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Peter van Nieuwenhuizen

Professor Peter van Nieuwenhuizen is honoured today:

"for the discovery of supergravity theory and research in its subsequent development. Prior to the discovery of supergravity, he made important contributions to the understanding of the quantum behaviour of ordinary gravity as well as matter coupled to gravity, through a systematic study of their divergence structure. The search for a gravity theory with better quantum behaviour, by inclusion of fermionic fields, eventually led to a highly non-trivial fusion of supersymmetry with gravity, culminating in the seminal paper with Sergio Ferrara and Daniel Z. Freedman in 1976, where the first supergravity theory was proposed. This theory combines, in a non-trivial fashion, the spin 2 graviton with a spin 3/2 particle called the gravitino to elevate supersymmetry to a local gauge symmetry. This led to an explosion of interest in quantum gravity and it transformed the subject, playing a significant role in very important developments in string theory as well as Kaluza-Klein theory. Professor van Nieuwenhuizen played a major role in the development of the subject, with his studies on the quantum aspects of supergravity, coupling of supergravity to matter, super Higgs effect, extended supergravity theories, conformal supergravity and many other aspects of the theory. In particular, he contributed to the construction of the ten-dimensional Einstein-Yang-Mills supergravity, which has been studied intensely in recent years as the low energy limit of the ten-dimensional heterotic string theory. Currently any grand unified theory incorporating gravity is based on a supergravity theory coupled to matter in four dimensions. These theories emerge naturally from the compactifications of the ten-dimensional heterotic string."

Professor Peter van Nieuwenhiuzen was born in Utrecht (The Netherlands) on 26 October 1938. He studied both physics and mathematics at the University of Utrecht, and in 1971 he obtained his Ph.D in physics with a thesis on Radiative Corrections to Muonic Processes under the supervision of Prof. M.Veltman. From 1965 to 1969 Professor van Nieuwenhuizen was Postdoctoral Fellow at the Dutch National Science Foundation. From 1969 to 1971 he was Fellow at CERN in the Theory Division and from 1971 to 1973 Juliot Curie Fellow at the University of Paris, Orsay in France. From 1973 to 1975 he was Research Associate at Brandeis University at Waltham, Massachusetts. From 1975 to 1985 he held different positions at the State University of New York at Stony Brook where he is now Leading Professor of Physics. Professor van Nieuwenhuizen is editor of the Journal of Modern Physics A, and was editor of the Journal of Mathematical Physics and Classical and Quantum Gravity. In 1985 he was appointed Teyler Professor of Physics at Leiden University. He is the author of 250 scientific publications; his Physics Report on Supergravity was on the CERN list of the 20 most referenced publications during the decade 1980-1990.

SOME PERSONAL RECOLLECTIONS ABOUT THE DISCOVERY OF SUPERGRAVITY

PETER VAN NIEUWENHUIZEN
Institute of Theoretical Physics
State University of New York at Stony Brook
Stony Brook, NY 11794-3840, USA.

1 Introduction.

It is a very great honor to stand here today, 18 years after the discovery of supergravity, to receive together with Dan Freedman and Sergio Ferrara, the Dirac medal and prize for the year 1993. I would like to thank Abdus Salam for his continuing strong support over the years of new theoretical ideas such as supersymmetry, supergravity and superstrings. In the early 1980's I helped organize with him and others a series of schools at Trieste on supergravity which later became the Trieste Spring Schools on strings. We had many meetings together and I recall, with pleasure, his intense interest in supergravity (on which he wrote many papers) as well as his sense of humor.

Before coming to the topic of my lecture, I would like to acknowledge the gratitude I feel for two other great physicists. First, Tini Veltman, my Ph.D. advisor: from him I learned to do Feynman graph calculations on the computer which I used in the final stages of the construction of supergravity. He will shudder at the thought that he indirectly contributed to the discoveryof supergravity because he has become, with Glashow and others, an outspoken critic of all super-things, but our friendship has only increased over the years. As to the validity of their criticism I can only say that interesting and clean problems in traditional areas of physics are nowadays very hard to find, whereas the new fields abound with such problems. The idea that for every boson there should be a fermionic partner, and vice-versa, is so radical that it repels some physicists, but it is not more radical than the prediction of Dirac in the 1930's that for every particle there should be an antiparticle. The recent dramatic precision of the unification of the running $SU(3) \times SU(2) \times U(1)$ coupling constants in the minimal supersymmetric extension of the standard model (precision 1 in 1000) clearly is an indirect manifestation of supersymmetry, but what the future of supersymmetry and supergravity will be, I cannot tell.

The other great physicist I feel very grateful to, is Frank Yang. Not only is he one of this century's greatest physicists (parity violation, Yang-Mills theory, Yang-Baxter equation etc.), but also he has managed to create an institute where, for the almost 20 years I have worked there, a very friendly and constructive atmosphere exists, among professors and students. Quite a difference from some other places where graduate students and junior faculty are often viewed as lower forms of life. When I was a high school student, my father came to me one day with Time magazine, where he had just read that two young Chinese physicists had been awarded the Nobel prize "for discovering that God is left-handed". He told me it must be marvelous to make such discoveries. I could hardly have imagined that one day I would be Frank's colleague and friend.

I realize that a lecture like the one today should not be a technical lecture on some of one's latest results, but rather a historical lecture looking back at the times when the discovery was made. My lecture will be in this vein, and among other anecdotes I will recall my encounter with Dirac and his reaction to supergravity.

