Gabriele Veneziano: A Concise Scientific Biography and an Interview

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Abstract. The aim of these notes is to present a broad brush profile of the scientific activity of Gabriele Veneziano, whose wide spectrum of interests and variety of contributions to fundamental theoretical physics is also reflected by the articles of his collaborators and friends in this book. We thank Gabriele for his kind help in preparing these notes, and for disclosing to us some aspects of his life that we were not aware of. The responsibility of any omission and imprecision will rest on the authors, of course, and we apologize in advance for the (unavoidable) incompleteness of Sect. 1, warning the reader that a full survey of all of Gabriele's activities is outside the scope of this introduction. Finally, we thank Gabriele for his patience in answering our questions that made possible the interview reported in Sect. 3 where, starting from the evocation of his past experience, he illustrates his personal point of view on the present status of fundamental physics, and his expectations for the future.

1 Biographical Notes

Gabriele Veneziano was born on September 7, 1942 in Florence (Italy). After completing his high-school studies (at the Liceo Scientifico "Leonardo da Vinci," Florence) he entered the University of Florence in 1960, where he started studying physics. He took his degrees (Laurea in Fisica) in 1965 defending a thesis on the applications of group theory to strong interactions, under the supervision of Professor Raoul Gatto. A short paper extracted from his thesis became his first scientific publication [1] (here, and in what follows, the quoted numbers refer to the list of publications of Gabriele Veneziano reported at the end of this chapter).

After graduating he won a scholarship of Angelo della Riccia to carry out research in the group directed by Raoul Gatto, who had gathered in Florence a number of brilliant young theorists (like Guido Altarelli, Franco Buccella, Giovanni Gallavotti, Luciano Maiani, and Giuliano Preparata, to

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mention a few). During that period he wrote a paper on saturation of current algebra sum rules [5] that attracted the attention of Professor Sergio Fubini. Meanwhile (after a conversation with Professor Giulio Racah) he had decided to continue his studies toward a PhD, choosing to apply to the Weizmann Institute of Science in Rehovot (Israel). In July 1966 he got married to Edy Pacifici and, after their honeymoon in Venice, they moved together to Israel.

His official advisor at the Weizmann Institute was Professor Harry J. Lipkin; however, his research activity was mainly carried out under the supervision of Professor Hector Rubinstein (see the contribution of Hector Rubinstein to this volume). In Israel he quickly completed his PhD studies, getting the degree at the end of 1967 (see Fig. 1). The PhD thesis was largely based on his research with Rubinstein and on work done in collaboration with Marco Ademollo (professor in Florence and visiting Harvard at that time) and Miguel Virasoro (who had joined the Weizmann group in the spring of 1967). That work developed important ideas initiated by Sergio Fubini and collaborators on a bootstrap approach to strong interactions based on "superconvergence" and "duality" (see, e.g., [13, 18]).

At the beginning of 1968 he was offered several post-doctoral positions in the United States, and he decided to accept the invitation of MIT (Boston) to join the newly formed Center for Theoretical Physics to which Sergio Fubini and Steven Weinberg had recently moved. Before starting the MIT appointment he spent the whole summer at the TH Division of CERN, where he completed the celebrated paper "Construction of a crossing-symmetric, Regge

