [100, 175, 176] is the following:

Fix a very ample class P in a compact Calabi-Yau 3-fold. Define a (mock?) modular form for a congruence subgroup of $SL(2,\mathbb{Z})$ associated to generating functions of DT invariants for boundstates of $D4D2D0$ branes with $D4$ charge P.

Some recent mathematical progress on this question has been made in [194, 195, 380, 381]. More broadly, modular properties associated to BPS degeneracies of d=4 N=4 black holes have been deeply investigated and understood. See [95] for a recent reference. For reasons explained in [281, 282] we also expect mock modular forms to play a role in the counting of d=4, N=2 black holes, so the important problem remains:

Investigate the (mock) modularity properties of generating functions of BPS degeneracies of black holes in d=4 N=2 supergravity.

4.1.3 3-manifolds, 3d mirror symmetry, and symplectic duality

We have learned many things about the category of boundary conditions in two-dimensional supersymmetric theories preserving supersymmetry. This leads to the theory of supersymmetric cycles (in math, the theory of calibrated submanifolds), the derived category of coherent sheaves, and homological mirror symmetry. Gaiotto and Witten investigated the half-supersymmetric boundary conditions for N=4 SYM and found a rich set of possibilities [177, 178, 179]. It is not known if their classification is complete, and this would be a good point to settle. More generally,

Extend the Gaiotto-Witten classification of supersymmetric boundary conditions to d=4 N=2 theories and to d=3 theories with extended supersymmetry.

As I have learned from T. Dimofte, classifying the category of supersymmetric boundary conditions in twisted d=3 N=4 theories (or at least for some clearly defined class of d=3 N=4 theories) should be the key to understanding "symplectic duality," a mathematical version of the physicist's "three-dimensional mirror symmetry" [241].

4.1.4 Knot theory

For over 30 years now knot theory has been the source of much interesting Physical Mathematics. [7] It continues to provide problems and inspire progress. At present the frontier concerns definitions of knot homologies, i.e., chain complexes whose homology groups are invariants of knots in three dimensions and whose Euler characters define knot polynomials such as those associated to three-dimensional Chern-Simons theory.

There are now many approaches both mathematical and physical and so an obvious problem is

Provide a unified viewpoint on the approaches to knot homology of Khovanov-Rozansky [253], Cautis-Kamnitzer [74], Seidel-Smith [359], Kronheimer-Mrowka [266], Webster [389], Gukov-Schwarz-Vafa [214], and Witten [421].

Of these various approaches, the most concrete and understandable to a physicist seems to be the gauge-theoretic approach of Witten. It has been further developed in [189], but yet further development is necessary to make the theory more computable, and progress here might ensue from progress on the problem:

Investigate the geometry and topology of the moduli spaces of solutions of the Haydys-Witten, the Kapustin-Witten, and the generalized Bogomolnyi equations.

In fact, all of these equations are specializations of the $Spin(7)$ instanton equations [84, 189, 226], and hence the above problem is closely related to those in the next subsection.

Much of the work on the categorification of knot polynomials has been limited to knots in S^3, but an obvious question is whether there is an analogous categorification of knot invariants in other three-manifolds. In general, the knot polynomials no longer have integer coefficients so it is not at all obvious what integers one wants to categorify. It is quite possible that physics can provide some very useful guidance here. A relatively straightforward problem is therefore

Generalize the formulation of effective Landau-Ginzburg theories of [189] to formulate knot homologies for knots in three-folds of the form $C \times S^1$ where C is a general Riemann surface.

The answer to this problem will surely involve the physics of d=4 N=2 theories of class S where C plays the role of the UV curve and the knots are defined by surface defects.

[7] Ironically, the mathematical theory of knots has its origins in physics, in the grand unified theory of the late 19th century, as championed by Kelvin and Tait. See ch. 2 of [265] for an account.

Finally, we know that the modular tensor categories associated with knot polynomials have had interesting applications to condensed matter physics and possibly to topologically protected quantum computation. It is natural to wonder whether there will be analogous applications of knot homologies to condensed matter theory and quantum computation. A crucial technical problem that must be overcome before such applications can be contemplated is:

Can one put a natural unitary structure on the knot homology groups?

4.1.5 Special holonomy in six, seven, and eight dimensions

There are many indications that there is a useful "three-tiered" field theoretic structure associated to gauge theories on 6,7,8 dimensional manifolds with holonomy groups $SU(3), G_2, Spin(7)$, respectively [126, 127]. There are also interesting analogues of form theories of gravity, based on [229, 230], that were proposed as a 7-dimensional "topological M-theory" analogue of topological string theory [115]. Moreover, analogously to the way the Yukawa couplings of heterotic theory or the prepotential of type II strings on Calabi-Yau "count" holomorphic curves in Calabi-Yau 3-folds [72, 118, 119], the superpotential for M-theory compactifications on G_2 manfiolds "counts" associative 3-folds [222]. All of this hints at a beautiful generalization of open-closed topological string theory with an M-theory analog in 6,7, and 8 dimensions. Mathematicians such as Donaldson, Joyce, Tian and their students have been making steady progress on this picture. The technical problems they are facing are immense. There are only constructions of G_2 metrics and G_2 instantons near various degenerations. There is no *known* analogue of the powerful tools of algebraic geometry. The moduli spaces of G_2 metrics and instantons are unknown and unexplored. The instanton and supersymmetric cycle conditions are known to be critical points of natural "Morse" functions, but technical issues related to noncompactness and bubbling are important when trying to define enumerative invariants. Sometimes physics introduces new degrees of freedom or new fields that help with such noncompactness problems. It is also possible that conformal field theory techniques could usefully be applied [364], or, perhaps, it might be useful to embed the equations in a broader M-theory context, as used, for example in [421]. More generally, we can ask,

Can physics help complete this program?

Are there useful and computable generating functions counting supersymmetric cycles in G_2 and $Spin(7)$ manifolds defined by holomorphic quantities in low energy effective actions associated with compactification on G_2 and $Spin(7)$ manifolds ?

Are there analogues of homological mirror symmetry and topological string theory for target spaces of G_2 and $Spin(7)$ holonomy?

4.2 Hyperkähler and Quaternionic-Kähler geometry

4.2.1 Construction of explicit hyperkähler metrics

In [180] a new construction of hyperkähler on certain classes of manifolds using twistor methods and a version of the Thermodynamic Bethe Ansatz was proposed. The manifolds in question are moduli spaces of vacua of circle compactifications of Seiberg-Witten theories on the Coulomb branch. For theories of class S they are moduli spaces of solutions to Hitchen equations. In [185] the methods were extended to produce actual solutions of the Hitchin systems themselves.

Can these methods be used to describe analytic formulae for hyperkähler metrics, even in the simplest new cases, such as ALG metrics?

It is possible that the methods apply to non-field theories such as the theories on probe M5-branes wrapping an elliptic curve of an elliptically fibered K3-manifold. (The physical justification of the existence of such extensions of field theory is unclear. See below.)

Can the twistor-TBA method be extended to produce an explicit smooth hyperkähler metrics on K3 surfaces ?

4.2.2 Construction of explicit quaternion-Kähler metrics

A close analogue of hyperkähler manifolds are quaternionic Kähler manifolds with holonomy in $(Sp(n) \times SU(2))/\mathbb{Z}_2$. They show up naturally as metrics determining the low energy effective actions of hypermultiplets in type II compactification on Calabi-Yau manifolds.

In [12, 13, 16] a program has been carried out that attempts to apply the twistor-TBA construction of hyperkähler metrics to the construction of quaternion Kähler metrics appearing in supergravity. Much of the discussion is formally the same. For example, one replaces Hitchin's theorem on the twistor formulation of hyperkähler metrics with LeBrun's theorem on the relation of quaternion Kähler metrics with holomorphic contact structures. There is, however, one very significant difference: The BPS degeneracies that enter into the TBA grow exponentially with the square of the charge, so the TBA integral equations are merely formal series. (These formal series still have much content and one can prove nontrivial things about them [15].) It is strongly suspected that this difficulty is a reflection of the need to include NS5-brane instanton effects [339]. Thus, the outstanding problem here is:

Show how to include NS5-brane instanton effects in the construction of quaternion-Kähler metrics associated with string compactification. Does this cure the problems with the twistor-TBA approach?

4.2.3 Monopole moduli spaces

Even well-investigated subjects like the moduli spaces of monopoles continue to yield beautiful new results: The relation of S-duality to the geometric Langlands program [246], the gauge-theoretic approach to knot homology [189, 421], the study of framed BPS states [52, 184], and recent studies of d=4, N=2, quiver gauge theories [327][328] all motivate a renewed study of singular monopoles. This raises many interesting questions about monopole moduli spaces. Here is one (which should not be too hard):

The moduli space of $SU(2)$ monopoles is isomorphic to the space of rational maps on $\mathbb{P}^1$ [26]. Generalize this picture to singular monopoles for arbitrary simple group and arbitrary 't Hooft defect.

The case of nonsingular monopoles was settled long ago [244].

The progress on wall-crossing in d=4 N=2 field theories clearly indicates that there is a rich theory of the zero-modes of certain Dirac-like operators on these noncompact spaces of which we only have glimpses of at present [52, 369, 382]. This raises a larger question: Unlike the case of general elliptic operators on compact manifolds there is no general index theorem for Dirac (or elliptic) operators on noncompact manifolds. For special classes of boundary conditions some very nice theorems exist [23, 56, 66, 71, 368] but there seems to be no broadly applicable useful theorem for the physicists to use. The boundary conditions of Atiyah, Patodi, and Singer are not always the appropriate ones to use. Moreover, it can even happen that the physicists need to discuss the dimensions of L^2 kernels of Dirac operators that are *not* Fredholm [190, 360, 390]! Special techniques can be applied in a case by case setting, but frustratingly, there is no general formula like Atiyah and Singer's:

Develop a general theory for the L^2-index of elliptic operators on noncompact spaces.

4.2.4 Line and surface defects and the geometry of (quantum) Hitchin moduli spaces

Many interesting questions circle around the categories of line and surface defects in class S theories, and these are closely related to the (quantum) geometry of the moduli space of Hitchin systems. As one example we cite Question 1.5 of [379]. It would be interesting to relate this to the category of line defects of class S theories, together with an exploration of the generalizations to higher rank systems. Moreover, the OPE's of line defects are expected to provide a categorification of the Skein algebra [184]. The quantum algebra of line defects is closely related to the quantization of Hitchin moduli spaces as cluster varieties, as discussed in the work of Fock and Goncharov [153], and should be closely related to the work of Nekrasov et. al. [322, 323, 324, 325, 326, 327, 328]. The elegant paper [424] gives a unified approach to understanding the deformation quantization of the algebra of holomorphic functions on Hitchin moduli spaces via line defects, on the one hand [184, 242], and the quantum integrable systems associated with Hitchin moduli spaces. The common theme is a deformed version of Rozansky-Witten theory. This seems to be a promising observation and deserves further exploration.

