A Century of Gravity: 1901–2000 (plus some 2001)

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This lecture consists of two parts. The first is a (totally unsystematic) survey of some of the high points in the evolution of gravity and its successors, primarily in the course of the past century. The second summarizes some new work on surprising properties of higher (> 1) spin fields in cosmological backgrounds: the presence of Λ gives rise to discrete sets of massive models endowed with gauge invariances, that divide the \( (m^2, \Lambda) \) plane into unitary and non-unitary phases. The unitary region common to fermions and bosons shrinks to flat space \( (\Lambda \to 0) \) as their spins increase.

1 Introduction

The scope implicit in this Conference’s title seemed to mandate a suitably sweeping topic. My initial choice, “100\(^2\), the Sequel”, would permit daring extrapolations from the present without risk of contradiction. I quickly realized, however, that it would also require truly long-term vision; the other permutation, 2100, being too close, I will instead go modestly backwards, and try to retrodict some of the high points, especially of the past century, in the development of gravitational physics. Indeed, this period has seen an amazing progression, bringing the subject from a prolonged stasis to the center (for good or ill) of almost all fundamental research since the standard model, leaping (at least initially) over the desert between GeV and Planck scales. Most of you will know most of this history, and will have your own emphasis and judgements; nevertheless it may be useful to think back to some of our smart (if ignorant) predecessors. In order to keep the historical account as neutral as possible (and partly from laziness), I will provide no references and mention no living names (you know who you are!).

Not being a historian, however, I will balance the talk with the usual stuff of lectures – new, 2001, physics – even if it means a considerable descent from the heroic deeds of yore. So the second, orthogonal, part of my talk will give a brief (referenced) account of current work, in collaboration with Andrew Waldron, on the gravity/particle theory interface, namely the properties of “partially massless” (the quotes are deserved) higher spin fields in cosmological backgrounds – deSitter (dS) or anti-deSitter (AdS). The new results here are quite surprising when compared with our usual Minkowski background concepts: There is partial masslessness and gauge invariance at tuned values of \( m^2 \neq 0 \), a phase plane with coordinates \( (m^2, \Lambda) \) divided by these gauge lines into unitarily allowed and forbidden regions. The region of

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common boson/fermion unitarity puts bounds on, or even forbids a cosmological constant! In presenting this research, I also hope to satisfy those who are history-averse: they can skip directly to it.

2 On Whose Shoulders?

As I launch into the past, I reemphasize that I have not tried to be historically accurate, instead attempting to remember what seems most seminal as one looks back over the past three centuries (in almost as few pages!). For this reason, I begin with a particularly impressionistic sketch of the prehistory, i.e., the origins of modern gravitational theory.

The immediate answer to all questions is of course “Newton”, and so it should be. But we must give their due to the ancient observers, particularly the Arabs, Chinese and Greeks, the mechanicians of the Middle Ages and the geometers/arithmeticians who helped bring mathematics to a stage sufficiently useful to physics. It was fortunate that our solar system sufficed as an arena, since it was both easily and secularly observable and received enough of society’s support (not always for disinterested reasons) to provide enough data. The great predecessors nearest to Newton (both as observers and theoreticians) include Brahe, Copernicus, Descartes, Galileo (who also performed notable terrestrial experiments) and Kepler. I would add amongst the theoreticians, Rømer, that ancestor of special relativity, who put \( c \) on the map, unwittingly undermining Newtonian gravity at its birth and adding his third to our triad of fundamental units. In Newton’s own shadow we must (at least) include Halley, Hooke and Leibnitz. We should also remember the vast development, stimulated by the Newtonian scheme, of mechanics and mathematics by Bessel, Euler, Laplace, Lagrange, Poisson and indeed most of the greats of the 18th and early 19th century. Their elaboration of its ramifications, such as perturbation theory, and the many-body problem were to be invaluable (as we have all learned) throughout physics, even unto the birth of quantum mechanics. This fruitful interplay between mathematics and physics (so marvelled at by Wigner) has always been present: The Gauss-Riemann constellation is its mid-19th century culmination, presaging (and making possible) the geometrization of physics in a very direct way. Hilbert, Poincaré, Weyl and their living successors have continued the tradition. But enough ancient history; I’m supposed to cover the 20th century!