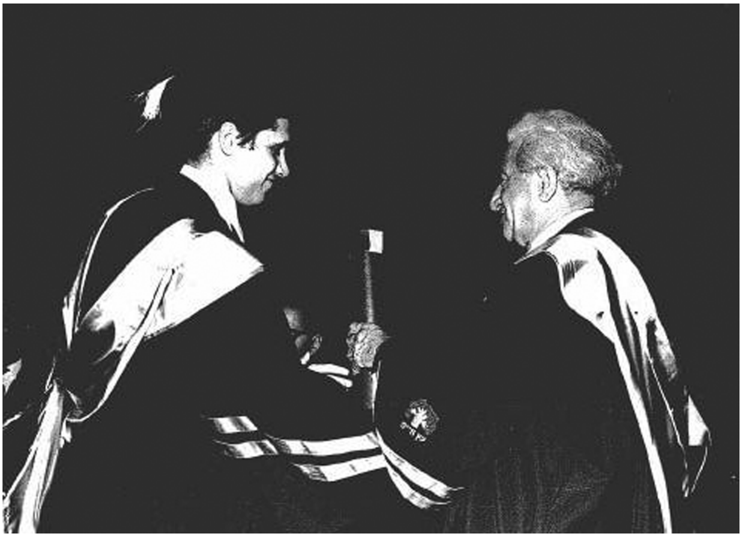

Fig. 1. Gabriele Veneziano (left) receiving his PhD diploma at the Weizmann Institute of Science (Rehovot, 1967)

behaved amplitude for linearly-rising trajectories" [20], in which he proposed the scattering amplitude that bears his name, and that is usually regarded as marking the birth of string theory. The model presented in his seminal paper incorporated most of the desired ingredients of an S-matrix theory of strong interactions, and it was largely quoted at the Vienna Conference on High Energy Physics, at the end of that summer.

At MIT he mainly worked with Sergio Fubini to develop generalizations of his earlier work that became known as "dual resonance models." Their work paved the way to the re-interpretation of such models as a theory of strings. In fact, some of the crucial features of string theory, such as the exponential degeneracy of the states [24, 25], the concept of "Fubini–Veneziano" vertex operator [28], and the algebraic structure underlying the Virasoro operators [31], were introduced by them in that period (see the contributions of Paolo di Vecchia, Adam Schwimmer, and Miguel Virasoro to this volume). In that period he also spent a summer at the Lawrence Berkeley Laboratory (California), where he contributed to an influential paper on the "twist" operator [26].

After the birth of his son Ariel (September 1970), and a one-term visit at the Institute for Advanced Studies in Princeton, he undertook a program invoking topological ideas in order to implement unitarity in the context of dual resonance models [30]. This led, in particular, to a model for the "Pomeron" [49], later developed by other researchers into the so-called dual parton model.

In 1972 he came back to the Weizemann Institute as a full professor. In the subsequent 4 years he also spent extended periods at CERN, pursuing the development of the topological unitarization ideas, meanwhile interpreted by Gerard 't Hooft as a $1/N$ expansion [53].

In 1976 he joined the TH Division of CERN, first as a scientific associate, then as a junior staff member (1977–1978), and, finally, as a senior staff member. Later he became Head of the TH Division (1994–1997). The beginning of this period was marked by the birth of his daughter Erika (July 1976), and by a change of direction of his research interests.

He started to work, in particular, on large N expansions in quantum chronodynamics (QCD) [62], its applications to baryon dynamics [64], and Bose–Einstein effects in jet physics [67] (see the contribution of Alberto Giovannini to this volume). Together with Daniele Amati and Roberto Petronzio, he proved the factorization theorem on collinear singularities in perturbative QCD, which forms the basis of the QCD parton model [69, 70] (see the contribution of Roberto Petronzio to this volume). This brought him naturally to devote his activity to the physics of QCD jets, writing some seminal papers with Kenichi Konishi and Akiwa Ukawa [71, 72] (the KUV jet calculus), and with Daniele Amati [74] (pre-confinement) (see the contributions of Marcello Ciafaloni and Giuseppe Marchesini to this volume).

Turning his attention to non-perturbative aspects of QCD, he tackled the $U(1)$ axial problem for a $1/N$ perspective, arriving at the celebrated (and even recently confirmed) Witten–Veneziano formula [77]. Related studies led to an estimate of the electric dipole moment of the neutron induced by a non-vanishing QCD θ-angle [78]. These results were encoded into an effective Lagrangian formalism developed with Paolo di Vecchia [79].

The effective Lagrangian formalism was later applied to supersymmetry (SUSY) Yang–Mills theories [93] and SUSY QCD [95], where the non-perturbative breakdown of non-renormalization theorems was first suggested. The superpotentials derived in those papers, in collaboration with Thomasz Taylor and Shimon Yankielowicz, are still being widely used and cited (often under some other names) in many contexts. The indications of those papers were confirmed by explicit calculations that he later performed with Giancarlo Rossi and collaborators, and that are summarized in [119] (see the contribution of Giancarlo Rossi to this volume). In that period he also pointed out the possible formulation of SUSY Yang–Mills theories in the lattice, and suggested an implementation [115] that is still being attempted.

