Physica Scripta. Vol. 24, 890–894, 1981

On the Future of Quantum Field Theory for Particle Physics

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Received November 3, 1980

Abstract

Some possible future developments of quantum field theory in particle physics are presented.

Is it appropriate that this lecture in which I am supposed to discuss Quantum Field Theory's future, is preceded by solemn, melancholous music in this lecture hall? Certainly, a decade ago that future looked brighter than now. Our unabridged enthusiasm for the new gauge theories for the elementary particles was presented to the world with clever salesmanship. Exposés appeared in the popular scientific press that, to some of us, looked like one big commercial (Fig. 1). Like in all commercials, the truth was twisted a little bit. Surely, the new model for the weak and electromagnetic interactions was renormalizable and to that it owes much of its success. The fact that it is anomaly-free is welcome though not an extremely crucial property. But if its advocates called it a unified theory I must object. The $SU(2) \times U(1)$ theory has two commuting gauge groups, both with their own independent coupling constants. The model could equally well apply to the *strong* interactions, describing ρ^+, ρ^0 and ρ^- as gauge particles. The $U(1)$ group is to be identified with electromagnetism and it only comes in the strong interaction theory by the way it is mixed with the strong $SU(2)$ gauge group, the Higgs field having accidently a $U(1)$ charge. That is not unification, and the situation with W^+, Z^0 and W^- is qualitatively the same. The tiny letters in Fig. 1 tell the more careful reader that unnatural cancellations are required to explain the Higgs mass-value.

Although this model is not what I think Einstein was hoping to achieve, the model is beautiful just because it probably contains lots of truth in it. The successful proposal of GIM and the subsequent discovery of charm do show that the theory was powerful and physics was served well by its invention. In my opinion however the best justification was hardly mentioned in the "commercials": the discovery that requirements such as unitarity and finiteness of the number of arbitrary constants of nature necessarily lead to theories of this sort.

It was natural that many investigations were inspired to continue along these lines, and the "probable" results of their future investigation were anticipated. Even more shouting commericals were launched announcing the expected beautiful results (Fig. 2).

We were promised unified theories with not only weak, electromagnetic and strong forces bundled into one unified description, but even gravity was threatened to be tamed. "Supergravity" became the key-word for that and "renormalizability" was promised again. The complexity of these models, purported even to explain the origin of the universe, would even have stunned Einstein in his dreams.

That hopeful enterprise was doomed to failure, and the advertisements were misleading. Renormalizability, even if it would ever be achieved, would not be sufficient to render such models respectable. That is because at energies beyond the Planck length perturbation expansion itself would be too divergent to make sense and predictive power would get lost. And it would never be possible in such models to explain the observed values of the elementary fermion masses. To many of us, therefore, it did not come as a surprise that the tone of the advertisements was gradually changing. See Fig. 3 which is self-explanatory. Some of the "little problems" are actually rather fundamental obstacles.

And so, it becomes more and more evident that the future of particle physics is not just a simple unified theory. Then what are we heading to? Usually, at such occasions, a reviewer of the future lists a number of urgent questions left by the present theories. Experience however tells us that, more often than not, such questions are not answered directly by ensuing developments. If an answer is found, it is by accident or via long detours. Nevertheless, since everybody else would do that in my place, let us give an incomplete list of the most urgent questions present day particle theory is facing.

Problems of purely mathematical nature are limited in particle physics. But some are very important:

(α) *Analytic methods for solving QCD*. Obviously the exact solution for the scattering matrix of, say, pure quantum chromodynamics, would be welcome but it is unlikely that that will be found. Slightly less unlikely is an exact analytic expression for pure chromodynamics of $SU(N)$ in the limit $N \to \infty$. The scattering matrix approaches the identity matrix but the particle spectrum should be computable. More pragmatic is to search for some systematic expansion procedure more useful than the ones proposed so far.

(β) *Numerical methods for solving QCD*. A program based on Monte-Carlo techniques is under way but there are fundamental problems. So far only properties of the vacuum (phase transitions) could be studied and some attempts were made to compute the string constant. It is hard to include fermions. If that can be done, and if with the advent of faster computers statistics will be improved, then this is a promising development.