3 The Heroic Era: 1901–1950

Let’s, rather arbitrarily, separate the beginnings of the new gravity from its modern and postmodern avatars. Most conveniently for our chronology, the obvious symbol for this division is the Planck length, conceived, providentially, at the start of my survey. [Planck’s Note, in the Berliner Berichte, introduced it almost as an aside.] The next few years belonged of course to special relativity, when \( c \) became a conversion factor, spacetime as \( D=3+1 \) was born, but geometry remained a rigid, \textit{a priori} fixed, background. Incidentally, it is here the
seeds of $D > 4$ were sown: $D=5$ was born in flat space in 1913 when Nordström did just the opposite of what Kaluza–Klein were to propose: he made electrodynamics part of a 5-vector in order to unify it with scalar gravity! While it was clear after 1905 that the instantaneous action-at-a-distance picture was doomed, that (at the very least) $\nabla^2$ had to be replaced by $\Box$, what emerged initially were other simple scalar gravity models (Abraham, Mie, Poincaré) also doomed at birth, but at least not by Rømer. Only Einstein kept his thoughts on the universality and accuracy (Eötvös) of the inertial/gravitational mass ratio, and very likely on the fact that systems with vanishing $T^\alpha_\alpha$ would not gravitate in scalar theories.

The overwhelming triumph of this period (after its gestation in 1909–1914) was of course Einstein’s general relativity (GR) in 1915. So overwhelming is this sudden revelation that we tend to forget some of its miraculous triumphs, e.g., its automatic explanation of the above mass ratios – an enormous quantitative feat (there are a lot of very accurately measured elevators out there!), or that because special relativity is built-in as the local tangent space, this (mostly) guaranteed causal signal propagation and the observed local properties of normal non-gravitational physics. Let us also pay tribute to that incredibly useful invention, the summation convention (whoever has seen Einstein’s notes from before and after this illumination can vouch for its effect on his work, let alone ours!). Einstein’s introduction, already in 1917, of the cosmological constant $\Lambda$ showed his instinctive understanding that, in physics, all that is not forbidden is compulsory (or at least we don’t yet have a natural forbiddenness for $\Lambda$, except in one model, unbroken $D=11$ supergravity).

Geometry⇔Dynamics has been the pillar of all subsequent unification dreams. Indeed, as in all matters, Einstein was ahead of his time: after writing $G_{\mu\nu} = \kappa^2 T_{\mu\nu}$, he immediately asked who ordered the right-hand side. Unfortunately for him, it was the right question at the wrong time and was to lose him in his later years, but we all still pursue it, with (we hope) deeper wisdom: some version of it will always be the question. Let me cite a much less known example of Einstein’s foresight to show that great steps also engender subtleties: Very early on he realized the dangers inherent in a dynamically determined geometry: how, he asked, could we be sure that it would always be well-behaved, namely causal? He understood the horrors of time travel and predicted that “good” sources would somehow not generate “bad” spaces, and so it has turned out.

The next triumphs were Schwarzschild’s rigorous black hole solution the following year, showing also that global properties count (as was realized only long after), actually preceded by Einstein’s description and calculation of the famous three tests. While we usually say that of these, only perihelion precession probes post-linearized effects, that is a bit narrow in that linearized theory is a fundamentally inconsistent (if very useful) model. [Birkhoff’s theorem, in this connection, was an early way to say there would be no monopole or dipole radiation.]