If I would have the time, I would in the second part of my lecture present a very simple proof of supergravity, much simpler than our (FvNF) original proof, or the equivalent first-order reformulation of Deser and Zumino (DZ). Now I must refer you to lectures I gave this month in Varenna. Those of you who have never studied or understood supergravity, will find there the simplest version I am aware of. It was constructed over the years by combining the ideas of quite a few people (Freedman, Ferrara, myself, Deser, Zumino, Townsend, Volkov, Soroka, MacDowell, Mansouri, Chamseddine, West and others). According to this approach, the action for $N = 1$ supergravity with a supercosmological constant can be written in the following Yang-Mills-like form by "gauging" the super anti-de Sitter algebra

$$\begin{aligned} I &= \int [R_{\mu\nu}{}^{mn}(M) R_{\rho\sigma}{}^{pq}(M) \epsilon_{mnpq} \\ &+ \bar{R}_{\mu\nu}(Q)\gamma_5 R_{\rho\sigma}(Q)] \epsilon^{\mu\nu\rho\sigma} d^4x \end{aligned} \tag{1}$$

provided one imposes the curvature constraint $R_{\mu\nu}{}^{m}(P) = 0$. The curvatures $R_{\mu\nu}{}^{mn}(M)$ and $R_{\mu\nu}{}^{\alpha}(Q)$ are Yang-Mills curvatures belonging to the Lorentz generators M_{mn} and the supersymmetry generators Q_α of the super-anti de Sitter algebra. One begins by first "gauging" the latter, i.e., by associating to each generator (M_{mn}, Q_α, P_m) a gauge field $(\omega_\mu{}^{mn}, \psi_\mu{}^\alpha, e_\mu{}^m)$ and constructing the corresponding Yang-Mills curvatures. But then one must impose the constraints $R_{\mu\nu}{}^m(P) = 0$. These constraints are a gauge choice which leaves only the diagonal subgroup in the direct product of Yang-Mills transformations corresponding to P_m and general coordinate transformations. They express the spin connection $\omega_\mu{}^{mn}$ as a complicated composite object depending on vielbein fields $e_\mu{}^m (m = 0, 3)$ and spin 3/2 gauge fields ("gravitinos") $\psi_\mu{}^\alpha(\alpha = 1, 4)$. The constraint $R_{\mu\nu}{}^m(P) = 0$ is also a field equation, namely the field equation of the spin connection itself, $\delta I/\delta\omega_\mu{}^{mn} = 0$. Imposing these constraints (equivalently: solving this field equation), one recovers the second-order formulation of FvNF, but a crucial simplification is that one can keep denoting $\omega_\mu{}^{mn}(e, \psi)$ by the symbol $\omega_\mu{}^{mn}$ (like in the first-order approach of

DZ) without ever expanding it, since the variation $\delta\omega_\mu{}^{mn}(e,\psi)$ is (of course) multiplied by the field equation $\delta I/\delta\omega_\mu{}^{mn}$ which vanishes identically. Of course, even with this simplification, the proof of invariance of the action is not totally trivial. If one does not impose the constraints and keeps $\omega_\mu{}^{mn}$ as an independent field, the transformation law of $\omega_\mu{}^{mn}$ is nonzero (and complicated) [1], as first correctly found by Deser and Zumino.

Supergravity can also be written in superspace. Superspace was invented by Salam and Strathdee as an application of the theory of coset manifiolds (the coset manifold is here $\{P_m, Q_\alpha, M_{mn}\}/\{M_{mn}\}$). In superspace one also needs constraints (on the supertorsions as first found by Wess and Zumino and solved by Siegel and Gates) but a simple geometrical derivation of all these constraints has not yet been found. In the geometrical approach to W gravity by Schoutens, Sevrin and myself, one has constraints on all curvatures, but here corresponding "W-superspace" is even unknown. Perhaps some of you can solve these intriguing problems.

2 Some historical recollections.

In this section I will recall how and why I came to supergravity. This is not a historical review where related work is discussed and compared with my own; rather it contains some personal recollections.

In the fall of 1975 I came to Stony Brook as an assistant professor and thereby became a colleague of Dan Freedman whom I had met at the Paris summer institute. The previous two years I had been at Brandeis University, busy applying the then recent covariant quantization rules of 't Hooft and Veltman to gravity, in collaboration with Stanley Deser, Marc Grisaru and others.

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1 Volkov and Soroka gauged the super Poincaré algebra in 1973, and treated $\omega_\mu{}^{mn}$ as an independent field, like DZ, but did not impose a constraint or field equation. Consequently, they found $\delta\omega_\mu{}^{mn} = 0$, which is incorrect, as with this law the action is not invariant.

These rules dispensed with the problems of operator ordering and unsolvable constraints which had been complicating the Hamiltonian approaches to quantum gravity, and now one could really calculate. Moreover, unitarity was guaranteed provided one introduced ghosts for the spacetime gauge symmetries, so the main problem was renormalizability. We had used a background field formalism to compute the one-loop divergences for all kinds of systems: the Maxwell-Einstein system, the Dirac-Einstein system, the Yang-Mills-Einstein system, QED coupled to gravity, gravitational lepton-lepton scattering, etc. Together with the earlier computation of 't Hooft and Veltman for pure gravity and gravity coupled to scalar fields, the results were uniformly disastrous: in all these cases (except pure gravity) there were one-loop divergences which were nonrenormalizable. For example, in the Maxwell-Einstein system, we found, using dimensional regularization, and imposing the Maxwell field equation $D^\mu F_{\mu\nu} = 0$ and the Einstein field equation $G_{\mu\nu} = -\frac{1}{2}T_{\mu\nu}$ (photon)

$$\Delta\mathcal{L} = \frac{\sqrt{-g}}{n-4}\,\frac{137}{60} R_{\mu\nu}R^{\mu\nu} \tag{2}$$

(The number 137 was curious but it was an integer, not α^{-1}.) Since the form of this counter term is different from the form of the terms in the original action, ordinary renormalizability could not be used to get rid of these divergences, and hence these divergences were unrenormalizable. There were some unexpected or, as we called it, "miraculous?" cancellations (which we attributed to the duality invariance of the action and of $T_{\mu\nu}$ (photon) $= F^2{}_{\mu\nu} +^* F^2{}_{\mu\nu}$ under $\delta F =^* F$) due to which $F_{\mu\nu}$ only appeared in the combination $T_{\rho\sigma}$ (photon) and $(D^\mu F_{\mu\nu})^2$ but not as $(F^2)^2, R_{\mu\nu\rho\sigma}F^{\mu\nu}F^{\rho\sigma}$ or RF^2. However, terms with $R_{\mu\nu}{}^2, R^2, T_{\mu\nu}{}^2, T_{\mu\nu}R^{\mu\nu}$ and $(D^\mu F_{\mu\nu})^2$ remained, and these yielded the above quoted final result after using the classical field equations.

