Superstring Theory
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In accordance with what is by now a hallowed tradition at the Symposium, I shall not follow the instructions of our leader. Instead of giving a true conference summary, I shall make some general remarks about where we stand in superstring theory.

I am not very well qualified to speak here, since I have not been active in research on superstrings. My connection with the subject has taken these forms:

1) I helped to create at Caltech, during the lean years of superstrings, what Sergio Fubini called a nature reserve for an endangered species — the superstring theorist. We used U.S. government money in part, but in part we benefited from my begging a gift from the Fleischmann Foundation (based on a yeast and whiskey fortune), which awarded the money just as it was expiring. Thus, as long as we satisfied the accountants that the money was spent on research rather than riotous living, there was no control over what kind of research we did — it could be as crazy as we liked. And, with John Schwarz resident and such visitors as the late and much regretted Joël Scherk, as well as Michael Green, Lars Brink, and many others, to say nothing of the presence of the inimitable Pierre Ramond, the ideas were as crazy as anyone would want. And it is with those ideas that we are concerned today.

2) About twenty years ago I made two oral suggestions. One was about the bootstrap program, which I liked in general, but found too restricted. I said repeatedly that Chew and Mandelstam and their associates were doing a good job, but should stop beating to death one intermediate state, the $\rho$ meson, and one channel ($\pi + \pi$), but instead look simultaneously at all intermediate, incoming and outgoing hadron states. In fact, I asked for an infinite number of narrow resonances as an approximation in all incoming and outgoing legs and all intermediate channels. The same list would be required to turn up on all legs and in all channels. Our postdocs Dolen, Hora, and Schmid helped to make such a program possible, with their influential “duality” paper. Veneziano’s ingenious scheme followed, and then the super-work of Ramond and of Neveu and Schwarz, and here we are.

3) Around the same time, 1966, after working for five years or so on current algebra, I suggested repeatedly that it would be wonderful if we could express the whole dynamics by means of current algebra, adjoining the energy density to the algebra of the internal charge and current densities, with the energy density expressed in terms of the charge and current densities, particularly in a light-cone frame.
In 1968, in a bar in Ankara, I met Sugawara, who told me he had created such a model. I was delighted, but over time all of us became discouraged when we learned that it had really nice properties only in two dimensions. Today, as David Olive described so eloquently, such two-dimensional systems have turned out to be fundamental. And, the expression "two-dimensional" now rouses great hope where once it led to disappointment.

Enough of the pre-history of string theory. The history was described with remarkable thoroughness by Mike Green. And it was shown by Paolo di Vecchia how remarkably relevant much of the past work is today, such as his research with del Giudice and Fubini and the vertex research of Sciuto.

Nowadays, we have a set of consistent superstring theories, with one in particular looking very promising as a candidate for a unified theory of all the particles and interactions. It has these encouraging properties:

1) It is a delicately balanced self-consistent theory of the dual or bootstrap type, without obvious tachyons,

2) apparently free from anomalies and finite in perturbation expansion, so leading neither to infinities nor to uncalculable renormalized quantities resulting from sweeping infinities under the rug,

3) incorporating the graviton in a quantum version of Einstein's theory of gravitation, as well as infinitely many other fields of various spins, including the kinds of fields we would need for quarks and leptons and vector bosons (the only known way to make quantum gravitation finite),

4) with a highly restricted set of possible "internal" symmetries, at least one of which, $E_8 \times E_8$, seems promising for agreement with the real world, especially the chiral structure of the standard model,

5) with the number of space-time dimensions determined, yielding a set of six extra dimensions, which, if they behave themselves and curl up into a tiny structure comparable in size with the Planck length, can explain an important part of the reduction of symmetry and can lead to initially massless modes that we can compare with the low-lying states observed experimentally and those we may observe at higher experimental energies. (We see, roughly speaking, how these low
energy modes out of an infinite sequence can, under the right conditions, come out to number around a hundred or several hundred, the first time that we have ever had such an understanding of why there are so many elementary particles in our experience.)

Anyway, how can a set of theories based on the number 496 possibly fail to contain the right one?

How will we know if we have the right theory? I have had an answer to this for a long time, and that answer is unchanged by the fact that there is now a good candidate. We will improve our understanding of the theory, satisfy ourselves as to its elegant and fragile self-consistency, and cultivate the ability to make approximate calculations. At the same time, we will continue to do experiments on old phenomena and on the search for new phenomena. Now suppose that the known phenomena, such as those in the standard model apart from Higgs bosons, are predicted correctly, with previously undetermined parameters calculable in many cases, that new phenomena come out as in the theory, to the accuracy of the experiments and calculations, and that no new phenomena are reliably found that contradict the theory. Then, even if there is no absolutely critical separate test of the theory as distinct from pieces of the theory, but because of the consistency and higher predictive power of the theory compared to the pieces, as calculations and experiments become more extensive and more accurate, and both become more expensive, with no disagreement developing, ultimately the human race will declare the theory to be the right one.

