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Some Comments on Physical Mathematics

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ABSTRACT: These are some thoughts that accompany a talk delivered at the APS Savannah meeting, April 5, 2014.

I have serious doubts about whether I deserve to be awarded the 2014 Heineman Prize. Nevertheless, I thank the APS and the selection committee for their recognition of the work I have been involved in, as well as the Heineman Foundation for its continued support of Mathematical Physics. Above all, I thank my many excellent collaborators and teachers for making possible my participation in some very rewarding scientific research. [1]

I have been asked to give a talk in this prize session, and so I will use the occasion to say a few words about Mathematical Physics, and its relation to the sub-discipline of Physical Mathematics. I will also comment on how some of the work mentioned in the citation illuminates this emergent field.

I will begin by framing the remarks in a much broader historical and philosophical context. 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 Galileo, Kepler, Leibniz, and Newton were neither physicists nor mathematicans. Rather they were Natural Philosophers. Even around the turn of the 19th century the same could still be said of Bernoulli, 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 [30]. He modestly recommends his somewhat-neglected dynamical theory of the electromagnetic field to the mathematical community. According to [12] 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. Both Poincaré and Hilbert made it the subject of their addresses to the first two meetings of the International Congress of Mathematicians in 1986 and 1900, respectively. Hilbert's address famously stated 23 problems for the 20th century and the sixth problem concerned the relation of Mathematics and Physics:

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 ...

[1] I would also like to thank F. Denef, N. Seiberg, and E. Witten for making some very useful comments on this essay.

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 some ultimate foundations of physics, we can certainly say that one common goal (but not the *only* goal) in Physical Mathematics is to elucidate the solution to that problem. I suspect we will be working on it for quite some time to come.

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 [10]:

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 [11]. He predicted, correctly, that new 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 [14]:

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.*

I will just mention a few more discussions which I have found particularly intriguing. In 1960 Wigner waxed philosophical in his famous essay "On the Unreasonable Effectiveness of Mathematics in the Physical Sciences" [44]. In 1972 Freeman Dyson wrote a beautiful essay on the subject, "Missed Opportunities," [12]. He then followed up a decade later in 1982 with another wonderful commentary, "Unfashionable Pursuits" [13].

In our own time the subject 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 which 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. 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

[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.

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.

I was instructed to speak about my own work in this talk, and so I will now zoom in, as it were, from the grand vistas of the above remarks to the minutia of individual research and to try to put some of the work of myself and my collaborators into the context of the still vast territory of Physical Mathematics. The citation for the Heineman prize was a bit vague, and that is as it should be: During my own career I've been on base many times, but I've never yet hit a grand slam. So, I will discuss two very different examples which illuminate this larger field of Physical Mathematics. Ironically, while they appear very different, in the last few years they have turned out to be intimately connected.

The first example is a story which, for me, began with the groundbreaking work of Belavin, Polyakov, and Zamolodchikov on two-dimensional conformal field theory [4]. A number of people, including Daniel Friedan, Stephen Shenker, Erik Verlinde, and myself recognized that there is a "Goldilocks" subset of two-dimensional conformal field theories which are amenable to a much more detailed structural analysis than is possible for the general CFT, but nevertheless exhibit very rich properties. These were called *rational conformal field theories* (RCFT's). In 1988 Nathan Seiberg and I proposed a classification of RCFT's using the monodromy and modular properties of the analytic functions, known as conformal blocks, which are used to construct the correlation functions of the theory [32, 33, 34]. The monodromy and modular data formed a beautiful mathematical structure known as a "modular tensor category." Physicists were at first resistant to the use of category theory, but in part thanks to deep insights of Alexei Kitaev and Michael Freedman and others, many physicists have embraced the subject and it remains vital today in attempts to understand topological phases of matter in condensed matter theory and in potential applications to topologically protected quantum computation. This mathematics also had more direct physical applications to the quantum Hall effect. For example it led to the proposal of a Pfaffian trial wavefunction which might be relevant to certain quantum Hall states with novel excitations satisfying nonabelian statistics [35].