(γ) Search for *new symmetries in Quantum Field Theory*. It seems as if we have not got all possible symmetries yet. Supersymmetry, for instance, came as a surprise. Are there others? Non-local? Could any of them explain the vanishing cosmological constant?

(δ) *Quantum gravity*. Understand and classify the various peculiar topological features relevant to quantum gravity.

Many urgent questions may be asked to the theoretical physicists.

(a) *Naturality*. Find respectable models for the observed particles that show no unnatural cancellations among unrelated constants of nature at energy scales below a given maximum.

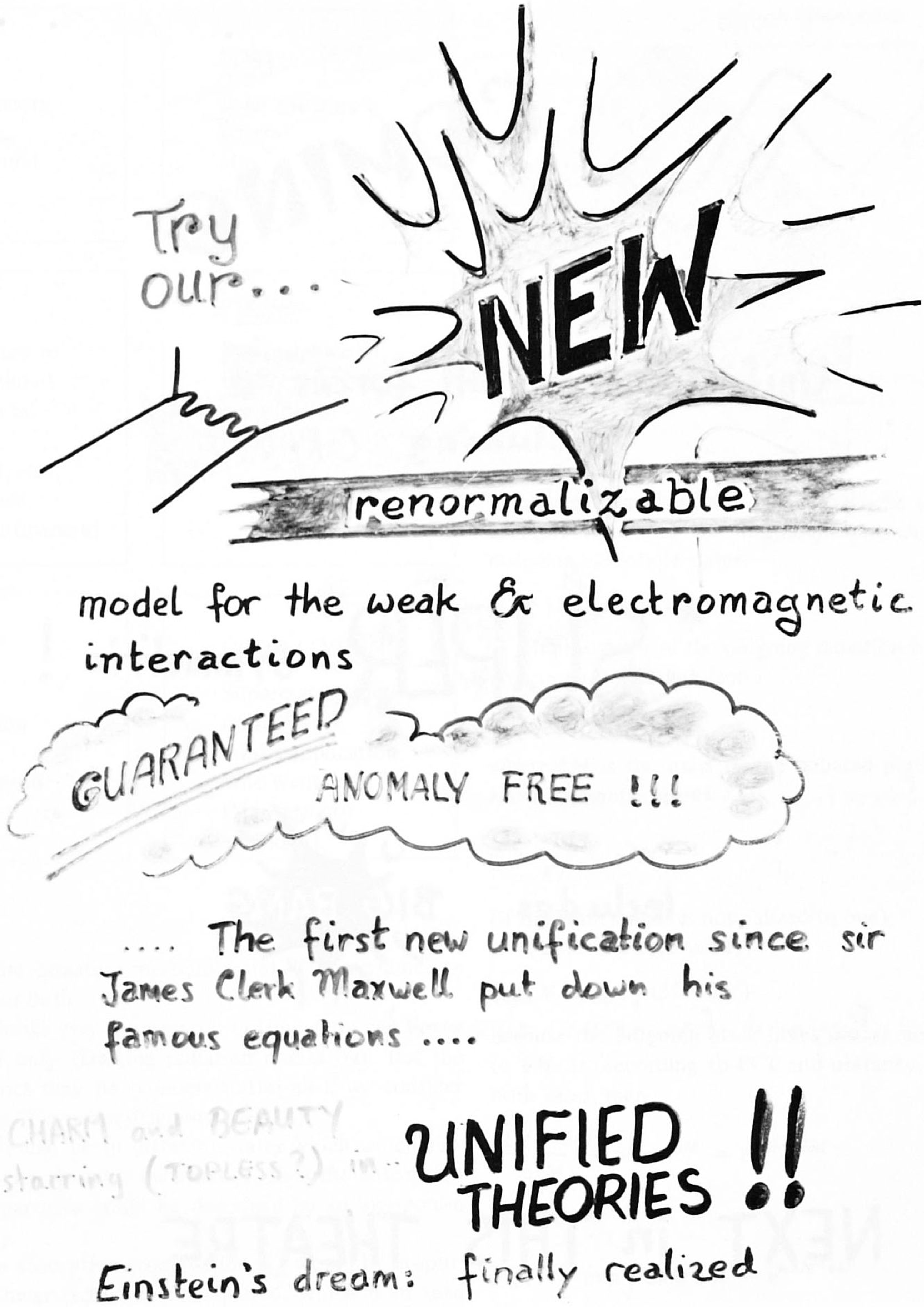

Fig. 1.

