

Reflections and Impressionistic Portrait at the Conference *Frontiers Beyond the Standard Model, FTPI, Oct. 2012*

Paraphrasing Feynman: Nature is more imaginative than any of us and all of us taken together. Thanks god, it keeps sending messages rich on surprises.

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1 In the beginning. Before 1972

In the beginning of my career in high energy physics (HEP), theory was lagging behind experiment and, by and large, its development was guided by experiment. Before the advent of the standard model (SM, at that time it was referred to as the Weinberg-Salam model) and quantum chromodynamics (QCD) none of hot theoretical topics of the day was particularly singled out: many directions of theoretical thought were considered to be equally respectable, and peacefully coexisted. The HEP theory community was distributed roughly speaking evenly between them. People understood that above several GeV theory had to be changed,¹ a few competing ideas as to possible changes were discussed but none was firmly established. The ignorance at short distances was usually parametrized either by nonrenormalizable operators in effective Lagrangians or, in loops, by an ultraviolet cut-off. Then theoretical predictions were confronted with experiment in order to determine the scale of “new physics.” Experiment was an ultimate judge of what was important in theory and what not. I do not think that anybody could even dream of making a statement that “the theory of everything” was within reach. The general belief was that such a theory (that would explain all mysteries of nature once and for all) could not exist. For this reason a lack of advance or even complete failure of this or that line of thought caused no trouble in the community. It only affected a few followers who could rather painlessly switch to other theories or topics. This was a wonderful time.

¹One of the reasons behind this understanding was the analysis of the K_L - K_S mass difference which led to the GIM mechanism implying in turn the existence of a charmed quark not heavier than ~ 2 GeV.

Experimental guidance started fading away after the November revolution of 1974 – the discovery of heavy charmonium. The role played by experiment continued to decrease steadily for quite some time, until it became almost invisible. In HEP theory this effect coincided with a transition (a crossover, rather than a phase transition) into a different mode of operation which I will call, somewhat conditionally, the giant resonance mode. In this mode each novel idea, once it appears, spreads in an explosive manner in the theoretical community, sucking into itself a majority of active theorists, especially young theorists. Naturally, alternative lines of thought by and large dry out. Then, before this given idea brings fruits in understanding of phenomena occurring in nature (both, due to the lack of experimental data and due to the fact that on the theory side crucial difficult problems are left behind, unsolved), a new novel idea arrives, the old one is abandoned, and a new majority jumps onto the new train. Note that I do not say here that this is good or bad. This is just the fact of life of the present-day theoretical community largely deprived of reference points provided by experiment.

That’s why such high expectations were associated with LHC.

2 Standard model

Currently there are no direct experimental data contradicting SM. Beginning with the discovery of neutral currents in 1973 and ending with that of the Higgs boson in 2012, through the precision electroweak measurements at LEP – everything we know today triumphantly confirms this model. Massive neutrinos and their mixing, which was absent in the earlier version, is naturally accommodated by SM. To this end no “new physics” is necessary. Note that existence of axions *per se*, if confirmed in the future, will require no restructuring of the model either, since the axions are neutral with regards to gauge interactions. One can just add them to the model as is.

The structure of the standard model is rather elegant and it is definitely self-contained. For all practical purposes we know how our world operates at distances of the order of 10^{-17} cm or larger (in many instances, to a high precision). However, curious mind never stops. Conceptual issues exist whose solution lies beyond SM. First and foremost, the mass hierarchy.²

There are rather many free parameters in the standard model, most of them are masses and quark (neutrino) mixing angles which are clearly associated with the mass matrix. A complete lack of any semblance of universality in this sector is striking. Masses span the interval from $\sim 10^{-2}$ eV (for neutrinos) to ~ 200 GeV for t quarks –

²One can also add an associated question of enormous suppression of the cosmological constant Λ .

thirteen orders of magnitude. This is not the end of the story, however. Indeed, it is widely believed that the only natural scale in physics is set by the Planck mass M_P which determines the strength (or should I say, weakness?) of gravity interactions at low energies and the energy scale at which gravity becomes strong, $M_P \sim 10^{19}$ GeV. This is, of course, assuming that no dramatic change occurs in physics on the way from the present-day ~ 1 TeV up to M_P , i.e. sixteen orders of magnitude.

This idea – that M_P is the only genuine scale in physics – is rather deeply rooted in the community, although sometimes people still try to pose a question: “What if this is not the case?”³

If the natural scale of all masses is indeed set by M_P , then the hierarchy problem becomes awful. Not only the masses in the matter sector are scattered over thirteen orders of magnitude, they are extremely small in the scale of M_P .

