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A brief history of Supergravity: the first 3 weeks

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Abstract

I summarize, at its 41$^{\rm st}$ – and what would have been Bruno's 94$^{\rm th}$ – birthday, the history of the discoveries of Supergravity.

1 Background

Briefly, the relevant background – and the ideas in the air – prior to Supergravity's (SUGRA's) inception lay in two new, if quite different, realms: supersymmetry (SUSY), and the, also emerging, difficulties in achieving consistent gravity-higher ($s > 1$) spin gauge field interaction. Indeed, the Western discoverers of SUSY, Julius Wess and Bruno Zumino [1], would frequently visit Boston from NYU to spread the SUSY gospel, which did get even our blasé attention after a while, especially since the simplest SUSY multiplet pattern $(s, s + 1/2)$ linking adjoining Fermi-Bose fields had no obvious reason to stop at the then studied $s = 0$ and $s = 1/2$ models. The specific idea of a SUGRA was also in the air, both in terms of spacetime-dependent SUSY parameters [1] and of (albeit too general at the time) superspace approaches [2]. Separately, I had been intrigued for some time by the woes of higher spin gauge fields interacting – as they must – with gravity [2,3], and was particularly struck by Buchdahl's [4] early, if classical, study of the first difficult, $s = 3/2$, case.

A rather different, but extremely potent, motivation was the striking improvement observed in the UV behavior of SUSY models, where infinities from the Bose/Fermi components miraculously cancel each other. It had been shown a bit earlier that quantum gravity coupled to matter was in dire need of miraculous cancellations, being non-renormalizable already at 1-loop level for spin $(0, 1/2, 1$ – both Maxwell and YM) matter sources [5], while the source-free system was known to diverge at the next, 2-loop, level [6]. That UV hope was an enticing – if still unresolved – carrot; indeed, SUGRA not only shares the one-loop finiteness of pure GR [7] – the only "matter" field to do so, but stays finite at 2 loops because there are no ($\sim$Riemann^3) invariant counterterms there. The (Riemann^4) stick, found a bit later [8], only strikes from 3 loops on. On a personal level, I had invited Zumino to lecture at the last – 1970 – Brandeis Theory Summer school; his notes were such a goldmine of deep QFT ideas that they are cited to this day. We stayed in touch thereafter, and his presence was a strong motivation to spend my Sabbatical term at CERN. After our initial SUGRA work, we wrote a number of related papers in the field, including one on the 2D basis of "Superstring SUGRA" [9]. In the eighties we also co-led an ITP Semester session: ours were longstanding scientific ties.