Technicolor allows one to go beyond 2000 GeV, but extension of these ideas beyond 50 000 GeV or so (extended technicolor) meets with vast and complex problems.

(b) The question of the *fermion masses*. What determines them? Technicolor? As yet we could only express them in terms of other ununderstood constants of nature.

(c) *PC violation and the θ problem*. Why do we have PC violation and why is the strong instanton angle θ so close to zero?

(d) The origin of the *quark/lepton generations*. Why all these Xerox copies of quark and lepton families? Is the answer again technicolor?

(e) *Supersymmetry and supergravity*. What is their physical interpretation? Can we make realistic supergravity theories? What do they imply?

(f) Again *Quantum gravity*, but now as a physical problem. What new physics can we put in to improve our understanding of this fundamental problem?

(g) *Cosmogony*. Can we understand how the universe started? How did the galaxies appear?

I could go on like this, but instead we can also ask questions to experimental physicists. Questions likely to be answered in the not too distant future are:

(A) *Intermediate vector bosons*. What are the properties of W and Z? What are their widths? Do we get sensible limits on the number of neutrino types?

(B) Does the *proton decay*? Into what? What is the lifetime?

(C) Do the *neutrinos* have *masses*? Do they oscillate? Which transitions occur?

(D) The *Higgs particle*. As yet it eluded direct or indirect

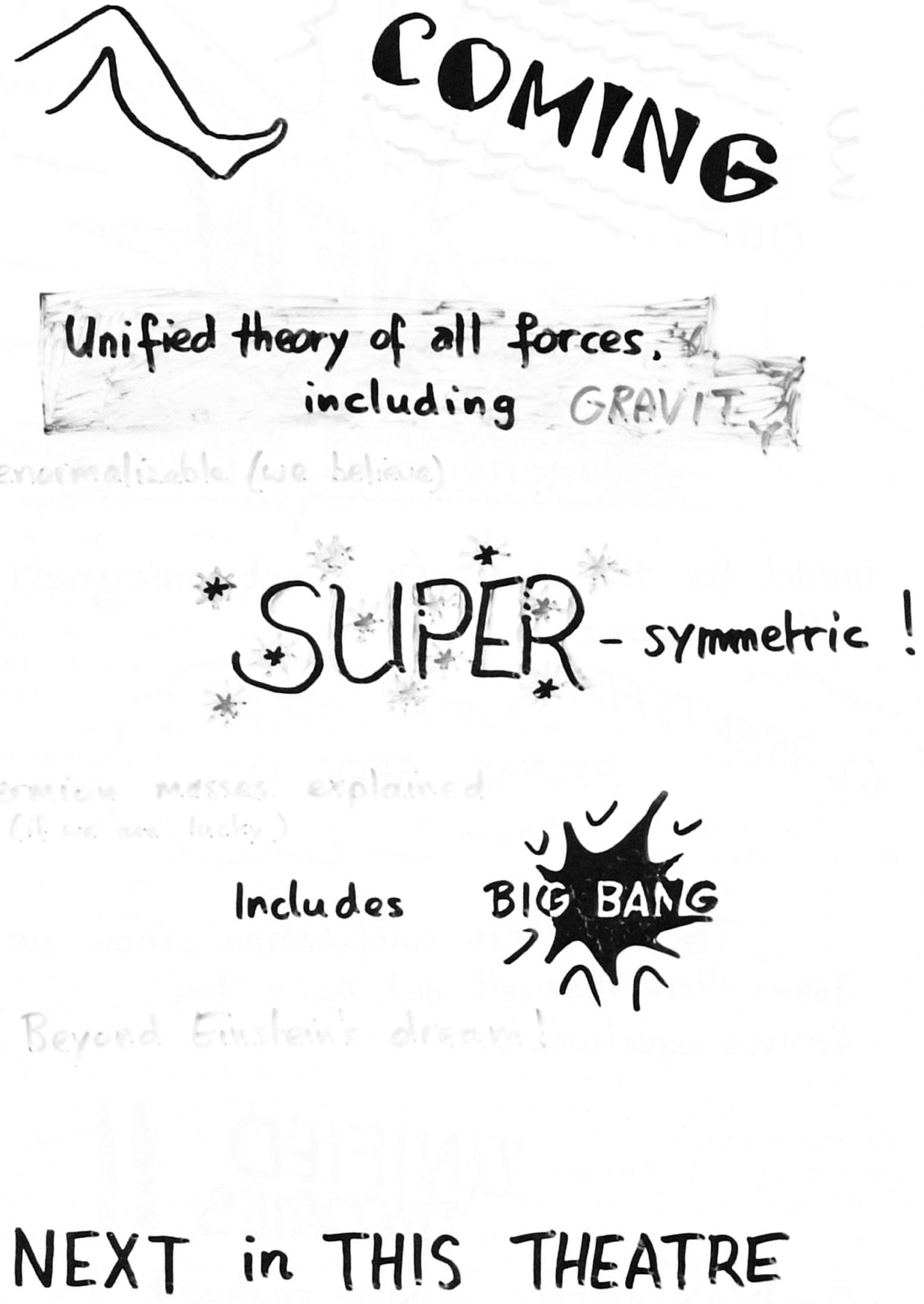

Fig. 2.

observation. Is its mass beyond 1000 GeV as technicolor theories indicate? Do they form jets in the TeV region?

(E) *New particles*. Look for expected and unexpected crazy objects (magnetic monopoles, superheavy stable particles; but also new particles with vacuum quantum numbers such as axions, techni-eta, etc.).

And so on, and so forth. But in particular the mathematical and theoretical questions are not so easy to ask. The most important question is what the right questions are. For instance the questions concerning fermion masses and generations will probably be answered in terms of some new theory that can only be obtained by looking at more carefully phrased questions first [1].