Even if we accept for the time being that our understanding of physics is not ripe enough to explain the mass hierarchy, the next question to ask is whether or not this hierarchy is stable. In other words, if we set the mass parameters (measured in a certain well-defined way) more or less as they are in some approximation, will quantum corrections dramatically shift them from the initial values dragging them toward the Planck scale?

For fermions (quarks and leptons) the stability situation is not bad, provided that we are at weak coupling. Indeed, quantum corrections to masses are logarithmic and proportional to the original mass. Therefore, the expansion parameter

$$\frac{\alpha}{4\pi} (\log M_{\text{uv}}/\mu_{\text{ir}}) \ll 1$$

even in the worst case scenario in which the ultraviolet cut-off parameter $M_{\text{uv}} \sim M_P$.

This is not the case, however, for the Higgs mass, or, alternatively, for the Higgs vacuum expectation value. Corresponding quantum corrections are quadratically divergent and, therefore, apparently drag these parameters toward the Planck scale, if there is no natural cut-off at a much lower scale. (Later I will say more about “naturalness.”)

This is a bad situation. There are two ways out: new physics at a scale much lower than M_P , but not lower than 1 TeV or so (that’s because below 1 TeV we see absolutely no indications to new physics), or extreme fine-tuning. The latter would mean that although each successive quantum correction produces a huge shift in mass,

³ And rightly so. I remember that in the very beginning of my physics career it was not unusual to assume that the Fermi four-fermion interaction extends all the way up to its unitary limit, $E \sim G_F^{-1/2}$, and that $G_F^{-1/2}$ is the only scale relevant to weak interactions. Some theorists invested their efforts in exploration of this scenario. Needless to say, it was abandoned with the advent of the standard model with its W and Z bosons. Now we know that electroweak scale is set by M_W , and at $E \sim M_W$ all cross sections are stabilized well below the unitary limit.

the shifts cancel in the total sum to a very high accuracy, so that the resulting overall shift is small or absent. Such a scenario is usually called “unnatural.” The criterion of naturalness is aesthetic, or, if you wish, philosophic. If you do not like it you can ignore it. Most people like it.

Numerical situation with the cosmological constant Λ might seem even worse. If $M_H/M_P \sim 10^{-17}$, for the cosmological constant we have $\Lambda^{1/4}/M_P \sim 10^{-31}$. To my mind, there is no conceptual difference in fine-tuning at the level of 17 or 31 orders of magnitude. One and the same hitherto unknown mechanism could be responsible in both cases.

3 Supersymmetry

Supersymmetry as a theoretical construction is known since early 1970s. Attempts at developing supersymmetry-based phenomenology started shortly after theoretical discovery of supersymmetry. In 1982 Witten pointed out that supersymmetry stabilizes the hierarchy problem. It introduces a new scale – that of supersymmetry breaking $M_{\mathcal{S}}$ – which, if low enough, allows one to stabilize the Higgs boson mass. In supersymmetry, the quadratically divergent integral is cut off not by M_P but by $M_{\mathcal{S}}$. In addition, the degree of fine-tuning in the cosmological constant reduces, since, as it became clear very early, the cosmological constant vanishes in the limit of exact supersymmetry.

Another reason for the advent of supersymmetry in phenomenology was the hope that it provides us with a sensible candidate for dark matter. If the R parity is conserved, the lightest superpartner must be stable, and if the lightest superpartner is neutralino, the dark matter problem ($\sim 25\%$ of Universe’s mass) could be solved by neutralinos.

After Witten’s publication, explorations in the framework MSSM, which became a basis for supersymmetric phenomenology, expanded in an explosive way. Although theoretically supersymmetry is a beautiful concept, the corresponding phenomenology was and still is less than elegant. Supersymmetry, if it exists, is definitely broken in nature. This breaking is parametrized by many free parameters. If in the standard model the number of free parameters is close to 20, in supersymmetric model it exceeds 100. Moreover, there are no deep theoretical reasons for the R parity conservation. If we allow R parity to be broken, extra free parameters appear and the dark matter motivation disappears, since in the absence of R parity there are no stable superpartners. Worse than that, in the absence of R parity we lose proton stability, generally speaking.

For many years supersymmetry-based phenomenology was in the focus of theo-

retical research. By and large people closed their eyes on the above aesthetic drawbacks in the race for natural stabilization of the mass hierarchy.