On the mathematical side, the subject of modular tensor categories is closely related to the application of physics to knot invariants. The invention of modular tensor categories in 1988 was one of several strands which Edward Witten tied up beautifully in his interpretation of the Jones polynomial of knots via three-dimensional Chern-Simons gauge theory [47]. This is clearly Physical Mathematics: Nobody is going to measure the Jones polynomial at the LHC. But it led, on the one hand, to profound developments in low-dimensionaly topology, such as the Reshetikhin-Turaev invariants of 3-manifolds, and on the other hand it provided an important example of "holography," a concept which has proven to be of great importance in our current understanding of the quantum physics of black holes and the gauge/gravity correspondence.

My second example has to do with a certain class of quantum field theories and string theories known as "theories with extended supersymmetry." The basic idea is that these theories have a global symmetry - known as "R-symmetry" - such as $SO(N)$ or $SU(N)$ such that supersymmetry operators transform nontrivially under R-symmetry. For example one could have N supersymmetry operators $(Q^1_\alpha, \dots, Q^N_\alpha)$, each squaring to translations and each exchanging bosons and fermions, but the $(Q^1_\alpha, \dots, Q^N_\alpha)$ also transform in the irreducible N-dimensional representation of $SO(N)$ or $SU(N)$.

The *kinematics* of theories with extended supersymmetry - i.e. the construction of field and particle multiplets and Lagrangians - were thoroughly investigated in the 1970's. Those constructions are very beautiful, and build on the great principles of symmetry that dominated 20th century physics. But for some time the investigation of the *dynamics* of these theories lay fallow. That began to change dramatically in 1994 with the resurgence of interest in strong-weak coupling duality symmetries and especiallly with the ground-breaking work of Seiberg and Witten on N=2 Quantum Field Theories [39, 40]. These developments amply demonstrated that theories with extended supersymmetry constitute yet another Goldilocks class of theories which are special enough to admit exact nontrivial results on their dynamics, but general enough to exhibit a host of nontrivial dynamical phenomena. With the benefit of twenty years of hindsight we can see that the promise of the Seiberg-Witten breakthrough is three-fold:

1. First, one can make exact statements about the long distance/low energy behavior of the theory. Especially, one can describe exactly how the massless particles in the theory interact at low energies in the limit of low energies. [4]

2. Second, one can make exact statements about the spectrum of the Hamiltonian for a subsector of the Hilbert space of states called the "BPS subspace."

3. Third, one can make exact statements about path integrals with extended observables known as line defects, surface defects, and interfaces. (Often called line operators, surface operators, and domain walls, respectively.)

One (oversimplified) way to encapsulate Seiberg and Witten's main result is this: They

[4] Technically, one can give exact expressions for the leading (two-derivative) terms in a derivative expansion of the low energy effective action.

studied an N=2 theory with $SU(2)$ gauge symmetry. In addition to the "W and Z bosons"

$$A_\mu = \begin{pmatrix} Z_\mu & W_\mu^+ \\ W_\mu^- & -Z_\mu \end{pmatrix} \tag{1}$$

the extended supersymmetry demands the presence of a complex scalar Higgs field

$$\Phi = \begin{pmatrix} \varphi & \varphi^+ \\ \varphi^- & -\varphi \end{pmatrix}. \tag{2}$$

Just as in the Higgs mechanism of Nature the field Φ develops a vacuum expectation value

$$\langle\Phi\rangle = \begin{pmatrix} u & 0 \\ 0 & -u \end{pmatrix} \tag{3}$$

and spontaneously breaks $SU(2)$ gauge symmetry to $U(1)$ gauge symmetry. [5] Just as in Nature, at low energies, the dynamics is governed by a version of Maxwell's theory. Unlike the case of Nature, in this model it is a version of Maxwell's theory with $N = 2$ supersymmetry. The unbroken supersymmetry has the crucial implication that, unlike Nature where the Higgs VEV is determined to be 246GeV, in this model the Higgs VEV u is *undetermined*, even at the quantum level, and there is hence a *manifold of vacua* $\mathcal{B}$. In the example of $SU(2)$ gauge theory the manifold $\mathcal{B}$ is just the complex plane, but other N=2 theories have more complicated manifolds $\mathcal{B}$ (with some very interesting geometry). Seiberg and Witten showed that the strength of Coulomb's law, i.e. the fine structure constant $\alpha(u)$, is an interesting function on $\mathcal{B}$, and moreover it can be exactly computed in terms of two functions $a(u)$ and $a_D(u)$ using a formula roughly like