Consistent cosmology started in the early twenties, with Friedmann and Lemaitre as some of its pioneers. In the same period, came the next big generalization: geometry-driven Kaluza–Klein unification by use of higher dimensions, for them the simplest step, $D \rightarrow 5$. After suffering many swings of fashion, $D > 4$ (or even $D >> 4$) seems to be here to
stay, as are indeed both $D=3$ and $D=2$! [As everyone at this Conference knows, the more sophisticated, Klein, version was born in Ann Arbor. Klein was simultaneously teaching GR and electromagnetism and tried to lighten his load by this stratagem; great as that version of history would be, the truth is that he was actually inspired by teaching mechanics, in fact the Hamilton–Jacobi method in background gravitational and electromagnetic fields.] Note also the concomitant introduction of topology, i.e., compactification of the fifth dimension, and of global geometry to quantize a physical parameter, here electric charge, a precursor to the quantization of other constants, such as the non-Abelian Chern–Simons coefficients.

After this intensively successful period, progress slowed, partly because of the irresistible appeal of quantum mechanics, QFT and particle physics, partly because the broad lines of the classical theory had been understood and no earthshakingly novel observations were at hand (except, as usual, in cosmology: Hubble expanded our world already in 1929). Indeed, things got moving again in a big way when GR begun to be investigated, first as a classical, and then more painfully as a quantum field theory, in the late fifties. To be sure, there was plenty of important prewar progress. A short list would include: the expression, by Fock, Klein, Schrödinger, Weyl and others around 1929 of spinor theory in curved space, a formal but significant start towards quantum gravity, let alone supergravity! Then there was (in no particular time order) the work of Oppenheimer and his school, and Chandrasekhar’s, on black holes, while “time travel” solutions, most notably Gödel’s provided suitable puzzles. On the mathematical side we may cite the beginning of rigorous analysis of the Einstein equations as a normal hyperbolic system, with propagating and perfectly real gravitational waves (something that was not at all obvious to many people at the time) by Darmois, Lichnerowicz and his school amongst others. Perhaps more significant to the quantum side of our story were the early recognitions, by Rosenfeld already in 1930, that GR had to be quantized if only because its matter sources are, and by Heisenberg in 1938 that the dimensionality of the Einstein coupling constant boded ill for the ultraviolet behavior of quantum gravity, just as it did for the then new Fermi weak interaction model. The thirties also saw the rebirth of the linearized approximation as the field theory of massless spin 2 at the hands of Fierz, Pauli and independently by Bronshtein in the USSR (before he disappeared in the Stalin purges). We must also not forget the modern understanding of the stress-tensor as a (covariantly) conserved current by Belinfante and Rosenfeld.

Important as it was, the progress I have traced from say 1925 to WWII was distinctly low-budget, with few adepts: it was a very unfashionable path to follow, and suffered from the absence of new experimental input as compared to lower spin physics. [To be sure, while Einstein didn’t discover GR due to immediate pressure of new data, his incorporation of hitherto unexplained observations certainly qualified as being experimentally driven.] What were the reactions of the great quantum innovators to GR? Some of the giants of the quantum era later turned into relativists – Schrödinger was perhaps the first (in fact he had also worked in GR long before 1925) although he foundered on the same dead end as Einstein; Dirac started much later and stayed (mostly) within the existing theory as a dynamical system, but was also driven by the “large number problems” to consider gravity with variable gravitational constant. Pauli, whose breakthrough was (at 18) with one of the first texts
on GR, essentially stayed on the sidelines thereafter. Jordan is best remembered for the scalar-tensor theory and Klein remained active in both $D=4$ and 5. Others, like Bohr, Born, Heisenberg, Wigner and Planck himself, never really entered GR. Fermi’s very first papers (he was inspired as a young student by the Italian geometry school) were in GR, and as might be expected, are useful to this very day. The converse reaction was Einstein’s refusal to enter the promised land of QM!

