JOURNAL DE PHYSIQUE
Colloque C3, supplément au n° 12, Tome 43, décembre 1982 page C3-755

THEORETICAL PERSPECTIVES

G. 't Hooft

Institute for Theoretical Physics, P.O. Box 80006, 3508 TA Utrecht, The Netherlands

When I was asked by the Organizing Committee to speak about "Theoretical Perspectives" this left me with two possibilities. One would be to discuss: "Perspectives in Particle Physics according to standard theory" and the other: "Future perspectives of Theoretical Particle Physics". Both are rather brave enterprises since they require extrapolations towards the future. And since my telephone line with the future has about as much static disturbance as anybody else's my extrapolations may well be entirely wrong.

I. Perspectives in Particle Physics according to standard theory.

About this first interpretation of my title I shall be brief. Anything I wanted to say on this subject has already been discussed in much more detail in the sessions we heard. The near future of particle physics looks great: Z^0 at 90 GeV and $W^\pm$ at 80 GeV. If all goes well we will learn as much from these new objects as we did from the ψ particles some years ago. One big difference is that this time theory is making rather precise predictions and does not run behind the facts like in '74. If things do not come out as predicted we will first blame the experimentalists, not the theorists. Our only worry should be perhaps that the predictions will be obeyed too well; nothing new will be learnt. R. Gatto[1] calls this "Scenario Zero": only minor errors in theoretical calculations need be corrected.

The Higgs particle

But there are many aspects of particle physics about which theory is still extremely vague. Clearly any experimental results in those areas will be of utmost importance. Going to higher energies we find as the first uncertain factor: the Higgs particle. Is it there? Is it light? Heavy? Composite? If the Higgs particle is light then it will be relatively (but not very[2]) easy to detect (scenario a[1]). This will leave Theory with the mystery of the parameter-conspiracy at higher energy scales: what caused this mass to be so low? More theoretical clues would be needed. If it is heavy, then this would be theoretically more challenging. We have

$$M_H^2 \propto \lambda F_H^2 \propto \lambda G_F^{-1},$$

where G_F is the Fermi constant. If M_H (the Higgs Mass) is large then the coupling constant λ must be large. Because of lack of asymptotic freedom this would imply the near presence of a new strong interaction regime[3,4] at $\sim$ 2000 GeV.

The new possible structure at the TeV range is very ill understood. Unfortunately the perspectives here are much less bright: will experimentalists ever be able to produce, detect and unravel the expected jets at 2000 GeV *(center of mass)* energy? Can they determine their quantum numbers? Even at 1 GeV this seems to be a difficult job. A problem may well be that costs of new machines will continue to rise as roughly the third power of the energy, whereas the theorist's interest is clearly only a logarithmic function of energy. It must be the greatest challenge for experimental particle physicists of the coming decades to surprise the theorists here.

Beyond 2000 TeV

At the TeV (c.m.) region Theory envisages at least four possibilities:

a). One of these Grand Unified Theories is correct[5]. Whether or not it holds all the way to the Planck mass is of secondary importance. Most important is that then there will be no structure at all until much higher energies (10^{15} GeV). The Higgs will be relatively light (comparable with the Z boson mass). Mathematically nothing seems to be wrong with this possibility. But we do not at all understand the physical reason for the unnatural cancellation of parameters required at the high (10^{15} GeV) mass scale[3,4]. One possible attitude here is that Nature provided a precedent in this respect: the cosmological coupling constant in Einstein's Lagrangian is also unnaturally small. Usually we argue however that that is just one more of these ununderstood aspects of quantum gravity but that such things should not be accepted in particle physics.

b). The supersymmetry option.[6] Nature may become supersymmetric beyond 2000 GeV. All particles come in super multiplets with mass splittings not much greater than 1000 GeV. Whether these splittings result from explicit or spontaneous supersymmetry breaking remains to be seen. No new strong interaction structure is then needed at or beyond 2000 GeV, and "naturalness" is saved. At least the supermultiplet arrangements of particles should then be unravelled by theorists and experimenters. Certainly many new particles will be needed.

c). "Technicolor" (or "meta color")[7]. The fundamental assumption here is that the Higgs particle is a two- (or more-) fermion composite. Here also there is a historical precedent: the Cooper pair in super conductors is just a "Higgs" particle. But for understanding fermion masses we need "extended technicolor" and we heard already what the difficulties are when such theories are exposed to phenomenological data.

d). I leave a fourth option: "hyper complexity". This is the extreme opposite of option a. The particle spectrum at and beyond 2000 GeV may be so rich that no presently known theories allow us to guess the details. This may seem to be just an "emergency exit" for theorists but truly it is a possibility that cannot be ruled out. Some speculations in this direction can be made[8].