It is better to predict the future by looking at those areas of particle physics that are in rapid motion right now. The Monte Carlo techniques (β) for instance are rapidly improving. Technicolor models have not at all been investigated as exhaustedly as "ordinary" unified theories.

Quantum field theory is suffering from only one really fundamental paradox: how can quantum theory be reconciled with the notions of general relativity, in short, how to formulate a theory of quantum gravity? In spite of rigorous assaults nobody came with a convincing theory. Still, I think not all possibilities have been exploited and it may yet be possible to get there by just thinking very hard, and without the aid of experimental data. Let me sketch what I think the beginning could be of a future "superunicosmified theory". It is called "quantum meladynamics", for the Greek word $\mu\epsilon\lambda\alpha\sigma$ = black. It is the quantization of black holes.

The principle on which it is founded is that there is no fundamental distinction between black holes and elementary particles. They are the same things but with different qunatum numbers.

WANTED:

A natural theory
for particles,
reliable beyond
2000 GeV.

LOST:

Solar neutrino's,
Where
are
you?

WANTED:

Exact solution to
any QCD related
theory such as:
SU(∞),
$Z(2)$ lattice, etc.
By preference
showing confinement

FOUND:

10^{10} galaxies.
Who
made them?
How?

WANTED:

A physical
interpretation
of our
multi-instanton
solution . . .

GOOD-LOOKING:

Supergravity theory
up to $N = 8$
awaits application.
Who wants me?
(Marriage not
excluded).

Fig. 3.

Further we require quantum mechanics and PCT invariance to be exactly valid for both.

A black hole looks very asymmetric in time: you can throw anything in, and only Hawking radiation comes out. But the quantum mechanics may be symmetric after all if we consider the corresponding "Feynman diagrams" (Fig. 4).