4 Modern and Postmodern 1950–2000

With the general postwar physics upsurge came a new wave here as well. Initially, much talent was deployed in generating, and classifying types of solutions; this has remained an industry, with such beautiful results as the rotating black hole geometry (that seems just now to have found its observational realization!), chaotic cosmologies, “radiative” metrics and their geometric representations, as well as the deepening understanding of black holes. From the late fifties, GR began to be studied as a gauge field theory, to which physical concepts such as energy and the usual apparatus of Hamiltonian dynamics could be applied, under appropriate asymptotic behavior. Likewise, the covariant formulation of the theory, including the (index-heavy) perturbative calculational rules began to be used for tree, and later loop, calculations. Indeed the complications peculiar to nonabelian theories, such as ghosts and their role in keeping the rules free of unitarity paradoxes were understood here in the early sixties. The seventies brought in a wider integration into the rest of QFT. For example, gravitational conformal anomalies began to be explored by the mid-seventies. These anomalies were in turn connected to black hole radiation as part of a new (and still very strong) industry of QFT on a fixed gravitational background, that enabled progress to be made in quantized matter-gravity interactions.

There was also a dark side to the new results: as Heisenberg had feared, the dimensional coupling constant of GR indeed gives rise to catastrophic ultraviolet loop effects. [The flip side is that the infrared end is totally unproblematic, precisely because stress tensors, i.e., vertices, contain a higher power of momentum than vector currents do.] Let’s recall rapidly what the generic problem is, say for a bosonic field (fermions are similar): The vertex (stress-tensor) behaves as $\sim p^\mu p^\nu$, while the propagator is $\sim p^{-2}$. Thus the insertion of an extra internal line into a given loop order diagram involves two new vertices and three new denominators, together with a new virtual $d^4k$ integration, a process that (barring miracles) leads to increasingly virulent infinities and hence counterterms with increasing powers of curvature at each loop order. That this is also the case in practice was rapidly borne out in three different sectors. First, the calculational rules were sufficiently streamlined to show (almost thirty years ago) that at one loop level gravity plus a scalar field is already non-renormalizable, requiring non-vanishing new counterterms. While pure GR, itself a self-interacting quantum system, also led to new infinities, by an accident of $D=4$ geometry, the corresponding terms could be removed by field redefinitions since they involved local terms quadratic in curvature. All these terms vanish when the external gravitational lines are...
on-shell \( (R_{\mu\nu} = 0) \) by the Gauss–Bonnet identity, which reduces the three possible quadratic terms (including \( \int R_{\mu\nu\alpha\beta}^2 \)) to just \( \int R^2 \) or \( \int R_{\mu\nu}^2 \). This ought no longer to be the case at 2 loops, but here the calculations become almost insuperable. Nevertheless, they were carried out around 1986 and the corresponding unremovable \( R_{\mu\nu\alpha\beta}^3 \) counterterms were present, as was independently verified a few years later. The final nails in the gravity plus matter coffin were driven in by a series of one-loop calculation (also in the early seventies) including Dirac particles, photons and Yang–Mills fields coupled to gravity: even for matter gauge theories there were no miracles at all, bearing out the need for something new, at least within the perturbative framework that is the only available tool in QFT. The inescapable conclusion is that GR (like the Fermi theory) is at best an effective low energy theory, and must be the limit of a more fundamental one.

Sure enough, something, indeed two new things, did show up. The first, string theory, had come up a bit earlier, emerging from strong interaction theory (the dual resonance model). That closed strings contained massless spin 2 excitations in the zero slope limit and that these could, indeed must, be gravitons was duly noted but remained unexploited until the string revolutions began in the ’80s. Less exotic, but more immediately accessible, was of course Supergravity (SUGRA), the local gauge generalization of ordinary flat space supersymmetry’s invariance under constant spinor transformations, turning the graviton into part of a doublet with a spin 3/2 field. Found exactly 25 years ago this Spring, the so called \( N=1, D=4 \) model was rapidly generalized both in dimension and in field content. At first it seemed just the ticket to finiteness: not only did it share pure GR’s 1-loop escape, but unlike GR it did not permit 2-loop (super-)gauge-invariant counterterms. Alas, it was shown very soon to have plenty of possible ones at 3 loops and up! So in this sense SUGRA was no better a gauge theory than GR plus ordinary matter, except to defer the infinities to a higher loop order. Indeed, quite recently it was shown explicitly that even the most beautiful and ultimate SUGRA, that of \( D=11 \), is already 2 loop nonrenormalizable, requiring specifiable infinite counterterms. Thus, the short-term dream for SUGRA as both a unified and finite theory has proven invalid at least as to finiteness. Nevertheless, many people continue to share the hope that \( D=11 \) SUGRA, so unique that it cannot have matter sources at all (a beautiful realization of Einstein’s dream about no right-hand side of the ultimate field equations) is, in some broken version, at least part of the next big step.