You saw me frantically taking notes and vainly imploring the speakers to leave their transparencies on the projector long enough to copy. I want to deny right now the false rumor that David Gross asked me to try to slow down the presentations because he couldn't follow them at that speed. I never had any real hope of summarizing the talks, and this experience convinced me I could not do it, partly on account of my profound ignorance, but also because the vertical projector was being used as a tachistoscope — from the Greek for shortest and seeing. Psychologists use the tachistoscope to shorten the interval of vision until the material is taken in only subliminally, below the time threshold for conscious knowledge of the contents. Only very indirect tests, such as word association, can then show that the material was taken in at all.

Advertisers have tried, despite the unproved character of the claims of the psychologists who advised them, to use this subliminal effect in movies and television to sell products. Such subliminal
advertising has been outlawed in many places, even without any proof that it works.

I think Luis Alvarez-Gaumé gets the subliminal award for this meeting. Of his brilliant talk on anomalies, virtually everything was taken in this way and carried all the more conviction, perhaps. What I was able to catch looks something like Fig. 1 – a typical page of my notes.

The rest got through only subliminally. If those advertising schemes really work, then when we get home we may find messages reverberating subconsciously in our heads: from van Nieuwenhuizen and de Wit ... "Supergravity will rise again ...", from Friedman ... "We do live on vector bundles in moduli space ...", and so forth.

Even if we do have the exact fundamental theory, then there is still quite a bit left to do in basic theoretical science. Our kind Swedish hosts need not close down their prize factory.

Let us adopt the many-worlds picture of quantum mechanics, with a quantum mechanical description of the whole universe including our information gathering system (ourselves, other living things, apparatus, objects we pick up, etc.) [I became a devotee of this approach around 1963 (after re-discovering it independently of Everett). Recently Stephen Hawking has done some very nice work using it, some of which he described to us here.] Say for convenience that there is a true Hamiltonian $H$ for string theory and that it is exact. Science depends also on two other kinds of information. We figure out by induction, as Hawking is trying to do, the density matrix $\rho(t_0)$ of the universe at a very early moment $t_0$ in its expansion. We would then have:

$$e^{-iH(t-t_0)}\rho(t_0)e^{iH(t-t_0)} = \rho(t)$$

if we ignored the existence of any information-gathering system or any necessary change in available information. Crudely speaking, assigning a specific time to each piece of information acquired about us, or by us, or for us, we apply a projection operator $P$ (Hawking used $\Pi$) which says that certain variables have certain values or ranges of values and not others. Then the a priori probability of a sequence of such $P$'s (given the condition

$$\sum_{\alpha_i} P_{\alpha_i} = 1$$
Fig. 1

φ = \frac{1}{2}A + \text{not high-valued}

3 3 3

IMPORTANT

1) φ → φ + \pi t.
2) ...

[Two more transparencies]

3 2

Nurture winding of solitons around a-cycles and b-cycles

\Rightarrow 3^{rd} term

2^{nd} term confronted by dynamics
for the different alternative projections \( \alpha_i \) is

\[
P(\alpha_1, \alpha_2, \alpha_3, \alpha_4, \ldots) \equiv \text{Tr}(\cdots P_2^{\alpha_2} e^{-iH(t_2-t_1)} P_1^{\alpha_1} e^{-iH(t_1-t_0)} \rho(t_0) \times e^{iH(t_1-t_0)} P_1^{\alpha_1} e^{iH(t_2-t_1)} P_2^{\alpha_2} \cdots),
\]

where we have assigned a time \( t_i \) to each measurement or observation \( P_i \). Using the definition

\[
0 P_n^{\alpha_n} \equiv e^{iH(t_n-t_0)} P_n^{\alpha_n} e^{-iH(t_n-t_0)},
\]

which refers all projection operators formally back to time \( t_0 \), we have

\[
P(\alpha_1, \alpha_2, \alpha_3, \ldots) \equiv \text{Tr}(\cdots P_3^{\alpha_3} P_2^{\alpha_2} P_1^{\alpha_1} \rho(t_0) P_1^{\alpha_1} P_2^{\alpha_2} P_3^{\alpha_3} \cdots)
\]

If we now, with our information-gathering system, lop off some branches by saying that the particular alternatives \( \alpha_1, \alpha_2, \alpha_3 \) have actually already occurred, then we use

\[
\frac{P(\alpha_1, \alpha_2, \alpha_3, \alpha_4, \alpha_5 \ldots)}{P(\alpha_1, \alpha_2, \alpha_3)}
\]

to predict the joint probability of \((\alpha_4, \alpha_5, \ldots)\) given that \( \alpha_1, \alpha_2, \alpha_3 \) have happened. The choices \( \alpha_1, \alpha_2, \alpha_3 \) constitute our information about this specific universe.