Similarly, the theory of branes (of all types (B,B,B), (A,B,A), etc. [246, 325]) are related to a host of interesting questions. Many of these make contact with Geometric Representation Theory. Among the many questions here we would like to call attention to one in particular. Some hyperkähler manifolds have a very interesting canonical hyperholomorphic line bundle [14, 225, 233, 320]. It has recently proven useful in some physical discussions [17]. Since the hyperholomorphic connection is so canonical one would imagine it should play an important role in physical considerations. A physical interpretation has been suggested in [106, 320] but a full understanding has yet to emerge.

4.3 Four-manifolds

Donaldson theory [124], it's interpretation in terms of topologically twisted N=2 SU(2) SYM [402], and its solution using the Seiberg-Witten breakthrough [125, 277, 305, 410] has been a wonderfully successful chapter in the development of Physical Mathematics.

The resounding success has given some physicists the impression that four-manifold theory has been "solved," but this is very far from being the case. Note that when $b_2^+ - b_1$ is even all the Seiberg-Witten invariants vanish. Can physics really be blind to "half" the world of four-manifolds? Indeed, there are plenty of remaining questions in four-manifold theory. For samples see the end of [125] or [151]. To choose one example, Fintushel and Stern have constructed families of 4-manifolds all of which are homeomorphic, all of which have the same Seiberg-Witten invariants, but are nevertheless strongly suspected to be non-diffeomorphic [152]. Another example of this situation are the Horikawa surfaces [31]. After some initial optimism following the introduction of Seiberg-Witten invariants mathematicians have realized that the classification of diffeomorphism types of simply-connected four-manifolds is far more complex than anyone had imagined. Will lightning strike twice? There are two natural ways in which physics could provide new invariants. One is a hint from the older work [285, 286, 287]:

Do general theories of class S lead to new four-manifold invariants, or new relations on the known four-manifold invariants? In particular, are there new 4-manifold invariants associated with correlators of topologically twisted superconformal theories?

A second line of attack [8] is to consider the categorification of Vafa-Witten invariants. Nontrivial issues of noncompactness of the moduli space of solutions to the Vafa-Witten equations need to be resolved before one can make progress on this idea.

In addition, a very interesting problem posed by Donaldson in [125] is to consider invariants for *families* of four-manifolds. The invariants would be cohomology classes on $BDiff(X)$ where X is the four-manifold. As explained at the end of [305] this can be approached from physics by considering the topologically twisted N=2 theory coupled to topologically twisted N=2 supergravity. Then $Qg_{\mu\nu} = \psi_{\mu\nu}$ and one naturally produces differential forms on the space of metrics from the path integral. Recent progress [134, 149] in coupling field theories to background Euclidean signature supergravities might be very useful in carrying out the details of this idea.

[8] This is based on discussions with E. Witten.

Finally, I have to ask. Donaldson famously showed that there are exotic differential structures on $\mathbb{R}^4$. Fintushel and Stern have shown that many simply connected compact four-manifolds have infinitely many smooth structures. So:

Do exotic differential structures on $\mathbb{R}^4$ play any role in physics?

The circumstantial evidence thus far is negative. There have certainly been many attempts to find applications of these structures in the past, but nothing compelling has emerged. Moreover, there have been ample opportunities for exotic differentiable structures on S^7 and S^{11} to play an important role in 11-dimensional supergravity, but again nothing compelling has emerged. Yet.

4.4 Geometric Representation Theory

Geometric Representation Theory (GRT) is the study of how geometry illuminates representation theory and vice versa. A paradigmatic example of a result in this field is the Borel-Weil-Bott theorem, identifying the irreducible representations of a compact Lie group G with the space of holomorphic sections of certain holomorphic line bundles over the flag manifold G/T (in a suitable complex structure). [9] The first example of that theorem is the statement that the Landau levels of an electron confined to a sphere surrounding a charge m monopole transform in an irreducible representation of $SU(2)$ of dimension $|m|+1$, and is widely used in physics in the context of the coherent state formalism [337]. Interesting special functions of mathematical physics (such as Legendre functions in the above example) naturally arise from this viewpoint, and the actions of certain differential operators on these functions is easily understood using group theory. This is a precursor to the theory of "D-modules."

All of this has an enormous generalization, with rich connections to physics. It is not possible to do it justice in a few short paragraphs, and I believe the time is ripe for a comprehensive text reviewing the many advances of the past twenty years. See [85, 197, 198, 316, 317, 318, 319] for some introductions. Typically, one studies cohomology or K-theory (equivariant, quantum,...) or derived categories, or D-modules, or .... of some natural varieties associated with groups, such as flag manifolds, (affine) Grassmannians, orbits of nilpotent elements and their Springer resolutions, Slodowy slices,... (which themselves often arise as moduli spaces of interesting equations such as monopole equations, Nahm equations, Hitchin equations, etc.). One finds rich algebraic structures associated with these cohomologies or categories. Remarkably these often have interesting physical interpretations.

For physicists unfamiliar with this subject the following simple observation should wet their appetites: Consider the solutions of the matrix equation $m^2 = 0$ where m is a 2×2 complex traceless matrix. From the Jordan form theorem it is easy to see that the solutions are of the form

$$m = \begin{pmatrix} x & y \\ z & -x \end{pmatrix} \tag{4.1}$$

[9] See [58] for a nice introduction.

where $x^2 + yz = 0$. But the equation $x^2 + yz = 0$ is just the famous A_1 singularity which, together with its Eguchi-Hanson hyperkähler resolution, plays such a wide variety of roles in string theory. On the other hand, the set of matrices m we have described is the (closure of) the nilpotent orbit inside $sl(2,\mathbb{C})$. This very old observation, and its natural generalizations, is an entry point for some of the interesting math referred to above.

We just highlight a few of the many connections between Physical Mathematics and GRT:

1. One strong impetus for the development of GRT is the Geometric Langlands Program (GLP). It should be no surprise, then, that the gauge theoretic interpretation of the GLP has led to a physical interpretation of many aspects of the theory. In a series of papers by Witten and collaborators one will find beautiful expository discussions of many mathematical constructions (e.g. Hecke operators, Kazhdan-Lusztig polynomials, braid group actions of the Weyl group, geometric Satake theorem, ...) used in GRT, all motivated by physical questions [161, 177, 178, 179, 215, 216, 217, 246, 419, 420].

2. Part of the argument relating d=4, N=4 SYM to the GLP involves compactification on a Riemann surface Σ, since the low energy theory that results is a sigma model with target space a Hitchin moduli space [50, 219]. Therefore, the Kapustin-Witten interpretation of the GLP can be viewed as a statement about the $(2,0)$ theory compactified on the four-manifold $T^2 \times \Sigma$. An obvious generalization then is to consider the appropriately twisted $(2,0)$ theory on $C \times \Sigma$. Class S theories defined by C have S-dualities related to the different pants decompositions of C [181], so it is natural to ask how the statement of the Geometric Langlands Program can be generalized to this broader set of theories. More generally, we could pose the problem: *Generalize the statement of the Geometric Langlands Correspondence to a duality following from compactification of the* $(2,0)$ *theory on more general four-manifolds.* It might be that one should restrict attention to four-manifolds which are products of surfaces, but with the appropriate language, perhaps relating different decompositions of four-manifolds, one could dream that there is an analog for all four-manifolds.

3. A closely related subject is the relation of representation theory to the geometry of quiver varieties, in particular Nakajima's results on q-deformed affine algebras and the equivariant K-theory of quiver varieties [317, 318]. Of particular interest to physicists is the special case of the moduli spaces of instantons on ALE spaces. There have been many attempts to interpret Nakajima's results on the algebras associated with the homology of these quiver moduli spaces in terms of quantum field theory and string theory. These include [130, 221, 384, 385]. None has been entirely satisfactory. The attempt [221] did introduce a general idea of an "algebra of BPS states," a concept based just on the BPS bound and general principles of S-matrix theory. Conjecturally, it is related to algebras associated with correspondence varieties such as those used in the definition of Hecke algebras, Ringel-Hall algebras, etc. Thus, the "algebra of BPS

states" is more of a general proposal than a concrete result. (Contrary to what is stated in [264], the general idea is not a failed attempt. In fact it was successfully and concretely carried out in a number of examples.) As mentioned above, ideas about cohomological Hall algebras are clearly very closely related to the algebra of BPS states in the context of quiver quantum mechanics and an interesting open problem is to explore the relation between the two notions. Recently, mathematicians have been exploring a categorified version of Nakajima's construction [74, 75]. It does not appear that there is an available physical interpretation of these constructions yet, but all the right elements are available, and it should be very interesting to provide such an interpretation.

4. Another closely related topic is the AGT conjecture and the role of W-algebra symmetry in the geometry of certain instanton moduli spaces [64, 65]. Indeed, some aspects of AGT were anticipated in the math literature exactly through these kinds of considerations. It is natural to ask whether the recent progress in deriving the AGT conjecture [89, 90, 325] can be used to rederive these results and extend them to other instanton moduli spaces.

5. Again, closely related, is the role quantum affine algebras play in considering "instanton partition functions" associated with 4,5, and 6-dimensional gauge theories [327, 328]. From a mathematical viewpoint one begins with certain generating functions for equivariant integrals of characterstic classes of certain "matter sheaves" over framed instanton moduli spaces of instantons on $\mathbb{C}P^2$. Taking appropriate limits of these generating functions produces functions satisfying a host of nontrivial *difference* (as opposed to differential) equations which can be neatly expressed using the characters of representations of quantum affine algebras. They can be considered as difference-equation generalizations of the equations for Seiberg-Witten curves, on the one hand, and as versions of the TQ equations of integrable lattice models on the other. Quantum groups were initially viewed as completely ungeometrical, and hence (to some) rather distasteful. The papers [318, 317, 327, 328] (which build on a large body of related work) show that quantum groups do arise naturally as "symmetries" of natural cohomology problems on instanton moduli spaces. These works raise two obvious questions: First, what can we say about instanton moduli spaces on other four-manifolds? Second, the physics underlying the approach of [328] is that of the D5 brane in type IIB supergravity. Can we learn anything from the S-duality to the NS5 brane (and hence the $(2,0)$ theory)?

6. The classification of surface defects [215] in four-dimensional N=4 supersymmetric Yang-Mills is closely related to the work [51]. It would be natural to try to use these aspects of GRT to try to classify surface defects in class S theories. Related to this is the classification of codimension two defects in the six-dimensional (2,0) theories. The paper [78] addresses this question, and makes constact with some rather sophisticated aspects of the theory of nilpotent orbits.

