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Can the superstring theory become physics?

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The bubble of Japan's economical prosperity has bursted, and the bubble of the superstring theory has also bursted. It seems that most of the present day's master-course graduate students no longer turn their face to the superstring.

The superstring had been very fashionable for several years since 1985 and lionized very much as the "theory of everything". It is quite questionable, however, how many researchers really believed so. I wish to take a questionnaire for the people who worked in the superstring in that time. I guess that most of them merely wanted not to miss the bus.

I wrote comments criticizing the superstring in the *Soryūsiron Kenkyū* (Research of Elementary Particles) 1) and in a magazine *Parity* 2) in 1986. Reading them again now, I do not feel any necessity of revising them. It seems that the superstring had been too much based on wishful expectations.

General relativity was a theory having the sturcture quite foreign to the mechanics established before that time. However, Einstein was quite right in believing the correctness of his theory before knowing the observational result of Eddington. The reasons are as follows: First, general relativity was constructed from the clear-cut first principles. Second, it included the already firmly-established Newtonian mechanics as an approximation in a natural way. Third, it explained the precession of perihelion of Mercury, which any other theory had been unable to explain, quantitatively without introducing any new adjustable parameter.

If the superstring is the "theory of everything", it is not inadequate to compare it with general relativity. It is remarkable, however, that the superstring has no ground corresponding to anyone of the above three. First, it was not constructed on the basis of any fundamental principle. Duality is a thing borrowed from hadron physics. The theory which cannot be formulated non-perturbatively is not a respectable theory. I believe that any correct quantum theory should be able to be formulated in the Heisenberg picture. As long as one does not give up the idea of adding the interaction part to the free-field theory, one will never attain the fundamental theory.

Second, it is quite unclear whether or not the superstring really includes the standard theory as an approximation. Certainly, it has a symmetry large enough to include $SU(3) \times SU(2) \times U(1)$. This fact was quite attractive because the $N = 8$ supergravity failed. But, of course, the relation to the standard theory cannot be inferred by this fact only. Although various scenarios aiming at deriving the standard theory from the superstring have been invented in the superstring phenomenology, it must be said that they all were too far from the logical inevitability.

Third, there is no way for verifying the superstring experimentally. This point is what Glashow, a boss of anti-superstring, emphasized loudly. In fact, it is extremely difficult to develop a new physical theory without experimental supports. But if one emphasizes this point too much, that will lead to the assertion of not considering quantum gravity at all. Einstein was very lucky; one cannot always expect such a godsend.

If there are experimental supports, such a very queer theory as Bohr's atomic model can be successful. If not, it is something like walking in a dark night without lamplight. In this case, it would be extremely difficult to attain the destination safely unless one is equipped with highly technological devices. That means that we need to keep the logical consistency based on the fundamental principles and the close and inevitable relationship to the already successful theories. I believe that we should absolutely refrain from modifying the theory by hand merely for the purpose of making both ends meet. Even if the theory yields some physically unreasonable results, we must avoid to make any deception. It is more important to clarify the structure of the theory than to fit them to experimental data. It may happen that the unphysical results obtained disappear by reinterpreting the theory. For example, the Yang-Mills theory failed as a gauge theory of isospin proposed originally, but it has become brilliantly successful as the standard theory.

The two posts with which the popularity of the superstring is supported are the characterization of the theory by the anomaly-free condition and the absence of ultraviolet divergences. But, in my opinion, the problem of ultraviolet divergences may not be taken into account so seriously in constructing a new theory. This is because it is a consequence derived on the basis of such an approximation method as perturbation theory and the finiteness of the perturbative solution does not give not only a sufficient condition for the finiteness of the exact solution but also a necessary condition for it. Originally, the divergence difficulty is of mathematical nature. The reason why the four-fermi interaction breaks down is not its unrenormalizability but the fact that the cross section exceeds its unitarity bound. There are many people who regard quantum gravity as a theory of the same level as the Fermi theory solely because both are unrenormalizable, but evidently they do not understand what is most essential. When one dicusses the naturalness in particle physics, the Planck mass is supposed to be the cutoff scale. Nevertheless, when one discusses quantum gravity itself, it is claimed that quantum Einstein gravity is not physically sensible because there arise infinitely many kinds of divergences if it is expanded in powers of the *inverse* of the Planck mass. It seems to me that the argument is not consistent.

It is worth being praised that the constructive field theory established the existence of nontrivial relativistic quantum field theories mathematically rigorously, albeit only in the lower-dimensional cases. Unfortunately, however, this fact seems to have acquired a curious kind of authority recently: The people have increased who believe that all quantum systems of infinite degrees of freedom must be formulated in the "constructive" way. But I cannot agree to such an opinion. The mathematically rigorous formulation should be done only at the final stage of the theory. When one wishes to construct a new physical theory, it is rather harmful to formulate it mathematically rigorously. To make it rigorous adequately is possible only after its structure has become completely clear. It is not preferable to make the theory under construction rigorous nervously, because the assumption which has been introduced just for the necessity of rigor but without physical ground may often kill the essence of physics. For example, if one assumes that the state-vector space is a Hilbert space of positive norm, one can no longer satisfactorily formulate quantum gauge theories and quantum gravity. Anyway, since it is a prohibitively difficult task to formulate realistic theories mathematically rigorously, it is probably not the right way to adhere to the "constructive" way. I think that the adequate mathematical structure of the theory will be gradually determined in the process of solving it.