Consistency conditions for bound state theories

The idea that presently known "elementary" particles are actually bound states composed of even more "elementary" constituents, has been speculated upon a lot. A problem here however is that an explanation must be found as to why these bound states are so much lighter than the constituent mass scale. One tries to find a symmetry principle[3,4] that forbids a mass term to develop. But here one finds restrictions. They are the so-called "anomaly consistency conditions"[3,9]. Let me explain these. One looks at all currents in the constituent theory (in particular those to which no gauge particles are coupled) and computes the Adler-Bell-Jackiw anomaly in their conservation laws. Now the fermionic bound states that come out of the system should reproduce the same anomaly. Thus

$$\sum_{\substack{\text{elementary}\\ \text{constituents}}} \pm \ \mathrm{Tr}\ T_{\{a} T_b T_{c\}} = \sum_{\substack{\text{fermionic}\\ \text{bound states}}} \pm \ \mathrm{Tr}\ T_{\{a} T_b T_{c\}}$$

The signs refer to the helicities of the fermionic particles. Most researchers agree upon this important restriction.

If a state occurs with both helicities then mass terms will be possible that lift these states, pairwise, away from the set of massless particles. Such pairs do not contribute in the equation.

Originally it was hoped that there would exist such a thing as an "effective Lagrangian for light bound states. A condition was derived[3,10] later called "persistent mass condition", and it was subsequently refuted[10]. Let me explain the philosophy behind this. One could hope that the effective Lagrangian would be determined essentially by small distance effects in the theory, and that it would be insensitive to phase transitions. The phase into which the system would finally condense would be determined solely by the values of the various parameters in this Lagrangian (for instance whether or not an effective potential has only one or many degenerate minima). The characteristic numbers of helicity states (indices) would then be independent of the values of mass parameters in the original Lagrangian: "persistent mass condition". Unfortunately this cannot be true. The two conditions combined yield beautifully unique but nonsensically fractional indices. Dropping the persistent mass condition implies dropping the notion of unambiguous effective Lagrangians and with that the hope of being able to make precise calculations and predictions this way.

Presumably then, the pairing mechanism just mentioned is not just a small distance effect. Without the second condition one has more freedom to speculate[11] the bound state spectrum (without much hope of verifying such speculations by calculations). But even so one finds that the algebra destroys most simple-minded "preon", "rishon" or "straton" models that were proposed on the basis of phenomenology. As a rule of thumb one may expect that the Lagrangian needed at the more "elementary" level is about as complex as the phenomenology one wishes to explain: large gauge groups and preon multiplets seem to be necessary. Is a hypercomplex world unavoidable then? Who knows.

Miscellaneous

There are many scattered areas in particle physics where standard theory gives perspectives. Or destroys them. Ordinary quantum mechanics dictates that magnetic monopoles are quantized in units of the Dirac charge:

$$g = \left(\frac{2\pi}{e}\right) . n = 68.5 \text{ n.e.}$$

Therefore the existence of a monopole with one Dirac unit is inconsistent with the existence of a fractional electric charge, because then the monopole quantum should be multiplied with the inverse of this fraction.[12] Therefore I would conclude that at least one of the two famous recent Stanford experiments[13,14] must be wrong. Well, there are other, rather exotic, ways out of this dilemma. One would be that the observed (?) fractional charge is also a magnetic monopole. That is because if two particles (1 and 2) carry magnetic charge then Dirac's condition should be replaced by

$$g_1 q_2 - g_2 q_1 = 2\pi n_{12}$$

where g_i are the magnetic charges and q_i the electric charges. n_{12} is an integer.*

Standard theory can still every now and then make surprise discoveries. One recent surprise is the discovery of the Rubakov effect in a monopole[15]. Another is Witten's discovery that SU(2) gauge theories are not allowed to have an odd number of chiral fermions.[16] Note that this is one more reason to believe in the existence of such a thing as a top quark.

II. Future perspectives of Theoretical Particle Physics.

The theoretical situation today is clearer than ever. A number of things are now absolutely standard, well known and understood:

1. We know the prescription how to build any renormalizable (gauge-) field theory[17].

----

* H. Georgi pointed out another exotic possibility: both the magnetic monopole and the fractional charge could be coupled to an entirely unknown new U(1) gauge field. We then have

$$\sum_i (g_1^i q_2^i - g_2^i q_1^i) = 2\pi n_{12}$$

where i refers to different U(1) groups.

2. We know how to perform any perturbative calculation in such theories.[18] In practice they may be tedious but we understand the properties of the integrals involved. There is even a little more than that: instanton effects look different from perturbative effects, but the saddle point techniques involved could be called perturbative in a broader sense.
3. The set of renormalizable field theories is denumerable.