Now the discovery of the 125 GeV Higgs boson, and nothing else at LHC, caught MSSM phenomenologists by surprise dramatically changing the overall picture and the state of minds in the community. A simple and elegant idea of a single scale M_S close to the electroweak scale turned out to be in contradiction with data! One could feel the mood of perplexity in the audience.

At the Lagrangian level MSSM predicts that $M_H < M_Z$, where $M_Z \approx 90$ GeV is the Z boson mass. To elevate the Higgs mass to the level of 125 GeV one needs a very large radiative correction (not much smaller than the tree-level term). A natural solution is to make the stop mass (i.e. the mass of the t quark superpartner) very heavy, perhaps from a few TeV to 10 TeV or heavier. In conjunction with the fact that superpartners are not seen at LHC, one must admit that (i) the scale of supersymmetry breaking is non-universal; (ii) it is likely to be very high, much higher than was expected 10 or even 5 years ago. If superpartners are much heavier than the electroweak scale we are back to square one as far as the original problem of the hierarchy stabilization is concerned. Already today we face the necessity of fine-tuning at the level of 10^{-2} or even 10^{-3} .

Of course, people do not give up their dreams easily. They hasten to modify MSSM in a contrived way to keep it viable. Split supersymmetry, spread supersymmetry – to name just a few alternatives that (all of a sudden) regained popularity. The version of MSSM which now goes under the name “natural” (not to be confused with the original naturalness of the 1980s) is as follows: the first and second generation superpartners are assumed to be very heavy, so that there are no observable consequences from their existence whatsoever. The stop mass is fine-tuned to obtain the correct Higgs mass⁴ ($m_{\tilde{t}} \sim 10$ TeV). Then superpartners are not expected to be observed at LHC. Their appearance is deferred till an era of a mythical ILC or some next-generation accelerator which may or may not materialize, certainly not soon in the present-day political climate. The original impetus for low-energy supersymmetry is declared dead by the majority.⁵

Is the current situation with phenomenological supersymmetry good or bad? I think it is good. The vicious circle of constrained MSSM is broken. It is time to stop blindly scanning the parameter space and start thinking and developing new ideas. It is a great time for ingenious young researchers. I can compare it with two early years of my career, just before the advent of the standard model.

⁴The expected stop mass $m_{\tilde{t}}$ can be somewhat lowered, down to a few TeV, at the price of introducing large and fine-tuned A terms.

⁵Some people still try to keep it afloat by developing contrived baroque-like aesthetically unappealing modifications.

4 Theoretical supersymmetry

This is an example of a complete success story. I use the word ‘theoretical’ as opposed to phenomenological supersymmetry discussed above which, as I tried to convey, at the moment has a rather murky status. Theoretical supersymmetry proved to be a powerful tool to deal with quantum field theory, especially at strong coupling, a regime which was considered intractable for decades (with some exceptions in two dimensions). Progress in this line of research, although slow, is absolutely steady.

It was noted in the early 1980s that special holomorphy properties of supersymmetric gauge theories allow one to obtain exact results in the so-called protected sectors. The gluino condensate was calculated and the exact β function was derived in this way. $\mathcal{N} = 2$ supersymmetry turned out to be even more powerful in this respect. Continuous advances in this direction resulted in a revolutionary breakthrough in 1994, when the Seiberg-Witten solution of $\mathcal{N} = 2$ super-Yang-Mills was found. This was the first ever *analytic* demonstration of the dual Meissner effect as an underlying mechanism for quark (color) confinement.

Equally important was the discovery of Seiberg’s duality. In fact, it was first detected in supersymmetric QCD and then elevated to string theories, of which I will talk later. Of course, people knew from the early days of field theory that gauge symmetry is not a symmetry in the conventional meaning of this word, rather it is a redundancy in the theoretical description. Seiberg’s duality explicitly demonstrated that different gauge theories, with distinct gauge groups, can lead to one and the same physics in the infrared. Needless to say, this can only happen if at least one theory in the dual pair is at strong coupling, so that the fields in the Lagrangian do not represent the asymptotic states of the theory. The Seiberg duality and its combination with the Seiberg-Witten solution led to far reaching consequences in understanding gauge theories.