It is impractical to attempt any detailed account of gravity’s progress (or at least annexation!) beyond this period: the postmodern era is very much unfinished business on the string, M-theory, brane and novel compactification fronts. We have gone long beyond pure GR, but with no realistic unification in place yet. This period has however been full of observational (and associated theoretical) triumphs. Three examples are: (1) the binary pulsars and gravitational radiation, with total and extremely accurate agreement with GR calculations. (2) The development and increasingly accurate vindication of the big bang picture, albeit with many novel puzzles from our apparently living in a deSitter cosmology to the fact that known matter is practically “in the noise” compared to as yet unknown but seemingly necessary new forms of it. (3) Some form of inflation as the key to “homogeneous and isotropic”. We await the start of gravitational wave observations from
LIGO and its several acronymic counterparts into a new form of astronomy, and expect continued improvement in a variety of observational regimes, both here and aloft. All this information will bring in a new phenomenology, if not yet the desired superunifications. So the new century (let alone millennium), can be safely predicted to be an exciting one!

5 Progress and Mysteries; a Summary

Let me close this survey with a (random) summary of what was said about the current big picture and of its mysteries. Some main lessons from the evolution of gravity have been:

1. Instantaneous scalar, linear, Newtonian gravity has been naturally required by special relativity and universality of coupling (including notably light bending) to transform into a propagating tensor theory, and (on consistency grounds) necessarily a self-interacting one. This necessity immediately yields Einstein theory, i.e., geometry, as was shown explicitly some 3 decades ago: dynamics and geometry are unavoidably unified.

2. Cosmology acquires a natural basis; and, for better or worse, a cosmological constant can be present. Many observed phenomena in the large find a satisfactory underlying explanation, others just the opposite.

3. Geometry as the generic road to unification. Invariances specify the correct dynamics in all areas, including the non-Abelian gauge theories of the standard model: there is now just one (generalized) T-shirt motto, \([D_\mu, D_\nu] = F_{\mu\nu}\).

4. Gravity reemerges as an experimentally driven science: the 3 “old” tests, supplemented by two more recent ones, time delay and lunar ranging; binary pulsars and the reality of gravitational waves; black hole observations, and of course the mapping of the big bang cosmos are just some examples.

5. The importance of thinking globally: topological invariants, coordinate identifications, and understanding of global properties of e.g., black holes; singularity theorems; Lorentz generators in asymptotically flat spaces, \(D=3\) gravity where dynamics is just global geometry, Chern–Simons terms in (odd-dimensional) physics. Finally, we mention \(D=2\) (super)gravity where only “the smile” is left, but just enough to generate the (super) string action in a precise way through its (lower spin) matter sources.

6. The liberation of dimensions: \(D = 4 + n\) as a post-Einstein road to geometrical unification, with mere Kaluza–Klein \(n=1\) now replaced by (at least) \(n=6\) or 7. The sensitivity of supergravity to \(D\), with its natural upper limit \(D=11\), along with superstrings’ \(D=10\). Compactifications on wildly different (not just KK Planck) scales, and with new building blocks, branes.
7. Gravity and Geometry redux: GR and SUGRA as mere local limits of much broader systems, e.g., strings or M-theory or their non-commutating generalizations.

The above list is both incomplete and not at all meant to convey relative importance in terms of prospects for the future survival or incorporation into the next breakthroughs, of any or all of these ideas.