$$\alpha(u)^{-1} \sim \mathrm{Im}\left(\frac{da_D(u)}{da(u)}\right). \tag{4}$$

The really exciting part of Seiberg and Witten's discovery was not so much the formula (4) (which was, in some sense, already well-known to experts on extended supersymmetry) but rather the method by which the functions $a(u)$ and $a_D(u)$ could be extracted. It turned out that they are "periods of an elliptic curve."

Briefly, what this means is that we can consider the equation for two complex variables y and z:

$$y^2 = z + u + \frac{1}{z} \tag{5}$$

Since y and z are complex there are four real unknowns in (y, z) and (5) is equivalent to two real equations. So the solution space is a two-dimensional surface. It turns out to be a

[5] Technically, we should actually work with gauge invariant quantities and define $u := \frac{1}{2}\langle \mathrm{Tr}\Phi^2\rangle$. So in a technically correct equation the diagonal matrix elements in (3) are in fact given by the function $\pm a(u)$ appearing in equation (6) below. In the limit of weak coupling/large vev $a(u) \sim \sqrt{u}$. We have written a slightly wrong formula in (3) because our discussion is aimed at nonexperts and we are trying to keep things simple.

(punctured) torus. Moreover, if one chooses the two basic noncontracible curves on a torus A and B then Seiberg and Witten's basic formula is that

$$\begin{aligned} a(u) &= \int_A y \frac{dz}{z} \\ a_D(u) &= \int_B y \frac{dz}{z} \end{aligned} \tag{6}$$

For our purposes here these mathematical details are not particularly important. The really crucial point is that the subject of elliptic curves is a profound and rich subfield of mathematics. Work on elliptic curves goes back to the early 19th century and continues vigorously to this day. For example, elliptic curves played an important role in the ultimate resolution of Fermat's Last Theorem.

This breakthrough nicely illustrates the theme of Physical Mathematics. Clearly, this is a profound advance in our understanding of theoretical physics. Moreover, the beautiful role played by elliptic curves and many related aspects of algebraic geometry illustrates what Wigner called "The Unreasonable Effectiveness of Mathematics in the Natural Sciences." But, then, in the hands of Witten, this physical insight was used to produce new and extremely powerful invariants of four-manifolds, now known as "Seiberg-Witten invariants" [48]. This is a powerful illustration of a converse to Wigner's dictum, namely, the Unreasonable Effectiveness of Physics in Mathematics. Later, drawing on an improved understanding of the relation between Donaldson invariants and Seiberg-Witten invariants [36] it was discovered that the existence of superconformal fixed points imply new results on the topology of four-manifolds [28, 29]. Yet again, a physical insight leads to an unexpected result in mathematics.

Let us return to the three-fold promise of the Seiberg-Witten breakthrough. I would guestimate that there have been well over ten-thousand physicist-years devoted to the intense investigation of four-dimensional N=2 field theories. Nevertheless, the full promise of the Seiberg-Witten breakthrough has not yet been fully realized. The two original Seiberg-Witten papers, and most of the immediate follow-up papers, were primarily focused on results of the first type in the above trichotomy. We can now describe the low energy dynamics for (infinitely) many N=2 theories, though still not for an arbitrary N=2 theory, although progress continues to this day [38]. For the second class of results, some important information was worked out in the 1990's but only for isolated cases. In the past eight years a much more systematic understanding of the "BPS spectrum" of N=2 theories has been elucidated. The third class of results is again a more recent development. I will now turn to a description of some of my own involvement in these two latter kinds of results.