[Of course, a shift $g_{\mu\nu} \to g_{\mu\nu} + \alpha R_{\mu\nu} + \beta g_{\mu\nu} R$ produces in the Einstein action terms like $R_{\mu\nu}{}^2$, but such field redefinitions do not modify the on-shell divergences.]

So it seemed, as it does today, that a perturbative approach to Einstein quantum gravity leads to non-renormalizable divergences. Physically, it was clear that due to the dimensionality of the gravitational coupling constant κ, one was expanding in powers of κk where k is a momentum, which is not a good expansion for ultraviolet divergences. That was the end of the story, so it seemed.

However, although in QED coupled to gravity the infinities did not cancel, there remained in several people's minds a lingering doubt that perhaps a magical combination of fields existed for which the infinities did cancel. The reason for this hope was that the coefficients of divergences proportional to ${R_{\mu\nu}}^2$ or ${T_{\mu\nu}}^2$ were always positive as followed from unitarity (whether due to fermion loops or boson loops) but that cross terms $R_{\mu\nu}T^{\mu\nu}$ had often an opposite sign when one used the Einstein equation $G_{\mu\nu} = -\frac{1}{2}T_{\mu\nu}$. The big question, of course, was what that magical combination of fields was.

It seemed highly probable that it should have an extra symmetry, beyond the spacetime symmetries (general coordinate (=Einstein) invariance and local Lorentz invariance), but it was not clear what that extra symmetry should be. Natural candidates were: local scale, or perhaps even local conformal symmetry, or the fermi-bose symmetry (also called supersymmetry, or "susy") discovered by Gel'fand and Lichtman (1971), Akulov and Volkov (1973) and Wess and Zumino (1974).[2]

One problem with the latter symmetry was that so far no local fermi-bose symmetry had been constructed, only a rigid one. In an early attempt in 1975, Arnowitt and Nath had proposed a gauge theory for supersymmetry in superspace ("supergauge theory" as they called it) which they obtained from Einstein gravity by simply

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2 Originally it was called supergauge symmetry, but because the parameter ϵ^α is constant, it was changed to global supersymmetry. However, to avoid the impression that global meant "defined on the whole manifold", the name was finally changed to rigid supersymmetry, and that is the present name.

letting everywhere all indices become super indices (with a bosonic and a fermionic part). This theory has no constraints and as a consequence it contains higher spin fields and ghosts, and for that reason it has been abandoned. Yet, I recall that already at that time two physicists suggested to study the one loop ultraviolet divergences for spin 3/2 fields coupled to gravity: Salam at the London conference of 1975, and Veltman. I did not get down to computing the divergences of this system, although I reported this as a research project at a conference at Christmas 1975 in Caracas, because I was a bit tired of all these long calculations which in the end always gave a negative result.

In the spring of 1976, Dan Freedman came back from the Ecole Normale Supérieure in Paris, where he had studied various topics in physics, as well as the remarkable food market on the rue Mouffetard. The year before he had with Bernard de Wit applied the low energy theorems of current algebra to spontaneously broken supersymmetric systems, in order to find out whether the neutrino could be the supersymmetric partner of the photon. Their conclusion was negative, as it remains today, although the argument today is not based on current algebra but on the simple fact that in the standard model they have different $SU(3) \times SU(2) \times U(1)$ quantum numbers. (The conjugate Higgs doublet has the same quantum numbers as the (ν_e, e^-) doublet, but there are no partners for the (ν_μ, μ^-) and (ν_τ, τ^-) doublets).

In the very friendly atmosphere of the Institute for Theoretical Physics at Stony Brook, we had lunch together every day in the common room, and much of the remaining time was spent near the coffee machine, which was next to my office. It was only natural that colleagues would enter my office in a relaxed mood with a cup of coffee in their hand, and begin discussing physics. In this way, Dan and I came into scientific contact. Dan suggested that we start looking into a gauge theory of supersymmetry, which I immediately fully embraced because it was something new, exciting, and still in the domain of gravity with spinors where I had spent so much time. In this way we started working together. In Paris, Dan had also met Sergio Ferrara, who was an expert in rigid supersymmetry, and who had suggested to construct a theory of local supersymmetry, and he joined us from CERN. In those days there was no e-mail, but we managed to stay in touch.

So, how should we start? The basic property of rigid supersymmetry was (and is) that the commutator of two supersymmetry transformations gives a translation, $\{Q_\alpha, Q_\beta\} = \gamma^\mu{}_{\alpha\beta} P_\mu$, so upon making supersymmetry local, we would expect to obtain a local translation. Now the concept of local translations looked to us very much like a general coordinate transformation, so we expected that a theory of local supersymmetry would necessarily contain gravity, and this explains the name supergravity for the gauge theory of supersymmetry. Conversely, in the presence of gravity a constant supersymmetry parameter becomes spacetime dependent after a local Lorentz rotation, hence rigid supersymmetry in the presence of gravity must turn into local supersymmetry.

