Reflections on Supersymmetry*

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Abstract: Supersymmetry is a theme with many facets that has dominated much of high energy physics over the past decades. In this contribution I present a very personal perspective on these developments, which has also been shaped in an important way by my interactions with Julius Wess.

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1 Introduction

When Misha Shifman asked me whether I would be willing to contribute a chapter to this book with some memories of my encounters with Julius Wess, I did not hesitate for very long. For one thing, I am honored to have my name included in such an illustrious list of early contributors and pioneers of supersymmetry. Secondly, because I feel privileged to have been a witness when for a (world-historically speaking) fleeting moment the provincial town of Karlsruhe became an epicenter of theoretical physics. It does not happen often that a beginning graduate student is so lucky, as I was, to land in a hot spot of new theory developments, especially in a place that never makes it into the top one hundred of international university rankings. Of course, there were several other such hot spots, and the contributions to this volume vividly and from different angles tell the story how the idea of supersymmetry evolved in such diverse places. For this reason I will not dwell too much on topics that are already extensively dealt with in the book, but instead offer my personal perspective, covering some aspects that have received less attention in the other contributions, especially with regard to $N = 8$ supergravity, as well as some thoughts concerning the current status of supersymmetry *vis-à-vis* theoretical high energy physics.

In writing this text I have decided not to include a list of references. The reason is simple: there would be either too few, or too many! At any rate, all references relevant to what I am saying can be easily located on the electronic `arXiv`, or alternatively by consulting the companion articles in this book.

2 Early years in Karlsruhe

Already soon after enrolling at the University of Karlsruhe for my undergraduate studies in physics it was clear to me that I would want to do my diploma thesis with Julius Wess who was at the forefront of exciting developments in theoretical physics. Even as students who knew hardly anything about the subject, we were keenly aware of this fact! So in 1974 I approached Julius with the request to become his diploma student. Yet before getting accepted I had to overcome a first hurdle, when he told me that before starting in earnest I would have to read the book *Introduction to the theory of quantized fields* by Bogoliubov-Shirkov, the then much used standard text on relativistic quantum field theory (QFT). This was clearly the device which he employed to scare away prospective students who were not 150% willing to commit. I remained undeterred even though I can now admit that I never read this formidable book to the end (and fortunately for me, Julius never checked). This was because I realized full well that if I was not accepted, the alternative would have been to settle for a boring diploma topic, or even (God forbid!) experimental work.

Thanks to the way the institute was run by Julius, life at the institute was very stimulating in many ways, not only with regard to physics. This included all kinds of social events and gatherings, perhaps most memorably the annual skiing excursions to Oberperfuss (Austria). The days in Oberperfuss were divided into physics sessions that took place in an old school building in the mornings and late afternoons, and ski outings in between. I was the only institute member who did not know how to ski, or even how to stand on skis properly. I still remember how on my first day I fell off the lift immediately on arriving at the top of the slope, feeling like the giant beetle in Kafka's novella "Die Verwandlung". Nevertheless, after practising for a couple of days I was proudly able to stand and slowly move on my skis, descending the slope by zigzagging across it horizontally (much to the annoyance of people racing down hill), which took me a good part of the afternoon.

At the institute in the Physikhochhaus the common room for postdocs and graduate students bore the label "Kinder" (children). Indeed we little ones had hardly any idea what the 'adults' (*alias* Julius and Bruno Zumino) were up to. But we greatly benefitted from the numerous prominent visitors. Lochlain O'Raifeartaigh, who discovered the first example of a Wess-Zumino type model that exhibits spontaneous breaking of supersymmetry, spent extended periods of time in Karlsruhe. Another highlight was a visit by Rudolf Haag, who started out his seminar with the question "Wo ist Martin Sohnius?". The main purpose of his visit was to work with Martin and Jan Łopuszański on their now famous classification of all supersymmetries of the S-matrix. Other visitors included André Martin and John Iliopoulos. John had been scheduled to give a supersymmetry seminar about Fayet-Iliopoulos breaking, but switched topics to talk about the c-quark which had just been discovered, and its proper interpretation in terms of the GIM mechanism. Pierre Fayet came to present the first version of a supersymmetric extension of the Standard Model (SM) of particle physics, inventing a whole new vocabulary with neologisms like 'photino', 'gluino' and the like. Of course, the most prominent visitor was Bruno Zumino (which made us joke that there also had to be a superpartner by the name of 'Zumo'). Bruno came to Karlsruhe regularly, but when he was around we rarely saw him, with no idea of what they were doing because they usually locked themselves up, or simply worked at Julius' home.