We see that the effective density matrix is not just \( \rho(t) \) as obtained above from \( \rho(t_0) \) and the Schrödinger equation. Whatever information has been gathered is used to project it, and after \( \alpha_1, \alpha_2, \) and \( \alpha_3 \) are known to have been selected in our universe, the effective density matrix is

\[
\frac{e^{-iH(t-t_0)} P_3^{\alpha_3} P_2^{\alpha_2} P_1^{\alpha_1} \rho(t_0) P_1^{\alpha_1} P_2^{\alpha_2} P_3^{\alpha_3} e^{iH(t-t_0)}}{\text{Tr}(P_3^{\alpha_3} P_2^{\alpha_2} P_1^{\alpha_1} \rho(t_0) P_1^{\alpha_1} P_2^{\alpha_2} P_3^{\alpha_3})}.
\]

The other process that alters the effective density matrix is coarse-graining, also described by Stephen. Certain variables are unknowable for fundamental or for practical reasons and they do not occur in the projection operators. In the probability formula, we factor the trace operation "Tr" into "Sp" over those unknowable variables and "tr" over all other variables,

\[
\text{Tr} \equiv \text{tr} \ Sp,
\]

and we are dealing effectively not with \( \rho \) but with \( Sp \rho \) of reduced dimensionality.
Thus, whereas mathematical entropy

\[ S_{\text{math}} \equiv -\text{Tr}\rho \ln \rho = -\text{tr}(\rho \ln \rho) \]

is conserved by the Schrödinger equation, coarse-grained entropy

\[ S_{c-g} \equiv -\text{tr}[(\rho \rho \ln \rho)] \]

is not and under suitable conditions it increases.

Now the environmental sciences depend not only on the fundamental elementary particle theory described by \( H \) (presumably superstrings!) and on the fundamental cosmological principle described by \( \rho(t_0) \), which together with \( H \) explains the regularity of the environmental sciences, but also on the results \( \alpha_1, \alpha_2, \alpha_3, \) etc., which include the detailed environmental information about individual galaxies, individual stars, individual planets, such as this one, life on this planet, human beings, their thought processes, cultural traditions, and history, and so forth.

Now it is conceivable that although our theory is parameter-free, some quantities in it might nevertheless be unspecified in the solution and come out to have quantum-mechanically probabilistic values. Hawking thinks the cosmological constant is of this kind. Some have conjectured that even the character of the tiny six-dimensional space, determining the symmetry-breaking pattern, might come out probabilistic. To the extent that any of this is so, elementary particle physics joins the ranks of the environmental sciences. Moreover, not only might some of the quantities be quantum-mechanically probabilistic, but some might, in the new inflation theory, even vary from one bubble to another, so that some day, as our horizon expands, we might be able to receive signals from another bubble that would tell of different values of those quantities there.

Some of us may hope that no quantity in our field has this arbitrary character, that elementary particle physics is really parameter-free, but we do not know whether or not that is so, even assuming that an apparently parameter-free superstring theory is correct.

Now, although, in using induction to find \( H \), we make use of what we know about the world, including our own existence, I hope that we do not use what some might call the strong anthropic
principle to help select H by the requirement that it allows us to be here. Some better criterion, involving elegance and simplicity, is to be hoped for, or we are abdicating our responsibilities (and maybe so is Nature!).

In the case of using induction to learn about ρ₀, the case for arguing from our own existence as well as from other information is a little stronger, although here too we might hope that something intrinsically simple might result, like Hartle's and Hawking's guess of a couple of years ago of what is essentially an initial ground state in the very early universe.

When we get to quantum-mechanically probabilistic or environmental information, which is the ensemble of what we know about this specific universe, anything goes and a "weak" anthropic principle seems quite reasonable. Even here, though, as Stephen said, it would nice although not necessary to find that what we see is highly probable.

Now let me give my impressions of the status of a number of questions about superstrings as a result of listening to the talks at this meeting.

What is the fundamental underlying principle or structure? Einstein proceeded, in formulating his theory of gravitation, from the equivalence principle to the fundamental idea of general relativity. Then he phoned up Marcel Grossmann (so to speak) and learned about Riemannian geometry, finally obtaining R_{μν} = 0, and so forth. Not only did he start with a principle, but when he got his equation it involved a quantity that went to the heart of the subject covered by the principle, the curvature tensor, or, in the action, the curvature scalar.

Now Einstein began with his elevator, and we begin with our equations. We are starting from them (or at least the preliminary formulation that is now available) and we are trying to find the elevator. We are seeking:

1) a fundamental principle
2) a mathematical formulation that involves quantities close to the fundamental principle, and
3) a closed form for the theory that also exhibits, presumably with the aid of a huge apparatus of auxiliary quantities, the symmetries of the system.

The fundamental principle, which greatly generalizes general relativity, I call field marshal relativity! To the extent that it fundamentally concerns topology, as it probably will, we may think
of field marshal topology, too! Ed Witten explained this morning how expanding general to field
marshal relativity will lead us to generalize the notion of topology.