Clearly, the subject of Geometric Representation Theory is vast and its connection to physics is profound. The plethora of results and constructions has become somewhat bewildering, and it would be quite interesting to see if the physical interpretations using the concepts of supersymmetric field theories, branes and homological mirror symmetry, and field theory dualities, could be turned around as organizing themes to give a *systematic* account of the entire subject of Geometric Representation Theory.

4.5 Generalizing Geometry

Once upon a millenium, "geometry" meant the Euclidean geometry of two and three dimensions. All that began to change in the 19th century, and new "geometries" have been explored ever since. One of the main insights of 20th century physics is the deeply geometrical aspect of fundamental physical laws. This is obvious in general relativity, but now modern particle physics is built upon the study of connections on fiber bundles, and hence all the modern fundamental theories of nature are geometrical. Even crucial results in quantum mechanics, like Wigner's theorem and adiabatic evolution are, when properly viewed, geometrical.

Attempts to understand string theory have led to many connections with geometry, as is clear from many other sections in this essay. However, string theory has encouraged a whole host of explicit generalizations of geometry. One of these, the "generalized geometry" explored by Hitchin and collaborators [232] is closely related to T-duality. It is based in part on special algebraic structures one can associate with the sum of cotangent and tangent bundles of a manifold. These structures, such as "generalized complex structures" in fact arise very naturally in the study of supersymmetric sigma models and have had applications in the interactions between Geometric Representation Theory and physics, as described above. Moreover, they have been extremely effective in solving the equations for supersymmetric backgrounds of 10- and 11-dimensional supergravity theories. See, for example, [205].

While the generalized geometries inspired by T-duality have clearly been very successful, there have been a number of attempts to generalize geometry in "stringy" ways with a large assortment of "non-geometrical" compactifications. Typically, this involves nontrivial families of conformal field theories with "transition functions" making use of nontrivial automorphisms of the conformal field theory. Among other things this has led to the subject of "double field theory," an attempt to make the T-duality symmetry of string field theory on tori completely manifest [234]. It is too early to say whether this subject will develop much further. One obvious question is whether it can be generalized to other U-duality symmetries and other string compactifications.

Another very intriguing development within the theme of string-theory inspired generalizations of geometry is the notion of geometries associated with higher spin particles. From a particle-physics viewpoint, general relativity is all about the classical field theory of a massless spin-two particle [391]. Since string theory predicts an interacting theory of many higher-spin particles it is natural to ask what the analogous geometrical theory of higher spin particles would be. This line of thought leads to the things like Vassiliev theory, a very popular subject of recent years. The line of thought also leads to the

old idea that the higher spin particles of string theory correspond, in some sense, to a tower of Goldstone bosons for some grand underlying symmetry, most of which is spontaneously broken by the background spacetimes we typically study. See, for examples [199, 200, 211, 272, 302, 303, 304, 401, 407] for some attempts to make that precise. (This is just a small sampling of papers attempting to make the idea more precise.) An important problem for the future is to make more direct contact of Vassiliev theory with string theory. For recent progress see [170].

Perhaps a good model to keep in mind when thinking about these issues is the "higher Teichmuller theory" of Fock and Goncharov and its relation to the "geometry associated to W-algebras." Even here, further clarification about just how the algebraic geometry of moduli spaces of complex flat connections is related to conformal field theory with W-symmetry would be most welcome.

4.6 Topology

4.6.1 Anomaly cancellation

There is a meta-theorem that states that all anomalies in any theory must cancel - *when that theory is mathematically consistent*! Historically, models of nature have been proposed that turned out to be anomalous. Moreover, the cancellation of anomalies has played an important role in the development of Physical Mathematics. It led to Wess-Zumino terms, the Green-Schwarz anomaly cancellation, and more generally to the notion that an effective action in a field theory should be a section of a line bundle, or, better, an invertible topological field theory. There are some very subtle topological questions, which remain to be answered, before we can be sure that there is no lurking inconsistency in our understanding of M-theory and string theory. In recent years S. Monnier has been making useful progress on questions concerning global anomalies, applying the newly available techniques of differential cohomology [296, 297]. We would like effective anomaly cancellation conditions - including all global anomalies - for the "general string background." Even defining a "general string background" is problematic, since people keep finding new constructions. But in type II theories it should definitely include arbitrary consistent configurations of branes (both D-branes and solitonic branes) with their excitations, arbitrary fluxes, and even arbitrary excitations of higher string modes. Moreover, one would like to include U-folds, T-folds, and the like. Since it is too difficult at the present moment to specify such a general background, we could usefully narrow down the to general type II orientifold backgrounds as defined in [122]:

Give a complete set of rules for complete cancellation of all anomalies, global and local, for the general D-brane configuration in the general type II orientifold background with RR fluxes.

Once the anomalies are cancelled we still need to know the background charge of orientifolds in order to construct consistent models that satisfy tadpole cancellation and hence we have the open problem:

Derive a useful formula for the RR charges of the general type II orientifold background as elements of twisted equivariant K-theory.

I should note that there is some controversy about the proper formula for the RR charge of orientifold planes, even at the level of rational cohomology. The authors of [122] derived a K-theoretic formula that generalizes a formula for the type I string derived by Freed and Hopkins [154]. The formula for the charge is defined as a Poincaré dual of a certain linear function on a twisted equivariant K-theory group (this follows from equation 7 of [154]) and hence is somewhat abstract and not very user-friendly for most string theorists. It is possible to give a more concrete formula in terms of the topology of the "orientifold planes" and their embedding in spacetime, but only after inverting the prime 2. But, especially for orientifolds, 2 is oddest and most important prime of all! It is probably not possible to localize the RR charge on the orientifold planes when 2 has not been inverted. There are many things to clean up in this subject. Aside from mathematical rectitude, a full discussion would probably prove useful to the ever-more-sophisticated efforts of string phenomenologists.

4.6.2 K,M,S

The Dirac quantization conditions on fluxes and branes in string theory and M-theory are associated with different generalized cohomology theories. In M-theory we use differential cohomology (based on classical singular cohomology) to model the C-field. On the other hand, in type II string theory we use differential K-theory to model the RR fields and currents. Moreover, the role of the B-field of type II string theory is generally understood to lead to a differential twisting of those differential K-theories. These facts lead to two questions, now well over 15 years old:

How are the two different Dirac quantizations compatible with the duality between M-theory and type IIA string theory?

How is the very different role for the RR fields and the NS B-field in the Dirac quantization law for type II RR fields compatible with S-duality?

One might hope that the different Dirac quantization laws apply in nonoverlapping regimes of validity. This is not the case, and the current understanding of the resolution of the tension is rather nontrivial, and somewhat technical [109, 110]. In my view, the current understanding is minimal.

Things are worse when we consider the tension between the S-duality of type IIB string theory and the role of the B-field in defining a twisting of K-theory. The sharpest tension I am aware of appears when we consider the case of orientifolds [416]. In a similar way, in F-theory when there are several kinds of (p, q) branes in play, the role of K-theory

in formulating the RR fields is not obviously correct. I am not aware of a compelling resolution of these puzzles.

4.6.3 Elliptic cohomology

Another important area of topology where Physical Mathematics ought to have an impact is elliptic cohomology and the theory of topological modular forms. Soon after the discovery of the elliptic genus Witten gave an interpretation in terms of a supertrace of a 2d quantum field theory with $(1,0)$ supersymmetry [400]. Mathematicians such as Hopkins developed a generalized cohomology theory known as tmf (topological modular forms), and Segal suggested a connection to conformal field theory [350]. Segal's ideas have been vigorously developed in recent years by Stolz and Teichner, who have developed a notion of conformal field theory that physicists would do well to pay much more attention to [370, 371]. One interpretation of these works is that they are providing a definition of a space of conformal field theories, and this space of theories turns out to have an interesting homotopy type.

A concrete question one could ask about the relation of elliptic cohomology and physics is:

Interpret the $(24)^2$*-periodicity of tmf in terms of conformal field theory.*

Another, possibly related question is

Define a mod-two index of the Dirac operator on loop space and interpret it in conformal field theory

In recent years the superconformal index has been very popular. In particular, the index of four-dimensional supersymmetric field theories can be written as a path integral on S^3. In close analogy with Witten's interpretation of the elliptic genus as an equivariant index of the Dirac operator on loop space then we can view the S^3-index as a quaternionic generalization. We can identify S^3 with the unit quaternions and the supersymmetry operators can be viewed as Dirac-like operators on the space of quaternionic maps into field space. As in the 2d case, one should work equivariantly with respect to the isometries of S^3 in order to get a well-defined index problem.

Does the S^3*-index of superconformal theories indicate the existence of a generalization of the theory of topological modular forms where the circle of CFT is replaced by the quaternionic circle* S^3*?*

4.6.4 Topological phases of matter

In recent years techniques familiar to both string theorists and to topologists have been brought to bear in some of the more theoretical corners of condensed matter theory, in the emerging subject of topological phases of matter. This is in part reflected by the leading role, played by topologists such as Michael Freedman and Kevin Walker at Microsoft's Station Q, in this subject.

One of the cleanest examples of this is the use of Bott periodicity and K-theory in discussing phases of free fermions. This was discussed extensively in the condensed matter theory literature. See [150, 255, 345, 372, 429, 430]. It was stressed in [157] and [314] that the 10-fold way should apply to *arbitrary* quantum mechanical systems and it does not rely on the assumption that the degrees of freedom are fermions nor does it rely on the presence or absence of interactions. Hence we may ask a simple question:

Can the 10-fold way be usefully applied to a broader class of physical systems ?

Moreover, as stressed by many authors on topological phases of matter, it is important to consider more refined classifications of phases of condensed matter systems. It has been suggested for some time by Kitaev that the appropriate framework is a novel generalized cohomology theory [256]. Following some recent progress on topological classification of phases in quantum field theories [218, 248, 249] there has been interesting progress on this idea [160, 250, 251, 252]. The key idea here is to include "gravitational" interactions in the low energy effective topological field theory, and then use cobordism theory to enumerate systematically the possibilities. In many ways this development goes back to the use of Milnor's construction of classifying spaces BG to give lattice definitions of topological gauge theories [112]. The various proposals recently made by Freed, Kapustin, and Kitaev, are not obviously equivalent (at least, not to me) and this raises the natural question of precisely how the proposals are related. The paper [160] makes an interesting suggestion of a relation between (fully extended) invertible topological field theories [10] on the one hand, and "phases with short range entanglement" [11] on the other. Once the connection to invertible topological field theories is made one can bring to bear the mathematics of cobordism theory to give a classification, as explained in [160]. Thus, an important problem is to explain in more microscopic terms the relation of short range entanglement and invertible topological field theories.