So, local supersymmetry predicted the existence of gravity, and that was for us one of the most attractive aspects of supergravity. Nowadays, people like to motivate their interest in supersymmetry by referring to the hierarchy problem which is solved by supersymmetry (provided one accepts some plausible assumptions which resolve the so-called μ-problem). Also, for supergravity the motivation has changed over time: whereas originally it was hoped that it might solve the nonrenormalizability problem of ordinary quantum gravity, nowadays one considers supergravity rather as the "low-energy" limit of superstring theory. The latter is finite and thus solves the problem of quantum gravity, but for phenomenology one needs the effective field theory which results at low energy, and this effective field theory inevitably caries along with it an infinite tower of higher-dimensional operators divided by powers of the string mass scale, and any truncation of this infinite tower is nonrenormalizable. In 1976, none of these interesting developments were known, of course.

Given that supergravity must contain at least gravity, we expected to need at least one other field, its fermionic partner which should be the gauge field of local supersymmetry. The gravitational field describes gravitons, with spin 2, or rather helicity ± 2, and from the theory of massless irreducible representations of the super Poincaré algebra it was known that susy required fermi-bose pairs with adjacent spins $(j, j+1/2)$. Clearly, we needed either a massless spin 3/2 field, or a massless spin 5/2 field. Any sensible person would begin with spin 3/2, and that is what we did. (Later it was found that one cannot couple massless spin 5/2 fields to gravity in a consistent way. At the level of algebra that is also clear: one would need spin 3/2 generators, but then the anticommutator of two such generators would produce a spin 3 generator, which is not known to exist in 4 dimensions. In 2 dimensions it exists and leads to W gravity, but that we did not know in 1976).

In fact, in the 1960's and 1970's many concepts which are now so well understood that they have become almost trivial, were then confusing. Just to illustrate this, I may tell an anecdote of the 1960's concerning quantization of gauge field theories. My advisor was (and is) referee of *Physics Letters B*, and received one day a paper by Faddeev-Popov dealing with path-integrals, quantization and gauge theories. Now path-integrals were little used in those days, so people were unfamiliar with them. He could not make much sense out of the article (it did not contain their ghosts in the quantum action but rather there was a determinant in the measure) but neither could he find anything obviously wrong with this paper, so he decided, after much hesitation, to accept it for publication. Fortunately (with hindsight), just imagine what would have happened if he had rejected this article.

Although a spin (3/2, 2) doublet seemed to us the obvious choice, massless spin 3/2 fields were in disrepute due to the Johnson-Zwanziger-Velo "theorem". They had observed that if one coupled complex massless spin 3/2 fields to electromagnetism, this coupling was inconsistent. The field equation was expected to be

$\gamma^{[\mu}\gamma^{\nu}\gamma^{\rho]}D_\nu\psi_\rho = 0$ with $D_\mu\psi_\nu = \partial_\mu\psi_\nu - ieA_\mu\psi_\nu$, so upon contracting with D_μ one would get $F_{\mu\nu} = 0$, clearly too strong a condition. These couplings also led to signals which traveled faster than light. We were never intimidated by those no-go theorems, because we believed that the case of spin (1, 3/2) is very different from the case of spin (3/2,2). In fact, by the time you have carefully formulated a no-go theorem, you can often see the solution and turn it into a "yes-go" theorem. (Later it was indeed found that one can couple spin 3/2 to spin 1 provided also gravity to present: this coupling leads to $N = 2$ extended supergravity, which is the susy extension of the Maxwell-Einstein system which unifies electromagnetism and gravity. When we get there, we shall of course come back to the question of " magical cancellation of infinities").

Although we decided to begin with the free field action for spin 3/2 fields and couple it in the usual way (the minimal way, like spin 1/2) to gravity, there arose immediately a problem: which action? We went to the library, and found a paper by Bargman and Wigner, who discussed free-field higher-spin theories, in particular some spin 3/2 theories. Most of them were really field equations with subsidiary conditions, so of no use for us. We were looking for an action with a vector-spinor field ψ_μ, because gauge fields have always the structure of ∂_μ times the parameter. Soon we found a gem of a paper with this field ψ_μ: the famous Rarita-Schwinger paper. These authors had entertained in 1941 the conjecture that the neutrino in β decay had spin 3/2 instead of spin 1/2, and computed the angular distribution of the neutrinos. The results were in complete disaccord with the experimental data, so that was the end of that idea, but for us this was no set-back: it seemed to us that rather than the action for neutrinos in flat space, Rarita and Schwinger had found the leading fermionic term of supergravity. Their free-field action reads (for our purposes we distinguish between curved indices of the gauge fields ψ_μ and flat indices of the constant Dirac matrices γ^m)

$$\mathcal{L}(RS) = -\frac{1}{2}\bar{\psi}_\mu \gamma^{[m}\gamma^n\gamma^{r]}\partial_\nu\psi_\rho\delta^\mu_m\delta^\nu_n\delta^\rho_r \tag{3}$$

and it has a local gauge invariance, namely $\delta\psi_\sigma = \partial_\sigma\epsilon(x)$ where $\epsilon(x)$ is a 4-component spinor, just what one needs for a gauge field of supersymmetry! (Recall that gauge fields always transform into the derivative of the parameter + more). Since the fermionic partner of the real graviton should be real,ψ_σ too should be somehow real. If the matrices $\gamma^0, \gamma^1, \gamma^2, \gamma^3$ (satisfying $\{\gamma^m, \gamma^n\} = 2\eta^{mn}$ with $\eta^{mn} = (-1, +1, +1, +1)$) should be real (a so-called Majorana representation of the Dirac matrices) then also ψ_σ can be taken real, and $\mathcal{L}(RS)$ is real. That seemed a problem to us, because then the conjugate momentum of ψ_μ would be a linear combination of $\psi_\nu{}^\beta$. (Later I learned about Dirac quantization which resolves this problem.) So, we decided to work with complex Dirac matrices but we still needed some reality condition on ψ_μ to avoid overcounting. Here we must make a short technical stop and discuss Majorana spinors.