When string theory was recognized as a promising candidate to unify gravity and gauge interactions (i.e., after the so-called Green–Schwarz revolution in 1984) he came back to the theory that he had to abandon (not without regret) when it appeared inappropriate as a theory of strong interactions. His studies (with various collaborators, including Amit Giveon, Jnan Maharana, and Eliezer Rabinovici) first concentrated on the following directions: the physical consequences of a fundamental length [111], the emergence of new field-theoretic and "stringy" symmetries [113] (see the contributions of Jnan Maharana and Eliezer Rabinovici to this volume), the possible phenomenological consequences of a light dilaton [125], and a background field approach to the study of the T-duality symmetry [133].

A more substantial activity in that period concerned the study of *gedanken* experiments on trans-Planckian string collisions, in collaboration with Daniele Amati and Marcello Ciafaloni [120]. The main purpose of such studies was the understanding of how string theory may reproduce general-relativistic results at large distances, while providing important corrections at string-size distances. The works possibly have applications to an effectively modified uncertainty principle [138] and to the problem of "information loss" in black-hole physics (see the contribution of Daniele Amati to this volume).

While working on string theory he kept alive his interest in the subject of strong interaction phenomenology, producing works on the "spin of the proton" puzzle [132] (see the contribution of Graham Shore to this volume), and on semi-inclusive hard processes [167] (see the contribution of Luca Trentadue to this volume).

Triggered by his wish to find novel applications of string theory (and new possible ways to test it), he then turned his interest toward primordial cosmology and its theoretical and observational challenges. Starting from the study of duality symmetries in cosmological backgrounds [148, 149, 151] (see the contribution of Krzysztof Meissner to this volume) he proposed, in collaboration with Maurizio Gasperini, the so-called pre-big bang scenario [161], which attracted considerable interest in the astrophysical community, stimulating the studies of new mechanisms of inflation (see the contribution of Maurizio Gasperini to this volume). The multiple implications of this scenario were the object of many subsequent studies with various collaborators (see the contributions of Alessandra Buonanno, Thibault Damour, Massimo Giovannini, and Carlo Ungarelli to this volume). Of particular relevance were the phenomenological predictions concerning the generation of magnetic seeds [175], the enhanced production of primordial gravitational waves [178], and the possible axionic origin of the cosmic microwave background (CMB) anisotropy [225], opening a unique observational window on string/Planck-scale physics.

Encouraged by the possibility of concrete experimental verifications of such a string cosmology scenario, he and Maurizio Gasperini also tackled the problem of understanding (or re-interpreting), in such a context, the big bang singularity, by applying either quantum cosmology techniques (in collaboration with Jnan Maharana [185]), or higher-order string corrections (in collaboration with Michele Maggiore [190]), or non-local effects of the quantum back reaction (in collaboration with Massimo Giovannini [235]). The study of the high-curvature, strong-coupling regime (also appropriate to brane inflation, see the contribution of Henry Tye to this volume) led him to obtain, as a by-product, unexpected results on entropy in collaboration with Ram Brustein [207, 213] (see the contribution of Ram Brustein to this volume), and unexpected connections with black-hole physics [211, 240], in collaboration with Thibault Damour. Later developments of the pre-big bang scenario also led to interesting (and testable, in principle) interpretations of the presently observed cosmic acceleration [219].

His most recent interests are mainly focused again on the $1/N$ expansion, with two different ramifications. The first concerns a new version of such expansion, capable of connecting QCD to supersymmetric theories [236], developed in collaboration with Adi Armoni and Mikhail Shifman. The obtained predictions for one-flavor QCD, in particular, have been confirmed by subsequent (phenomenological or lattice) computations (see the contribution of Adi Armoni and Mikhail Shifman to this volume). The second, developed in collaboration with Enrico Onofri and Jacek Wosiek, deals with a Hamiltonian approach to large N dynamics, which, while still limited to quantum mechanics, has already produced interesting results in different branches of mathematical physics, like combinatorics and statistical mechanics [246].