Quite generally, the RG should define a "map" from field theories (or more general theories) in the UV to topological field theories in the far IR. We would like to understand this "map" much better. This is an exciting and fast-moving area and I expect to see good progress in the near future. In my view, the current vigorous trialogue between theoretical condensed matter, string theory and quantum field theory, and mathematics, fits nicely under the umbrella of Physical Mathematics.

4.7 Geometry and Representation Theory of Some Infinite Dimensional Groups

Experience from two-dimensional field theories shows that the geometry and representation theory of infinite-dimensional groups can be intimately related with quantum field theory.

[10] Since topological field theories can be multiplied, and since there is a unit (the trivial theory) one can define a notion of an "invertible" topological field theory [155]. In general, the sum over topological sectors in any path integral involves weights which can be understood as a coupling to such an invertible theory [155]. It has been shown by Seiberg and collaborators that such couplings of a QFT to an invertible TQFT can have important physical consequences [11, 38, 249, 357].

[11] A nice definition of such phases has been given by Kitaev for spin systems. Regrettably the term is used in several different ways in the literature and again, it is not obvious to me that these different uses are equivalent.

In particular, the representation theory of the diffeomorphism group of the circle, and the loop groups of compact Lie groups [343] forms the basis of WZW conformal field theories, which in turn are the building blocks of RCFT's.

One might expect that in more general field theories the geometry and representation theory of diffeomorphism groups of other manifolds, and the groups of automorphisms of principle G-bundles should play a more central role than they do in current formulations. Not much is known about this representation theory, and there are even some discouraging no-go theorems [343]. Nevertheless, it is physically clear that current algebra should make sense in higher dimensions.

Another set of infinite-dimensional groups which have recently received some interesting attention, are the BMS groups. I would expect that the representation theory of these groups to be interesting and useful in applications to quantum gravity.

4.8 Supergeometry

Anyone who has tried to understand superstring perturbation theory has run into the subtleties of the super-algebraic geometry of super-Riemann surfaces. This was recently revived by Witten [422, 423]. Some of the subtleties have to do with global issues related to proper integration over the modes of the gravitino. This raises the obvious question:

Are there analogous subtleties in the path integral of supergravity in higher dimensions?

Moreover, superconformal symmetries are central in many sub-disciplines of Physical Mathematics. A useful perspective on some groups, such as Poincaré and Galilean groups is that of Felix Klein's Erlangen program. This was in part an attempt to give a unified view of the zoo of different non-Euclidean geometries discovered during the 19th century. Klein proposed to define geometries by the groups of automorphisms preserving geometrical structures. Thus, for example, n-dimensional projective geometry is the study of those geometrical structures (say in $\mathbb{P}^n$) invariant under $PGL(n+1)$. Fixing a plane at infinity defines affine geometry, and we can rigidify further to Euclidean geometry by considering $\mathcal{T} \rtimes SO(n)$ where $\mathcal{T}$ is the group of translations. The program adapts itself well to physics: The Poincaré and Galilean groups fit in snugly. [12] Now, the conformal group is the group of automorphisms of an affine space that preserves causal structure. That is, it is the group of transformations that takes light cones to light cones. What about the superconformal group?

Is there a super-Erlangen program for super-geometries, in particular, for superconformal geometries? That is, can we define precisely supergeometries of which the superconformal groups are the groups of super automorphisms?

These should be the background geometries on which theories such as the $(2,0)$ theory are defined.

[12] See, for example, Section 2 of [157].

5. Defining Quantum Field Theory

Physics is very interesting: There are many, many interesting theorems. Unfortunately, there are no definitions. - David Kazhdan

We often hear that we do not really "know" what string theory or M-theory "is." That is undoubtedly true. But it is generally assumed by physicists that we do "know" what quantum field theory "is." The 20th century has bequeathed to us the descriptions we learned in our textbooks. Now we can move on to investigate wonderful new phenomena and explore particularly rich examples of QFT. I would like to suggest that there are in fact indications that there are serious deficiencies in our current understanding of what QFT "is." I am not saying that what is in textbooks on QFT is wrong, but rather that there is growing evidence that what is in textbooks is seriously incomplete and that there exists at least one currently unknown formulation of QFT that will shed light on several issues that many of us find perplexing. I will list a brief summary of the evidence, and then comment on each item more extensively: [13]

1. Some QFT's have no action principle and no obvious "fundamental" degrees of freedom.
2. Some QFT's have many action principles, with completely different "fundamental" degrees of freedom.
3. Even when there is an action principle, interacting QFT's with running coupling constants are not clearly and rigorously mathematically defined.
4. S-matrix amplitudes exhibit remarkable properties making startling connections to mathematical fields not traditionally related to physics. Moreover, they encode physical properties such as locality in highly nontrivial ways.
5. The universe of "theories" is *not* a disjoint union of string theory and QFT. From AdS/CFT we believe that some field theories are equivalent to string theories. On the other hand, there are other "theories," which are neither local quantum field theories nor traditional string theories (with gravity).
6. Many field theoretic phenomena have *geometrical reformulations*, reducing highly nontrivial facts of field theory to simple geometrical constructions.
7. A fully local theory should associate physical quantities with subspaces of all codimension and have coherent gluing rules for gluing of such subspaces.
8. Even when there is an action principle, a QFT is not completely defined by an action and a list of local operators. One must include (higher categories of) defects of all dimensions.

I will now comment on each of these items.

[13] Nati Seiberg has been voicing very similar opinions for some years now, and I found his list of evidence [358] helpful in preparing my own.

5.1 Theories without actions

A striking prediction of string theory from the mid 1990's (in the hands of E. Witten, A. Strominger, and N. Seiberg) is that there is a class of six-dimensional interacting conformal quantum field theories known as the "(2,0)-theories." [14]

Many beautiful results in Physical Mathematics obtained in the past few years - such as the AGT conjecture, the connections to (quantum) integrable systems, the connections to cluster algebras and cluster varieties, and Witten's gauge-theoretic approach to knot homology - can be traced to the very existence of these theories. On the other hand, these six-dimensional theories have not yet been fully formulated in any systematic way. There is no analog of a statement for nonabelian gauge theory like: "Make sense of the path integral over connections on a principal bundle weighted by the Yang-Mills action." We don't even know if there are fundamental degrees of freedom in the form of field multiplets governed by equations of motion or an action principle! (Some leading practitioners are skeptical that such multiplets and equations of motion even exist, others suggest various higher algebraic structures are the key.) [15]

The current formulations of the theories always involve fairly elaborate limiting processes, which are hard to carry out in very concrete and explicit terms:

1. A family of type IIB string theories defined on a family of spacetimes that develops an ADE singularity has a "decoupled" low-energy field-theoretic sector with six-dimensional $(2,0)$ superconformal invariance [411]. [16]

2. A family of parallel M5-branes such that the distance d between them goes to zero has, at low energies E on the order of $\sqrt{d/\ell_M^3}$ a decoupled field-theoretic sector with $(2,0)$ superconformal invariance [375].

3. SQM on instanton moduli spaces of instanton number k in the large N limit can be used to compute certain correlators in the $(2,0)$ theory [7].

4. There is a claim that a large N, strong coupling, and large Higgs vev limit of certain d=4 N=2 SYM theories exists and can be used to study some physical questions about the $(2,0)$ theory [21].

5. There are claims that one can "UV complete" 5d SYM into a UV theory by the inclusion of "instanton particle effects." [132, 269, 334].

[14] For reviews see [313] or [376]. For some useful talks see [133, 312, 333].

[15] It is often, but wrongly, stated that the abelian $(2,0)$ theories have no action principle. On the contrary, they have infinitely many action principles [4, 33, 34, 45, 228, 335, 336, 338]. What does appear to be true is that there is no *local* and relativistically invariant action for an abelian self-dual field. It would be useful to formulate and prove a sharp no-go theorem to this effect.

[16] To be slightly more precise, one considers limit which sends the type IIB string coupling to zero and while simultaneously moving along a one-parameter family of metrics on the resolution of the ADE singularity where the area of the exceptional curves scales to zero. The limit is defined so that the strings obtained by wrapping D3 branes around those exceptional curves have a constant tension.

It is perhaps possible to formulate a list of axioms the theories should obey. Such a list might even characterize the theories uniquely [313]. [17] In some strictly logical sense this might be a definition, but even if brought to a successful conclusion it would hardly be satisfactory. It would be more of a description of the answer than a compelling UV formulation. As stressed to me by Edward Witten, thanks to the AdS/CFT correspondence, we do have an understanding of the complete solution of the theory in the large rank limit for $\mathfrak{su}(N)$ theories, but this required completely new dimensions and new ideas.

5.2 Theories with many actions

One lesson we have learned over and over again is that there can be different action principles and different "fundamental degrees of freedom" that describe a single theory. A simple example is that of bosonization in certain 1+1 dimensional QFTs. Another example is nonabelian bosonization, where the WZW nonlinear sigma model whose target space is a compact simple Lie group is equivalent (at level $k = 1$) to a nonlinear sigma model on the maximal torus. (In mathematics this is known as the Frenkel-Kac-Segal construction.) In fact, in this case there is so much symmetry that one typically solves the theory without making use of the action at all. For reviews see [111, 168]. Over the years much more dramatic examples have been produced of various dual pairs of theories. A typical example would be a pair of theories related by a strong-weak coupling duality. In addition, very different UV Lagrangian theories can produce equivalent IR physics as in the famous Seiberg and Seiberg-like dualities in divers dimensions.

5.3 The trouble with scale dependence

A large number of mathematicians have understood TFT very deeply, and gone on to develop it in marvelous ways. Many mathematicians have mastered free field theories very well. A few mathematicians have understood perturbative field theory - much better than most physicists. Nevertheless, no mathematicians have given a clear and rigorous defintion of interacting QFT's with running coupling constants. [18] The issue of scale transformations and the renormalization group remains beyond the toolkit of most mathematically-trained practitioners of Physical Mathematics. The reason for this is the lack of a very clear and satisfying mathematical formulation of the Wilsonian approach to field theory. Why are they having such a hard time understanding and formulating interacting QFT? I would suggest it might be because the physicists themselves are not thinking about it properly.

5.4 S matrix magic

It has been known for well over thirty years that the S-matrix can exihibit surprising mathematical properties and can be a very effective way to approach the solution of certain quantum field theories [425]. Such S-matrices were the origin of a tremendous amount of

[17] It should be stressed that there are several nontrivial steps that need to be filled in to complete such a list of axioms.