If a black hole can be in different states which we characterize, for simplicity, just by the mass M, then the absorption and creation of particles could be described by an interaction Hamiltonian, H_{int}.

Classically, the absorption cross-section σ_{in} would be proportional to the Schwartzschild radius squared, which is in turn proportional to mass squared:

$$\sigma_{\text{in}} \propto R^2 \propto M^2$$

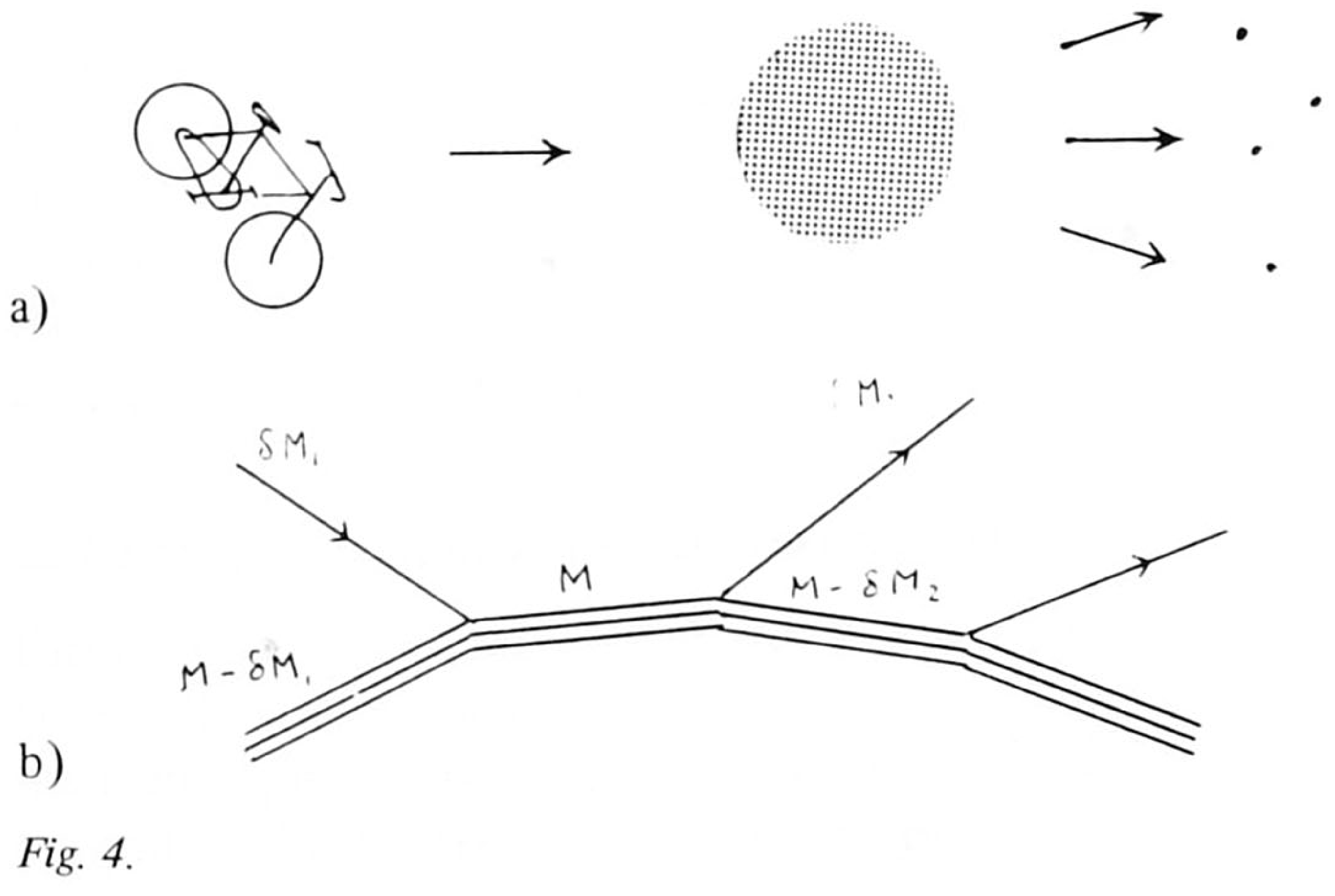

Fig. 4.