Since 2004 he holds the prestigious Chair of Elementary Particles, Gravitation and Cosmology at the Collège de France, in Paris (see Fig. 2).

We give below a schematic summary of his professional career, his administrative appointments at CERN, his positions and associations, and his prizes and honors.

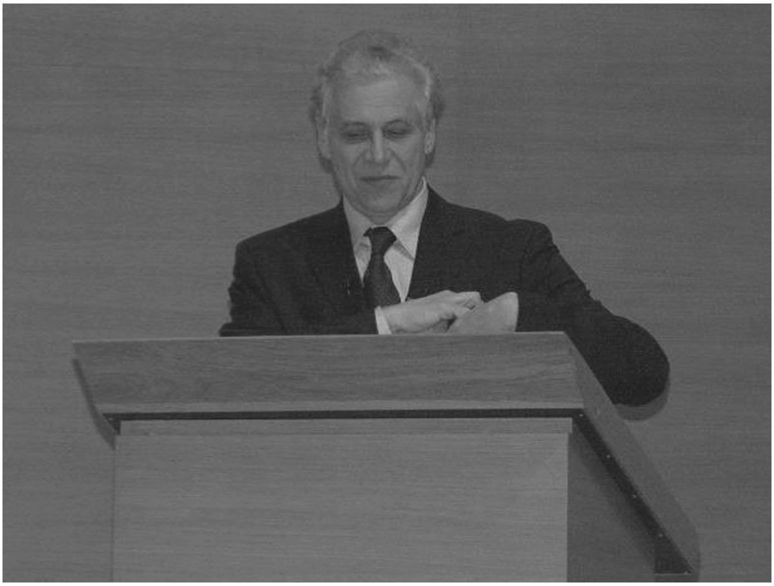

Fig. 2. Gabriele Veneziano giving the Inaugural Lecture at the Collège de France. Paris, February 17, 2005 (Photo Suzy Vascotto)

1.1 Professional Career

- Research Associate at MIT, Cambridge (USA), 1968–1969
- Visiting Assistant Professor at MIT, 1969–1970
- Visiting Associate Professor at MIT, 1970–1972
- Full Professor at Weizmann Institute of Science, Rehovot (Israel), 1971–1975
- Amos-de Shalit Professor of Physics at Weizmann Institute of Science, 1975–1977
- Junior Staff Member at CERN, TH Division, Geneva, 1977–1978
- Senior Staff Member, CERN, Geneva, 1978–2207
- Head of Theory Division, CERN, Geneva, 1994–1997
- Professor at Collège de France, Paris, since 2004

1.2 Administrative Appointments at CERN

- Member of the SPS (Super Proton Synchrotron) Committee, 1983–1986
- CERN representative to Plenary ECFA (European Committee for Future Accelerators), 1987–1990
- Chairman of the Academic Training Committee, 1990–1994
- Division Leader of the Theory Division, 1994–1997

- Member of the Scientific Information Policy Board, 1997–2000
- Member of the Archives Committee, 2001–2004
- Chairman of the Pauli Committee, 2003–2007

1.3 Positions and Associations

- Recipient of a Chaire Condorcet at LPTENS (Laboratoire de Physique Théorique de l'Ecole Normale Superieure), Paris, 1994
- Co-director (with Gerard 't Hooft and Antonino Zichichi) of the International School on Subnuclear Physics in Erice, Sicily, 1996–2001
- Recipient of a Chaire Blaise Pascal at LPT (Laboratoire de Physique Théorique), Université Paris Sud, Orsay, and IHES (Institut des Hautes Etudes Scientifiques), Bures-sur-Yvette (France), 2000–2002
- Academic Staff Member at Kavli Institute of Theoretical Physics, University of California, Santa Barbara, 2003
- Chairman of the Advisory Committee of the Galileo Galilei Institute in Arcetri (Italy), since 2005
- Member of Accademia delle Scienze di Torino (Italy), since 1994
- Member of Accademia Nazionale dei Lincei, Roma, since 1996
- Member of Académie des Sciences of the Institut de France, Paris, since 2002

1.4 Prizes and Honors

- I. Ya. Pomeranchuk Prize, ITEP, Moscow (May 1999).