[18] Of course there are rigorous definitions of scalar field theories in $1 + 1$ dimensions with polynomial interactions. But the methods used here do not seem to generalize easily to more challenging interacting theories in 3 and 4 dimensions.

Physical Mathematics in the context of integrable field theories, and the closely related mathematical subject of quantum groups. At least for such field theories, it was clear that the standard approaches to quantum field theories were relatively unilluminating.

More recently, remarkable mathematical structures have been discovered in investigations of S-matrix amplitudes of Yang-Mills theories [22, 147, 202]. Aspects of cluster algebras and cluster varieties, in particular the algebraic geometry of Grassmannians, and the mathematics of polylogarithms and motives play a prominent role. According to these authors, these results lead to radical new formulations of the principle of locality. One concrete question we could ask in this subject is

Can the integral expressions for $N = 4$ SYM scattering amplitudes as integrals over the positive Grassmannian (or its generalizations) be derived from the $\alpha' \to 0$ limit of string theory expressions involving integrals over moduli spaces of Riemann surfaces?

Another community of mathematicians has been exploring perturbative field theory amplitudes are relating them to period integrals associated with motives. They find a beautiful mathematical structure associated to a Hopf algebra of Feynman diagrams. Moreover, there is a Birkhoff factorization, which geometrizes very beautifully the intricate BHPZ regularization and renormalization procedure of perturbative field theory. See, for examples, [67, 68, 69, 283, 284] for a representative sampling of the literature.

Clearly, these two communities are uncovering some unexpected and very interesting mathematical structure in the S-matrix of field theories, and it seems likely that this direction will be a source of much future progress. These two communities should communicate with each other more than they do.

5.5 Oddball theories

In addition to the $(2,0)$ *field theories*, string theory constructions have motivated other limits, allegedly constructing quantum theories that are neither string theories with gravity nor local quantum field theories.

Perhaps the most notable of these are the "little string theories" obtained by taking a $g_{string} \to 0$ limit of the theory on an NS5 brane holding ℓ_{string} fixed. Such theories are thought to exhibit T-duality since the longitudinal T-dual of an NS5 brane is another NS5 brane. Two useful reviews are [8, 9]. Very little is known about these theories and surely an important direction for future research is to develop these theories further.

It is possible that one can also define limits of physics on D-branes "probing" compact geometries. If such theories exist they could have exotic properties. If one could consistently decouple gravity from a D3 brane probing the base of an elliptically fibered K3 surface, as in the setup of [35], the resulting theory would be a d=4 theory with N=2 supersymmetry with a compact Coulomb branch. The BPS degeneracies should in principle lead to a formula for the K3 metric using the twistor-TBA method mentioned above. [19]

[19] This can also be viewed as an M5 brane theory wrapping the elliptic fiber of an elliptically fibered K3. Such a theory would be related to the hypothetical "little m theory" whose existence was suggested in [276]. The existence of such a limit was called into question in [355]. Clearly there are some important issues to sort out here.

Finally, we note that certain noncommutative field theories (which, again, can be obtained from suitable limits of string theory) exhibit phenomena that violate several aspects of the standard wisdom of quantum field theories [295]. In particular, these are theories with arbitrarily high numbers of derivatives in the action, and yet are renormalizable. Moreover they violate effective field theory reasoning in surprising ways: Even in a massive theory there can be IR divergences.

5.6 Geometrization of field theoretic phenomena

There has always been an uneasy relation between fundamentally geometric and more abstract or algebraic approaches to physics. This was part of the debate between "wave mechanics" and "matrix mechanics" in 1926 and is at the heart of the difficulties in formulating quantum gravity. One remarkable aspect of brane physics has been the geometrization of various field theoretic phenomena. Examples abound:

1. Separated parallel branes geometrize spontaneous symmetry breaking by an adjoint-valued Higgs field [412].

2. Quite generally, many moduli spaces of vacua of supersymmetric field theories can be understood quite simply in terms of configuration spaces of branes.

3. Branes probing and ending on other branes geometrizes the ADHM construction of instantons, together with its equivariant generalizations to ALE and ALF spaces, the Nahm transform, and the Nahm pole for monopoles. For some sample papers see [108, 128, 129].

4. Brane motion geometrizes Seiberg duality and other field theory dualities [143, 144, 201].

5. Brane-bending geometrizes the renormalization group flow of certain field theories [414]. AdS/CFT also geometrizes the renormalization group [98, 367].

6. S-duality of d=4 N=4 theories is neatly explained as a geometrical symmetry by compactification from six dimensions. The S-duality group is just the modular group of the torus. [386, 387].

The list goes on and on. Should not all these be a special cases of a more unified and geometrical formulation of quantum field theories?

A particularly interesting example of geometrization of field-theoretic dualities is the relation of the S-duality groupoid of d=4 N=2 theories of class S to the modular groupoid of a Riemann surface [181, 414]. In particular, Davide Gaiotto's paper [181] can be read as a proposal that there is a generalization of a modular tensor category "valued in four-dimensional theories." See [311] for some discussion of what this might mean. An interesting open problem is to make these ideas more mathematically precise. It might be one useful route to a geometric reformulation of QFT.

5.7 Locality, locality, locality

Thanks to the work of Atiyah [27], Segal [349], and many others, mathematicians understand Topological Field Theory (TFT) very deeply. Indeed, in some sense they understand it better than most physicists. The axioms of TFT elegantly summarize the primitive properties we expect from any path integral, and thus constitute a very basic implementation of the key idea of *locality*. But we can push this notion of locality further. For example, just as we can sew together space-time evolutions by composing evolution operators, we should be able to divide up space into pieces and glue together physical quantities to produce the Hilbert space of states associated to a closed spatial manifold. This can be done in TFT, but is challenging to carry out rigorously, even in free field theory [351]. Indeed, it is the subject of much current work on entanglement entropy. Now, mathematicians have not stopped there. They have taken this notion of locality to its ultimate logical conclusion in the very sophisticated "cobordism hypothesis" and the notion of "extended topological field theories" [32, 158, 247]. The resulting formalism makes use of higher category theory. A version of the cobordism hypothesis of [32] was proved by M. Hopkins and J. Lurie, and by J. Lurie, for some classes of TFT's in [158, 278]. [20] The incorporation of this grander structure into QFT is not standard in physics - it is nowhere to be found in the standard texts on the subject. It clearly lies outside the approaches to QFT through the Wightman axioms or the Haag-Kastler axioms. It is far from fully investigated even in the case of topological or free field theories.

5.8 Theories without defects are defective

One very nice physical interpretation of the extended topological field theories was suggested by Kapustin terms of defects [247] (See also Lecture 1 of [313].) We learn an important lesson: There is more data in defining a QFT than a table of local operators and their correlation functions! We must include the spectrum of extended operators such as line defects, surface defects, and so on, all the way up to the category of boundary conditions. Some concrete examples of how the spectrum of line defects and surface defects can distinguish quantum field theories (in addition to phases of quantum field theories) were given in [11, 184, 218, 249]. Once again, the textbook definitions of conformal field theory are clearly inadequate. Just as there are correlation functions of local operators there are also analogues of correlators of defects. Moreover, at least in some topological and supersymmetric examples there are analogues of "OPE's" of defects. In some examples the "OPE coefficients" are not simple numbers but can be vector spaces [184, 246] or even quantum field theories themselves [186]. These statements are often justified by invoking

[20] Technically, they should be "fully dualizable." This in particular implies that the Hilbert space of the theory on a compact space is finite-dimensional. We know that there are very interesting partially-defined topological field theories, such as two-dimensional Yang-Mills theory at zero area and the closely related theories used to obtain the S^3-index for theories of class S [171, 172, 173, 174]. Thus, one concrete problem for the future would be: *Formulate and prove a version of the cobordism hypothesis for q-deformed two-dimensional Yang-Mills theory at* $A = 0$. An even more ambitious project would be to generalize to area-dependent theories such as $YM2_q$ with positive area.

changes of boundary conditions in a path integral. But then what do we do when there is no action principle? Once again, it is clear we need a more flexible notion of QFT.

5.9 So, what should we do about this?

How can we take these remarks and actually find the hypothetical reformation(s) of QFT? One possible route is via a better understanding of the proper mathematical framework for the AdS/CFT correspondence.

The AdS/CFT correspondence and its broader gravity/gauge generalization is very deep and has had a profound impact on string theory and beyond, influencing both condensed matter and nuclear physics. What has its impact been on Physical Mathematics? There are several possible answers to this:

1. The well-known Chern-Simons-Witten/RCFT correspondence between 3-dimensional Chern-Simons theory and conformal field theory is an important prototype for the AdS/CFT correspondence [142, 300, 404, 405, 418]. The CSW/RCFT correspondence has indubitably had a profound impact on Physical Mathematics. Since gravity is locally trivial in three dimensions in some sense it *is* the AdS/CFT correspondence, albeit in a very special case. The CSW/RCFT correspondence has played some role in the more standard examples of the AdS/CFT duality between string theory on $AdS_3 \times S^3 \times M_4$ and the superconformal sigma models on $\mathrm{Sym}^N(M_4)$, with $M_4 = K3, T^4$. Close analogues of CSW/RCFT exist in higher dimensions for the theory of singletons [44, 159, 213, 279, 309, 349, 417, 419]. Nevertheless, in spite of all this, the CSW/RCFT correspondence is definitely not the full fledged AdS/CFT correspondence!

2. There have been beautiful applications of integrable systems theory and in particular integrable spin chains to the AdS/CFT correspondence [42, 268]. The results are amazing, the methods are powerful. But it is not the full fledged AdS/CFT correspondence!

3. The AdS/CFT correspondence inspired important work in the differential geometry of Sasaki-Einstein manifolds [169, 257, 315]. But this does not give a mathematical framework for the full fledged AdS/CFT correspondence!

4. The large N transitions of Gopakumar and Vafa [203] have indeed had an enormous impact on topological string theory. Nevertheless, while there are many elements of the AdS/CFT correspondence in this story, it is a topological version or caricature of AdS/CFT rather than the full fledged correspondence!

In spite of all this, I would say that, in proportion to its depth, the AdS/CFT correspondence has had remarkably *little* impact on the kind of Physical Mathematics discussed in Section §4 above. Unlike its impact on the rest of string theory, it has *not* had a transformative impact. Moreover, the AdS/CFT correspondence obviously links geometry to

field theory and for these two reasons I would suggest that an important open problem (admittedly rather vague) is

Give a clear and general formulation of the proper mathematical setting for AdS/CFT.