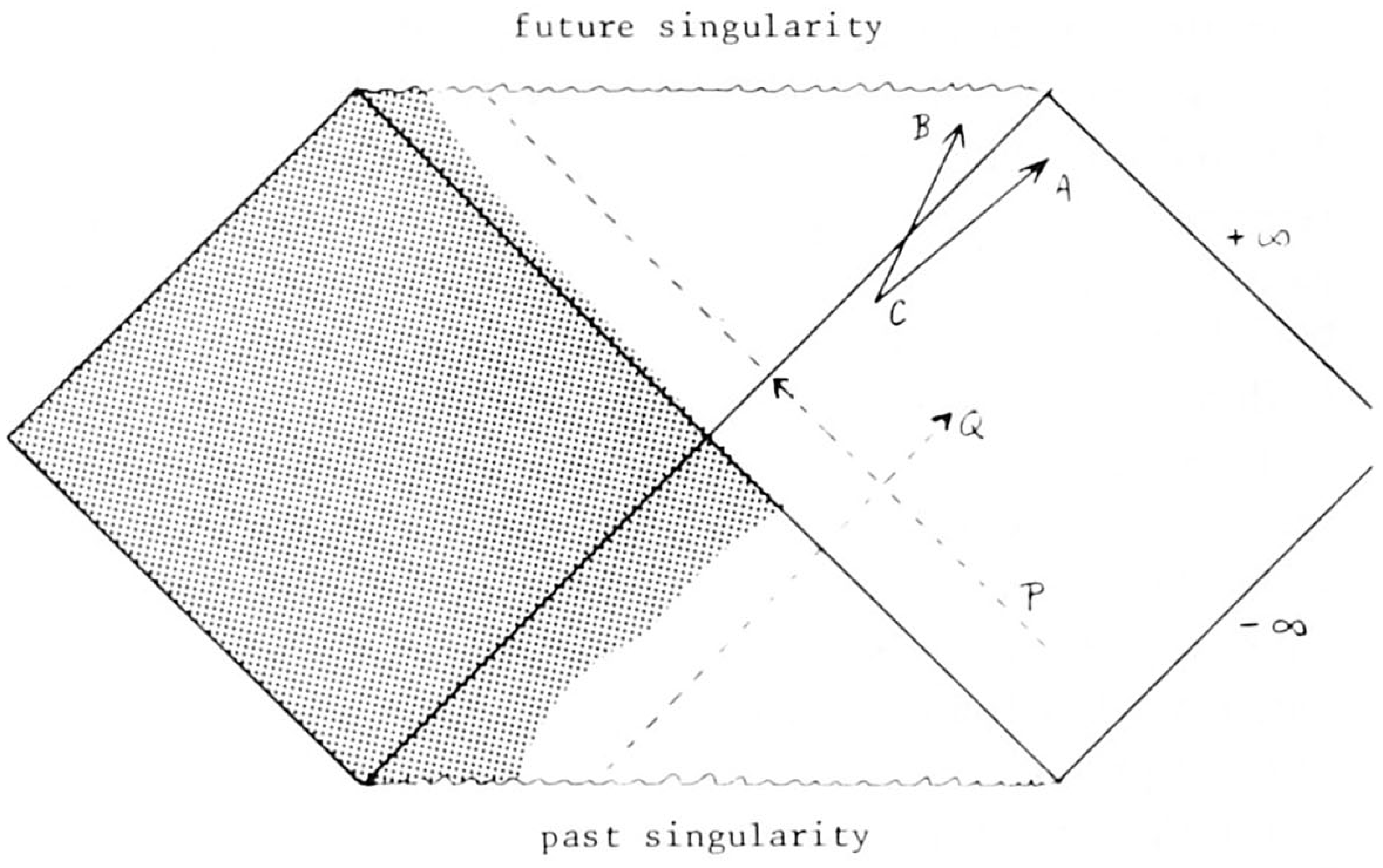

Fig. 5.