  Motivation: "*For his outstanding contributions to quantum field theory and theory of strings.*"

- Gold Medal of the Italian Republic (Diploma di prima classe riservati ai Benemeriti della Scienza e della Cultura), Rome (June 2000).
- Dannie Heineman Prize of the American Physical Society (May 2004).

  Motivation: "*For his pioneering discoveries in dual resonance models which, partly through his own efforts, have developed into string theory and a basis for the quantum theory of gravity.*"

- Enrico Fermi Prize of the Italian Physical Society (September 2005).
- Einstein Medal of the Albert Einstein Gesellschaft, Berne (June 2006).

  Motivation: "*The laureate has made significant contributions to the understanding of string theory.*"

- Commendatore dell'Ordine al Merito della Repubblica Italiana (February 2007).
- Oskar Klein Medal of the Swedish Royal Academy of Sciences, Stockholm (June 2007).

2 List of Collaborators of Gabriele Veneziano (Updated to 2006)

Here we report, in alphabetical order (and to the best of our knowledge), all authors who have published a paper in collaboration with Gabriele Veneziano. Their number is impressive (for a theoretical physicist), and we apologize in advance for any possible omission.

M. Ademollo	D. Amati	A. Armoni
R. Barbieri	A. Bassetto	M. Bishari
V. Bozza	V. Branchina	R. Brustein
F. Buccella	A. Buonanno	L. Caneschi
M. Ciafaloni	R. Crewther	G. Curci
T. Damour	A. C. Davis	V. De Alfaro
D. De Florian	E. Del Giudice	C. DeTar
A. Di Giacomo	P. Di Vecchia	M. J. Duff
R. Durrer	S. Elitzur	M. Fabbrichesi
K. Fabricius	S. Ferrara	V. Ferrari
S. Foffa	D. Freedman	J. Freeman
S. Fubini	G. Furlan	E. Gabathuler
M. Gasperini	R. Gatto	A. Giovannini
M. Giovannini	L. Giusti	A. Giveon
D. Gordon	A. Ghosh	D. Graudenz
M. Grazzini	M. B. Green	N. S. Han
R. Iengo	C. E. Jones	F. Karsch
E. Kohlprath	K. Konishi	T. Kubota
G. Longhi	F. E. Low	R. Madden
M. Maggiore	N. Magnoli	J. Maharana
G. Marchesini	A. Masiero	K. A. Meissner
A. Melchiorri	Y. Meurice	V. F. Mukhanov
S. Narison	F. Nicodemi	S. Okubo
L. B. Okun	E. Onofri	P. Pavlopoulos
P. Pendenza	R. Petronzio	R. Pettorino
F. Piazza	G. Pollifrone	E. Predazzi
E. Rabinovoci	R. Ricci	M. Roncadelli
C. Rosenzweig	G. C. Rossi	H. Rubinstein
M. Sakellariadou	N. Sanchez	M. M. Schaap
A. Schwimmer	L. Sertorio	M. Shifman
G. Shore	T. Taylor	M. Testa
L. Trentadue	H. Tye	A. Ukawa
C. Ungarelli	G. Vilkowisky	F. Vernizzi
M. Virasoro	S. Weinberg	E. Witten
J. Wosiek	S. Yankielowicz	J. E. Young
Y. Zarmi		

3 An Interview with Gabriele Veneziano

MG & JM: Hi Gabriele, and thank you very much for sparing us your valuable time, accepting to answer our questions. We had the privilege of preparing this collection of papers written by your close collaborators, and we would like to ask you a few questions concerning your personal experience with physics during over four decades of successful work. We are interested in your feelings and perspectives about the present status of the research activity in fundamental physics, and your hopes and expectations for the future. But let us start with the past. When (and why) did you decide to devote your professional activity to physics?

