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Grand challenges in mathematical physics

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It is no easy task to write in a few lines a comprehensible list of Grand Challenges in the Frontiers of Mathematical Physics. This is such an extensive discipline, comprising at the very least (and non-exclusively, of course): the mathematical formulation of classical mechanics, relevant mathematical aspects of statistical mechanics, hydrodynamics, acoustics, thermodynamics, classical and quantum field theories (QFT), in particular the study of symmetry principles in gauge theories, classical and quantum group theory, string and M theories, mathematical issues in quantum mechanics and quantum information theory, special functions of mathematical physics, distribution theory in physics, the foundations of the so-called axiomatic and algebraic quantum field theory, and other, zeta and other special functions of mathematical physics, classical and quantum chaos, fractals, modern relations between combinatorics and physics, mathematical cosmology, the foundations of lattice field theories, theories involving random matrices, etc. The list could be made longer and longer and yet, it would be absolutely impossible not to still miss many important connections between physics and mathematics where some relevant input by mathematical physicists could be recorded. In short, ours aims at being the widest vision of the field one can imagine, with its enormously rich and overwhelming frontiers, and the grand challenges we are going to list below will necessarily be intersecting with many of the grand challenges of the different disciplines of present day physics and mathematics themselves. This landscape is what I will now try to briefly explore in what follows.

In order to seek inspiration for the challenging future, and for what we can expect or for how things may evolve from now on, I will first throw a view toward the past of the discipline, very briefly highlighting a couple of landmarks and past glorious moments. I bet very few colleagues would now consider Bernhard Riemann to have been a mathematical-physicist, and maybe even less scientists know today that, in his time, Riemann was considered to be a physicist, rather than a mathematician! As is quite well known, to his prodigious genius we owe such fundamental ideas as the extremely useful concept of a multidimensional space, also that of an infinite dimensional one, which endowed with metrics and norms by Banach and Hilbert were the basis for the rigorous mathematical Foundation, and past and present development, of the whole of Quantum Mechanics, and now of string theories (and of almost any theory one can think of). Riemannian geometry, on the other hand, formed the mathematical body and is deeply imprinted in the conception itself of Einstein's General Relativity, the very basis of modern cosmology. Not to speak of the zeta function which brings to mind so many important and diverse applications that I would need lots more space than I here have to properly put it into context. In this retrospect, the works of von Neumann, Dirac, Wigner and many others, that I cannot possibly detail, were utmost crucial in the development of a golden age of mathematical physics [for a partial list of the most classical books see (Whittaker and Watson, 1927; Weyl and Robertson, 1931; Titchmarsh, 1939; von Neumann and Beyer, 1955; Courant and Hilbert, 1989), and for some other relevant references (Reed and Simon, 1972–1977; Margenau and Murphy, 1976; Thirring and Harrell, 1978–1983; Geroch, 1985; Glimm and Jaffe, 1987; Arfken and Weber, 1995; Kato, 1995; Haag, 1996; Arnold et al., 1997; Bender and Orszag, 1999; Morse and Feshbach, 1999; Boas, 2006; Françoise et al., 2006; Abraham and Marsden, 2008)].

Looking now into the future of the discipline in trying to define the Grand Challenges of the Frontiers in Mathematical Physics, and owing to the importance of the numerous interconnections recently having been established in the interface between physics and mathematics (information theory, knot theory, gravity, thermodynamics, hydrodynamics and QFT), we need mention, to start, the string-brane-M theory development. Even if the truly Grand Challenge of the formulation of a theory of everything (TOE), a unification of all interactions through the gauge (and holographic) principles seems to be very hard to reach any soon (Ellis, 1986; Zalsow, unpublished; Smolin, 2006), one should properly recognize the advances obtained already and exemplified, e.g., by the many interconnections already mentioned. This seems to be a very promising path to follow (Weinberg, 1993; Baez and Muniain, 1994; Holloway, 2005; Duff, 2011). The investigation of some most basic issues in the problem of the quantization of fields, as deformation quantization, symmetric spaces, and other, is also very important for the formulation of a final paradigm. Another Grand Challenge that has to do with these developments is the question about Einstein's gravity being or not an emergent theory, specifically, from thermodynamics, and also the emergence of space-time itself. Those are of course very physical and fundamental questions, but in their formulation they have an extremely heavy mathematical content and can be considered to fall, at least in part, in the domains of the mathematical-physics of the future.