With all really deep progress comes a set of mysteries and gravity is no exception. Here is one such enumeration:

1. One of the true formal triumphs that deserved to have its own place in our first list is certainly SUGRA, particularly its role as the “Dirac square root” of geometry. Just knowing that GR has such a square root has given us enormous insights, for example that its energy must be positive and flat space its vacuum. Yet after a quarter-century SUGRA’s role and scope have not yet been fully understood, particularly its most mysterious maximal $D=11$ version. That $D$ must be bounded by 11 is really a consequence of the well-known problems (unavoidable in $D > 11$) of coupling higher ($s > 2$) spin gauge fields to gravity. Rather, the mysteries seem to lie in the $D=11$ theory’s uniqueness: neither supersymmetric “matter” (lower spin multiplets) to provide a “right-hand side”, nor even a cosmological term can be present. It requires a (cubic) Chern–Simons term for its 3-form field, one that is (perversely) $P$-even. Is this model really a “mere” QFT (remember that all SUGRAs are necessarily quantum rather than classical theories due to the fermionic $s = 3/2$ companion of the graviton) or (as is now thought) on a footing with e.g., string theories? We do know, as mentioned earlier, that it is not a magically perturbatively finite QFT.

2. Compactification: the freedom of living in $D > 4$ both in QFT and in string theory brings with it the onus of providing a credible mechanism for landing in the observed corner that is our $D=4$ world. This has been one of the leading challenges of the past decades.

3. Black hole physics. Despite tantalizing clues, and much beautiful speculation, it seems fair to say that we do not yet have sufficient understanding of the statistical mechanics basis for thermodynamic laws, nor of the black hole radiation and information loss problem, or of the holography – surface degrees of freedom connection, amongst others.

4. The cosmological constant problem. This one is too well-known to require mention, but apart from the grotesque discrepancy between naive zero-point matter energy and the observed size of the universe, things are in some ways worse now that observation seemingly dictates a non-vanishing deSitter (dS) value of $\Lambda$. This means that any future fundamental way to set $\Lambda$ strictly to zero will now require a plausible symmetry breaking. In addition, if we must accept $\Lambda > 0$, we must also cope with the formal horrors of doing physics in a dS universe with its intrinsic horizon and other “non-Minkowskian” properties (not that $\Lambda < 0$, anti-dS, is any better!).
5. Assuming that string, or M-theory or their successors do indeed incorporate classical GR in a satisfactory and finite way, in that it indeed emerges as some zero slope effective limit, then we are still stuck at the old desert crossing dilemma: Do these deeper “geometries” really also contain the physics of the standard model in a unique, recognizable way, and if so how is the gap of 18 or so orders of magnitude accounted for? It is after all quite possible that we have been deluding ourselves in assuming that whatever legitimizes GR (or SUGRA) by incorporating it in some consistent quantum scheme necessarily also includes precisely the matter we study at the standard model level. This criticism, often heard in the early days of string theory, is not yet entirely obsolete.

The above problems, and especially those we have not even conceived of, should provide fun for the next century. Of course, the real fun will be the rest of our millenium when we try to incorporate (or do away with) the other – sub-Planck scale – desert! But as Bohr reminded us, prediction, especially of the future, is treacherous and he even proved it: a couple of years before the 1925 QM revolution, Bohr was supposed to have doubted that any true improvement over the old quantum theory would come in his lifetime. Might the (next) TOF also be just around the corner?

6 A 2001 Sample: Surprises from Higher Spin Fields in (A)dS

There should be at least some original physics even in a survey, and as threatened, here is my 2001 contribution. I propose to summarize some current work [1, 2, 3, 4] at the gravity-particle interface, namely exotic properties of ordinary familiar free fields living in cosmological spaces.