GV: *In senior high school in Florence I had a very good teacher of maths and physics, Tebaldo Liverani. He clearly loved those subjects (more maths than physics, he once admitted) and enjoyed teaching them. Probably under his influence, in 1960, myself and two other students in my class decided to enroll at the local university for a degree in either maths or physics. During the summer we had long debates on what to choose, and, eventually, we all opted for physics. I believe that none of us has regretted the choice. This little story tells us how important good-quality teaching is, and not only at the academic level, quite the contrary.*

MG & JM: Do you remember any professor who played a crucial role in influencing your career, both during your studies and at the beginning of your research activity? What should be, in your opinion, the main objectives of undergraduate and graduate courses in physics?

GV: *Besides the high school teacher I have just mentioned, I remember some very good courses at the university, in particular by Professors Mand and Toraldo di Francia. Then, while I was entering my third year, Professor Raoul Gatto arrived to Florence, together with a group of brilliant young theorists, mainly from Rome. His teaching and his presence made me turn in the direction of theoretical particle physics. Without him around I would have probably yielded to some gentle pressure to become a high-energy experimentalist. Later on, at the Weizmann Institute, Hector Rubinstein had a very positive influence on my research. And, finally, at MIT, I learned a lot from working with/under Sergio Fubini. Gatto, Rubinstein, and Fubini had rather different styles in doing theoretical physics and I tried to pick up what I appreciated most from each one of them. Whether I succeeded or not, I certainly owe a lot to all three. What I have appreciated most in all my teachers and mentors has been their passion in doing research together with their professionality. Both are very important attitudes to communicate to the new generations, more important than just giving them a long series of notions. Particularly important is to inject into students a critical, yet constructive, attitude in doing research. Nothing should be taken for granted until it is understood at the deepest possible level.*

MG & JM: Among the many scientific institutions you have visited, where did you find the most pleasant atmosphere and facility of work? What do you think should be of primary care for a laboratory, an institute, or a department of physics in order to encourage the creativity and productivity of its researchers?

GV: *The group in Florence under professor Gatto was a fantastic one. The atmosphere at the Weizmann Institute, particularly in 1966–1968, was also extremely congenial for doing research. Work at the Center for Theoretical Physics at MIT was also carried out under optimal conditions, and the same has always been true for the TH division at CERN. All these places shared the virtue of giving the physicists the time and the means to carry out their research in complete freedom, without administrative burdens and without any demand of short-term results. For instance, at MIT, Fubini and I were working on a program (dual resonance models) which was far from fashionable at the time, but no one tried to push us out of it. I have always been very lucky with the places where I have been working, but also, I must say, with the historical period in which I embarked in theoretical particle physics. A posteriori we can say that the years 1965–1975 were a "golden decade" in theoretical particle physics. We still live, to a large extent, on the great heritage of that period: the standard model, its possible extensions, and string theory.*

MG & JM: You have deeply influenced, in many ways, the past development of fundamental theoretical physics. From your perspective, are you satisfied with the present approach to the physics of fundamental interactions? In particular, what is your attitude toward the main contemporary theoretical "paradigmas"?

GV: *You ask me to stick my neck out. Well, in my opinion, theorists, on the basis of their recent successes with the standard model, have grown a little too arrogant. Some of the ideas around are very well motivated and even beautiful, but it is very hard to find the right way without the input of new data (it is even hard with the data, to be sure, see, e.g., the case of neutrino masses and mixing!). For this reason I am not too excited about the huge activity that is going on in building models for data . . . that are not there yet. Perhaps it would be better to wait until those become available and, meanwhile, to put more effort on some of the outstanding theoretical and phenomenological problems that are already in the data, both in particle physics and in cosmology. Just to mention a few: confinement and dynamical symmetry breaking in QCD, and the origin of primordial—as well as of the present—cosmological acceleration. As an example, I don't think that enough effort has been devoted to trying to solve the first two problems I mentioned above at least in the large-N limit. I am pretty convinced that both analytic and numerical large-N techniques can and should be improved. A similar criticism could apply to present mathematical–physics research, mainly concentrated these days on string theory. It looks to me as if we forgot that the main "raison d' être" of modern string theory is the con-*