Another stimulating and promising development in theoretical supersymmetry is associated with extended objects, such as domain walls (branes) and strings. Studying dynamics of the BPS-saturated strings people came across a few surprises. First, dynamics on the string world sheet can be highly nontrivial. Being strongly coupled, effective world-sheet models are solvable (at least, some of them) because they are two-dimensional. And – the most remarkable feature – the solution of these two-dimensional models provide us with unambiguous (exact) information on aspects of four-dimensional bulk theories. This phenomenon is now known as $2D - 4D$ correspondence.

In certain instances supersymmetry-based results in conjunction with a $1/N$ expansion lead to exact predictions for non-supersymmetric theories. Planar equivalence between $\mathcal{N} = 1$ super-Yang-Mills and the the so-called orientifold theory presents the

most clear-cut example. At $N = 3$ the orientifold theory corresponding to $\mathcal{N} = 1$ super-Yang-Mills is just one-flavor QCD.

5 Quantum chromodynamics

It is hard to believe now that before 1972 it was not uncommon to interpret field theory in four dimensions as a set of a few “sacred” presumptions and prescriptions, such as uniqueness of the vacuum state, the absence of vacuum condensates (a special procedure of normal ordering in the Lagrangian and various relevant composite operators was always implied to ensure this property), and so on. By and large, quantum field theory was reduced to a theory of small field oscillations near zero, with the subsequent quantization of propagating waves, supplemented by a tricky and rather obscure procedure of renormalization. It was little more (if at all) than a set of Feynman graphs. Because of the Landau zero charge no self-consistent field theories in four dimensions were known. Generally speaking, people (at least in my surroundings) looked at those who were stuck with field theory as to losers.⁶

Just for an illustration let me present a quotation from Andrey Linde’s memoir: “...The difference between weak and electromagnetic interactions arises after a non-zero vacuum average $\langle\phi\rangle$ appears in the scalar field. But according to quantum field theory, these averages should always exactly equal zero. Many people at the time said the average $\langle\phi\rangle$ made no sense and that the spontaneous symmetry breaking mechanism should simply be understood as a heuristic trick, necessary only to guess special relations between masses and coupling constants of various fields for which the theory becomes renormalizable.”

The advent of QCD changed all that. We learned that what you see in the Lagrangian is not necessarily what can be detected; that the vacuum structure can be complex, vacuum need not be unique; that small harmonic oscillations near vacuum are insufficient to explain strong dynamics; that nonperturbative physics is rich and important; that there is a variety of diverse regimes (or phases) that can be implemented in field theory, such as Coulomb, Higgs, confinement, oblique confinement, conformal and more; that the dual Meissner effect presents a typical mechanism leading to confinement of quarks (color); that Wilson’s operator product expansion (OPE) can be adjusted to perfectly fit QCD, and then renormalization is readily (and trivially) understood as an evolution process from short to large distances. OPE-based methods proved to be useful in many problems of practical interest, in particular, in heavy quark physics.

⁶Arkady Vainshtein reminded me of important exceptions – effective field theories, such as the theory of Goldstone bosons.

In non-Abelian gauge theories one of the most profound and fruitful discoveries that shook the HEP community was that of 't Hooft, who pointed out that $1/N$ is a (hidden) expansion parameter in QCD and Yang-Mills theories in general, corresponding to the expansion in topologies of the underlying Feynman graphs. Thus, there emerged a natural – albeit qualitative – correspondence between QCD and a string-like picture, with $g_s \sim 1/N$ where g_s is the string coupling constant. Moreover, the domain wall tension in super-Yang-Mills was shown to scale as $N \sim 1/g_s$, which served as a basis for identification of these domain walls with the string theory branes.

A few times in the last two decades many believed, with excitement and enthusiasm, that the existing theory was at the verge of, if not the exact solution of QCD, at the very least, its solution in the planar limit (i.e. $N \rightarrow \infty$ with the fixed 't Hooft coupling). I vividly remember these days. Alas ... these high expectations never came true. The range of natural phenomena that are described by QCD is so diverse and complex, that such a universal solution seems unlikely (to me). Nevertheless, our understanding continues to grow, in a non-revolutionary manner, in particular due to penetration of supersymmetry-based methods and proliferation of $1/N$ expansions.

6 Grand unification

If we let three gauge coupling, as they are known at our energies, run, assuming low-energy supersymmetry and nothing else at higher energies, all three become equal to each other to a reasonable degree of accuracy at the energy scale $\sim 10^{16}$ GeV. This scenario, which is also known as a Great Desert scenario, culminating in a beautiful unified $O(10)$ gauge group at the “right” end of the desert, instead of $SU(3) \times SU(2) \times U(1)$ at our, “left” end, became deeply routed in the minds during three decades of its existence. The presence of a noncompact $U(1)$ is indeed an unpleasant theoretical feature of SM, for a number of reasons.