In autumn 1975 I finished my diploma thesis on spontaneous breaking of global supersymmetry[1], which satisfied Julius at least to the extent that he offered me the possibility of staying as a PhD student with him. As a topic for my doctoral thesis he suggested the problem of putting supersymmetry on the lattice. This looked like a win-win problem for an aspiring PhD student, because lattice gauge theory was the other freshly hot topic in particle physics at the time. But, as so often, the devil turned out to hide in the details. The idea did not really fly because of one main obstacle: the Leibniz rule is no longer valid for lattice derivatives, but nonetheless absolutely needed to re-assemble terms into total derivatives, as required for supersymmetry invariance of the action. This effectively meant that there was no way of putting supersymmetry on the lattice without doing violence to it (and the problem gets worse if one wants to incorporate gauge symmetry). My attempts to get around the difficulty never led to fully satisfactory answers: for instance, the so-called SLAC derivative (the Fourier transform of the saw-tooth function $[k^\mu]$) which retains a rudimentary version of the Leibniz rule, is too non-local on the lattice to be of much use. Looking for a way out, I turned to constructive QFT, with the idea of proving rigorously the existence of Wess-Zumino model in the framework of constructive quantum field theory (recall that to this day we have no example of a fully constructed interacting QFT in four dimensions satisfying all the Wightman axioms!). At first this looked promising because the model required only one divergent (wave-function) renormalization, unlike QED or ϕ^4 theory which need three. Consequently, one needed to study only a one-dimensional renormalization map, a cutoff dependent map between bare and renormalized parameters considered in earlier work by R. Schrader. As one of the by-products this required reformulating supersymmetry in Euclidean signature, a challenge because the replacement of $SL(2,\mathbb{C})$ by $SU(2) \times SU(2)$ implies a doubling of the fermions, in apparent conflict with the boson-fermion balance of the theory. While others concluded from this that $N = 1$ supersymmetry cannot be Euclideanized, my proposal simply amounted to the statement that the correct Euclideanization procedure is the one that yields the proper Euclidean continuation of the Minkowskian supersymmetry Ward identities, in accord with the Osterwalder-Schrader reconstruction theorem. These efforts earned me a seminar invitation to CERN, although I doubt that I was able to convince anyone, most importantly Bruno, of the relevance of what I was saying. In the

[1]We did not publish any of our results because L. O'Raifeartaigh scooped us by getting there first! Instead, my first paper (in 1976) introduced a non-relativistic quantum mechanical version of supersymmetry, with a supersymmetric spin system built solely from Majorana fermions. This model has some similarity with the (supersymmetric) SYK model that became popular only much later.

end my 'constructive' attempts were also stymied because the joint UV and IR regulators required to rigorously define the functional (path) integral break supersymmetry so badly that one could not really exploit the symmetry for its full worth. But at least I found a new way to characterize supersymmetry without the use of anticommuting variables, just in terms of the bosonic functional measure that is obtained by integrating out fermions.

Meanwhile others at the institute were busy working on all kinds of different problems spawned by the discovery of supersymmetry, *e.g.* renormalization of supersymmetric gauge theories (O. Piguet and K. Sibold), or possible generalizations of supersymmetry to parasymmetry (N. Dragon). Considerable effort was spent on searching for off-shell superspace versions of $N = 2$ gauge theories and supergravity in work involving R. Grimm and M. Sohnius, as well as Julius himself. As you can see, we had a whole new world in front of us to explore! Of course, Bruno and Julius continued their joint work, during the time I was there mostly concentrating on the superspace of formulation of $N = 1$ supergravity, building on the basic insight that even flat superspace requires torsion. I still recall when one morning Julius skipped the usual Austrian niceties, by greeting me simply and directly with the statement "$\mathcal{L} = E$" – meaning that the supergravity Lagrangian in superspace is nothing but the supervielbein Berezinian. I could see how happy he was with this result, because what could be simpler than that for a gravitational Lagrangian? A small fly in the ointment was the fact that one still had to take into account the superspace constraints, so the seemingly simple result could not be exploited directly to calculate loop corrections in supergravity.