A Majorana spinor $\psi^\alpha(\alpha = 1, 4)$ satisfies the property that its Majorana conjugate $\bar{\psi}_M \equiv \psi^T C$ (with the charge conjugation matrix C defined by $C\gamma^m C^{-1} = -(\gamma^m)^T$) is equal to its Dirac conjugate $\bar{\psi}_D \equiv \psi^\dagger i\gamma^0$. It is easy to show that $\bar{\psi}_M$ and $\bar{\psi}_D$ transform in the same way under Lorentz transformations, and satisfy the same Dirac equation. So $\bar{\psi}_\mu$ in $\mathcal{L}(RS)$ is both equal to $\psi^T_\mu C$ and $\psi^\dagger_\mu i\gamma^0$; and this shows that the action is hermitian and that $\bar{\psi}_\mu\gamma^{[m}\gamma^n\gamma^{r]}\psi_\rho$ is symmetric in μ and ρ. For what follows it is also important to know that $\bar{\psi}_\mu\gamma^m\psi_\nu$ is antisymmetric in (μ, ν).

In 4 dimensions one can write $\gamma^{[m}\gamma^n\gamma^{r]}$ as $\epsilon^{mnrs}\gamma_5\gamma_s$ (as it in fact occurs in the Rarita-Schwinger paper) and this is useful because putting the Rarita-Schwinger action in curved space (coupling it to gravity), the ϵ-tensor becomes a density and eliminates the need to add the usual factor $\sqrt{-g}$. Furthermore, as I knew from the Einstein-Dirac system, we had to replace $\delta_m{}^\mu$ by "vierbein fields" $e_m{}^\mu$ (tetrads, later called "vielbein" fields by Gell-Mann at the EST conference in San Francisco because vier=four and viel=many in German) and finally we had to replace the curl $\partial_\nu\psi_\rho - \partial_\rho\psi_\nu$ by $D_\nu\psi_\rho - D_\rho\psi_\nu$ where D_ν is a suitable gravitationally covariant derivative. So

$$\mathcal{L}(RS, \text{ gravity }) = -\frac{1}{2}\epsilon^{\mu\nu\rho\sigma}\bar{\psi}_\mu\gamma_5\gamma_\nu D_\rho\psi_\sigma \tag{4}$$

The symbol $\epsilon^{\mu\nu\rho\sigma}$ is ± 1 or 0, and a density while the factor $1/2$ is arbitrary but customary for real (bosonic or fermionic) fields.

The problem was, of course, what that suitable covariant derivative D_ρ was. We knew (for example from Weinberg's book on general relativity) that one possibility was

$$D_\rho\psi_\sigma = \partial_\rho\psi_\sigma - \Gamma_{\rho\sigma}{}^\tau(g)\psi_\tau + \frac{1}{4}\omega_\rho{}^{mn}(e)\gamma_m\gamma_n\psi_\sigma \tag{5}$$

where $\Gamma_{\rho\sigma}{}^\tau(g)$ is the usual Christoffel symbol and $\omega_\rho{}^{mn}(e)$ the spin connection, related to $\Gamma_{\rho\sigma}{}^\tau(g)$ by the "vielbein postulate"

$$\bar{D}_\rho e_\sigma{}^m \equiv \partial_\rho e_\sigma{}^m - \Gamma_{\rho\sigma}{}^\tau(g)e_\tau{}^m + \omega_\rho{}^m{}_n(e)e^n{}_\sigma = 0. \tag{6}$$

But we also studied papers by Hehl and collaborators, who introduced torsion in theories involving bosonic matter fields, where they wrote

$$\Gamma_{\mu\nu}{}^\rho = \Gamma_{\mu\nu}{}^\rho(g) + K_{\mu\nu}{}^\rho \tag{7}$$

with $K_{\mu\nu}{}^\rho = -K_{\nu\mu}{}^\rho$ the "contorsion tensor". We adopted this procedure for our problem and wrote $\omega_\mu{}^{mn} = \omega_\mu{}^{mn}(e) + 3$ terms involving the contorsion tensor, omitting $\Gamma_{\rho\sigma}{}^\tau(g)$ in (4) because it cancelled in the curl. By this ansatz as starting point we already committed ourselves to what is now called second-order formalism (with gravitino torsion).

So, our starting point was

$$\begin{aligned} S^{(0)} &= \int d^4x[\mathcal{L}_2 + \mathcal{L}_{3/2}] = \\ & \int d^4x[-\frac{\sqrt{-g}}{\kappa^2}R - \frac{1}{2}\epsilon^{\mu\nu\rho\sigma}\bar{\psi}_\mu\gamma_5\gamma_\nu D_\rho\psi_\sigma] \end{aligned} \tag{8}$$

where $\gamma_\nu = \gamma_n e_\nu{}^n$ with constant γ_n. The Einstein-Hilbert action is $R = R_{\mu\nu}{}^{mn} e_m{}^\nu e_n{}^\mu$ and $R_{\mu\nu}{}^{mn} = \partial_\mu \omega_\nu{}^{mn} + \omega_\mu{}^m{}_k \omega_\nu{}^k{}_n - \mu \leftrightarrow \nu$ with $\omega_\mu{}^{mn} = \omega_\mu{}^{mn}(e)$ to lowest order in κ. As lowest order supersymmetry transformation rules we took

$$\delta^{(0)} \psi_\mu = \frac{1}{\kappa} D_\mu \epsilon = \frac{1}{\kappa}(\partial_\mu \epsilon + \frac{1}{4} \omega_\mu{}^{mn}(e) \gamma_m \gamma_n \epsilon) \tag{9}$$

with again $\omega_\mu{}^{mn}(e)$ but anticipating further terms, and

$$\delta^{(0)} e_\mu{}^m = \alpha \kappa \bar{\epsilon} \gamma^m \psi_\mu, \ \alpha \text{ a constant.} \tag{10}$$