The light-cone string field formulation is presumably a closed form of the theory, in that sense
the most satisfactory available with on-shell and off-shell parts, but it exhibits few symmetries and
does not exhibit in any simple way the underlying principles, assuming they exist.

The Polyakov formulation, which uses the action principle of Brink, di Vecchia, and Howe,
involves an explicit sum over topologies, gives only perturbation theory at the moment, and seems
to require an arbitrary set of background fields around which to expand, but it does have an enormous
appeal, following on all the two-dimensional work starting with Nambu and Goto, then Goddard,
Goldstone, Rebbi, Thorn, etc. In the Polyakov formulation, the contribution from any genus of
2-dimensional Riemann surface in the D-dimensional space is always given by the same formula
(generalizing the area), with interactions coming simply from the $g$ handles or loops.

The two-dimensional surface has an unbelievable amount of knowledge of what happens in D-
dimensional space, as we heard over and over in the talks of Alvarez-Gaumé, Callan, and many
others. For instance, information about the diffeomorphism group in ten dimensions (which has,
mysteriously, $992 = 2 \times 496$ disconnected components) is somehow available. Full conformal in-
variance in two dimensions, including modular invariance, seems to be the principal source of this
uncanny awareness. Are there other secret channels of information? How does the two-dimensional
surface, even with modular invariance, get to be so smart? Can full conformal invariance, including
modular invariance, in two dimensions account for a large chunk of field marshal relativity?

The duality property that characterizes string theories and led to their discovery is known,
partly from perturbation theory results, to be closely connected to the full conformal invariance
of the two-dimensional theory. Thus there is a hint that the search for the principle underlying
superstring theory may bring us back to the vicinity of where we started, the duality version of the
bootstrap.

It is a striking feature of superstring theory that the complete nonlinear gauge-invariant sub-
theories of gravitational and Yang-Mills fields arise through the conspiracy of contributions from
initially massless fields and from others that have initial masses ranging upward from the Planck
mass. Here, too, the duality property is in a sense what brings order to the results.
Although our theories seem to involve a space of ten dimensions of which six must curl up to form a tiny structure, the actual formulation is sometimes with 26 dimensions (for example, the original formulation of the heterotic string, to say nothing of the tachyon-containing Veneziano model), $10 + 32$ (fermionic) dimensions, ten dimensions plus auxiliary coordinates, $10 + 496$ dimensions, or $10 + 496 + 16$ (fermionic) dimensions (as for Kallosh in her elegant formulation of superspace), etc. And, of course, ultimately we get four dimensions. Maybe, with dimension D so protean, we should really regard the all-wise two-dimensions as fundamental.

If so, then what we arrive at next is the space of equivalence-classes of metrics on the 2-dimensional Riemann surface, Teichmüller or Schottky space or moduli space, depending on what we do with a certain discrete group. Teichmüller ignores it, moduli divide by it, Schottky divides by part of it. Staley Mandelstam's tour de force of calculation was carried out there, and other speakers referred to it. Probably moduli space is the most useful version.

The most ambitious attempt to shift the action away from D-dimensional space to this moduli space is found in the provocative work of Friedan and Shenker. Here the D-dimensional space fades from view, and by making it abstract one hopes to let it organize itself as it wants, perhaps even including dimensional reduction (or "compactification") to four dimensions. Instead, one constructs, in a generalized moduli space for a surface with any number of handles, a vector bundle $W$, labeled by highest weights $h^a$ of Virasoro algebra representations, and one uses a locally flat metric $h_{ab}$ on such a bundle. The quantity $Z$, which, in the case of a Minkowski metric for the D-dimensional space, is the vacuum-to-vacuum amplitude and which becomes the partition function for a Euclidean metric, is given by a formula

$$Z = \int dm \, d\bar{m} \psi^\dagger(\bar{m}) h_{ab} \psi_a(m),$$

where $m$ and $\bar{m}$ are moduli; $\psi_a(m)$ and $\psi^\dagger(\bar{m})$ describe the kinematics in terms of left-moving and right-moving quantum numbers respectively; and $h_{ab}$ describes how these left-moving and right-moving propagations join at the handles on the generalized moduli space. In the case of D-dimensional Minkowski space, one can presumably vary the $\psi$'s and the $h$ in ways that preserve the covariant constant character of $h$. Such variations, which would correspond in the D-dimensional space to various string backgrounds, can encode state information that most of us would encode in more conventional ways, and may produce the S-matrix out of the vacuum-to-vacuum amplitude $Z$. 
(In the case of a Euclidean metric for D-dimensional space, it may not be possible to vary $h$ and the $\psi$'s in this way, and in order to get the S-matrix one might have to pick apart the sum and integral in the formula for $Z$ and look for particle singularities, a much clumsier procedure.)

The work of Friedan and Shenker can be thought of as carrying out the "conformal bootstrap" program of Polyakov et al. using full conformal invariance in two dimensions, including modular invariance. Whether the degree of abstraction employed by Friedan and Shenker is needed or not, the pursuit of the underlying principle and the elegant formulation in moduli space, with de-emphasis of the D-dimensional space, seems like one very promising path to follow.