The claim that the superstring theory is free of divergences in each order of perturbation theory is probably true. The argument for showing this is based on the locally Euclidean manifolds. It seems to be supposed that there is no problem for assuming so, because the amplitude can be analytically continued to the locally Lorentzian manifolds. Of course, the analytic continuation preserves all equalities such as $S^\dagger S = SS^\dagger = 1$, but does *not* preserve any inequalities such as the positivity of probability. Certain non-polynomial interaction-Lagrangian theories become free of divergences by means of analytic continuation, but such theories violate the positivity of probability owing to the reason stated above. I wonder if the non-occurence of the same trouble is assured in the case of the string theory. It seems to me that the people working in the string theory do not wish to touch on this problem.

Finally, I want to state some comments on the anomaly cancellation, which is a signboard of the superstring. The critical dimensionality $D = 26$ or $D = 10$ is determined by the formula of the vanishihg central charge as the condition for the disappearance of conformal anomaly. I feel, however, that this reasoning is valid only in the case in which the gauge fixing term involves no derivative, as is so in the conformal gauge. The reason for this is as follows.

The free scalar fields coupled with the two-dimensional quantum gravity can be identified with the bosonic string theory without interaction. Recently, Mitsuo Abe and I 3) have succeeded in constructing the exact solution to the two-dimensional quantum gravity in the covariant gauge (de Donder gauge). Anomaly appears also in the covariant gauge, but the way of its appearance is different from that in the conformal-gauge case. Since no constraint is involved in the covariant-gauge case, the anomaly cannot be eliminated by such a condition as the vanishing central charge.

Some years ago, Düsedau 4) and others investigated the covariant-gauge two-dimensional quantum gravity in lowest order of perturbation theory. They showed that, even in the covariant-gauge case, $D = 26$ can be derived from a condition similar to that in the conformal-gauge case. We have reexamined their papers and found that the derivation of their conclusion was not necessarily adequate. 5) The basic quantity in their reasoning is the symmetric energy-momentum tensor including the gravitational ghosts; it is not an observable quantitiy and its definition is *not unique.* Owing to the ambiguity of its definition, the condition for the absence of anomaly can be changed at human will. Thus the anomaly-free condition becomes meaningless in the covariant-gauge case. Such a situation is encountered only when the the gauge-fixing term contains derivatives.

Is it a justifiable proposition or nothing more than a creed that the notion of the critical dimension is well-defined independently to the gauge choice? If the answer to this question is already known, I would like to hear it.

References

1) N. Nakanishi, Soryūsiron Kenkyū **72** (1986), 345.
2) N. Nakanishi, Parity **1** (1986), No.9.
3) M. Abe and N. Nakanishi, Prog. Theor. Phys. **87** (1992), 757.
4) D. W. Dūsedau, Phys. Lett. **B 188** (1987), 51.
5) M. Abe and N. Nakanishi, Mod. Phys. Lett. **7** (1992), 1799.

Note added in October 2006

After writing the above essay, I, in collaboration with Abe, further investigated the question whether or not the notion of the critical dimension is well-defined in the covariant gauge.

1. Mödritsch 6) criticized our paper Ref. 5. He claimed the uniqueness of conformal anomaly by starting with a paricular generally-covariantized action for deriving the symmetric energy-momentum tensor. But the ambuiguity was merely transferred to that of choosing a particular non-flat action from the infinitly many non-flat actions which have the same flat limit. 7)

2. Takahashi 8) derived $D = 26$ uniquely by calculating the two-point functions of the gravitational ghosts, that is, without using the energy-momentum tensor. However, his calculation was incorrect owing to his way of dimensional regularization in which the dimensionality n of *external* lines was set equal to 2 from the outset (The term proportional to $n - 2 \to 0$ cannot be neglected owing to the loop divergence.). 9),10)

3. According to the claim of Kraemmer and Rebhan, 11) the gauge invariance of conformal anomaly is proved on the basis of BRS invariance of the action, but the relevant quantity is what is obtained by Euler-differentiating the generally-covariantized action with respect to the background gravitational field. We pointed out that this Euler differentiation does not commute with the BRS tranformation and that the existence of conformal anomaly itself can be shown only after making this Euler differentiation. 10),12)

4. According to the well-known paper of Kato and Ogawa, 13) the Noether BRS charge of the conformal-gauge two-dimensional quantum gravity is not nilpotent unless $D = 26$. We have found, however, that a strictly BRS-invariant exact solution with a nilpotent BRS charge exists for *any* value of D. 14),15) This curious phenomenon takes place owing to the appearance of the anomaly for a field equation. The field-equation anomaly disappears for

$D = 26$ in the conformal gauge. In the covariant gauge, however, the field-equation anomaly does not disappear for any particular value of D.

Thus the critical dimension of the string theory is *not* a natural consequence of the two-dimensional quantum gravity. I conjecture that the string theory cannot be consistently formulated in the covariant gauge. [For a critical review on conformal anomaly, see Ref. 10.]

References

6) W. Mödritsch, Mod. Phys. Lett **A 9** (1994), 241; 861(E).
7) M. Abe and N. Nakanishi, Mod. Phys. Lett **A 9** (1994), 3505.
8) T. Takahashi, Prog. Theor. Phys. **95** (1996), 995.
9) M. Abe and N. Nakanishi, Prog. Theor. Phys. **96** (1996), 1281.
10) M. Abe and N. Nakanishi, Int. J. Mod. Phys. **A 13** (1996), 3081.
11) U. Kraemmer and A. Rebhan, Nucl. Phys. **B 315** (1989), 717.
12) M. Abe and N. Nakanishi, Prog. Theor. Phys. **100** (1998), 411.
13) M. Kato and K. Ogawa, Nucl. Phys. **B 212** (1983), 443.
14) M. Abe and N. Nakanishi, Int. J. Mod. Phys. **A 14** (1999), 521.
15) M. Abe and N. Nakanishi, Int. J. Mod. Phys. **A 14** (1999), 1357.