The *BPS states* [6] are certain exact eigenstates of the Hamiltonian whose energy eigenvalues we can compute exactly. We can do this because they are annihilated by some linear combinations of supersymmetry operators, and the supersymmetry algebra then implies

[6] The term was coined by Seiberg and Witten to honor Bogomolnyi, Prasad, and Sommerfield. The importance of BPS states was first recognized by David Olive and Edward Witten in [45]. The term "BPS monopole" is much older and in fact goes back at least to [22]. The relation between these is that a semiclassical description of some BPS states is given by BPS monopoles.

that their energy is given in terms of relatively accessible and computable quantities such as the functions $a(u)$ and $a_D(u)$. For example, in the $SU(2)$ N=2 gauge theory a state ψ with electric charge q and magnetic charge p has a mass given by

$$M(\psi) = |qa(u) + pa_D(u)|. \tag{7}$$

The function $Z_\psi(u) = qa(u) + pa_D(u)$ on $\mathcal{B}$ is known as the *central charge* of the state.

The formula (7) tells us the mass a BPS state of charges $\gamma = (q,p) \in \mathbb{Z}^2$ would have, *provided* such a state exists. It does *not* tell us which charges γ are actually populated in the Hilbert space! The problem of determining which states are in fact populated is the problem of finding the *BPS spectrum*. The second class of results in the above trichotomy concerns finding the BPS spectrum. It is not an easy problem.

An essential feature of the BPS spectrum, which eventually led to the key progress of the past eight years, is that the BPS spectrum is only *piecewise continuous* as a function of $u \in \mathcal{B}$. The essential physical phenomenon here is that two BPS states, say of charges $\gamma_1, \gamma_2 \in \mathbb{Z}^2$ can interact and form a boundstate, which is itself a BPS state. [7] Since the electric and magnetic charges are additive, the new state has electromagnetic charge $\gamma_1 + \gamma_2$, and since [8] $Z_\gamma(u)$ is linear in these charges the boundstate has binding energy:

$$|Z_{\gamma_1}(u) + Z_{\gamma_2}(u)| - |Z_{\gamma_1}(u)| - |Z_{\gamma_2}(u)| \leq 0. \tag{8}$$

We are interested in where, as a function of u, the boundstate can decay. This can only happen when the binding energy is zero, which in turn only happens when $Z_{\gamma_1}(u)$ are $Z_{\gamma_2}(u)$ are parallel complex numbers. The condition of "marginal stability" then becomes:

$$\mathrm{Im} Z_{\gamma_1}(u)\overline{Z_{\gamma_2}(u)} = 0 \qquad \& \qquad \mathrm{Re} Z_{\gamma_1}(u)\overline{Z_{\gamma_2}(u)} > 0. \tag{9}$$

This is one real equation, (and an open condition) and hence describes a wall inside $\mathcal{B}$. [9] If BPS boundstates are going to cease existing as we vary u, they must do it along this wall of marginal stability.

There is a simple physical picture that explains how such BPS states can stop existing as the parameter u is changed. It was independently discovered by several groups [27, 31, 24] and by myself and Frederik Denef [9]. If we take a semiclassical view then some BPS states can be described as dyonic solitons in the spontaneously broken gauge theory. Two such dyons can interact via the electromagnetic field as well as via the Higgs field. When these forces balance a boundstate can form. Denef derived a beautiful formula for the boundstate

[7] This crucial observation has a long history. It was first observed in the context of quantum field theories in $1+1$ dimensions with (two-dimensional) N=2 supersymmetry [6] and was then more deeply investigated in the two-dimensional context by S. Cecotti and C. Vafa [7]. It then played a crucial role in verifying the consistency of the Seiberg-Witten description of low-energy dynamics in [39]. Then M. Douglas and collaborators began studying stability issues of BPS states in string compactifications in [5] and in many subsequent works. See [2] for a review of the progress from this period and its relation to Kontsevich's homological mirror symmetry conjecture [25].

[8] We replace ψ by its electromagnetic charge γ since that is the only relevant aspect for computing Z_ψ.