We heard Ed Witten describe this morning how moduli space is fantastically economical compared to the usual space of field theory and we heard him ask, like Friedan, for a situation in which space-time gets to determine its own background and its own topology.

Another interesting path, also discussed this morning by Ed Witten, is that of string field theory, as illustrated by the work of HIKKO (as described by Kikkawa), Ramond, Banks and Peskin, Siegel and Zwiebach, Neveu and West, etc., as well as the rather different work of Witten himself (which, so far, is restricted to open strings). In all of these approaches, the idea is to use auxiliary variables to allow the manifest expression of gauge-invariance and relativistic covariance and presumably supersymmetry as well, thus improving on the light-cone field theory formalism and perhaps laying bare some of the underlying principle. The HIKKO group seems to have accomplished its purpose even with closed strings, but other groups seem still to have some difficulty understanding their achievement.

Witten, working with the non-commutative geometry of Alain Connes, has an elegant formulation for open strings, in which, by special tricks, involving the $\sigma$-midpoint of each line in the diagram

![Diagram]

he has made his algebra associative. That associativity is likely to be impossible for the closed case,
and perhaps the special tricks need to be made less special and the resulting nonassociativity dealt with (rendering it harmless) so as to prepare for the closed string case.

It appears that whereas the light-cone people have strings overlapping at a point (especially clear for closed strings, where one string out of the three at the vertex has to be wasp-waisted like $8$ or $\infty$) and a $\phi^4$ term for open strings, Witten has no $\phi^4$ term for open strings and deals with overlap on a line, which for closed strings presumably will look like

\[
\begin{array}{c}
\includegraphics[width=0.2\textwidth]{diagram1.png}
\end{array}
\]

or

\[
\begin{array}{c}
\includegraphics[width=0.2\textwidth]{diagram2.png}
\end{array}
\]

even if only one point is "fixed." HIKKO's results seem to be like those in the light cone case. About the results of Ramond and the others, I am not clear. The wasp-waisted restriction seems very special for a formalism that is supposed to show the full symmetry and ultimately lead us to understand field marshal relativity.

Another fascinating branch of research involves the effective classical field theory for the initially massless fields, at $\tilde{\alpha} = 0$, but to all orders in $\alpha'$ if we are strong enough. This effective field theory appeared over and over, in the talks by Callan, Deser, Kallosh, Ferrara, and many others. It can be obtained, as Callan showed us, as a fixed point of two-dimensional theories with the massless fields as parametric backgrounds.

This approach, complicated as the result must be, and difficult as summing the series in $\alpha'$ must be, may throw light on many relevant theoretical questions. John Schwarz said as much this morning. It may even, through its renormalization group behavior, throw light on the mysterious
small quantities that ought to appear in the theory if it is right. In any case, the classical method helps us to understand the relation to earlier theories of gravitation.

We must now discuss the fact that there are several different superstring theories: Type I, with open and closed strings, using the group \( SO(32) \); Types IIA and IIB, with closed strings only; heterotic superstring theory (type III/II), using either \( SO(32) \) or \( E_8 \times E_8 \); and finally, the honorary superstring theory based on \( SO(16) \times SO(16) \), with world-sheet supersymmetry but no explicit space-time supersymmetry.

At various times, a radical idea has surfaced and then dropped out of sight again that all the superstring theories might arise in a physical way from the Veneziano model in 26 dimensions, with fermions as dynamical solitons, supersymmetry as a dynamical symmetry, ten dimensions coming from dimensional reduction, and so forth. More convincing at the moment are the mathematical relationships that are emerging among the different theories and between them and the Veneziano model. The simplest example is the construction of the \( SO(16) \times SO(16) \) theory by the orbifold method from the heterotic superstring theory.

Some forms of physical relationship might still be demonstrated. There is even the half-joking suggestion of Ed Witten that the two \( SO(32) \) theories might be physically equivalent, in such a way that the dilaton field \( \phi \) in one formulation is related to \( \phi^{-1} \) in the other.

In any case, we are faced with the question of how Nature chooses among the physically inequivalent superstring theories, if we assume that one of them is right. Among the possibilities are the following:

1) Nature has arbitrarily chosen the one that agrees with our observations. That seems unpleasantly close to the strong anthropic principle. Of course, of the known superstring theories, the one that seems most likely to describe the world we know is the \( E_8 \times E_8 \) heterotic theory, but it would be satisfying to have some principle underlying the choice.

2) We might consider a generalization of what we heard from Holger Beck Nielsen; namely, that at energies much higher than \( 10^{20} \) GeV there is a random set of theories, which are attracted below \( 10^{20} \) GeV to the right superstring theory!

3) There is a beautiful principle, even stronger than field marshal relativity, that not only
requires a superstring theory but fixes the choice of the theory as well.