Needless to say, in the absence of the low-energy supersymmetry the Great Desert scenario will have to be reconsidered. Will unification of the gauge couplings survive in some form?

7 Large extra dimensions

Ten years ago this was the hit of the day. String theory tells us that the number of dimensions may, generally speaking, be larger than four, for instance ten in the superstring theory. Then six extra dimensions must be compact. Such a solution was suggested e.g. in the “quartet” paper that started the first string revolution in 1985.

In this paper it was tacitly assumed that the size of the compactified dimensions is of the order of M_P^{-1} .

A priori this does not have to be the case: the patterns of compactification could be of a multistep/multiscale type and quite contrived. It was suggested that at least one extra dimension could have (in a sense) “a macroscopic” size, with all matter fields trapped on a surface of a 3-brane with thickness of the order of $1/\text{TeV}$.

The conceptual design was very attractive, transforming a gigantic hierarchy of the mass spectrum into a relatively modest hierarchy of a geometrical nature (the interbrane distances and brane thicknesses). Various interesting geometric separation mechanisms explaining the mass hierarchies, the pattern of the CKM matrix, suppression of unwanted flavor-changing decays and so on were worked out – e.g. fat branes and warped scenarios, to name just a few.

As it often happens, however, the devil was in the details. Unfortunately no extra-dimension model which would be theoretically elegant and concise on the one hand, and fully consistent with the existing phenomenology, on the other hand, has ever appeared. Of course, the criterion ‘elegant’ is relative, and one can argue whether this or that model on the theoretical scene was elegant enough. What is unquestionable is the fact that now, ten years later, there are no indications to any of the large extra dimension models. Nothing pointing out in this direction came from LHC so far. Original enthusiasm seems to fade away. Will it be resurrected? Only if future LHC data will give it a chance.

The role of supersymmetry in the large extra dimension models is subsidiary, if at all, the Great Desert is gone, and unification of all three gauge couplings may or may not occur. In no way it can be considered as a *fait accompli* as is the case in low-energy supersymmetry.

8 String theory

String theory appeared as an extension of the dual resonance model of hadrons in the early 1970, and by mid-1980 it raised expectations for the advent of “the theory of everything” to Olympic heights. Now we see that these heights are unsustainable. Perhaps, this was the greatest mistake of the string-theory practitioners. They cornered themselves by promising to give answers to each and every question that arises in the realm of fundamental physics, including the hierarchy problem, the incredible smallness of the cosmological constant, and the diversity of the mixing angles. I think, by now, the “theory-of-everything-doers” are in disarray, and a less formal branch of

string theory is in crisis.⁷

At the same time, leaving aside extreme and unsupported hype of the previous decades, we should say that string theory, as a qualitative extension of field theory, exhibits a very rich mathematical structure, and provides us with new, and in a sense superior, understanding of mathematical physics and quantum field theory. It would be a shame not to explore this structure. And, sure enough, it was explored by serious string theorists.

The lessons we learned are quite illuminating. First and foremost we learned that physics does not end in four dimensions: in certain instances it is advantageous to look at four dimensional physics from a higher-dimensional perspective. Surprisingly a relatively simple geometric structure designed in higher dimensions – let us call it string and brane engineering – leads to highly nontrivial insights regarding the general (and sometimes, even quite specific) aspects of supersymmetric gauge theories, and even two-dimensional sigma models. A significant number of advances in field theory, including miracles in $\mathcal{N} = 4$ super-Yang-Mills, that we witnessed in the last decade or so, came from the string-theory side. It turns out that a simple action – abandoning the strive to explain all of the world at the fundamental level, all at once, liberates string theory from the dead end it put itself in, and puts it onto a comfortable highway.

Much excitement was caused by the gauge-string duality, sometimes referred to as a holographic description. Since 1980s Polyakov was insisting that QCD had to be reducible to a string theory in 4+1 dimensions. He followed this road step by step, for years practically alone, eventually arriving at the conclusion that confinement in QCD could be described as a problem in quantum gravity. This paradigm culminated in Maldacena’s observation (in the late 1990’s) that dynamics of $\mathcal{N} = 4$ super-Yang-Mills in four dimensions (viewed as a boundary of a multidimensional bulk) at large N can be read off from the solution of a string theory in the bulk. In particular, in the limit of the large ’t Hooft coupling, the bulk string theory degenerates and becomes supergravity, which one needs to solve in the classical approximation. Needless to say, searches for a classical solution of the supergravity equations are infinitely simpler than solving quantum field theory in the strong coupling regime.