To get a better idea of what I vaguely have in mind, let me remind the reader that when Atiyah and Segal formulated topological field theory axioms they gave a geometrical framework that clearly could include more general quantum field theories: One changes, for example, the geometrical bordism category appropriately. Their clear, concise, and general picture of what a field theory "is," has had a transformative effect on the way many mathematicians approach Physical Mathematics. But it is also clearly not the full story. Simple facts that physicists take for granted, as well as nontrivial dynamical phenomena, are not readily understood from this approach. I am suggesting that some analogous progress that would address these shortcomings could be accomplished if someone could explain to the mathematical community what the AdS/CFT correspondence really "is."

6. Exploring Quantum Field Theory

"*A dog does not define a cat, but when it sees one it knows what to do.*" - Andre Weil.

♣Need to authenticate this quote. ♣

6.1 Classification questions

Even if we don't have a fully satisfactory definition of quantum field theory, that does not stop physicists from asking how to classify them. It is clearly best to start with conformal theories, and even better to start with superconformal theories. If we begin in six dimensions with the maximal supersymmetry we are classifying the $(2,0)$ superconformal theories. In addition to the abelian (noninteracting) theories the classification of the simple, interacting, superconformal field theories is thought to follow a beautiful ADE classification scheme. (It would be good to have a clear argument why this classification is complete. Perhaps it follows from anomaly cancellation arguments.) So, there is the very natural question:

How far can we go in classifying superconformal theories as we reduce spacetime dimension and the amount of supersymmetry?

The first reduction, to six-dimensional $(1,0)$ superconformal theories is a currently active subject of investigation [192, 227] but as yet there is no crisp classification scheme. As we move down to five dimensions known constructions such as the use of webs of (p,q) 5-branes suggests that there is quite a zoo. Can it be tamed?

A particularly interesting case is the classification of d=4 N=2 theories. For example, we could ask: Are all d=4 N=2 theories of class S or "limits" of class S? [21] Thanks to

[21] The standard counterexample, which is often cited (and which I first learned from Davide Gaiotto), is a quiver gauge theory for a quiver corresponding to an exceptional Dynkin diagram with gauge group ranks proportional to the McKay weights. There is no known way to realize these theories in class S, but that falls far short of a proof.

AdS/CFT it is possible that those families of theories with large N limits can be classified by examining their holographically dual geometries [182], but even here questions remain regarding the boundary conditions for the Toda equations used in [182].

As we move down to two dimensions we encounter the very old question of the classification of two dimensional superconformal theories. Of course, the answer will depend on the definition of quantum field theory. For example, Stolz and Teichner have an answer [370, 371] that might not be recognized by most physicists.

Another approach to classification is the conformal bootstrap. Of course, this has been used with spectacular success for two-dimensional theories [43, 111]. Steady progress has been made in the past few years in the three and four-dimensional case. See, for some examples [145, 146, 344]. Clearly, there is much more that remains to do here; this will surely be an active direction or the future. One question, which has not yet been addressed much in the literature, is this: In the case of two-dimensional theories we know that in order to define theories that are consistent on all Riemann surfaces it does *not* suffice simply to solve the crossing symmetry constraints on the four-point function. One must also impose conditions from consistency of correlation functions at genus one.

In the context of three- and four-dimensional conformal field theories, find a set of consistency conditions on correlation functions (in addition to the standard bootstrap equations) so that they can be defined on arbitrary conformal classes of three- and four-dimensional Riemannian manifolds, respectively.

6.2 Geometry on the space of field theories

The profound results of D. Friedan [163, 164] and A. Zamolodchikov [426] on the geometry of the space of two-dimensional quantum field theories have made it quite clear that we should define a *space* of quantum field theories and explore its geometry. Indeed, Friedan has even proposed to use properties of the space of all two-dimensional models as a starting point for a unified view of physics that is meant to be an alternative to the more standard string-theoretic approach [165].

It would be good to give a concrete and rigorous definition of the "space of quantum field theories" both in two and in higher dimensions. One tool, which has been used to explore families of quantum field theories, is the construction of quantities that decay monotonically under renormalization group flow. There are known quantities in two [426], three [73], and four [260] dimensions, generally called c, F, a, respectively. An obvious problem for the future is

Find monotonically decreasing quantities for renormalization group flow of five and six dimensional theories.

As discussed above, we do not understand these higher dimensional theories in the UV very well so this problem is probably out of reach at the moment. A curious aspect of the known monotonically decreasing quantities is that c, a in $2, 4$ dimensions are defined in

terms of local correlation functions of the energy-momentum tensor while F is a nonlocal quantity.

Is there a more unified view on the monotonically decreasing quantities? Is there a locally defined quantity in three dimensions that decreases under RG flow? Are there other functionally independent monotonically decreasing quantities?

In two-dimensional theories it is known that one can enrich the story by considering so-called boundary conformal field theories: One considers a conformal field theory defined on a Riemann surface with boundary, with boundary interactions breaking the conformal invariance. In this situation there is a so-called g-function that is known to undergo gradient flow [2, 3, 166, 267]. [22] It is natural to ask if there is an analogous story in higher dimensional field theory:

Investigate higher dimensional analogues of boundary conformal field theories. Are there analogues of the gradient flow equation for the g-function?

7. String theory compactification

Despite an enormous amount of work, there is plenty left to do in deriving reliable physical predictions from string theory and M-theory compactifications. This subject has been of enduring interest for over 40 years, and is the source of much of the lively interactions with algebraic geometry. It is also the origin of phenomena such as mirror symmetry.

Roughly speaking string theory and M-theory compactifications can be classified along the following lines:

1. Weakly coupled heterotic strings
2. Weakly coupled type II strings, (with D-branes, fluxes, and orientifold planes).
3. Geometric compactifications of 11-dimensional M-theory: G_2 and $Spin(7)$ manifolds, generalizations with fluxes, and heterotic M-theory compactifications.
4. Nongeometric "compactifications," obtained by replacing the small space of Kaluza-Klein with a more abstract notion such as a CFT.

This classification scheme is somewhat useless, and extremely rough. Moreover, because of the web of dualities there are compactifications admitting several descriptions (especially if one allows for continuous extension of parameters such as couplings and other

[22] The g-theorem had its origins both in the work in condensed matter physics of Affleck and Ludwig [2, 3] as well as in an attempt at a background-independent formulation of string field theory [193, 267, 362, 363, 408, 409]. Indeed, there is a long history of connections between attempts to understand better the geometry of two-dimensional quantum field theories and attempts to understand string field theory.

moduli). One might well ask for a more useful and complete classification of sting/M-theory compactifications. Some initial attempts in this direction were made in the "String Vacuum Project" [374].

Within each class above there are many many unsolved problems.

For example, in the framework of heterotic compactifications the current understanding of (two-dimensional) (0,2) conformal field theories is poor, although it is improving [361]. To choose but one example of problems in this class:

Formulate a rule for establishing nontrivial equivalences between $(0,2)$ *conformal field theories.* [23]

In the case of type II compactifications, we still do not know the answers to basic questions about the space of compactifications of type II compactifications with four-dimensional N=1 (or even N=2) Poincaré supersymmetry. For example, it is not even known whether there are a finite number of topological types of compact Calabi-Yau 3-folds. In the 1990's there was an ambitious conjecture that the space of compactifications would be connected [210]. However, subsequent constructions made it clear that there would be components of this space separated by infinite (Zamolodchikov) distance [79, 80, 99, 271, 415]. As discussed below, questions of complete anomaly cancellation and the computation of the full low energy supergravity (even in $d = 4, N = 2$ compactifications) remain unanswered.

A special case of the M-theory compactifications is the vast topic of F-theory compactifications, with all their beautiful applications of the algebraic geometry of elliptic fibrations to string theory. An ambitious program to make phenomenologically viable F-theory compactifications was proposed in [40, 41, 123] and has inspired a fairly sizable body of work. See [289] for a representative example. Nevertheless, crucial technical problems related to the dynamical behavior of FI terms remain to be solved [259]. Another extremely important class of compactifications are the heterotic M-theory compactifications [236, 237], which has been vigorously pursued especially by the Penn group. See [59, 61] for some recent examples. [24]

In spite of all the effort that has been devoted to compactification of string theory and M-theory we still have not got the answer to a very basic problem:

[23] Sometimes this is phrased as the problem of establishing "$(0,2)$ mirror symmetry." The idea is that equivalences between $(0,2)$ CFT's associated to a Calabi-Yau manfiold with holomorphic vector bundle might define nontrivial equivalences between different manifolds with bundle. As stressed to me by N. Seiberg, the term "mirror symmetry" should be reserved for a duality that acts nontrivially on the R-symmetry group.

[24] There is a technical problem in heterotic M-theory that should be clarified. There should be a systematic derivative expansion of the supersymmetric LEEA for M-theory on an interval. The expansion begins with 11-dimensional supergravity in the bulk and the E_8 super-Yang-Mills action on each boundary. The first nontrivial corrections were worked out in [237]. The problem is that certain boundary corrections were proportional to the delta functions $\delta(x^{11})$ and $\delta(x^{11} - L)$. Naively these come in squared at the next order. We can expect that with supersymmetric boundary conditions there will be supersymmetric corrections that cancel these, and potentially higher, divergences. But those corrections have not been worked out.

Can we construct compactifications of string theory or M-theory with unbroken four-dimensional N=1 Poincaré supersymmetry or deSitter symmetry with small cosmological constant, with a parametrically controlled hierarchy of scales to the KK scale and parametric control of all quantum corrections?

Judging by the enormous popularity of the KKLT and warped throat compactifications [39, 103, 105], I expect that most string theorists will claim this problem has been solved. The stress above is on having a controlled approximation scheme, such that we can compute quantum corrections to alleged leading order approximations. [25] Moreover, when taking into account supersymmetry breaking and "uplifting" to deSitter backgrounds we must ensure that deep issues of quantum gravity do not render the entire approach through effective potentials meaningless [37]. We also must make sure that the backgrounds are truly stable to perturbations, and this too has been called into question [46, 47, 48, 245]. Because of these, and many other technical issues, I remain unconvinced that the above problem has been adequately solved. This might just reflect my own failings and limitations. But many scientists whom I respect also remain unconvinced. In view of this a closely related question must be posed:

Has the time come yet for the String Vacuum Project [374] ?

An important question we should ask before trying to address this one is:

Do we know all the ingredients one should use in string compactification?