Speaking of Nielsen, he mentioned how if Lorentz invariance is some sort of low energy fluke then he can argue for attraction to $3 + 1$ dimensions.

Now in superstring theory, where we are restricted to ten dimensions, do we really understand why we have $9 + 1$?

Is the choice of $9 + 1$ associated with the the $1 + 1$ character of $2$-space if $2$-space is really fundamental?

Of course, a situation with all space dimensions would be awkward for physicists, except for Steve Hawking, who seems to live in Euclidean space, and only now and then deigns to toy with a Wick-Feynman rotation to get to a different signature in order to make the rest of us feel better.

Two or more time dimensions would allow us to have closed paths in time and really go back and interact with our ancestors. Of course, we could not kill our grandfathers, as Gerard ’t Hooft remarked (in a somewhat different connection) – instead, one would need, as in any scheme that abandons forward causality, a self-consistent loop condition for all events, as in Heinlein’s classic science-fiction story “By His Bootstraps,” where the hero turned out to be his own grandfather.

Actually, we do understand, more or less, why the signature has to be $9 + 1$. First of all, the Virasoro condition is capable of canceling all the ghosts in the spectrum of superstring theory when there is only one time dimension. When the signature is different, that cancellation is not known to work. Of course, one is supposed to be able to trade ghosts off against a lack of causality, and causality loses a good deal of its usual meaning when there are two or more time dimensions, but still the absence of ghosts supplies a plausible argument for the signature.

In addition, we need the sixteen-component Weyl-Majorana spinor in order to make at least most of the superstring theories work, and that spinor requires a signature of $9 + 1, 5 + 5$, or $1 + 9$. It should not be hard to get rid of the two extra cases!

Next, let us turn to the crucial question of why the cosmological constant is so small, not more than $10^{-118}$ in Planck units. If it turns out to be arbitrary and we have to dial it to that value, we will be dealing with the largest fudge factor in the history of science. It would be nicest for it to be zero and for us to find a reason why it has to be zero. In any case, as we consider successive steps in
the spontaneous breaking of symmetry (especially supersymmetry), most of us would like to have $\Lambda$ kept zero by symmetry considerations as long as possible, even though we don't see how to retain the equation $\Lambda = 0$ until the end.

Thus, in the original ten dimensions, theorists are unhappy about the $SO(16) \times SO(16)$ theory because in the absence of supersymmetry there is no argument for the vanishing of $\Lambda$ even before the reduction to four dimensions.

In the supersymmetric theories, of course we have $\Lambda = 0$ to start with. Then, with the curling up of six dimensions, if we have $N = 1$ supersymmetry, involving $SU_3$ holonomy of the spin connection, we still retain $\Lambda = 0$. Finally, with spontaneous breaking of supersymmetry, we lose it in any picture that we know.

Is there, as Ed Witten has speculated, some fundamentally different way of spontaneously breaking supersymmetry?

Of course Hawking, in answer to my question, made it clear that he doesn't expect $\Lambda$ to vanish fundamentally. He has at times advocated letting it go to a large value and then dragging it back to zero through a space-time foam effect. I never understood that very well. Now he mentions merely that it may be quantum-mechanically probabilistic, either with high probability of being zero or else required to be near zero by the weak anthropic principle (acting strongly!), in which case, as in his answer to Ed Witten, we should be able to detect a non-zero value for $\Lambda$, and perhaps have already done so.

Another thing to try, of course, is to let $\Lambda$ vary with time in a cosmological solution. That raises the question, do we necessarily have a Minkowski solution to our equations simply because we seem to be getting closer to one as the universe expands? (After all, we still have a density that is equal to or close to the critical density as was the case in the early moments of the expansion.) Is a realistic cosmological solution perhaps a necessity and not a luxury?

Anyway, if we study a cosmological solution with evolution, there is the possibility of time-variation of $\Lambda$. The expected values of the two dilatons $\phi$ and

\[
\begin{bmatrix}
g_{5,5} & \cdots & g_{5,19} \\
\vdots & \cdots & \vdots \\
g_{10,5} & \cdots & g_{10,10}
\end{bmatrix}
\]
could both vary, say as powers of time, and even make $\Lambda$ vary. If, for example, in the course of "new inflation" of the radius of the universe by a factor of $10^{29}$, $\Lambda$ acquired a factor of $(10^{29})^{-4}$, the situation might be favorable. Actually, no one has ever managed to obtain such a small factor. Moreover, many of the observable coupling constants have a tendency to vary, too, in a way that contradicts history, unless some exceedingly delicate juggling of the two dilatons occurs, which seems unlikely.

We should, I think, pay more attention than we often do to what an evolutionary cosmological theory would look like, but I have no confidence that doing so will solve the puzzle of the cosmological constant.