Although we were all captivated by supersymmetry and in no doubt about its ultimate relevance, no one in Karlsruhe in those days thought that the available supersymmetric models could be directly relevant to real physics. Rather, the general attitude was that essential pieces were still missing from the picture – just like Yang-Mills theories are not viable without the crucial extra ingredients discovered only much later, namely the Brout-Englert-Higgs mechanism, and the subtle dynamical mechanism of confinement. The main advantage of supersymmetry emphasized in those days was not so much the absence of quadratic divergences (which became the dominant narrative only later in connection with the hierarchy problem), but rather the fact that it overcame the Coleman-Mandula no-go theorem prohibiting the fusion of space-time and internal symmetries. The $N = 1$ models that became fashionable for phenomenology in the 80ies precisely do *not* exploit this feature, which would require extended ($N \geq 2$) supersymmetry. This is the basic reason for the (to me, esthetically unappealing) need to double up the known particle spectrum with unseen superpartners possessing the same internal quantum numbers.

3 Beyond simple supersymmetry

After finishing my PhD in October 1978 and an unsuccessful application for a CERN fellowship I moved to Heidelberg, where I spent most of my time learning QCD and perturbative quantization of gauge theories. I had already reconciled myself to the idea of spending the next few years there, when quite unexpectedly I was informed that I could finally join CERN because another fellowship candidate had declined the offer from CERN. So we moved to Geneva in November 1979. In the following years I continued to meet Julius regularly, rejoining the Karlsruhe institute as an associate professor for the two years 1986 - 1988. After that contacts became less frequent, as Julius accepted a joint offer from LMU Munich and the MPI für Physik, and finally moved there in 1990. Physicswise, he had quit supersymmetry activities to start exploring new ground, turning his full attention to non-commutative geometry where, as he told me more than once, he saw a new El Dorado for theoretical physics.

At CERN my main focus switched to supergravity, an interest that had been ignited during a remarkable mathematical physics (M∩P) conference in Lausanne in 1978 that I was able to attend. It was above all the brilliant performance of the French team represented by Bernard Julia and Joël Scherk that got me hooked. They both talked about their recent progress with maximal supergravity. Bernard reported on his discovery with Eugène Cremmer of the hidden exceptional $E_{7(7)}$ symmetry of maximal $N = 8$ supergravity, while Joël gave a wildly funny presentation of their recent construction of $D = 11$ supergravity, drawing all kinds of parallels between the superworld and Nietzschean philosophy. This is when my ‘extremist view’ on supersymmetry slowly started shaping up: if it is to be supersymmetry, then either as much of it as possible, or none at all! For this reason I decided not to get involved in the activities on $N = 1$ supersymmetry model building, both with regard to further theoretical developments such as developing the tensor calculus for supergravity matter couplings, or concurrent efforts to apply the theory to the phenomenology of the SM and its extensions. I rather concentrated my efforts on learning extended supergravity in the component (not superspace) formalism. In doing my first steps I was greatly helped by Paul Townsend, whose subtle sense of humor and very British understatement I have always admired, and Peter van Nieuwenhuizen, one of the fathers of supergravity, who generously accepted me as the ignoramus newcomer into the collaboration.