[9] Technically it is a real codimension one subvariety.

radius in terms of the central charges [8]:

$$R(u) = \langle \gamma_1, \gamma_2 \rangle \frac{|Z_{\gamma_1}(u) + Z_{\gamma_2}(u)|}{2\text{Im}\left(Z_{\gamma_1}(u)\overline{Z_{\gamma_2}(u)}\right)} \tag{10}$$

where $\langle \gamma_1, \gamma_2 \rangle = p_1 q_2 - p_2 q_1$ measures the angular momentum in the electromagnetic field of the dyonic pair. Clearly the boundstate radius should be positive, $R(u) > 0$, and hence that boundstate can only exist when

$$\langle \gamma_1, \gamma_2 \rangle \text{Im}\left(Z_{\gamma_1}(u)\overline{Z_{\gamma_2}(u)}\right) > 0. \tag{11}$$

As the wall of marginal stability is approached the boundstate radius goes to infinity: The boundstate leaves the physical Hilbert space, and the space of BPS states is thus discontinuous as a function of the vev u. This is known as the *wall-crossing phenomenon*.

We would like to understand how to describe the wall-crossing quantitatively. This is difficult in part because now only can BPS states form two-particle BPS boundstates but they can also form multiparticle BPS boundstates. It turns out to be very useful to replace the vector space $\mathcal{H}^{\text{BPS}}_{(q,p)}$ by an integer $\Omega_{(q,p)}$, the so-called *BPS index*, which is a signed sum over the space. Properly viewed, the BPS index is an example of what is known as a *Witten index*[46], so let us take a moment to recall this concept.

Suppose that a Hilbert space $\mathcal{H}$ has a decomposition $\mathcal{H} = \mathcal{H}^0 \oplus \mathcal{H}^1$ (thought of as "bosonic" and "fermionic" spaces) together with a self-adjoint operator which takes the block-form

$$D = \begin{pmatrix} 0 & Q \\ Q^\dagger & 0 \end{pmatrix} \tag{12}$$

so that the Hamiltonian is the positive semidefinite operator D^2. Put differently, we can introduce a "fermionic parity," often denoted $(-1)^F$ which in 2×2 block notation is just

$$(-1)^F = \begin{pmatrix} 1 & 0 \\ 0 & -1 \end{pmatrix} \tag{13}$$

and hence anticommutes with D. The Witten index is then

$$\text{Tr}_{\mathcal{H}}(-1)^F e^{-\beta D^2} \tag{14}$$

and it has the beautiful property that it is independent of β, and furthermore is robust to changes in parameters (such as lengths and couplings) that are used to define D. The essential reason for this is that if a normalizable energy eigenstate ψ_E with $E > 0$ has definite fermionic parity then $\frac{1}{\sqrt{E}} D\psi_E$ is another normalizable energy eigenstate with the same energy E but opposite fermionic parity. This index is closely related to the index of elliptic operators, and more generally of Fredholm operators in mathematics, again establishing a link to the profound and rich subject of index theory in mathematics, which began to flower in the early 1960's.

Although the Witten index is robust against changes of parameters, if a change of parameters of D involves some large change of potential energy for large values of fields (or in mathematical terms, if a change in D violates the Fredholm condition) then the index can change. In the context of N=2 theories this is in fact what happens when u crosses a wall of marginal stability. Using the physical insights described above Denef and I derived a formula for the jump in the BPS index due to many, but not all, kinds of BPS boundstates [9]. Shortly after our paper appeared two mathematicians, Maxim Kontsevich and Yan Soibelman introduced a formula of a completely new type for the change of BPS indices which covers the general case [26]. [10] More importantly, Kontsevich and Soibelman introduced some completely new ideas and techniques into the story. Among other things, they introduced a certain *nonabelian* group, and, for each charge $\gamma = (q, p)$, a group element $K_{q,p}$. They observed that the phases of the central charges $Z_{q,p} = qa(u) + pa_D(u)$ can be ordered in a clockwise or counterclockwise fashion and that the ordering switches from clockwise to counterclockwise as u passes through a wall of marginal stability. They then claimed that

$$\prod_{\circlearrowleft} K_{(q,p)}^{\Omega^-_{(q,p)}} = \prod_{\circlearrowright} K_{(q,p)}^{\Omega^+_{(q,p)}} \tag{15}$$

where $\Omega^{\pm}_{(q,p)}$ are the BPS indices of states of charge (q, p) on either side of the wall.