2 Action

When I arrived, as a visiting member, on the evening of April 1,1976, Bruno was awaiting me in the CERN cafeteria and we instantly began our nonstop 3-week endeavor – especially at night, when we filled the CERN lecture hall's immense blackboards with our seemingly endless Fierz identity slogs! [We were once startled by another insomniac, Claud Lovelace, lurking in the curtains.] I had to go back to the US at dawn on the 22^{nd} for a short stay; fortunately, we were done with all calculations and writeup by then, and Bruno was to hand our manuscript [10] to Raoul Gatto, the local Editor of Phys. Lett. B, CERN's then "house organ". One reason our work succeeded so rapidly was that we were both familiar with two papers by the great mathematician Hermann Weyl on coupling (Dirac) spinors to gravity [11]. The first and oldest of these, showing how to do it, is well-known; the second – directly relevant for us – some two decades later, in 1950, was, and remains, an obscure gloss on the first. That was the year I began my student subscription to the Physical Review and pretended to read the fancy theory papers, in particular that one. Weyl noticed, in that short afterthought, that spinors can couple to affinities two ways, namely "first" and "second" order: either the affinities are regarded as independent variables or expressed as vierbein affinities, and that those two ways differed by (non-derivative) terms quartic in the spinors – the torsion. Weyl's notation was rather cumbersome, and ours were real Majorana vector-, rather than Weyl's complex Dirac-, spinors, so we worked that dictionary out for ourselves, even though our aim was precisely to avoid dealing with the horrors of torsion by sticking to first order. I was of course a rabid first order fan, as that was the basis of the 1959-62 "ADM" formulation of GR, and Bruno was ambidextrous as well. [Indeed, we could have saved a lot of chalk by not worrying about the transformation properties of the independent (in first order) affinity, which is essentially just a Lagrange multiplier.] The beauty of our approach (pace Boltzmann's dictum that elegance is for tailors, not physicists) is that the entire action in [10] is the two-term sum of the (first order) Einstein and (the modern version of) minimally gravitationally coupled Rarita-Schwinger-Davydov-Ginzburg actions (another independent, West-East this time, discovery). The latter part just involves a covariant curl of the vector-spinor (actually only of its spinor part, of course) using the independent, non-metric, affinity, as does the Einstein ("Palatini") action. That's it – neither quartic spinor nor auxiliary field debris! This deceptively naive form of the coupled field equations obeyed the required "Bianchi" identities – that their divergences vanished, both for the gravitational and spin 3/2 field equations. That of the latter is mandated by SUGRA's invariance under the local, fermionic gauge transformation generalizing that of the flat space massless 3/2 field (again, simply replacing an ordinary partial- by a covariant- derivative) while the vierbein transforms like all SUSY bosons, with no derivatives of the gauge parameter. The vanishing of the spin 3/2 field equation's divergence is in fact related to the other influence I mentioned at the start, Buchdahl's [4] remark that the massless spin 3/2 equation is only consistent in Ricci-flat spaces, because its divergence is proportional to the 2-index Ricci tensor that arises from the resulting covariant derivative commutator. He thought this meant this matter field could not consistently live in a General Relativity background (except as a test field), since the Ricci tensor was proportional to its stress tensor, so could not vanish. What this classical study omitted of course was the power of Fierz identities in (necessarily) quantized vector-spinor fields to show that this stress-tensor contribution vanishes identically (as it had better, not being gauge-invariant by itself). It also sealed the fate, as was later shown explicitly [12], of "hypergravities", systems like $(2, 5/2)$ because the spin 5/2 tensor-spinor field equation's divergence has more indices, hence unavoidably brings in the full Riemann (rather than just Ricci) tensor, whose Weyl tensor part is left undetermined by the Einstein equations. This completes the (admittedly all too compressed, but accurate) survey of the motivation, history, and not least – conceptual plus technical – tools (and sweat, at our already advanced physics ages) involved in [10].

3 Re-action

When we started, Bruno had just received (in those pre-internet days) a preprint of the initial version of the other proto-SUGRA group [13], attempting to stake a claim to ownership of SUGRA, based on lowest order coupling in the traditional second order formalism. However, there is a long, instructive, history of would-be extended gauge invariant systems: Lowest order coupling always works, only to fail at next order – the real test of consistency only occurs after the presumed first step's effect impinges on the next one; indeed, the $(2, 5/2)$ model is of this type. Further, the $(3/2, 2)$ idea was in any case already very much in play, as mentioned earlier. After we had finished, Bruno phoned me in the US to report that a second version of [13] had just arrived, stating that an extensive computer calculation of their quartic (in the fermions) remainder terms had finally shown them all to vanish; we instantly, and unilaterally, decided to cite their results in our published version. As we have seen, both results were entirely on a par in timing, while all aspects of the model's constructions were mutually independent technically and conceptually. This was yet another example of simultaneous scientific discoveries, unsurprisingly when the right ideas are in the air. Most familiar in recent times are the three separate inventions of the "Higgs" in the sixties, and most relevant to our theme, the East-West groups who found SUSY in the late '60s and early '70s, or the still earlier West-East spin-3/2 pioneers.

There is usually a reasonably fair consensus in such cases as to who did what when and deserves what share of the credit. To be sure, at the essential, scientific, level, each group is intrinsically rewarded just by seeing the dazzling new light for the first time, and the real (as against alternate) truth is graven in the Platonic heavens. Here on earth, however, a Whig twist occasionally prevails, as it seems to have done here. This anniversary provides a chance to correct it, as Bruno often wished.

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