Of course the mention of the curling up of six dimensions brings us to the question of how six dimensions are selected for such a fate. Can we revive the attractive suggestion that the field strength $H_{\mu\nu\rho}$ of the second-rank antisymmetric tensor potential $B_{\mu\nu}$ has an expected value that is proportional to the fundamental third-rank antisymmetric tensor (analogous to the Kronecker $\epsilon_{ijk}$) in a complex three-dimensional space? Supposedly this idea has been killed by arguments relating to the quantization of the expected value $\langle H \rangle$ in Planck units (by a mechanism similar to that responsible for the quantization of magnetic charge) and the resulting large violation of supersymmetry. If the idea can not be revived, is there another suggestion to replace it?

Given that six dimensions are curled up, what kind of space do they form and why? Much research is carried out on the phenomenological consequences of particular six-dimensional structures that are thought to obey the equations of motion or to do so approximately, but what is the principle that determines the choice? Is there a condition of least quantum-corrected action or least energy or something of that kind that selects a unique structure? Or is there a fundamental degeneracy that makes the choice a probabilistic one and thus "environmental"?

Why not orbifolds? So many discussions of orbifolds have been apologetic, although not Jeff Harvey's at this Symposium. Many theorists think of orbifolds as the "poor man's Calabi-Yau spaces." Why? They are corners of the space of spaces, not generic, like typical Calabi-Yau spaces, but so what? They are solutions of the classical equations, while a Calabi-Yau space has to be equipped with a non-Ricci-flat metric in order to be a solution. For the standard embedding, an orbifold has a neighborhood of such modified Calabi-Yau solutions; but in the case of non-standard
embeddings this is not known to be true and may never be true, because of the problem of world-sheet instantons.

In the end, we need a real understanding of what the field theory instructs us to do about the dimensional reduction, including the answer to the following question. Can we get away with the assumption that in the dimensional reduction the zero modes (initially massless particles most of which acquire mass later) can be associated, as in weak coupling, with field components that have zero mass in the free string theory? In $N = 8$ supergravity, some theorists were playing with mixing in with these zero modes other modes initially at or above the Planck mass in a desperate attempt to get agreement with observation. With the advent of $E_8 \times E_8$ superstring theory, the relief was so great that the mass operators of the initially massless fields could apparently do the job that nearly everyone has relied on them without worrying about the possibility that strong coupling might mix in higher modes in an essential way. Is there a theorem that prevents such a situation from arising?

The importance of advanced mathematics in dealing with all the questions we have discussed is stupefying. Theoretical physics has been reunited with pure mathematics during the past decade in a most dramatic way, after an estrangement of half a century or so; and fundamental theoretical physics has been rejoined with the core of pure mathematics, where geometry, analysis, and algebra (even number theory) come together. David Olive, in his beautiful presentation, mentioned the fact that string theory is bound to reveal new relations between algebra and geometry and may thus affect modern mathematics in important ways.

I hope that the trend in mathematical teaching, writing, and editing will continue to recoil from the extreme of Bourbakisme, so that explanations and non-trivial examples can be presented and physicists (to say nothing of other scientists) can once more have a fighting chance of understanding what mathematicians are up to, as they did early in the twentieth century.

My attitude toward pure mathematics has undergone a great change. I no longer regard it as merely a game with rules made up by the mathematicians and with rewards going to those who make up the rules with the richest apparent consequences. Despite the fact that many mathematicians spurn the connection with Nature (which led me in the past to say that mathematics bore the same sort of relation to science that masturbation does to sex), they are in fact investigating a real science of their own, with an elusive definition, but one that somehow concerns the rules for all possible
systems or structures that Nature might employ. Rich and self-consistent structures are not so easy to come by, and that is why superstring theory, although not discovered by the usual inductive procedure based principally on experimental evidence, may prove to be right anyway.

Reference to experimental evidence brings us finally to the topic of phenomenology.

In a very nice talk, Pierre Fayet reminded us, among other things, of his $R$ symmetry and how a multiplicative version of it should survive the spontaneous breaking of supersymmetry and of additive $R$ symmetry and should give rise to the absolute stability of the lightest bosino (say the photino), which may thus well provide a sizeable chunk of the dark matter in our galactic cluster or in the universe.

Fayet also discussed possible symmetry relations between vector bosons and higgsons in the case of massive supermultiplets. It would be interesting to see if anything of that kind arises from superstring theory, or if there is any observational evidence of such relations.

John Ellis discussed a wide variety of phenomenological issues, including the comparison with observation of various collapsed six-dimensional spaces. Assuming $N = 1$ supersymmetry and $SU_3$ holonomy for both the spin connection and the internal space, he treated two quite different situations:

1) Spin and internal connections identified, standard $(2,2)$ embedding, giving $E_6 \times E_6$ before other symmetry breaking mechanisms are invoked. Here there is a choice of an orbifold solution or a modified Calabi-Yau solution. In either case John is unhappy about the many extra particles required by the representations of the group $E_6$, although he has no strong and direct counter-argument.