I think it is fair to say that when equation (15) was announced most workers in the field were astonished and asked *Who ordered that?* In retrospect, we now understand that a very similar formula was already written by Sergio Cecotti and Cumrun Vafa in 1992 for BPS states in 1+1 dimensional QFT's [7], but it took some time for people to appreciate that. In any case, soon after the Kontsevich-Soibelman announcement, I undertook, together with Davide Gaiotto and Andy Neitzke to try to understand the origin of equation (15) in "more physical terms." A very fruitful collaboration resulted [15, 16, 17, 18, 19, 20]. As a result of our project we now have several physically-motivated ways of understanding (15). In one way of understanding the formula, we can see in retrospect that the physical mechanism explained before captures the essential physics of the wall-crossing formula and a systematic implementation of that insight indeed leads to (15) [17, 1]. More importantly, on the journey to this understanding we found many new unexpected results, both mathematical and physical, of interest to a wider community of scientists. Among these were

1. New constructions of hyperkähler manifolds. That is, new ways of constructing solutions to Einstein's equations on certain special manifolds (closely related to the manifold of vacua $\mathcal{B}$).

2. Connections to Hitchin systems and cluster algebras.

3. Connections to the theory of integrable systems.

4. Exact results for line defect and surface defect correlators.

[10] Technically, Kontsevich and Soibelman were working with generalized Donaldson-Thomas invariants associated to a Calabi-Yau category. But these purely mathematical invariants seem to coincide with the physically determined quantities relevant to BPS states. It is another example of the miracles that make Physical Mathematics so lively.

As just one representative example, here is an exact formula for the expectation value of a "supersymmetric Wilson-Polyakov loop." This illustrates a result of the third type in the trichotomy mentioned above.

We consider an $SU(2)$ N=2 gauge theory at finite temperature which in a path integral means that there are three spatial dimensions and the Euclidean time direction is periodically identified with period $\beta = 1/kT$. We consider the holonomy, or Wilson loop, of the gauge field around this circular direction (at any point) in space. More precisely, we consider the path integral with periodic boundary conditions for the boson and fermion fields together with an insertion of

$$\mathrm{Tr}_2 \mathrm{Pexp} \int_{S^1_\beta \times \{\vec{0}\}} \left(\frac{\Phi}{2\zeta} + A + \frac{\zeta}{2}\bar{\Phi} \right). \tag{16}$$

Here ζ is an arbitrary nonzero complex number. We can state the result for the path integral exactly. It can be expressed as

$$\mathcal{Y}_e + \frac{1}{\mathcal{Y}_e} + \mathcal{Y}_m \tag{17}$$

where $\mathcal{Y}_e$ and $\mathcal{Y}_m$ are functions of u, ζ, β and another angle θ. [11] The functions $\mathcal{Y}_e$ and $\mathcal{Y}_m$ can be determined by a system of integral equations [15] which are formally identical to the Thermodynamic Bethe Ansatz equations introduced in the theory of two-dimensional integrable systems by Al. Zamolodchikov [50]. For large values of u (which correspond to weak coupling of the gauge theory)

$$\mathcal{Y}_e \sim e^{\frac{\beta}{2\zeta} a(u) + i\theta_e + \frac{1}{2}\beta\zeta\overline{a(u)}} + \cdots \tag{18}$$

where the corrections as well as the function $\mathcal{Y}_m$, are due to instanton effects, are exponentially small, and are exactly computable. From the mathematical side, the functions $\mathcal{Y}_e$ and $\mathcal{Y}_m$ turn out to have great geometrical significance related to cluster algebras and hyperkähler geometry.