This latter law was not obvious, but it was linear in fields, just like in rigid susy where one has δ (boson) $\sim$ (fermion) ϵ. An alternative, $\delta e_\mu{}^m = \alpha \kappa \bar{\epsilon} \gamma_\mu \psi_\nu e^\nu{}_n \eta^{nm}$ we rejected because it was not linear in fields. The law for $\delta^{(0)} \psi_\mu$ was also to lowest order in fields and for constant ϵ of the expected form δ (fermion) $=$ ∂ (boson) ϵ, since $\omega_\mu{}^{mn}(e)$ contains to lowest order indeed only terms of the form ∂ (boson), namely derivatives of the vielbein field.

The first test came immediately: are there encouraging cancellations in $\delta I^{(0)}$? One obtains from varying the vielbeins in $\mathcal{L}_2$, using $g_{\mu\nu} = e_\mu{}^m e_\nu{}^n \eta_{mn}$,

$$\delta \mathcal{L}_2 = \frac{1}{\kappa^2}(R^{\mu\nu} - \frac{1}{2} g^{\mu\nu} R) \delta g_{\mu\nu} = (R^{\mu\nu} - \frac{1}{2} g^{\mu\nu} R) \frac{\alpha}{\kappa} (\bar{\epsilon} \gamma_\mu \psi_\nu + \bar{\epsilon} \gamma_\nu \psi_\mu) \tag{11}$$

On the other hand, varying $\bar{\psi}_\mu$ and ψ_σ in $\mathcal{L}_{3/2}$, gave

$$\begin{aligned} \delta \mathcal{L}_{3/2} &= -\frac{\epsilon^{\mu\nu\rho\sigma}}{2\kappa} (\bar{\psi}_\mu \gamma_5 \gamma_\nu D_\rho D_\sigma \epsilon + (D_\mu \bar{\epsilon}) \gamma_5 \gamma_\nu D_\rho \psi_\sigma) \\ &= \frac{\epsilon^{\mu\nu\rho\sigma}}{16\kappa} R_{\rho\sigma}{}^{mn} (\bar{\epsilon} \gamma_5 \{\gamma_\nu, \gamma_m \gamma_n\} \psi_\mu) \end{aligned} \tag{12}$$

where we partially integrated the derivative on $D_\mu \bar{\epsilon}$, used $[D_\rho, D_\sigma]\epsilon = \frac{1}{4} R_{\rho\sigma}{}^{mn} \gamma_m \gamma_n \epsilon$, and finally used the Majorana property $\bar{\psi}_\mu \gamma_5 \gamma_\nu \gamma_m \gamma_n \epsilon = -\bar{\epsilon} \gamma_n \gamma_m \gamma_\nu \gamma_5 \psi_\mu$. We then found a fantastic cancellation ("heart warming" we called it): the variations of $\mathcal{L}_2$ and $\mathcal{L}_{3/2}$ in (11) and (12) actually cancelled. To see this, one may replace $\{\gamma_\nu, \gamma_m\gamma_n\}$ by $2e_\nu{}^t\epsilon_{tmns}\gamma_5\gamma_s$, use $\gamma_5{}^2 = 1$ and write $\epsilon^{\mu\nu\rho\sigma}\epsilon_{rmns}$ as a product of four vielbeins fields, properly antisymmetrized. Then, for suitable α, these variations cancelled.

However, this was only the beginning of a whole series of cancellations which were needed to prove that the final action was susy. Not yet taken into account were: the variations of $\omega_\mu{}^{mn}(e)$, the derivative $D_\mu e_\nu{}^r$ picked up in the process of partial integration and the variation of $e_\nu{}^r$ in γ_ν. We solved this problem by adding new suitable terms of higher order in κ to action and transformation laws each time when the variations of the action did not cancel. ("The Noether method", see below). This was tedious work, which required a steady hand in manipulations with Dirac matrices and Riemannian geometry. Every morning I could hear Dan coming into the institute, humming always the same two sentences, "In heaven there is no beer, that's why we drink it here", but we actually did not drink any beer, but worked very hard, at least 12 hours a day, weekends included, for several months. We never knew whether our approach would work, and many times we thought supergravity was dead, only to find the next day a solution which brought it back to life. An amusing incident happened when at some point we found that a sum of five terms involving Riemann tensors and complicated spinor structures had to cancel. By taking special values for indices and fields, we got strong indications that they did. We started reading J. Schouten's famous book, but did not find there an explanation, and then went to some mathematicians, who got very interested and thought we might have discovered some new identity. Eventually, we realized the truth was much more pedestrian: in 4 dimensions a tensor with 5 indices, totally antisymmetrized, always vanishes. Yet, as a tribute to this episode, we introduced the verb to "Schoutenize" which indicates the interchange of indices which results from this identity, and even today this word can be found in the literature.

In this way we pushed, with a lot of algebra, the proof of invariance up to the level of five gravitino fields and one ϵ in $\delta\mathcal{L}$. This last calculation was so complicated that only a computer seemed able to do it.

We had at that time a connection to the big computer at Brookhaven National Laboratory, at least big for those years. I started writing a simple Fortran program, to collect all variations and check whether the coefficients of all independent spinor combinations were zero. Rather than work with Majorana spinors, we rewrote them as 2 component Weyl spinors since this saved memory , and wrote all terms in the form

$$t^{abcdefgh}(\psi_a^\dagger \sigma_b \psi_c)(\psi_d^\dagger \sigma_e \psi_f)(\epsilon^\dagger \sigma_g \psi_h) \tag{13}$$

where t is an integer-valued tensor constructed from ϵ symbols and Kronecker deltas. In some test runs we found output values like 0.1875. I was puzzled, but for Dan it was obvious that this was $\frac{3}{16}$ (the factor $\frac{1}{16}$ we later traced to our normalization of spinors) and he still sees this as a characteristic difference between a European and American education. (Americans measure length in units of 1/16 of an inch, and students are trained to convert this into decimals.)