Quantum-mechanically, σ_{in} would be proportional to the absolute square of the matrix-element and to the density of outgoing black-hole states:

$$\sigma_{\text{in}} \propto |H_{\text{int}}|^2 \rho(M)$$

The intensity of the outgoing radiation is proportional to the area and a Boltzmann factor:

$$\Gamma_{\text{out}} \propto M^2\, \mathrm{e}^{-\beta\delta M}$$

where δM is the mass of the radiated particle. β is the inverse Hawking temperature:

$$\beta = \frac{1}{kT} = 8\pi M$$

(if the Planck mass is normalized to one).

Quantum-mechanically,

$$\Gamma_{\text{out}} \propto |H_{\text{int}}|^2 \rho(M - \delta M)$$

because the outgoing black holes lost an amount of mass equal to δM. If, according to PCT and unitarity, H_{int} is the same in both cases, then

$$\frac{\rho(M-\delta M)}{\rho(M)} = \mathrm{e}^{-\beta\delta M} = \mathrm{e}^{-8\pi M\delta M}$$

We find ρ:

$$\rho(M) = \exp \int 8\pi M\, \mathrm{d}M = \mathrm{e}^{4\pi M^2 + C}$$

where C is a dimensionless constant.

For low M this formula can only be some approximation, telling us roughly what the spectrum looks like. We see that the black hole spectrum becomes very dense at high M.

Of course what I did here was derive the enthropy of a black hole and that was known for a long time. But the above mentioned principle has more profound consequences.

Figure 5 shows the Penrose diagram enabling Hawking to derive his famous radiation phenomenon. The shaded part is the collapsing star, leaving a black hole. An oscillating field at C propagates partly to A, partly to B. This splitting mixes positive with negative frequencies and hence particles and antiparticles emerge at $+\infty$, as Hawking radiation. Those components that vanish into the future singularity (B) seem to be lost forever. Hawking argues that this loss of information is the reason why the outgoing particles are thermal, i.e., they form a quantum mechanical mixed state, while pure particles went in.

Now this is in contradiction with our equivalence principle. The information should not get lost. How does Nature recover it? The only possible, even likely, answer is that the radiation reappears at the past singularity. A bicycle thrown in along the path P, may come out along the path Q, not necessarily as a bicycle, but as a complicated quantummechanical super position of things, such that the information is retained.

Is causality violated then? Not necessarily. P and Q intersect very close to the horizon, so it is as if some scattering took place at the horizon and it seems that that would not be impossible physically. Clearly though, a thorough investigation of what causality restrictions would imply for such an assumption would be necessary [1].

Another suggestive observation to be made is the following. We are used to thinking that Quantum Gravity at distances much *shorter* than the Planck length is totally unaccessible to us. We even do not know whether space–time is continuous or discrete. However, in terms of Feynman graphs it should be possible to say something: if we consider energy-exchanges much larger than the Planck mass, then these extremely massive gravitons would become black holes themselves. If the exchanged particle is to be identified with a black hole, then arguments such as crossing symmetry could tell us what the matrix-elements are: the sub-Planck structure of space–time may be determined after all.

Quantum Meladynamics is not yet a theory. I was just expressing my personal vision as to how possibly quantum gravity may be attacked. I do not see at present how this could lead to a "superunicosmified" theory. Very probably this idea will follow many others: into the drain. However, we all need our personal philosophies, our beliefs (or superstitions) about how to obtain new interesting insights in our physical world.

We need them, but probably they are irrelevant. I have the impression that whoever of us has been successful in the past in some branch of physics has been lucky for being at the right place at the right moment, rather than for having a superior philosophy. Fortunately, the community of physicists is a very heterogeneous one. We are all working according to different views and philosophies. "God" or whoever invented this universe does not care at all about the morality of playing with dice or being natural. He/she/it is just laughing its head off at us. Every once in a while however somebody will be at the right place at the right moment and make a small or large step disclosing the world of elementary particles. I do not think that that has to go with much glamour. But we will all be there and watch it. Future for particle physics will be fun.

References

1. Author not yet known (to be published).