The above result is just one of many from my project with Gaiotto and Neitzke, and that project is just one of many collaborations and projects in a community that has created a wonderful burst of activity surrounding the subject of four-dimensional N=2 theories. There have been perhaps one or two hundred scientists who, for several years now, have been uncovering many delightful and surprising results. Like a beautiful flower which continues to unfold and dazzle, the deeper the probe, the richer the emergent mathematics. In addition to the relations of four-dimensional N=2 theories to hyperkähler geometry, cluster algebras, cluster varieties and integrable systems several other remarkable links to subjects in pure mathematics have been discovered by many mathematicians and physicists. The full list is too long to mention here but some prominent examples include deep relations to geometric representation theory and nontrivial connections with modular tensor categories and two-dimensional conformal field theory.

[11] Technically, $\theta = (\theta_e, \theta_m)$ is a pair of angles given the boundary condition for the holonomy of the unbroken Maxwell gauge field and dual gauge field around the Euclidean time circle as $\vec{x} \to \infty$. This data is part of the specification of the vacuum of the theory.

Looking to the future, it is clear that there is much left to understand about N=2 theories. I will just mention one out of very many directions for future research. The BPS index gives a series of many interesting integers. In general in physics, when a physical quantity turns out to be integral we should look more deeply for an interesting vector space whose dimension (or perhaps graded-dimension) gives that integer, and for mathematical structures related to that vector space. Sometimes this process is called "categorification." An (imperfect) example of this process might be the following. The binding energies of the hydrogen atom are of the form $-\mathrm{Ry}/N$ where $N = 1, 4, 9, \ldots$ is a perfect square and $\mathrm{Ry} = \frac{1}{2}mc^2\alpha^2$ is the Rydberg. [12] It is indeed the dimension of a vector space of eigenfunctions of the Schrödinger operator and one of the key mathematical structures is that it is a direct sum of irreducible representations of $SU(2)$ of dimensions $1, 3, 5, \ldots, 2\sqrt{N} - 1$. In the Quantum Hall Effect, the quantization of the Hall conductance gives the dimension of a degenerate set of ground states when the system has doubly-periodic boundary conditions. So too, we would like to go beyond the BPS indices and understand better underlying vector spaces that lead to these integers. That is one focus of much current research, and there has been some recent progress on the issue [21].

In view of the extraordinary richness of the N=2 theories one might well wonder if there is some simplifying and unifying viewpoint on all the luxuriant connections to mathematics and integrable field theories. It is widely believed by many mathematicians and physicists that there is: 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." Many of the beautiful connections alluded to above 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." Indeed the very mention of the (2,0) theories is greeted by some scientists with an indulgent smile. But many of us take them seriously. An important problem for the future is a deeper understanding and formulation of these theories. [13]

Looking further to the future, we should not forget that the very existence of the (2,0) theory is but a corollary of the existence of string theory. Work on the fundamental principles underlying string theory has noticeably waned. The problem has been put aside - temporarily. But, ultimately, Physical Mathematics must return to this grand question.

Finally, I would like to zoom out, as it were, and return to the much broader viewpoint with which I began. I will conclude by addressing a debate of a more political nature.

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

[12] And we work in the nonrelativistic approximation.

[13] For reviews giving a more extensive explanation of these matters the reader could consult my review talk at Strings2011 in Uppsala, my review talk at the 2012 International Congress on Mathematics and Physics in Aalborg, or my 2012 Felix Klein lectures delivered in Bonn. They are all available on my home page. I would like to stress that there are several viewpoints on this vibrant subject held by several other mathematicians and physicists which are equally if not more valid. For a good example, see the review by Yuji Tachikawa [42].

mathematical rigor. The most reasoned objections along these lines were raised by Jaffe and Quinn in [23]. The excellent responses of Atiyah et. al. and Thurston need not be repeated here [3, 43]. Moreover, the proof is in the pudding: In the years since this debate broke out 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 in 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 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 and atomic structure? 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 which can be tested, agreed upon, and verified. A mathematical discovery can be, in and of itself, a great intellectual achievement. Therefore, I think I can confidently predict that Physical Mathematics will be a long-standing fixture of the intellectual landscape, and is likely to remain an important beacon for progress in both Physics and in Mathematics, for some time to come.

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