I would expect that most people would answer this in the affirmative. Most people would say that we have all the relevant tools and we simply must use them in sufficiently ingenius combinations to produce physically acceptable compactifications. However, the upheavals of the D-brane revolution should give us pause: We now know there were important ingredients that were missing in the early 1990's and we did *not* have all the relevant tools at that time. Moreover, at that time there were clear warning signs from the study of perturbative string theory that such ingredients were missing. We can only recognize these warning signs for what they were given 20-20 hindsight. Among these warning signs were:

1. The confusing combinatorics of string perturbation theory with Dirichlet boundary conditions [206],

2. The odd behavior of open strings under T-duality [97],

3. The asymptotic growth of the string perturbation series and its suggestion of $\exp[-C/g_{string}]$ effects [365],

[25] For example, in a type II compactification with fluxes and orientifolds it would be important to have a well-defined string perturbation theory. Presumably the pure spinor formalism offers the best hopes because of the presence of RR background fluxes. However, the existence of warping factors renders the applicability of the perturbation questionable.

4. The difficulties associated with including nontrivial RR backgrounds in string perturbation theory, and the odd fact that no states had RR charge.

5. The peculiar status of 11-dimensional supergravity

We should be on the alert for similar warning signs today. Four potential thunderclouds on the horizon are:

1. As mentioned above our understanding of 2d quantum field theories with (0,2) symmetry is poor. In particular we do not have a good understanding of the target space geometry associated with the IR limit of such theories.

2. There are hints of nonperturbative degrees of freedom confined within orientifold planes. For example, in the Sen limit of F-theory (p,q) 7-branes combine to form an orientifold plane. What happened to the nonperturbative degrees of freedom of the 7-branes?

3. S. Shenker's argument [365] that the $(2g)!$ growth of string perturbation theory (at order g) requires nonperturbative effects with weight $\sim \exp[-\frac{const}{g}]$ would seem to apply to the heterotic string. However, the only known branes in the heterotic string have tension $\sim \frac{1}{g^2}$.

4. Finally, the Romans mass has no obvious lift to 11-dimensions. Why not?

When there are thunderclouds on the horizon it does not necessarily follow that there will be a storm. [26] It could be that the focus of the community on (two-dimensional) $(2,0)$ theories is a lamppost effect, and the community should be considering much broader compactification schemes based on internal spaces having nothing to do with Calabi-Yau or perturbations of Calabi-Yau spaces. The heterotic $\exp[-C/g]$ effects might be simple consequences of known dualities [342, 366]. A simple and straightforward argument shows that the IIA string coupling cannot get strong for solutions of IIA supergravity with weak curvature [10] and nonzero Romans mass. (The key point is the quantization of the Romans mass.) This might indicate that looking for M-theory lifts of the Romans mass and D8 branes is simply a poorly posed problem - they are intrinsically 10-dimensional objects which should not have 11-dimensional lifts.

8. String Field Theory

String field theory was originally intended as a way to approach nonperturbative string theory. That's not how the discovery of nonperturbative effects in string transpired. Nevertheless, the problem of giving a systematic description of *all* nonperturbative effects in string theory remains open, and string field theory might still be a route to solving that problem.

[26] I thank E. Silverstein for shooting down 75% of my storm clouds.

Thanks to the work of Schnabl [347] and subsequent work by others there has been progress in understanding analytic solutions to the classical equations of bosonic open string field theory [399]. Yet, even within this context a very important open problem remains:

Can one find analytic solutions of the open string field theory equations of motion (without adding Chan-Paton factors) corresponding to a D-brane with nonabelian gauge degrees of freedom?

Recent progress and the state of the art on this problem can be found in [148].

Impressive progress in understanding the meaning and effects of open string tachyons led to an improved understanding of D-branes, their K-theoretic charges, and the relation to noncommutativity. But what about closed string tachyons?

Develop a theory of the endpoint of closed string tachyon condensation.

The papers [1, 223] made a fair start by studying the implications of tachyons associated with certain nonsupersymmetric orbifolds. These are tachyonic fields whose support is localized in spacetime, and a reasonably satisfactory picture emerged for the endpoint of the condensation. But this was just a baby step. The full theory of closed string tachyon condensation is completely open and completely mysterious.

9. Noncommutativity and spacetime

One of the great lessons from string theory has been that we must use much more flexible and abstract notions of what constitutes a spacetime than have been previously used in physics. This was already clear over twenty years ago with the discoveries of mirror symmetry, T-duality, and topology change in CY moduli space. For some nice recent discussions see [291], and [352]. The latter article even views noncommutative spacetime as an inevitable consequence of quantum field theory itself.

In particular, in the context of type II string theory, the D-brane construction has taught us that we should replace the notion of spacetime by a category of branes. Spacetime is then, somehow, a derived concept. One very simple and clean baby example where this idea can be made completely precise is open/closed 2d topological field theory [310]. The fact that D-branes should be objects in a category follows from very simple and basic sewing arguments. In the context of open/closed 2d TFT one sees very beautifully and simply how the closed string sector is derived from the open string sector through the Hochschild (or cyclic) cohomology of the category of branes. This beautiful fact continues to be true at the level of cochain-valued TFT [92, 310]. As Segal points out in [352], there is a nice analog of this phenomenon in homotopy theory: Given a category $\mathcal{C}$ you can produce a topological space $|\mathcal{C}|$. (Objects are 0-cells, morphisms are 1-cells, pairs of composable morphisms are 2-cells, etc.) Now, given a manifold with an open cover there is a naturally associated category. (The set of objects is the disjoint union of the charts and morphisms encode how the charts are glued.) The space of that category has the same homotopy type

as the space you started with. So, at least at the level of homotopy type, the space can be recovered from the more abstract category. String theory is providing some generalization of that idea. Indeed, a key aspect of the Matrix theory approach to nonperturbative string theory is to put the open string matrix models on a primary logical footing [36].

One central feature of the viewpoint that spacetime should be derived from D-brane categories is that spacetime should - in some way - be viewed as noncommutative. There are many related hints that this should be the case. For example transverse coordinates of coincident D-branes are noncommutating matrices and it is only when looking at the classical moduli space of groundstates that we find commutative coordinates [412]. Indeed, the traditional noncommutative geometry as described in [87, 204] has played a very direct role in the physics of D-branes [88, 131, 321, 356]. But these ideas have not yet been successfully applied to the great questions of what happens beyond black hole horizons and what is the nature of cosmological singularities. Therefore, I suspect that we have not yet fully understood the mathematically precise sense in which space*time* should be noncommutative.

One aspect of string theory where our ignorance is quite profound is in the subject of time-dependent backgrounds. Attempts to understand cosmological singularities in string theory have run into technical challenges. See, for examples [91, 94, 238, 239, 273, 273, 274, 275, 290]. Perhaps we need to understand the nature of time itself better. Any child will ask: If there was a big bang, what happened before the origin of time? One natural way to approach that question would be to understand in what sense time itself is an emergent concept, and one natural way to make sense of such a notion is to understand how pseudo-Riemannian geometry can emerge from more fundamental and abstract notions such as categories of branes.

10. Exceptional Structures

We can divide physicists into two types: Those who believe the world is special and inevitable, and those who believe it is random and accidental. Steven Weinberg's correct prediction of the cosmological constant certainly gives the latter camp a strong upper hand. If that latter camp is right it would seem that winning the next Nobel Prize in high energy theoretical physics is equivalent to winning the Lottery. It might well be this is just the way things are.

On the other hand, it must be said that much of Physical Mathematics has a predilection for special, sporadic, and exceptional structures. Superconformal field theories and supergravity theories are closely related to magical properties of low-dimensional Clifford algebras, leading to startling connections with platonic solids, triality symmetries, division algebras (including the octonions), exceptional groups, Freudenthal-Tits magic squares, and so on. Moreover, certain conformal field theories have famously been closely related to the sporadic finite simple groups, especially the monster group.

Even if we live in a random world, it cannot be denied that it contains within it some exceptional gems of rare beauty, which can only inspire a sense of awe. One could write an extensive essay on the numerous "coincidences" and "accidents" associated with excep-

tional structures appearing in Physical Mathematics that call for deeper understanding. I cannot forecast what stormy weather our field is destined to endure, but I can confidently forecast abundant moonshine in the years ahead.

Among the most notable recent examples of such structures is the Mathieu Moonshine phenomenon associated with K3 sigma models and mock modular forms. Four years after its discovery [140] it remains largely mysterious, despite very intense efforts of first-rate scientists to find a natural explanation. Indeed, the mystery has only deepened with the extension to umbral moonshine [81]. The state of the art is summarized in [346]. Here is a fairly concrete question in this general area:

Is there an algebraic structure on the BPS states (spacetime or worldsheet) of string theory compactifications involving K3 surfaces whose automorphism group is naturally related to M_{24}?

In the heady days after the invention of the heterotic string many of the exceptional "accidents" were adduced as evidence of deeper structure. Some of these "accidents", such that the relation of surface singularities to Lie groups and the Kodaira classification of elliptic fibrations have been incorporated beautifully into enhanced gauge symmetries in string theory and F-theory compactification. But other "accidents" remain unexplained and unutilized. My favorite question in this class would be:

Is there a physical interpretation of the fact that the group of 11-dimensional exotic spheres, $\mathbb{Z}_{992}$, is cyclic with order four times the dimension of E_8?

Finally, we might ask whether there are applications of various Moonshine phenomena to laboratory experiments. There are claims that neutron scattering from cobalt niobate detects the first two Perron-Frobenius eigenvalues of the Cartan matrix of E_8 [54, 428]. If E_8 appears, can the Monster be far behind?

11. Should we count in number theory?

It is truly striking that techniques used in number theory have, again and again, turned out to be useful in the developments in Physical Mathematics in the past decades. I will merely list a few instances of which I am aware (this list is definitely incomplete):

1. Automorphic forms for numerous arithmetic groups have played an important role in conformal field theory and string theory. They arise in partition functions and effective actions and the automorphy is usually due to actions of large diffeomorphisms and duality symmetries. The literature here is too huge to cite. It should be pointed out that methods of analytic number theory developed for the Langlands program have been extremely effective in deriving U-duality invariant couplings in low energy effective actions [207, 208, 209, 329, 330].

2. Another aspect of analytic number theory, namely Rademacher sums have also been playing an important role, beginning with [114]. Following important work of Duncan

and Frenkel [135] (which was in turn motivated by a possible role of the Monster in 3d gravity [280, 418]) there have been many new applications of Rademacher sums to physics. See [308] for a review of the older work and [82] for a relatively recent review. Very recently, detailed facts about Kloosterman sums, needed for the original proposal of [114] to make sense, have been proved in [96].

3. An interesting variation on the theory of automorphic forms is the theory of mock modular forms, a subject going back to Ramanujan. Recently, mock modular forms have been appearing in counting problems of BPS states related to supersymmetric black holes [95, 281, 282], in elliptic genera of (noncompact) K3 models [138, 139, 383], as well as in investigations of Mathieu Moonshine [81, 82, 83, 224].