Owing to (another kind of) space constraints, I will begin at the end, namely state some of the main results: A non-vanishing $\Lambda$ (for us $\Lambda$ has dimension $m^2$) necessarily alters the kinematics of free massive higher ($s > 1$) spin fields in such a way that (for appropriate discrete values of the $m^2/\Lambda$ ratio) there are “partially massless” models, whose novel gauge invariances can successively eliminate all possible sets of lower helicities: These eliminated sets not only include all those from $(s - 1)$ down to zero or 1/2 as in the usual flat space massless gauge models, but also just helicity 0 or 1/2, or $(1/2,3/2)$ or $(0,1)$ all the way up. These discrete “tuned” lines act as “phase transition” in the $(m^2, \Lambda)$ plane, separating regions that are either forbidden and permitted according to whether they contain excitations that violate unitarity or not: Essentially, there is only one allowed, unitary, region for bosons and one for fermions. The two are mostly complementary, but possess an interesting overlap about the $\Lambda = 0$ axis in this plane. In the limit, as fields of arbitrarily high spin (independent of details of the masses) are included, the unitarity region on each side narrows precisely to the $\Lambda = 0$ line: no $\Lambda \neq 0$ cosmological constant space can permit infinite higher spin towers. Here is the picture that is worth all the above words:
Figure 1: The top/bottom halves of the half-plane represent dS/AdS (and also bosons/fermions) respectively. The $m^2 = 0$ vertical is the familiar massless helicity $\pm s$ system, while the other lines in dS represent truncated (bosonic) multiplets of partial gauge invariance: the lowest has no helicity zero, the next no helicities $(0, \pm 1)$, etc. Apart from these discrete lines, bosonic unitarity is preserved only in the region below the lowest line, namely that including flat space (the horizontal) and all of AdS. In the AdS sector, it is the topmost line that represents the pure gauge helicity $\pm s$ fermion, while the whole region below it, including the partially massless lines, is non-unitary. Thus, for fermions, only the region above the top line, including the flat space horizontal and all of dS, is allowed. Hence the overlap between permitted regions straddles the $\Lambda = 0$ horizontal and shrinks down to it as the spins in the tower of spinning particles grow; only $\Lambda = 0$ is allowed for generic ($m^2$ not growing as $s^2$) infinite towers.

Specifically, one may deduce [4] from the fact that the transition line gauge models can be shown to all have null propagation (another remarkable property of these partially
massive theories) that the unitary region is bounded by the lines

\[ 3m_0^2 = \Lambda s(s-1) ; \quad 3m_f^2 = -\Lambda(s-1/2)^2 \]  

(1)

respectively. [Since the relevant bosons/fermion regions are dS/AdS (\(\Lambda > 0/\Lambda < 0\)), the signs in (1) are correct as shown.] Clearly, as \(s\) increases, then (unless the masses conspire to rise as high as quadratically with spin) the allowed \(\Lambda\) must clearly vanish in both half angles above, \(i.e.,\) infinite towers of higher spins can only exist in \(\Lambda = 0\) spaces. Whether the above mechanism has any relevance to the infinite towers of higher spins in string slope expansions is far from clear. It is also not clear how robust it is within a QFT framework, once interactions and dynamical gravity are turned on. But this dramatic picture certainly provides a sample of how particle kinematics can be affected by cosmological backgrounds, allowing only certain partial gauge theories and a restricted range of \(m^2\) for a given \(\Lambda\).

How does all this “level-splitting” compared to the \(\Lambda = 0\) picture arise? At the action level (rather than in terms of representations of the appropriate (A)dS algebras), it is due to lifting of degeneracy among successive Bianchi identities on higher spin field operators. I can only sketch the simplest example here, spin 2, the lowest nontrivial case. In a dS background (denoted by a bar), the successive divergences of the field equations read

\[ \bar{D}_\mu G^{\mu\nu} = -m^2 \bar{D}_\mu (h^{\mu\nu} - \bar{g}^{\mu\nu}h_\alpha^\alpha) , \]  

(2a)

\[ \bar{D}_\mu \bar{D}_\nu G^{\mu\nu} + \frac{1}{2} m^2 G^\mu_\mu = 3/2m^2 (m^2 - 2/3 \Lambda)h_\alpha^\alpha . \]  