Around that time I started working with Bernard de Wit, aiming right away for maximal $N = 8$ supergravity. Not only had Bernard done important preparatory work, but he also entrusted me with a big pile of hand-written notes. I spent the following weeks assiduously working my way through these notes, until I reached a stage where I could no longer see the forest for all the trees. Since our initial attempts at gauging the maximal $N = 8$ theory thus failed badly, we turned to $N = 5$ supergravity as a kind of stopgap measure, a theory that by itself is only of limited interest (except perhaps as a testing ground for checking unexplained cancellations and UV finiteness properties of extended supergravities beyond four loops, as I learnt very recently from Zvi Bern). There we finally got things to work! Having convinced ourselves that $N = 8$ would still refuse to cede, we decided to include an appendix in our $N = 5$ paper explaining in all technical detail why $N = 8$ supergravity could not be gauged. The paper was practically finished, so we decided to follow our usual habit of having another cup of coffee in the CERN cafeteria before releasing the paper. And there it suddenly struck us like a bolt of lightning out of the sky: we had made an elementary mistake in the calculation – so trivial that I cannot even remember now what it was! Needless to say that we rushed back to get to work right away on revising the paper. But then it still took several months to complete the construction of gauged $N = 8$ supergravity

Optimism that $N = 8$ supergravity might have something to do with the real world was still running high in 1982: on my first visit to DAMTP none less than Stephen Hawking assured me that, in his opinion, this was the best candidate for a unified theory. He immediately put a graduate student (Nick Warner) on the problem of finding suitable 'realistic' symmetry breaking vacua of the $N = 8$ potential. Although I kept coming to Cambridge as a visitor numerous times afterwards, I could never quite figure out whether Stephen continued to stand by this claim, or whether he had secretly switched allegiance to string theory at some point. Independently, at around the same time there were attempts to relate the $N = 8$ supergravity to 'real physics', most notably by Ellis, Gaillard, and Zumino, who tried to exploit a conjecture by Cremmer and Julia, according to which the (chiral!) SU(8) R-symmetry of the theory could become dynamical. There was a short-lived burst of excitement when they found three $\bar{\mathbf{5}} \oplus \mathbf{10}$ fermion multiplets of SU(5), but those were accompanied by huge towers of 'junk particles' that were obviously useless for realistic applications. Another, and to me even more intriguing, proposal was made by M. Gell-Mann, and I will come back to it below. Furthermore, hopes for an 'easy' argument that $N = 8$

supergravity could be finite to all orders were dashed when it was realized in a series of papers by Howe and Lindström, Kallosh and later Howe, Stelle and Townsend that the theory admits (linearized) counterterms from three loops onward [2]. So at that point it looked like all roads were blocked.

On top of these failings there was another major development that pushed $N = 8$ supergravity aside as a leading candidate theory for unification. This was the advent of heterotic string theory in 1984 which rolled over the CERN theory division like a tsunami, burying everything else underneath it. People were busily scurrying to jump on the bandwaggon, being certain that all problems would soon be solved, with an almost unique path from the heterotic theory to SM low energy physics. The expectation that physics would be over within a few weeks resulted in a coherent state of collective inebriation that in retrospect looks rather absurd to me. Nevertheless, the general attitude from that moment on and for many years thereafter remained that anyone *not* working on string theory and the heterotic string was completely out of the loop. Too bad for our poor colleagues who had invested all their stock into non-string approaches to quantum gravity!

In spite of these developments I decided to stick with maximally supersymmetric theories, and to persist with my decision not to join the booming MSSM and string model building industry (which at one point prompted John Ellis to exclaim "Hermann, we'll never turn you into a good phenomenologist!"). Excursions into string theory proper were only sporadic, perhaps most notably with our proposal (with A. Casher, F. Englert and A. Taormina) to derive all consistent superstring theories from the $D = 26$ bosonic string, including a mechanism to get space-time fermions out of the bosons, and thus explain the emergence of supersymmetry from a purely bosonic theory. Somewhat to my surprise, this proposal generated a huge amount of attention, getting me an invitation from M. Gell-Mann to present these ideas at his newly founded Santa Fe Institute. There, however, I failed to convince the fully assembled string establishment of the virtues of our insights. The main drawback was that despite all efforts our construction remained entirely kinematical – like most subsequent constructions of string vacua! – with no compelling dynamical explanation why the $D = 26$ string should do such a thing. Towards the end of my stay at CERN I also

[2]To be sure, the question of finiteness (or not) of $N = 8$ supergravity remains up in the air, in spite of stunning computational advances by Z. Bern, L. Dixon and collaborators, with results now reaching up to five loops. However, for all I know, the seven-loop calculation that might finally settle the question remains beyond reach. *Idem* for the question whether the known 'seed' counterterms can be made fully compatible with non-linear supersymmetry and non-linear $E_{7(7)}$ invariance.

