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PHYSICS AND MATHEMATICS:
A HAPPILY EVOLVING MARRIAGE?

by Gabriele VENEZIANO

Abstract. – The interplay of physics and mathematics started affecting my student life some forty years ago, and has remained a personal struggle since.

1. High School Days

Forty years ago, when the IHÉS was created, I was in my third year as a high school student in Florence. I was doing well in most subjects, typically through hard work, and, with almost no effort, I was doing particularly well in maths and physics. These two subjects were taught, in Italian high schools, by the same Professor. Very luckily, my class had been assigned to an excellent teacher whose name I still remember: Prof. Liverani.

I recall very well how he gave me the feeling he loved those two subjects and how he could transmit this love to (some of) us. I was then surprised when, one day, very possibly in 1958, I heard him make, apologetically, a confession to his class (recorded here as I remember it today...):

I wish I would love physics as much as I love maths, but I can't.
I am sorry if this is reflected in the way I teach them.

He was really a modest man! In 1960, when I graduated from high school, Prof. Liverani tried to convince my parents that I should continue my studies at the Scuola Normale Superiore in Pisa, and, of course, in mathematics. This was my first struggle between physics and maths. In spite of Prof. Liverani's advice, during the summer of 1960 the decision was taken: I would register as a physics student at the University of Florence! Prof. Liverani was disappointed, of course. Many years later, my mother once told me, she met him accidentally in the street. He had read that his former student had made a career in theoretical physics and was very happy...

2. University of Florence

After three years of study in the physics Department – which I still remember as an incredible relief from the hard high school studies in Latin, history and the like – I was ready to look for a thesis advisor. Another lucky coincidence occurred: Professor Raoul Gatto had just moved from Cagliari to Florence taking with him a crowd of (ex) students of his from the University of Rome. Among them there were names now familiar to all of us: Altarelli, Gallavotti, Maiani. I asked Gatto to become his student and he agreed to give me a chance.

The following three years spent in Gatto's group were most crucial in forming me as a theorist. The question of what an optimal relationship between theoretical physics and maths should be was coming repeatedly to my mind. How much time should I devote to study physics; how much to learning maths? In those days group theory was important in order to follow what was going on. They were the years of $SU(3)_F$, of $SU(6)_W$, of $\tilde{U}(12)$. Obviously, Gatto and his group were at hand with those techniques but, I think, there was a healthy distance between us and group theory. Again, as with Professor Liverani, a sentence by Prof. Gatto got stuck in my memory. It was something like:

A good theorist should not study maths, should just know it!

In other words, physics was always at the forefront of theoretical research in Gatto's group, but, of course, mathematical rigour (by physics standards) was demanded. Mathematics was seen by us just as a tool, a necessary instrument, but, as a painter should not fall in love with his brush, we were not supposed to fall in love with the mathematics involved in our physical problem, or to divert too much our attention from the latter. With these concepts in mind I left for Ph. D. studies at the Weizmann Institute in Rehovot, Israel, in the fall of 1966. I was expecting to find there a similar approach to theoretical physics in Haim Harari, Harry Lipkin, Hector Rubinstein ... and I did.

3. Weizmann Institute

At first, my graduate-student work at the Weizmann Institute followed lines similar to those of my research in Florence. A turning point took place in the summer of 1967, in Erice's summer school, where Murray Gell-Mann was giving some very inspiring lectures. In one of them he mentioned, "en passant", a recent phenomenological observation by Dolen, Horn and Schmid, noticing a "duality" between the *s*-channel-Resonance and the *t*-channel-Regge-pole description of pion-nucleon scattering. It occurred to me that, if this was a general feature of strongly interacting systems, a similar duality had to occur in "gedanken" meson-meson reactions which had the theoretical advantage of exhibiting the same quantum numbers in both the *s* and *t*-channels. This would then lead to some self-consistency (or bootstrap) conditions which could eventually determine physically measured (or measurable) quantities.

Passing through Florence from Erice, I managed to get Marco Ademollo involved in this idea. Back at Weizmann, Hector Rubinstein and Miguel Virasoro enthusiastically joined in. During the academic year 1967-1968 our "gang of four" found that the Resonance-Regge duality bootstrap could be made to work to very high accuracy, much better than what one could have anticipated. Furthermore, the emerging picture was quite simple: mesons had to lie on linearly rising parallel Regge trajectories with a universal slope $\alpha' \sim 1\text{GeV}^{-2}$.

This "empirical" conclusion made me think that a simple, closed mathematical formula, encoding all those results, had to exist. Indeed, we did have something like a closed formula, but it was describing the *imaginary part* of an analytic function and was not particularly suggestive. In retrospect, the crucial step which had to be made was to consider the scattering amplitude itself, truly an *analytic function*, and not just its imaginary part. Once this was done, Resonance-Regge duality became Resonance-Resonance duality in the $s - t$ channels, i.e. crossing symmetry in $s \leftrightarrow t$. The scattering amplitude had just to be symmetric in the exchange of s and t, meromorphic (to describe narrow resonances), and Regge-behaved on the average, i.e. away from its poles. There was only one such function fitting all the requirements: it was Euler's Beta-function!