struction of a fully consistent quantum theory of gravity. Most of the present activity deals with very special (static, supersymmetric) solutions that fail to address the issue of what happens to generic solutions that approach those of General Relativity in some limit, but should look very different near the ubiquitous singularities of the classical solutions. What happens, in string theory, to the big bang singularity? Or to the one inside a black-hole horizon? These are tough problems, of course, but it looks to me that our community tends to ignore these issues in favor of tackling some easier problems (more for sociological than for scientific reasons, I guess). Let me repeat a motto I have voiced a few times: "Let's find tools for our problems, rather than problems for our tools!" In the case of the singularities, for instance, new techniques should be searched for studying string theory in geometries whose curvature radius is much below the string scale. I have the feeling that, by some appropriate duality, this problem should not be too different from that of large curvature radii, which we are already able to deal with. Would it not be wonderful to know about the fate of the big bang singularity—and thus of the beginning of time—in string theory?

MG & JM: A frequently voiced criticism of string theory (see, e.g., L. Smolin's recent book) is that string is not science since it cannot make predictions and, therefore, cannot be falsified. What is your opinion on this?

GV: *I completely disagree. It is fair to say that, at present, we are unable to extract reliable testable predictions from string theory, but this is only due to our present incomplete understanding of such a complicated theory. After all, how many decades had to pass before we could go from Yang–Mills theory to a theory of the weak interactions? For instance, it is often said that, in order to test string theory, one would need such high energies that no (human-built) accelerator will ever be able to produce. But (besides the fact that the Universe itself has provided such enormous energies right after the big bang, and may well have kept some imprint of string theory since then) it is not true that the predictions of string theory are just in the high-energy domain. String theory contains—at the lowest level of approximation—many massless scalar fields that could deeply influence low-energy physics by inducing violations of the equivalence principle, deviations from Newton's law, or space and/or time variations of fundamental constants. The problem is that we are presently unable to understand whether (some of) those massless particles stay massless after the theory is completely solved. If the answer is yes, then superstring theory will be falsified for the same main reason that the old hadronic string was abandoned: strong interactions are short range, but the old string insisted on having massless particles! Another generic prediction of string theory is the existence of extra dimensions of space. If those are not too small they could be revealed at accelerator experiments. But even if they are tiny they could have affected very early cosmology, leaving an imprint of today's cosmological observables.*

MG & JM: What would you like the LHS to discover? And what do you think the LHC will actually discover?

GV: *The best gift the LHC could deliver is . . . surprises. The worst would be just a confirmation of the Standard Model by the discovery of a light Higgs boson and nothing else. Unfortunately, given the striking phenomenological successes of the Standard Model, the latter possibility is not easy to exclude. It would amount to some fine-tuning of the Standard Model's parameters, true, but the cosmological constant problem has accustomed us to much worse than that. Another item in any theorist's wish list is the discovery, by the LHC, of a good dark-matter candidate, even better if this will have to do with discovering supersymmetry. Personally, I am quite convinced that supersymmetry will play a role in particle physics, sooner or later. The problem, if supersymmetry lies at too high an energy scale to be reached at the LHC, is that we may never find the motivations (and resources) to push toward the next energy frontier. If I should bet my own money on something, I would say that the LHC will find more than the standard Higgs but not quite what we theorists are expecting or hoping for (like extra dimensions or strong gravity). For instance, I am not fully convinced that the ideas of a dynamical symmetry breaking (of "technicolor" type), or of some compositeness of leptons and quarks explaining the origin of the three families, can already be put to rest. We have not yet understood the non-perturbative dynamics of QCD: how can we be sure that a different gauge theory cannot solve one or both of those questions?*

MG & JM: What are your main suggestions and recommendations to young people at the beginning of their research activity in the field of fundamental theoretical physics?