wrote one paper with Sergio Fubini, showing how to take the 'square root' of the Yang-Mills action in eight dimensions, in an attempt to gain better access to the maximal ($N = 4$) super-Yang-Mills theory. This led to the discovery of what we called the octonionic instanton. Sergio, one of the true pioneers of string theory, was a very kind person. He often told me what life was like at MIT when they did their ground-breaking work there, and hardly anyone would bother to show up at their seminars. Talk about volatility in the public perception of what is good theory!

Over the following years I devoted much time to Kaluza-Klein supergravity and the question of consistent truncations beyond the linearized approximation, mostly in collaboration with B. de Wit (I think the importance of this problem is still not fully appreciated in work on the phenomenology of effective low energy actions obtained from higher dimensions). Further topics were all kinds of things exceptional, such as quasi-conformal realizations and minimal unitary representations of exceptional non-compact groups, and $E_{8(8)}$ in particular, where I learnt a lot from Murat Günaydin, and the construction of maximal gauged supergravity in $D = 3$ with Henning Samtleben, where again $E_{8(8)}$ played a pivotal role. However, my greatest fascination remained with B. Julia's early papers conjecturing the appearance of the *infinite-dimensional* exceptional symmetries E_9 and E_{10} in the further reduction to two and one dimensions. Of these, E_9 symmetry (or more precisely, $E_{9(9)}$) is now understood to be an extension of the Geroch group in general relativity, but E_{10}, the maximal rank hyperbolic Kac-Moody algebra, remains a total enigma. What struck me most there was the unexpected link between two seemingly disparate developments in physics and pure mathematics. Namely, in both cases the search for distinguished structures leads to unique answers. On the physics side, trying to make Einstein's theory as (super-)symmetric as possible, one arrives at a unique answer ($D = 11$ maximal supergravity), while on the other side mathematicians identified a uniquely distinguished Lie algebra, the maximally extended hyperbolic Kac–Moody algebra E_{10}, by way of by classifying Dynkin diagrams. In fact, E_{10} has been shown to contain all simply laced hyperbolic Kac-Moody algebras as subalgebras, and is thus an all-encompassing mathematical entity – much like $D = 11$ supergravity, which keeps popping up in all discussions of M theory.

4 Supersymmetry and fundamental physics

The last time I saw Julius Wess, who had meanwhile moved to Hamburg, was in Israel on the occasion of Eliezer Rabinovici's and Shimon Yankielowicz' 60th birthday celebrations in Jerusalem and Tel Aviv[3]. Julius seemed to be in great shape, and we agreed that he should soon visit me in Potsdam. So I was absolutely shocked when I learnt that he had suffered a stroke shortly after; he died in 2007, and the planned visit never came to pass. Tragically, he thus also missed the chance to see the outcome of the LHC experiment which he had been eagerly awaiting: over the years he had become increasingly confident that LHC would find superparticles (whereas Bruno Zumino seemed more skeptical, as far as I could tell from his review talks that I was able to attend). Despite his optimism, and surely being aware that experimental confirmation of supersymmetry would earn them a very nice reward, Julius was always modest enough to admit the possibility that his expectations could turn out to be totally wrong. There was no question for him that experiment is the ultimate arbiter, and that either way one would have to accept the outcome of the experiment.

Half a century has now passed since the discovery of supersymmetry. During this time the subject has developed enormously, with stupendous advances on many fronts, some of which are also documented in this book. Supersymmetry has been a major driving force of developments in mathematical physics and pure mathematics. So it is definitely here to stay! Nevertheless, we now (in 2023) have to face up to the fact that supersymmetry, at least in the form championed over many years, is off the table as a realistic option for real life particle physics. 15 years of LHC searches have not produced a shred of evidence for superpartners of any kind. Quite to the contrary, the integrated results from LHC strongly indicate that the SM could happily live up to the Planck scale more or less *as is*, and *without* supersymmetry or other major modifications.[4]

So where are we to go from here? Should we simply give up on the idea that supersymmetry is of any relevance to the real world, as demanded by a growing chorus of voices? Or pin our hopes on the next (100 TeV?) collider that, if it is ever built, will finally reveal that Nature *is* supersymmetric? If such hopes

[3] This was the first time I broke ranks with the supersymmetry community when reporting about my work with K. Meissner, where we propose that it is *conformal symmetry* rather than supersymmetry that stabilizes the electroweak scale.