That we have started with "stringy" concepts does not at all mean that these

Challenges will have any priority for Frontiers in Mathematical Physics, nor the contrary, at any rate. Our aim is actually to try, once and for all, to overcome this artificial division into "string" and "non-string" mathematical physicists, or any other kind of classification or subdivision whatsoever. The interconnections already mentioned do not address some other, very important Challenges in the Frontiers in Mathematical Physics as, say, in hydrodynamics those having to do with turbulent flow and the resolution itself of the Navier-Stokes equation. Also, those associated grand problems as climate prediction, large scale oceanic currents, construction of a mathematical model to describe glass physics, plasma physics, the proper modelization of the Solar magnetic field, etc. Moreover, in quantum information theory, the paradigm of the loss of information in black holes will need further attention. Moreover, the quantization of gravity could be approached by other theories, as loop quantum gravity, involving heavy mathematical methods as well, random partitions (triangulations), and theories on the lattice.

Furthermore, in mathematical cosmology we easily identify a number of Grand Challenges having to do with key fundamental issues, one of them being the feasibility of alternative mathematical formulations of gravity (modified gravity theories) at very large scales as true (or at least essentially more approximate) descriptions of our universe. Another Grand Challenge here is to find a mathematical model for the very origin of the universe, a final inflationary paradigm, etc., once the initial (mathematical) singularity idea has been declared obsolete since it cannot correspond to the ultimate physical answer, owing to quantum corrections (and, possibly, unknown new physics) we are sure to encounter when we approach (and go further below) the Planck scale. By the way, the mathematical analysis of future singularities and their quantum corrections in models for the evolution of our universe also constitute an essential field of study.

Some important Grand Challenges have to do with the quantum Hall conductance, Ising, Hubbard, Potts, sigma, and O(N) models, and other models (exponents and dimensions, extended states), entropy production, the quantum Heisenberg ferromagnet, spin glasses, Bose-Einstein condensation, and the Gross-Pitaevskii equation. Also, several different problems in random matrix theories constitute a very challenging field of study for the next future. The consideration of Grand Challenges associated with the zeta function, the most famous one of them, i.e., the Riemann conjecture, having been declared the probably most important problem in mathematics ever, bring us to consider its crucial use in physics as a regularization tool in QFT, its importance in quantum chaos, in particular when considering the non-trivial zeros as corresponding to a physical dynamical system, and its possible use toward developing a physical approach to solve the Riemann conjecture itself. Also, the extension, by Selberg, Ruelle and many others, of the concept of zeta function in order to solve genuine problems of the dynamical systems themselves. In the study of quantum vacuum fluctuations and many different spectral problems, zeta functions and other special functions of mathematical physics also play a fundamental role. A proper renormalization, which we still do not have, of this vacuum energy, could lead to a feasible resolution of the problem of the cosmological constant. More mathematical Grand Challenges have to do also with the existence and mass gap of Yang-Mills theories.

Other Grand Challenges of the Frontiers in Mathematical Physics are in fact double-faced, since they appear to be as important for mathematics as they are for physics, like the *P* vs. *NP* issue, the complexity conundrum, catastrophe theory modeling and physics, to finally reach the extremely appealing Grand Challenge of the proper modelization of the process of consciousness and, before that, of other extremely important biological processes at the level of genomics, proteomics and the cell, the treatment of illnesses, etc., not to forget the intriguing issue of artificial life. To wit, a number of mathematical physicists are involved in these studies with interesting results already. We could still go on, and I am also pretty sure a number of colleagues will blame me for having forgotten this or that issue, as important at least as the ones I have mentioned here. I apologize in advance for these non-deliberated omissions.

It is time to summarize by saying that any interesting work on a subject having to do, in the most general sense of the concept, with mathematical physics will be seriously considered for publication in Frontiers in Mathematical Physics.

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