GV: *To think with their own heads rather than follow the fashion. They should learn of course what has been done by the previous generation, but to follow the latter's prejudices will not help bring out the new ideas we badly need in order to solve the outstanding problems still facing us.*

MG & JM: Do you remember any amusing episode or anecdote concerning your scientific life that you would like to share with us and with the readers?

GV: *An amusing one is the drink I had with Feynman in Caltech after his talk at a conference on QCD. It must have been around 1979–1980. I had been invited to present some results obtained at CERN about how quark and gluon jets evolve and lead, eventually, to a state that looks almost ready to convert into low-mass hadrons. I gave the talk, which was well received. Feynman was in the audience, but I do not remember any question by him, either at the end of my talk or in private afterward. The next day Feynman gave his talk. Apparently he had rewritten it overnight and, consequently, was not very well prepared; but it was brilliant, as usual. His talk was largely inspired by mine and Feynman kept mentioning my results over and over again. I remember he was even mispelling the name of Petronzio by quoting "Veneziano and*

Petronziano," surely joking Mr. Feynman? After the end of the session, I told Feynman I had enjoyed his talk. He must not have been very satisfied with it, since he answered: well, that's because I quoted you all the time, isn't it? And then he added: come, let's have a drink, I want to understand better what exactly you have done. So we went to a nearby pub, had coffee (or was it beer?) and I started to tell him about my work. At some point, before I had finished, he interrupted me and said: "But then you have been cheating me! I thought you had done much more! This is nothing but the Altarelli–Parisi stuff!" I had to sweat a lot to convince him that, indeed, I had done more. Did he get convinced? I am not sure. But at some point I stumbled on his English (too good for me, I guess). I asked him what he meant by a "freying jet," an expression he had used many times. To explain, he pointed at my shirt and said: well your poor-Italian-physicist's shirt is freying...I got it. He also said: you know, he should get together and fix them, referring to some colleagues in Caltech who had also been doing jet physics. I thought that "fixing" them would mean to attack them badly, so I asked "Why be nasty?" But then he reassured me: no, I mean we should just correct what they are doing incorrectly... This was indeed my first and last substantial encounter with Feynman, a person I admired very much for his tremendous talent as a physicist but also for being so straight, so simple, and yet so deep, as a man.

MG & JM: To which subject(s), in particular, would you like to dedicate your future scientific activity?

GV: *Probably the wisest thing for me to do would be to retire from active research and give more time to teaching and to writing. However, for me doing research is a little bit like being addicted to a drug (I'm not sure since I've never been!). It will be difficult to stop abruptly. I would really like to know, for instance, what happens to spacetime singularities in string theory, to understand the origin of cosmic acceleration, and to solve QCD in some suitable large-N limit. But all this sounds like wishful thinking doesn't it?*

MG & JM: Finally, how do you imagine the path that fundamental physics and cosmology will follow in the future? What do you expect, in particular, from string theory and/or M-theory? Is, in your opinion, a successful "theory of everything" really within our reach in a foreseeable future?

GV: *Who said that it is difficult to make predictions, particularly for the future? But, if I have to make some guess, or a bet, I would say that, probably, the new accelerator data will not confirm our simplest theoretical ideas and, in particular, will suggest that there is more structure in today's "elementary particles" than we presently assume. In other words, the desert will blossom. The difficulties we are experiencing with getting the right model from string theory could mean that, like the old strings did not succeed in describing hadrons, the new ones will fail to describe quarks and leptons. Also, about the hierarchy problem, we could be on the wrong track with low-energy SUSY. Possibly, the solutions of the hierarchy and cosmological constant problem are not unrelated.*

Will we arrive one day at a "final theory" and to the end of theoretical physics? I do not think we will ever arrive at a "final theory" (I have given many talks about "Dreams of a Finite Theory" instead) but we may very well come to the end of some branch of physics because of "practical" reasons. I think that Feynman said once that a certain branch of physics may terminate the day the effort to make a tiny step forward (experimentally or theoretically) will be too large to be able to afford it. We may be (slowly!) approaching that limit in high-energy accelerator physics, but I am old enough for not being afraid of it.

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