[4] Another hint in this direction is the following. While supersymmetry strongly prefers BPS-type static configurations (such as AdS space), it is neither compatible with a positive cosmological constant nor with a time-dependent cosmology – but this is what we see when we look out of the window!

rely on further pursuing the (no longer 'low' energy) $N = 1$ supersymmetry option I am afraid they may well be disappointed, because the main motivating argument, namely solving the hierarchy problem, has largely evaporated. On the other hand, looking at the current disarray and confusion reigning in 'Beyond the SM' particle physics, I feel that simply abandoning the idea of supersymmetry for 'real' physics altogether would throw out the baby with the bath. Without *some* guiding principle, we may simply end up in an unnavigable multiverse of ideas (just look at the wildly diverse tableau of approaches to quantum gravity!), with no prospects of validating or falsifying any of them all the way.

To end this contribution on a more speculative note, let me outline where I see a possible way forward. This is to search for concepts *beyond* space-time supersymmetry, while taking the hints from what we have learnt from supersymmetry studies over the years. In other words, throw out the bath, but try to save the baby! And here I would first of all like to point out the remarkable fact that, with the possible exception of the asymptotic safety program, and despite their differences, *all* current approaches to quantum gravity are united in the belief that something dramatic happens to space or space-time near the Planck scale, in the sense that classical space-time must dissolve or 'de-emerge' there. This is in analogy with continuum fluid dynamics which emerges from an underlying discrete structure of atoms and molecules. But if there is no space-time to begin with, the basic superalgebra

$$\left\{Q^i_\alpha \, , \, \bar{Q}_{\dot{\beta}j}\right\} \, = \, 2\delta^i_j \sigma^\mu_{\;\alpha\dot{\beta}} P_\mu$$

and possibly even the distinction between bosons and fermions, cannot be fundamental. If consequently space-time, and concomitant concepts such as general covariance and gauge symmetries are to be emergent, the one million (or better: one billion?) dollar question is: *What* do they emerge *from*?

While there are no answers, there is also no lack of ideas and suggestions. One of them is a specific proposal that I have been involved in with Thibault Damour, Marc Henneaux and Axel Kleinschmidt, and which proceeds from the well-known BKL (short for: Belinski-Khalatnikov-Lifshitz) analysis of cosmological solutions of Einstein's equations near a space-like (cosmological) singularity. According to this analysis spatial points decouple near the singularity, effectively realizing a dimensional reduction to one (time) dimension at the singularity. But this is exactly the reduction for which one expects the hidden symmetry of maximal supergravity to get enlarged to the maximally extended hyperbolic Kac–Moody symmetry E_{10}! The hypothesis is therefore that the true symmetry reveals itself only in a 'near singularity limit', in perfect analogy with the full electroweak symmetry becoming manifest only in the high energy limit. So E_{10}, not supersymmetry, would be the key player! Towards a more concrete realization of this idea we have proposed a 'space-less' one-dimensional sigma model, mapping the supergravity dynamics in space-time to a null-geodesic motion on the infinite-dimensional coset manifold $E_{10}/K(E_{10})$. While capturing some essential features of maximal supergravity this ansatz remains incomplete for many reasons. The outstanding challenge is to explain in full detail how E_{10} can replace space-time based QFT and the emergence of classical concepts such as space-time and gauge symmetries from a purely algebraic construct.[5] One would expect quantum theory to play an essential role in understanding how this happens. A further difficulty is that we have hardly any idea how to physically interpret the states associated with the imaginary roots of E_{10}, starting out with the 'dual graviton'.[6]