One of the things that upsets me most these days is to hear that I found the Beta-function ansatz almost by chance, perhaps while browsing through a book on special functions. I hope it is clear from the above account that there is nothing further from the truth. Had one not found a simple solution for the (average) imaginary part, and recognized the importance of imposing crossing on the full scattering amplitude, the Beta-function would have stayed idle in maths books for some time...

4. Dual Resonance Models at MIT

Having managed to express/interpret a year of surprising results via a simple and elegant formula was a clear sign of being on an interesting track. Soon after having published my paper at CERN in the summer of 1968, I left for the US for my first post-doc: none less than Sergio Fubini, Francis Low and Steven Weinberg had offered me the position at the Center for Theoretical Physics of MIT six months earlier...

Although I had frequent discussion with all three, Sergio Fubini was to become my real mentor. I had already met Sergio on a couple of occasions before moving to the US and he had made an enormous impression on me. I must have made some on him too, since he had been, so I understood, the one who had insisted most for offering the post-doctoral position to this unknown young man.

We wasted no time before starting to work on what we baptized "Dual Resonance Models" (DRM), after the original Beta-function had been generalized to represent multi-particle reactions. The problem was to extract new physical information from those mathematical formulae. Sergio was a master at that and, perhaps for the first time as a theorist, I felt some difficulty in catching up with a collaborator. Sergio was really a volcano of physical and mathematical ideas. Almost everyday he would come up with some brilliant new insight into the problem at hand. Sometime it would be an interpretation of a mathematical result but, equally frequently, it would be a brilliant mathematical trick. I don't think that Sergio's mathematical culture was/is exceptional by modern theorists' standards. I rather think that he had/has an innate mathematical ingenuity allowing him to make mathematical guesses that later turn out to be correct (some of my younger collaborators say something similar of my own physics guesses).

With Sergio I understood that mathematics can be more than a tool: it can be an essential part of the process by which one makes progress in theoretical physics. For that to happen it is not enough to "know" maths, I believe, one has to be able to "invent" it while the physical problem unwinds. I believe that, in spite of what Professor Liverani thought, I do not quite have that quality at the necessary level. Sergio does. The nice complementarity I found in Sergio's qualities may explain the level of the work we did together during my first two years at MIT (1968-1970) while the structure of DRM was unravelled. Together, we understood the high degeneracy of the levels, the mechanism for decoupling ghosts, the algebraic structure underlying this mechanism, which brought us a small step from finding the "Virasoro Algebra", etc. In the following two years, while I moved into more phenomenological directions, Sergio kept working on the construction of physical states (the so-called DDF states) in order to try to prove a no-ghost theorem, i.e. positive-definiteness of the norm of physical DRM states.

I mention this particularly because of an amusing episode. At some point, people in the MIT maths department heard of the theoretical work Sergio and I were doing, that it contained some amusing maths, and decided they wanted to hear about it. We thus gave two or three lectures to them about vertex operators, Virasoro algebra, physical states, etc. We also presented to them a well-defined mathematical problem: proving the above-mentioned positivity of the norm of the space of physical states. As we now know, the no-ghost theorem was proven later by Richard Brower and, independently, by Peter Goddard and Charles Thorn, i.e. by three physicists, not by mathematicians. I am not sure which is the lesson to be drawn here. Does physical intuition help solving (some) mathematical problems? I tend to believe so, but I am not really qualified to answer such kind of questions.

5. Back to Phenomenology and QCD

Going back to my last two years at MIT (1970-1972), I shifted more towards phenomenology with DRM, especially in connection with some interesting data coming from multiparticle production in high energy hadronic collisions. They could be interpreted according to Al Muller's generalization of Regge theory. Since DRM had Regge-Mueller behaviour I wanted to test those ideas, but there was an important obstacle: in order to describe multiparticle production one had to add loops to the original tree-level DRM amplitudes. Although it was in principle known how to construct DRM loops, their expression was not very manageable and thus I proceeded more phenomenologically. I worked quite intensively on this problem for a number of years (including my first two years back in Rehovot, 1972-1974) with some success, but no really startling results. With hindsight, I had the right physical intuition, but I missed a crucial mathematical trick for making what I was doing more systematic, simpler, and cleaner: the idea of a 1/N expansion. I had to wait till 't Hooft's 1974 paper before realizing that this is what I had been implicitly doing for years. I had recognized intuitively the physical importance of diagram topology, but had not realized that a 1/N expansion was the way to do the proper book-keeping both in Quantum Field Theory and in DRM. Again, alas, my mathematical ingenuity had been proven not to match my physical intuition!