Taking into account antisymmetry relations between the spinors, we needed to compute about a thousand coefficients, each of which should come out zero. We spent an enormous time simplifying the program in order to reduce the costs of computing time, (which was in the end of the order of 50 dollars) and we got it down to about 3 minutes. Many trial runs were made to get rid of all bugs, but after days of work, one night everything was ready, and now it was up or down. I was sitting alone that night in the computer room, except for a colleague (Junn-Ming Wang), who often worked late. It was late (2 o'clock at night) and after starting the decisive run and waiting the expected 3 minutes, the results came in. As always the first few hundred entries were zero, but that was no reason for optimism because we already knew that these terms were zero. However, zeros kept coming, and I started making strange noises. Jimmy asked me what was going on, and I told him that I needed still a few hundred zeros, and if there was at least one nonzero entry, all our work would be in vain. The zeros kept coming, the tension mounted and then the program came to the end with only having produced zeros. It worked, supergravity existed!! Instead of being happy I was very, very tired. I phoned Dan, who was in a hotel in Chicago for a conference and who had told me to inform him of the result, no matter what the time was, and he said "Oh, that is wonderful" in also a very tired voice. I then went home, and felt depressed. In fact, I have often heard that physicists feel depressed just after a major discovery; perhaps that is the physicists' equivalent of post-partum depression.

However, the next days we became again enthusiastic. It was clear that an almost endless series of problems lay ahead of us, each problem even more interesting than the previous one. We had to redo for this new gauge theory all that one had done in the past for Yang-Mills gauge theories. The first problem was, of course, the coupling of matter to supergravity. By then it was summer 1976, and I went to Europe (Paris) while Dan went to Aspen. We decided that each should press on with research in supergravity. In Paris, I met for the first time Sergio Ferrara, with his usual cigar, and suggested to him that we try to couple scalar fields to supergravity. That was the first time I noticed his superb instinct for making the right choices, for he told me that my suggestion was excellent and we certainly should try to couple scalars, but perhaps spin 1 fields were even more interesting because of the extra Maxwell gauge invariance. Since I had no strong feelings one way or the other, I accepted his proposal. Later it was found that the coupling to scalars is much more complicated than the coupling to vectors. So the choice of vectors was very lucky. In that collaboration also Joel Scherk joined. At some point we got stuck because we were left with a term proportional to $F_{\mu\alpha}\epsilon^{\alpha\nu\rho\sigma}F_{\rho\sigma}$, but Joel remembered that he had passed a summer in Cambridge deriving (under a tree, but not being hit by apples!) all kind of identities for fun, and he vaguely remembered that there was something interesting with this term. He went to a pile of notebooks in the corner of his office, and produced from the middle a notebook in which he found that this term is proportional to ${\delta_\mu}^\nu$. Joel did work for years with us; he was absolutely creative, and his death in 1980 was a great blow to all workers in supergravity, and to me personally, as I had become very close to him.

This brings me to a point I want to stress here, and which I think is not at all sufficiently understood by physicists outside the circle of supergravity practitioners. From 1976 on, a group of young, enormously enthusiastic physicists did work that, in my opinion, is of an almost unique high standard in physics. Some older physicists have told me later that they also tried to enter the field, but that as soon as they sat down to begin this study, a flood of new papers by these young physicists deflated their energy. The drawback of this situation has been that relatively few senior physicists were involved with supergravity, so that when these young people needed a faculty position they had not always the backing from the establishment which they should have had. Still, looking around, I see that most of them have become professors, and almost all of them are still as active today as then.

The coupling of matter to gravity (and also all subsequent couplings, and also the construction of the gauge action itself) was achieved by using the "Noether method", where one evaluates $\delta\mathcal{L}$ order by order in κ, and when $\delta\mathcal{L}$ is nonzero, one adds further terms to the action and/or transformation laws such that up to that level in κ $\delta\mathcal{L}$ becomes zero. For example, if $\delta\mathcal{L}$ contains a term $\partial_\mu\epsilon$ one could add a new term to the action obtained by replacing $\partial_\mu\epsilon$ by $-\kappa\psi_\mu$ since varying ψ_μ into $\frac{1}{\kappa}\partial_\mu\epsilon$ in the new term would cancel the old variation. However, this would not work with a term like $\bar{\psi}_\mu\gamma^m\partial^\mu\epsilon$ since $\bar{\psi}_\mu\gamma^m\psi^\mu = 0$, so there were fermionic integrability conditions. As a byproduct we also found two alternative derivations of supergravity: 1) by starting with rigidly susy matter and then making ϵ local and at the same time introducing the gauge fields of supergravity, 2) by starting with the S-matrix and 3-point couplings and deducing the 4-point and higher couplings by imposing gauge-invariance (transversality). These approaches are well-known in ordinary gauge field theories, and it was comforting to see that they also worked well here.

In the fall of 1976, after the coupling of spin (1, 1/2) and later spin (0, 1/2) matter of supergravity, another interesting system to consider was the coupling of a rigidly susy spin (3/2, 1) matter system to supergravity which is a spin (3/2,2) system. It seemed to Ferrara and me that there should in the end be an extra O(2) symmetry in the action between both gravitinos, and that is what we found. The resulting system was "N=2 extended supergravity" with $N = 2$ gravitinos. This theory unifies electromagnetism and gravity ("Einstein's dream") by adding gravitinos as "glue". Later, Dan constructed $N = 3$ extended supergravity with Ashok Das, and discovered that one can couple the spin 1 fields to the other fields as an $SO(3)$ Yang-Mills system, provided one also added a supercosmological constant. And then the $N = 4$ and $N = 8$ (and $N = 5, 6, 7$) extended supergravities were constructed.