What is clear, however, is that E_{10} goes beyond supersymmetry in the sense that it 'knows' everything that supersymmetry 'knows'. For instance, the information about the bosonic constituents of the maximal supergravity multiplets ($D = 11$, IIA, IIB,..) can be directly read off from the E_{10} Dynkin diagram, simply by 'slicing' it in different ways w.r.t. its various finite subdiagrams. Likewise, the fermionic sectors of the maximal supergravity multiplets are governed by the maximal compact (or 'involutory') subgroup $K(E_{10})$ of E_{10}, an infinite prolongation of the R-symmetry groups of maximal supergravities. For instance, the IIA and IIB fermion multiplets at a given spatial point are again obtained by decomposing the $K(E_{10})$ under its finite-dimensional R-symmetry subgroups. Moreover, one can expect that due to the unique properties of its root lattice (being the unique even self-dual Lorentzian lattice in ten dimensions), E_{10} will eventually play a key role in ensuring the (non-perturbative) quantum consistency of the theory, in the same way that the perturbative finiteness and consistency of string theory are ensured by *modular invariance*, whereas supersymmetry is mainly needed to get rid of tachyons. So there is some evidence that, at the most basic level, duality and modular symmetries are indeed more important than supersymmetry.

There is yet another, and different, argument pointing in the same direction, though perhaps more obliquely. In 1983, M. Gell-Mann proposed what he described as "a last ditch attempt to salvage the haplon interpretation of $N = 8$ supergravity". The SO(8) gauge group of the theory contains a vector-like SU(3)

[5] P. West has proposed a conceptually very different scheme based on the indefinite (but non-hyperbolic) Kac–Moody algebra E_{11}. His scheme does not invoke dimensional reduction.

[6] One might also note that the $E_{10}/K(E_{10})$ sigma model does not admit static cosmological solutions.

$\times\,$U(1), but it is immediately evident that this SU(3) cannot be identified with the color symmetry SU(3)$_c$. Noticing that after the removal of eight Goldstinos (to break all supersymmetries) one is left with the right number $48 = 3 \times 16$ of spin-$\frac{1}{2}$ fermions (corresponding to three generations of quarks and leptons, including right-chiral neutrinos), he had the very strange idea that the supergravity SU(3) should be identified with the diagonal subgroup of SU(3)$_c$ and a hypothetical family symmetry SU(3)$_f$. Putting the quarks and leptons into the appropriate representations of SU(3)$_c$ and SU(3)$_f$ (there is only one way to do this properly) he obtained exact agreement! In addition, the electric charge assignments almost work, in that the U(1) charges are systematically off by a 'spurion charge' $\pm\frac{1}{6}$. So the proposal fails only by very little, and in a very systematic fashion. Not long after, Nick Warner and I were able to show that this scheme is *dynamically* realized at one of the stationary points of the $N = 8$ potential. For a few days we were ecstatic, trying very hard to push the agreement further – *alas*, in vain. Since the proposal was greeted with understandable skepticism by many colleagues (who had mostly placed their bets with $N = 1$ supersymmetry), I eventually decided to put it aside, since I did not want to get trapped in a quixotic chase after a mirage.

Nevertheless, the idea stuck in my mind. After all, if there are really only 48 fundamental spin-$\frac{1}{2}$ fermions, no more, no less, what other scheme could possibly explain this, with a *unique* answer as the outcome? Besides, the scheme can be immediately and easily falsified if only one extra fundamental spin-$\frac{1}{2}$ fermion were to be detected. In 2015, when it began to dawn on everyone that LHC would not produce the predicted cornucopia of new particles, Krzysztof Meissner and I therefore decided to have another look, by asking ourselves what it would take to rectify the mismatch of electric charges. As it turned out, this could be achieved by a surprisingly simple deformation of the U(1) group by the generator

$$\mathcal{I} := \frac{1}{2}\Big(T \wedge \mathbb{I} \wedge \mathbb{I} + \mathbb{I} \wedge T \wedge \mathbb{I} + \mathbb{I} \wedge \mathbb{I} \wedge T + T \wedge T \wedge T\Big)$$

acting on the tri-spinor χ^{ijk} of $N = 8$ supergravity which transforms in the $\mathbf{56} \equiv \mathbf{8} \wedge \mathbf{8} \wedge \mathbf{8}$ of SU(8). Here, the SO(8) matrix T represents the imaginary unit in the SU(3)$\times$U(1) breaking, with $T^2 = -\mathbb{I}$, which in turn implies $\mathcal{I}^2 = -\mathbb{I}$. Because the triple wedge product $T \wedge T \wedge T$ is *not* an SU(8) element, there is no way of incorporating this deformation into $N\!=\!8$ supergravity. The 'spurion shift' associated with $\mathcal{I}$ is therefore incompatible with supersymmetry.