2) Spin and internal connections different, non-standard embedding, say $(2,0)$, with various small groups as possibilities. Here only orbifold solutions are known and, as I mentioned earlier, that might be a rule. For these cases John expressed a concern that there be enough symmetry beyond $SU_3 \times SU_2 \times U_1$ (at least another $U_1$) so that the singlet neutral scalar field $N$ couple to something capable of preventing its acquiring a mass comparable with the Planck mass; if $N$ is kept relatively light, then it can play a role in Higgs phenomena as required in his analysis.

John expressed his conviction, encouraging to experimentalists, that the supergap for standard model particles will be small and the superpartners accessible, at least some day, to experimental
discovery. Otherwise, he does not see how the electro-weak parameters can be prevented from acquiring uncontrollably large radiative corrections. This argument clearly needs to be understood at the deepest level possible.

In connection with small parameters, we may well ask how the superstring theory will predict such tiny numbers as \( \frac{m_{\text{int.boson}}}{m_{\text{Planck}}} \approx 10^{-17} \) or \( \frac{m_e}{m_{\text{Planck}}} \approx 10^{-22} \) or possibly even \( \frac{m_{\nu}}{m_{\text{Planck}}} \approx 10^{-28} \). If the theory is really parameter-free (i.e., there are no arbitrary vacuum expected values), then these numbers must ultimately arise from expressions resembling \( \exp[-O(1)/\alpha] \), where \( \alpha \) is a coupling constant. The problem then is to understand the formulae and the fact that the \( \alpha \)'s are around \( 10^{-1} \) or \( 10^{-2} \).

Since we do not at the moment understand how to distinguish such numbers from unity, it is hard to be sure that the standard model supergap really is tiny compared to the Planck mass, as argued by Ellis, although it is certainly plausible. Of course, the gravitino mass provides another kind of supergap, which can perhaps be fairly large even if the other one is small.

Can we connect the small values of \( \alpha \) parameters to the smallness of \( \frac{1}{C_2} \)? John Preskill at Caltech has been toying with the idea that even for groups other than \( SU_N \) there can be an expansion analogous to the one in \( \frac{1}{N} \), in particular an expansion in \( \frac{1}{C_2} \), where \( C_2 \) is the value of the second degree Casimir operator for the adjoint representation. For an exceptional group like \( E_8 \times E_8 \) or \( E_6 \), the quantity \( C_2 \) is not really a variable but it is large, and the "expansion" in its reciprocal does seem to lead to diagram restrictions as for \( SU_N \). Can the resulting restrictions help to understand supersymmetry breaking or other phenomena? Can the smallness of \( \frac{1}{C_2} \) be used elsewhere, for example in a renormalization group study of the nonlinear effective action of the initially massless fields? Can it be related to the smallness of the various \( \alpha \)'s and ultimately to the smallness of the masses of the standard particles? What about the various symmetry breaking mechanisms that enter as we move down from the original internal group to \( SU_3 \times SU_2 \times U_1 \) times various \( U_1 \)'s?

In general, for comparing predictions with observation, there will be high energy experiments to explore thresholds for new particles, such as superpartners of known particles, or extra members of extended families, or symmetry-breaking particles; there will be comparisons with left-over cosmological effects from the early universe; and finally a very few experiments, mostly at low energies, to detect virtual effects of phenomena near the Planck mass (effects like proton decay). All of these will
have to be exploited. Are there any rare processes that we could detect at high energy accelerators that probe indirectly the fundamental scale of the theory? Or are intensities too low for that to make any sense?

It is a splendid privilege to be able to understand, even if sometimes dimly, the remarkable ideas that are being discussed here, and to share the excitement of wondering what will happen to these questions: the ultimate principles, the mystery of why two dimensions are so wise, the proper formulation, the rule for choosing the right theory, the mechanism of curling up six dimensions, the choice of how much is determined and how much, if anything, is probabilistic, the puzzle of the cosmological constant, the origin of the small parameters and the steps in symmetry breaking, the comparison with high energy searches (for instance for superpartners), the comparison with the properties of the cosmos, and the comparison with low energy (or high energy) experiments on rare events that probe the fundamental length.

It is an experience repeated frequently in our lives, but always wonderful, to meet with friends and colleagues from so many parts of the world to think together about the fundamental laws of the universe. Many of our fellow human beings are trapped in parochial ideas and sometimes in national and regional rivalries, and even hatreds, with little appreciation of how cooperation and competition together form a harmonious and adaptive pattern. They share with us the every-day concerns that all human beings have, but without our additional commitment, along with other scientists, scholars, artists, and some others to contribute together to the cultural achievements of the human race. We should do all we can, and more than we actually now do, to educate our fellow citizens about these two special facets of life, each of transcendent importance, that we are privileged to share.

In closing, let me thank the Nobel Foundation, the organizers (headed by Professors Nilsson and Brink), post-docs, students, and secretaries; the musicians and those who put on a splendid banquet for us; and the other remarkably attractive and friendly people that we have met in our hotel and in the shops and restaurants of Marstrand. I think I can speak for all of us in saying that we are delighted with the hospitality we have received.