Of course, the quantization was a topic of major interest. It turned out that the covariant quantization rules of 't Hooft and Veltman could once more be applied, with as gauge-fixing term for susy the expression $\bar{\psi} \cdot \gamma \partial\!\!\!/ \gamma \cdot \psi$, leading to commuting spinorial Faddeev-Popov ghosts. However, because the gauge algebra[3] was "open", one needed an unusual 4-ghost coupling to restore unitarity. A direct Feynman graph calculation revealed that the coupling of supergravity to spin $(0, 1/2)$ or spin $(1/2, 1)$ matter was in general nonrenormalizable, but that in the extended sugras, the infinities cancelled at the one-loop level. So, here finally we found a "magical combination of fields". For me the latter

3 Open gauge algebras, field dependent structure functions, auxiliary fields which close the gauge algebra, and that all in the context of superalgebras has become a whole new field in mathematics.

result was very gratifying because (i) it showed that supergravity was at least one-loop finite, and (ii) it also showed that my previous one-loop calculations for matter-supergravity systems with nonvanishing divergences were correct because they were used as input into this calculation. After the one-loop divergences were found to cancel in the $N = 2$ and $N = 4$ extended supergravities, the question was of course: do they cancel at the 2-loop level? I had a bet with a very good friend for a crate of champagne that they would cancel. Marc Grisaru found a nice argument that they do. [4] Then Stanley Deser and Kelly Stelle found that at 3-loop level one could write down a possible counterterm, but till today nobody has computed its coefficient. Most people believe that its coefficient is nonzero, but nobody knows. (The counterterm is of the generic form R^3. Also in 6 dimensions the one-loop counterterm is of this form, and I have shown that there its coefficient is nonzero. However, I do not think this gives information on the 4-dimensional situation, and it would be interesting if somebody would compute the 4-dimensional coefficient).

Incidentally, the name gravitino has also some history to it. With Marc Grisaru and Hugh Pendleton, I looked into the S-matrix of supergravity, and found relations between various cross-sections such as graviton-graviton scattering and the scattering of two massless spin 3/2 particles. At a short visit to Caltech, Gell-Mann had looked with me in dictionaries for a venerable name for these particles and had come up with "hemitrion" ("half-3"). So, in that S-matrix paper, we wrote "hemitrion-hemitrion scattering", but the editors of *Physical Review* did not allow this neo nomen, and we had to revert to "massless Rarita-Schwinger-massless Rarita-Schwinger scattering". It was Sidney Coleman and Heinz Pagels who coined "gravitino". (Actually, I was surprised some years later to read in a letter of recommendation that Sidney wrote that he was uninterested in gravity and superinterested in supergravity. He seems to have changed his mind a bit).

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4 I got in the end only one bottle of champagne from my friend.

While all this work on supergravity was going on, our students had a golden time, because (unlike today) there were far more exciting and doable problems than people. We also gave many seminars. I recall a few interesting occasions. On one occasion, I was to discuss (at the request of the chairman of that department) the progress in supergravity, and after he had introduced me (with the usual statement that he hoped to have pronounced my name correctly), he whispered to me, "Oh, I forgot to tell you, but please do not use the Dirac equation or other such difficult things because our faculty is mostly specialized in ..." (some other field). That required some improvisation on the spot ! On another occasion, I was in Tallahassee, where to my delight Professor Dirac was in the audience. To my even greater delight, when the chairman asked at the end of my lecture if anybody wanted to ask a question, Dirac raised his hand. "How many anticommuting variables does your theory have?". I quickly thought: at each point in spacetime a real 4-component spinor ($\epsilon^{\alpha}(x)$), so I answered: "Infinity to the fourth power." "That is a large number", he replied. I waited for a further comment, but no more was forthcoming. Later, he told me that Feynman graph calculations were in his opinion not the way to quantum theory; rather, they were like the coupling of Bohr-orbits in the early days of quantum mechanics. I was invited that evening for a dinner at his home, and as I knew that he was not an effusive speaker, I was not surprised that he only turned to me at the end of the dinner to ask me "Have you ever read (the book) the Red Rose?". I said I had not, and again no further comment was forthcoming. I have a short movie from that visit where you can see Dirac swimming among the mangroves. There were also alligators nearby, and I was too afraid to swim, but he told me there was no danger. A last recollection I have is that he told me that he found life in the USA a bit different from life in England. "Did you know that if you buy here a grand piano you get a gun for free?" I have now lived in the States for 20 years, and must agree with him that it sometimes is a bit different from Europe, but it is a very positive optimistic country and as a physicist I appreciate that young people are treated equally to older people, and that there is not much secrecy in appointments or promotions.

These were a few recollections of the exciting early days of supergravity. Supergravity then went on: there came a Kaluza-Klein era, and a 2-dimensional era with σ models, and supersymmetric quantum mechanics, and then came superstring theory which is also a kind of supergravity theory as it is also based on a local fermi-bose symmetry. We have now reached a level of sophistication where we should be able to explain nature around us, and, as always in fundamental science, many people become somewhat pessimistic about the chances of success. Some people go even further these days and say that particle physics is dead. Also that has been said before. I would like to state that the unification of running coupling constants I mentioned before is a clear though indirect manifestation of the existence in nature of rigid susy. Also gravity exists. Rigid susy plus gravity is supergravity, that we showed in 1976. For these reasons, I must conclude that supergravity exists and will be detected. I am confident that nature is aware of our efforts.