But now the remarkable fact (demonstrated jointly with Axel Kleinschmidt) is that, at least in the one-dimensional reduction, this deformation *can* be incorporated into $K(\mathrm{E}_{10})$. This could mean that matching the observed SM fermion spectrum with fundamental theory requires more than merely picking the right Calabi-Yau manifold, GUT group or fermion multiplets, but may involve understanding in detail how such structures could emerge from a space-time-less pre-geometric context, and that we may have to go all the way to fancy infinite-dimensional symmetries such as E_{10} and $K(E_{10})$ to make things work. Then the question is no longer whether $N = 8$ supergravity is the right theory. Rather, it is whether and how E_{10} can replace space-time supersymmetry as a guiding principle, and what kind of pre-geometric theory it is that lies beyond $N = 8$ supergravity and realizes these symmetries. And, most importantly, whether and how the partial match with SM physics can be completed.

Since it could easily take another 50 years to figure all this out (assuming there is any truth in these ideas), Krzysztof and I have recently started looking for observational ways to test this hypothesis, by concentrating on the massive gravitinos which are the only other fundamental fermions that would emerge from the $N = 8$ supermultiplet. Unlike the MSSM gravitino, these gravitinos do carry SM charges and would thus participate in (non-chiral) SM interactions, which makes them directly detectable at least in principle. Taking into account the U(1) shift (which again requires $K(E_{10})$) we have a new dark matter candidate here, but of a very unusual type. The gravitinos would have to be fractionally charged, extremely massive, and at the same time extremely rare (with an abundance of roughly one gravitino in a cube of $\mathcal{O}(10\,km)$ side length), so as to have escaped detection until now. We have argued that, if such gravitinos were really found, we would be able to address two outstanding open problems in astrophysics, namely (1) explaining the origin of supermassive primordial black holes in the very early universe (via the condensation and gravitational collapse of lumps of supermassive gravitinos in the early radiation period), and (2) understanding the origin of the ultra-high energy cosmic particles that have been observed over many years at the Pierre Auger Observatory in Argentina (via the mutual annihilation of supermassive gravitinos in the 'skin' of neutron stars). We are currently exploring ways to search for such particles in upcoming underground experiments, in particular JUNO. There *are* options beyond high energy colliders!

5 Conclusions

Independently of whether the ideas sketched above are on the right track or not, I remain attached to the Einsteinian point of view that we should try to understand and explain first of all *our* universe and *our* low energy world, and that in the end there should emerge a more or less unique answer. I believe that 50 years of supersymmetry have brought us a wee bit closer to this goal, though not as close as many would have wished. Of course, this point of view runs counter to currently prevalent views according to which the only way out of the vacuum dilemma of string theory is the multiverse. But if Nature must pick the 'right' answer at random from a huge ($> 10^{272\,000}$?) number of possibilities, I see no hope that we would ever be able to confirm or refute such a theory.

Already in 1929, and in connection with his first attempts at unification, Albert Einstein published an article in which he states with wonderful and characteristic lucidity what the criteria should be of a 'good' unified theory: (1) to describe as far as possible all phenomena and their inherent links, and (2) to do so on the basis of a minimal number of assumptions and logically independent basic concepts. The second of these goals, also known as the principle of Occam's razor, he refers to as "logical unity" ("logische Einheitlichkeit"), and goes on to say: "Roughly but truthfully, one might say: we not only want to understand *how* Nature works, but we are also after the perhaps utopian and presumptuous goal of understanding *why* Nature is the way it is, and not otherwise."

To which I have nothing more to add!

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