4. The S-duality of N=4 SYM compactified on a Riemann surface is deeply related to the geometric Langlands program [161, 215, 216, 246]. The geometric Langlands program was formulated as a way to gain insight into the original Langlands program of number theory.

5. Elliptic curves and families of elliptic curves (over $\mathbb{Z}$) play an important role in number theory, in many ways. Elliptic curves and families of elliptic curves (over $\mathbb{C}$) also play an important role in Physical Mathematics, in many ways. Two prominent examples are Seiberg-Witten theory and F-theory. Even the Mordell-Weil group of families of elliptic curves have been playing a significant role in recent work in F-theory.

6. In fact some physical constructions even point to special roles played by elliptic curves with complex multiplication. One of these is the attractor mechanism [306, 307]. Moreover, the image of RCFT compactifications of the heterotic string under heterotic/F-theory duality [306, 307]. It has been conjectured that, more generally, rational points in the moduli space of elliptically fibered K3 surfaces give rational nonlinear sigma models [212, 392, 393].

7. The Weil conjectures are often used as a technical *tool* for computing enumerative invariants (such as, for example, Euler characters of moduli spaces of sheaves) used to test duality conjectures. This leaves one asking if something as profound as the Weil conjectures shouldn't be playing a more fundamental role in the physical problem.

8. The Bloch group of algebraic K-theory has been closely related to new constructions with 3d supersymmetric theories [116, 117].

9. Constructions of Arakelov and Faltings, which were used in the proof of the Mordell conjecture turn out to be useful in proving bosonization formulae of 2d CFT on arbitrary Riemann surfaces [20]. Indeed Faltings' famous δ-invariant finds a natural interpretation in terms of the bosonization formulae [20, 55, 394]. (As noted in [235], there should be interesting analogues for higher dimensional self-dual fields.) Recently, similar number-theoretic quantities have proven useful in formulating two-loop superstring amplitudes [107].

10. A group known as the Grothendieck-Teichmuller group appears to be closely related to the absolute Galois group of $\bar{\mathbb{Q}}$ over $\mathbb{Q}$ [348], on the one hand, and is possibly related to the monodromy of conformal blocks in RCFT [101]. More recently, this group, and Grothendieck's *dessins d'enfants* have been related to four-dimensional supersymmetric field theories in an interesting way [24].

11. In a very interesting paper [388] Walcher has proposed that the BPS states of certain theories in 1+1 dimensions with (2,2) supersymmetry naturally carry an arithmetic structure: The central charges are valued in a Galois extension of $\mathbb{Q}$ and, conjecturally, the Galois group acts on the spaces of BPS states. The paper [388] approaches such phenomena through the use of open string mirror symmetry to compute the critical points of superpotentials on D-branes.

All this begs the obvious broad question:

Is there a deeper connection between string theory and number theory?

Perhaps a more useful concrete question is

Is there a natural role for L-functions in BPS-state counting problems?

For one set of speculations see [294].

On the negative side, there is no obvious unifying theme in the above connections to number theory. Moreover, there has been very little technology transfer from string theory to number theory. I know of two minor examples. One is the one-loop computation of [220] where a standard technique in perturbative string theory led to some useful technology for understanding the "lifting" of automorphic forms [53]. Another recent example is a discussion of the "fundamental lemma" using new ideas from the gauge-theoretic interpretation of the geometric Langlands program [162].

One obvious objection to any deeper relation to, say, the Weil conjectures or the modularity theorem for rational elliptic curves is that number theoretic questions depend strongly on the arithmetic nature of the coefficients of equations defining a variety. The Mordell theorem might hold an important lesson here. It says that the number of integer solutions to an equation for a curve over defined over $\mathbb{Z}$ will be finite if the genus of the curve, *as an equation over* $\mathbb{C}$, is greater than one. This is extremely striking - the topology of the the variety defined over $\mathbb{C}$ controls a basic number-theoretic property. One might wonder, similarly, if physical properties associated to the varieties that appear in string compactifications, or D-brane constructions, might control arithmetic properties of those varieties.

12. Keep true to the dreams of thy youth: M-theory

Looking further to the future, we should not overlook the big elephant in the room: We still have no fundamental formulation of "M-theory" - the hypothetical theory of which 11-dimensional supergravity and the five string theories are all special limiting cases. Work on

formulating the fundamental principles underlying M-theory has noticeably waned. A good start was given by the Matrix theory approach of Banks, Fischler, Shenker and Susskind [36]. (See [341, 377] for reviews.) We have every reason to expect that this theory produces the correct scattering amplitudes of modes in the 11-dimensional supergravity multiplet in 11-dimensional Minkowski space - even at energies sufficiently large that black holes should be created. (This latter phenomenon has never been explicitly demonstrated). But Matrix theory is only a beginning and does not give us the whole picture of M-theory. The program ran into increasing technical difficulties when more complicated compactifications were investigated. (For example, compactification on a six-dimensional torus is not very well understood at all. This is again related to "little m theory" mentioned above.) Moreover, to my mind, as it has thus far been practiced it has an important flaw: It has not led to much significant new mathematics. If history is a good guide, then we should expect that anything as profound and far-reaching as a fully satisfactory formulation of M-theory is surely going to lead to new and novel mathematics. Regrettably, it is a problem the community seems to have put aside - temporarily. But, ultimately, Physical Mathematics must return to this grand issue.

13. Money and jobs

When I discuss the topics of this essay with my colleagues they often turn the conversation to the political consequences of declaring that there exists an independent field of endeavor that should be called Physical Mathematics. They often remind me that most mathematicians are not as fully blessed with the opportunities for pursuing their research that many theoretical physicists enjoy. (Several mathematicians have expressed to me their frustration with this inequity.)

One is tempted to take the high moral ground with which Poincaré opened his ICM address [340]:

On vous a sans doute souvent demandé à quoi servent les mathématiques et si ces délicates constructions que nous tirons tout entiéres de notre esprit ne sont pas artificielles et enfantées par notre caprice.
Parmi les personnes qui font cette question, je dois faire une distinction; les gens pratiques réclament seulement de nous le moyen de gagner de l'argent. Ceux-là ne méritent pas qu'on leur réponde; c'est à eux plutôt qu'il conviendrait de demander à quoi bon accumuler tant de richesses et si, pour avoir le temps de les acquérir, il faut négliger l'art et la science qui seuls nous font des âmes capables d'en jouir
et propter vitam vivendi perdere causas.

You undoubtedly have often been asked what is the use of mathematics, and whether the tricky manipulations that we pull out of our minds aren't artificial and the product of our whims.

From among the people who pose this question, I should single out one group: the practical people, who demand from us only the means of making money. They don't deserve a response; we should rather ask them what good it is to accumulate such riches and whether, in order to make the time to acquire them, one has to neglect art and science, which alone make our souls able to enjoy them,
and lose the reason for living in order to live

As a practical American, I cannot wholly agree. Our field can only prosper if people have a warm house, a comfortable dinner, and the time to pursue their research. The real problem here is simple: It is not that the support for physicists exploring mathematics is too high, but rather that the support for mathematicians exploring physics is too low. While the problem is simple, the solution is not so simple.

One important and helpful trend of recent decades that addresses this problem has been the creation of many independent institutions which provide important opportunities for meetings, workshops, and programs, as well as retreats and safe havens from those unwelcome distractions from research with which our world is so plentifully supplied. A few examples would be the Aspen Center for Physics, the Benasque Center for Science, the IHES in Paris, the Institute for Advanced Study in Princeton, the Isaac Newton Institute for Mathematical Sciences in Cambridge, the KITP at Santa Barbara, the KIPMU in Tokyo, the Mathematical Research Institute at Oberwolfach, the Mathematical Sciences Research Institute in Berkeley, the Perimeter Institute in Waterloo, and the Simons Center for Geometry and Physics at Stony Brook. I think the emergence and proliferation of these and other similar research-oriented institutions is a Good Thing. Some of these institutions are well-endowed, while others are run on a shoestring. All of them should be encouraged in all possible ways.

14. An uneasy marriage

"*We should not confuse rigor with rigor mortis.*" - Isidore Singer.

♣Need to authenticate this quote. ♣

Physical Mathematics is sometimes viewed with suspicion by both physicists and mathematicians. On the one hand, mathematicians regard it as deficient, for lack of proper mathematical rigor. The most reasoned objections along these lines were raised by Jaffe and Quinn in [243]. The excellent responses of Atiyah et. al. and Thurston need not be repeated here [28, 378]. Moreover, the proof is in the pudding: In the years since this debate erupted there have been many spectacular successes scored by Physical Mathematics, thanks again to the unreasonable effectiveness of Physics in the Mathematical Sciences. Nevertheless, Jaffe and Quinn raised some reasonable points, and we should not lose sight of that.

On the other hand, the relative lack of reliance of Physical Mathematics on laboratory experiments is viewed - with some justification - as dangerous by many physicists. The dangers of relying on "pure thought" when divining the secrets of Nature are well-known and illustrated by multitudinous examples. To choose but one: Addressing an important debate of his time regarding human anatomy, Descartes gave a coherent logical proof that

the human heart is a furnace, and not a pump. Our response to this objection by physicists must be more nuanced, and is a two-part response.

First, the ebullient statements of Dirac and Einstein quoted above were founded on their past spectacular accomplishments. Einstein was reluctant to acknowledge that the Michelson-Morley experiment had a significant influence on his road to special relativity. And he was right: Once Maxwell's equations are properly understood mathematically, special relativity is an inevitable consequence. Even deeper reflection on the meaning of relativity led him inexorably to the general relativistic formulation of gravity. Dirac was led to his amazing insights into the existence of anti-matter through the pure mathematics of Clifford algebras and the Dirac operator (admittedly prompted by the enigma of the electron's spin - an unexpected and surprising experimental discovery). The danger with this view, of course, is the spectre of human arrogance. We must ask: Would any community of humans pursuing Hilbert's 6th problem of 1900 have discovered the bizarre mathematical structure we call Quantum Mechanics without the insistent promptings and chastening from experiments on blackbody radiation, atomic structure, and spectral lines? My guess is that it would never have happened based on purely mathematical reasoning.

But that does not mean we should abandon the search! Mathematical beauty is not an infallible guide, but more often than not it has been a very useful tool. And this brings me to the second response: In the search itself great mathematics is created. Mathematical truth is something that can be tested, agreed upon, and verified. To reiterate the creed of Physical Mathematics: A mathematical discovery can be, in and of itself, a great intellectual achievement, making the search itself worthwhile.

Looking to the future, with some anxiety and trepidation I guess an' fear. Physical Mathematics has the potential to be a long-standing fixture of the intellectual landscape, a great beacon for progress in both Physics and in Mathematics, for some time to come, but only if we are mindfully adaptable and vigilant.

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