Unfortunately – a usual story – when fashion permeates physics, people in search of easy and fast ways to Olympus tend to overdo. For instance, much effort is being invested in holographic description in condensed matter dynamics (at strong coupling). People pick up a supergravity solution in higher dimensions and try to find out whether or not it corresponds to any sensible physical problem which may or may not arise in a condensed matter system. To my mind, this strategy, known as the

⁷A more formal branch evolved to become a part of mathematics or (in certain occasions) mathematical physics.

“solution in search of a problem” is a dead end again. Attempts to replace developing deep insights in relevant dynamics by guesses very rarely lead to success.

9 Landscape

The idea of a landscape of vacua (which came from string theory) is probably the most dramatic change of paradigms from the Newton times. In a sense, it was born out of desperation. The searches for a unique solution for our world, a unique string vacuum, ended in failure. No guiding principle was found to limit the number of vacua, let alone, reduce the result to a unique vacuum which would contain in it all information currently available, including all mass hierarchies, all coupling constants, and so on. Because string theory proclaimed itself to be the *ultimate* theory nothing less than that was acceptable.

At this point a U turn occurred when some theorists suggested to convert a failure of the original program into a triumph. Observing that quasistable vacua of string theory, as they are known now, are extremely abundant, they said: “The more numerous they are the better!” If the number of vacua is 10^{500} (you can put in the exponent 1000, 10000 or any other large number you like) then various critical parameters (all masses, coupling constants, generation numbers, chirality structure, gauge groups, etc.), being randomly scattered in these vacua, will give rise to a huge variety of distinct universes. Some of them (perhaps, just one or two) are quite exceptional. These are highly non-generic vacua where we find our worlds’ parameters. We simply happen to live in such a vacuum. In other universes life is impossible, there are no theorists to derive laws of nature.

Thus, there is no point in trying to understand the world order: the mass hierarchies, the smallness of the cosmological constant, the absence of the fourth generation, you name it. Nor such attempts will be meaningful in the future. All this is an environmental coincidence. Just take it as is and live happily ever after.

This is nothing else than the anthropic principle in its extreme realization, with a religious (or philosophical, if you put it milder) flavor.

Indeed, even if this is true, we will never know. All “extra” universes are causally disconnected from our, so there is no physical way to confirm their existence or non-existence in experiment. So, this part of the landscape paradigm is the act of belief in today’s string theory, not supported by any evidence, and not to be supported by evidence in the future.

The second conclusion – that one should abandon the search for a rational (non-environmental) understanding of the mass hierarchy/stability and the smallness of the cosmological constant – may well be true in a limited sense but for a totally

different reason than the landscape paradigm. It may well be that at the moment we do not have enough data to develop a theory. Or, perhaps, a message has been already sent to us but we failed to read it. In other words, the theory is not ripe enough to offer an explanation. Then there is some hope that if the search is resumed in the future it will be more successful than now.

10 Curiosity

Surprisingly, or perhaps not so surprisingly, the $H \rightarrow \gamma\gamma$ decay was discussed in quite a few talks. A factor of 1.8 excess from the SM prediction (which – I hasten to add – is well within 2σ) caused a stir and gave rise to some speculations that it opened a window to new physics. Such a speculation seems premature. If only the discrepancy were at the level of 3σ or more! Could VVZ and I imagine, 33 years ago, when we calculated this decay rate and established its connection with the β function – could we imagine then that this calculation would become an important reference point, a new physics counter of sorts?

11 A lost generation?

It is easy to estimate the total number of active high-energy theorists. Every day hep-th and hep-ph bring us about thirty new papers. Assuming that on average an active theorist publishes 3-4 papers per year, we get 2500 to 3000 theorists. The majority of them are young theorists in their thirties or early forties. During their careers many of them never worked on any issues beyond supersymmetry-based phenomenology or string theory. Given the crises (or, at least, huge question marks) in these two areas we currently face, there seems to be a serious problem in the community. Usually such times of uncertainty as to the direction of future research offer wide opportunities to young people, in the prime of their careers. To grab these opportunities a certain reorientation and reeducation are apparently needed. Will this happen?

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