The work of 't Hooft was teaching not only how to make my considerations more systematic, it was also showing how to get the DRM duality diagrams from the 1/N expansion of Quantum Chromodynamics (QCD). There were already indications that QCD was superior to DRM for describing strong interactions; however, there were also good points in favour of the DRM approach, including the approximate validity of the narrow-resonance approximation and of linear Regge trajectories. Once 't Hooft had shown how QCD could easily get that as well, I became immediately convinced that QCD was the right description and that Nature had just tricked us into believing in DRM as a fundamental new theory, while it was just an effective description of QCD at large distance (meanwhile DRM's had been reformulated as a theory of strings and QCD, in the large-distance regime, was expected to produce string-like objects because of quark-gluon confinement). I then generalized 't Hooft's work and showed that DRM and Regge-Mueller-Gribov theories all emerge from different 1/N expansions in QCD. This is an example of how having (missing) the right mathematical setup can considerably speed up (slow down) progress in theoretical physics. By 1976 I was working full time in QCD: jet physics, U(1) problem, supersymmetric extensions... and I did not get back to strings until, after the Green-Schwarz revolution in 1984, they appeared once more to have a chance of being physically relevant.

6. The Superstring Era

The last 14 years of (super)string theory are an example of a perfect marriage between theoretical physics and mathematics. I think that never before have we witnessed such a cross-fertilization between these two fields of human knowledge. Other authors in this volume will certainly describe this interplay at length and in depth; I will refrain from doing the same. I will limit myself, again, to some personal – and necessarily biased – remarks connected with those of my previous discussion.

The mathematics of strings is wonderful, fascinating: most fields of modern maths, from algebraic topology to number or knot theory, find in string theory a perfect arena. Other areas of mathematics advanced as a result of developments in string theory. Mathematicians look at string theory for inspiration: while visiting the IAS in Princeton, recently, I met two mathematicians who were there to edit some notes from Ed Witten's lectures for mathematicians. If string theory were to turn out to have nothing to do with Nature (which I refuse to accept) it would still go down in history as a gigantic achievement in mathematical physics.

My discussion here will be at a different level. What kind of evolution did I witness, through thirty years of string theory, of the relationship between theoretical physics and mathematics? In the late 60's I would call that relation "Gatto-like": mathematics was just a useful tool allowing us to express in an elegant, concise way the outcome of a long process of physics-driven research. In the early 70's it shifted to "Fubini-like": mathematics had to be used "on-line", or even invented on the spot according to the problem ahead of us. With superstrings of the 80's and 90's we seem to have entered a new phase that can be termed "Witten-like": mathematical rigour and elegance are becoming a driving force, a true guiding principle for theoretical research.

There are at least two valid reasons for that. The first is that we learn from past experience: most of the advancements in string theory have been triggered by formal/mathematical developments which allowed us, each time, to gain further insight into its physical content. The second has to do with the evolution of particle physics experiments: these have become more and more complex, costly and, therefore, rare. Furthermore, the Standard Model of non-gravitational phenomena has proven so successful that, so far, no real evidence for new physics (with the possible exception of neutrino masses) has been found. Lacking the goal of explaining new data, theorists turn to other criteria for checking the validity of their work: mathematical consistency is (and has always been) one of those; but also mathematical sophistication and elegance is taken as such. Theoretical research is becoming (in this field) increasingly mathematics-driven.

I am almost happy with this: however, I cannot refrain from closing with a slightly critical note. The reasons for resurrecting strings after about ten years of oblivion were genuinely physical: the urge to add gravity to the standard model in a way consistent with quantum mechanics and general relativity, and also, perhaps to a lesser extent, the desire to eliminate infinities from Quantum Field Theory. After 14 years of superstrings many mathematical properties were exposed, but not much progress has taken place on the physical issues which should find their solution in any consistent treatment of quantum gravity. At a rather low-brow level one can ask how the singularities of classical general relativity are cured in string theory. At a more conceptual level we may ask whether the principles of quantum mechanics itself can/should be reformulated when space and time themselves become quantum. There are hints that new forms of the uncertainty principle hold in string theory, and that some non-commutative geometry replaces standard geometry below a certain quantum scale.

Only a small fraction of the effort made today in string theory aims in those directions. Why? Are those problems uninteresting? Certainly not! When E. Witten wanted to explain to the public at large the importance of string theory (*The New Republic*, Dec. 29, 1997) he mentioned just one issue that string theory can hope to answer: the origin of space and time at (or before) the Big Bang. But these are hard, physical problems, whose solution may need many years of unsuccessful attempts. In that process we will have to dirty our hands, use heuristic, non-rigorous arguments – and possibly dubious mathematics – before, slowly, the correct path emerges and a proper mathematical framework appears. Most fundamental discoveries in physics came that way (quantum mechanics is an example, general relativity being the obvious counterexample), pushed by apparently insurmountable problems. We do have such problems in gravitational physics: the singularity problem, the cosmological constant, the information paradox, the quantum description of the Universe as a whole, etc. My feeling is that we should encourage long-term investment in those problems. I have recently expressed this by saying, at the risk of making a few enemies:

Let us invent
tools that suit our problems
rather than
problems that suit our tools.

This, I think, is where the subtle border between a healthy and an unhealthy marriage of physics and mathematics actually lies.

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