(2b)

where \(G^{\mu\nu}\) is the linearized Einstein tensor of the massive field \(h_{\mu\nu}\) (it is unique if we require \(\bar{D}^\mu G_{\mu\nu} \equiv 0\)). The first divergence is as in flat space and eliminates helicity \(\pm 1\) if \(m^2 = 0\). However, the second divergence shows that while helicity 0 is eliminated as well at \(m^2 = 0\) (the usual massless spin 2 gauge invariances that leave only helicity \(\pm 2\)), it is also possible to tune \(m^2\) to \(2/3 \Lambda\), eliminating helicity 0 but not helicity \(\pm 1\), and leaving only the 4 excitations \((\pm 2, \pm 1)\), due to a novel (scalar) gauge invariance at this point, namely under

\[ \delta h_{\mu\nu} = (\bar{D}_\mu \bar{D}_\nu + \bar{D}_\nu \bar{D}_\mu + 2\Lambda/3\bar{g}_{\mu\nu})\alpha . \]  

(3)

That the region lying between the \(m^2 = 0\) and \(m^2 = 2/3\Lambda\) line is non-unitary was discovered in \([\text{3}]\) by considering equal time commutators among field components and their behavior as a function of \((m^2, \Lambda)\). It can be understood in a different way by tracing the behavior of the helicity 0 mode in the \((m^2, \Lambda)\) phase of Figure 1. Clearly, the theory has five normal flat space helicity excitations (for \(m^2 \neq 0\)) at \(\Lambda = 0\), the horizontal line. As one comes up in dS space, the helicity 0 mode first vanishes on \(m^2 = 2/3 \Lambda\) and then reappears on the other side, but with ghost behavior (see below). This loss of unitarity affects the whole region until we reach the \(m^2 = 0\) line, where the helicity 0 mode again vanishes (along with helicity \(\pm 1\)); this limit is just linearized dS gravity, which was analyzed long ago \([\text{4}]\).

Let me sketch finally a simple but I trust instructive Hamiltonian analysis \([\text{3}]\) of how this occurs: The free theory’s action is (as usual) the sum of those of the separate helicities. Because helicity \((\pm 2, \pm 1)\) modes are entirely independent of the linear, scalar, Hamiltonian
constraint, only the helicity zero part need be considered. After some field redefinitions (canonical transformations) the helicity zero Lagrangian reduces to the form

\[ L_{h=0}(p, q) \equiv p \dot{q} - \frac{1}{2} [\nu^2 p^2 + \nu^{-2} q(-\nabla^2 + \mu^2)q] \] (4)

where \( \nu^2 \equiv m^2 - \frac{2}{3} \Lambda, \mu^2 \equiv m^2 - \frac{3}{4} \Lambda \) and \( \nabla^2 \) contains a time-dependent factor \( f^{-2}(t) \) in the synchronous gauge we are using here,

\[ ds^2 = -dt^2 + f^2(t)dx_i^2, \quad \ln f = \sqrt{\frac{\Lambda}{3t}}. \] (5)

The shift in mass value from \( m^2 \) to \( \mu^2 \) is essential both to establish (non-trivially) positive energy and null propagation [4] at both lines, but in the present context, \( \nu^2 \) is the operative factor. So long as \( \nu^2 > 0 \), we may trivially remove it by an obvious rescaling of \( (p, q) \) and obtain a perfectly unitary model. But when \( \nu^2 \) becomes negative (the “bad” region) then we can no longer rescale \( (p, q) \) by \( \nu \) without either introducing imaginary fields or rescaling by \( |\nu| \) and accepting a negative helicity zero Hamiltonian. The quantum version of this non-unitarity also emerges, as mentioned, from a study of the equal time commutators [6], that displays a concomitant ghost for \( \nu^2 < 0 \). Precisely at \( \nu^2 = 0 \), the whole excitation is easily shown to vanish even before the desired form (4) is even reached, just as helicity 0 quickly drops out of vector actions precisely at \( m^2 = 0 \) in flat space. Entirely similar results hold for higher integer spins where more indices allow for more sets of helicity deletions (starting with the lowest), as well as (in AdS) for spinors with \( s > 3/2 \), as symbolized in our Figure.

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