Other things are <u>not understood</u> at all and <u>nearly hopeless</u>:

1. The values of masses and coupling constants are not predicted. Precisely because perturbation theory is understood so well we know that there is no theoretical limitation. Any "theory" claiming to understand mass- and coupling constant relations must be based on dubious arguments of esthetics. The logic is never convincing. Constructing bound state theories seems to be of little help because, as stated,the underlying theories are not simpler than the phenomenology they attempt to explain so that they contain as many free parameters as there are unknowns.
2. How to perform reliably non-perturbative calculations. This is not to say that presently popular and widely practiced methods, such as those of refs 19) and 20) or the lattice Monte-Carlo procedures[21], would not be of enormous value. The point is that they involve speculations that one has to believe on faith. Does the continuum limit really exist and reproduce QCD? We would like to have mathematically rigorous methods without these or even more dubious assumptions.

3. We are still very far away from any quantum theory of gravity.

So there is a wide gap between the completely understood and the apparently hopelessly impossible. If there are perspectives in "Theoretical Theory" then that should be in finding little islands in the gap that some time may serve as foundations for a bridge. If history is a guide, then the various ununderstood problems may well be all connected.

The lattice. Universality

The lattice has been discussed in several other sessions at this conference[21]. Here I mainly wish to stress that there is a fundamental underlying assumption when these methods are applied to quantum field theory. A lattice Lagrangian essentially of the form

$$\mathcal{L} = \frac{1}{g^2} \sum_{\text{plaquettes}} \mathrm{Tr}\left[UUU^{+}U^{+}\right] ,$$

where U are unitary matrix variables defined on the lattice links, is considered on a lattice with lattice spacings of length a. In the "continuum limit" it should reproduce the Lagrangian

$$\mathcal{L} \to -\frac{1}{4} G^{a}_{\mu\nu} G^{a}_{\mu\nu} ,$$

if the identification

$$U_{\mu}(x) \to \exp\left[iga \frac{\lambda^{c}}{2} A^{c}_{\mu}(x)\right] ,$$

is made. The theory is "regularized" with a cut-off $M \simeq 1/a$.

Now according to the renormalization group the limit $a \downarrow o$; $g \downarrow o$ must be taken such that

$$g^2 \to \frac{16\pi^2}{\beta_o \log \frac{1}{\Lambda^2 a^2} + (\beta_1/\beta_o) \log\log \frac{1}{\Lambda^2 a^2}}$$

with Λ (= lattice lambda parameter) kept fixed. Does this limit exist? If perturbation expansion is performed with respect to g^2, or rather a running coupling constant $g^2(\mu)$ then everything seems to be in order. But this expansion is fundamentally divergent. We do not know whether the expansion uniquely determines the theory. So maybe the limit does not exist, or maybe it depends on the details of the lattice (cubic, triangular, body-centered cubes, etc). The uniqueness of the limit (for subtracted, gauge-invariant Green's functions) is called "universality" and if universality indeed holds then the lattice Lagrangian is a good way to define the popular theory called "quantum-chromodynamics". If it holds then it should be possible to prove this. And then it should and probably will be possible to calculate hadronic features with great accuracy.

But what if universality does not hold? If different lattices give different proton mass-to-string constant ratios (I am not talking about 1% but about discrepencies of the order of 100%)? This may still be the case because universality has not yet been checked accurately (although attempts are under way[22]. This would be a highly interesting situation because it would imply that hadronic properties are still determined by a kind of physics that we do not understand. I come back to this later.

Quenching

An intriguing observation has been made by Eguchi and Kawai[23], later criticised and refined, also on the basis of earlier work[24]. Suppose one considers the expectation value of a Wilson loop in a closed box. Now we could divide the box into smaller boxes with additional periodic boundary conditions. The Wilson loop expression then compares with an expression for a loop folded up inside one of the smaller boxes. But its total area may not have changed. And in SU(N) theories, with N large, the surface spanned by the loop does not interact with itself. So one may expect that the Wilson loop expression will not have altered. This is what Eguchi and Kawai find, working on a lattice. They reduce the system to the extreme: a $1\times1\times1\times1$ lattice.

At first sight this looks like an enormous simplification, bringing the exact solution of SU(∞) gauge theories within range. Unfortunately that is probably just an illusion. For one thing: one should not fold the Wilson loop too many times because then the number of intersections might beat the damping $1/N^2$ factor for the interactions. In a different formalism it was found that the energy-momentum operator P_μ can be rewritten as an NxN matrix, which after diagonalization reads:

$$P_\mu = \begin{pmatrix} P_\mu^1 & & o \\ & \ddots & \\ o & & P_\mu^N \end{pmatrix}$$

But in a volume $V = (\Delta x)^4$ we want a set of eigenvalues separated by distances $\Delta p = \hbar/\Delta x$. So the number of eigenvalues required increases with $(1/\Delta p)^4 \propto V$. Thus the unfolded volume V cannot become larger than N. Apparently all space-time dependent structure of the theory has been traded for an N dependent structure. For computing large Wilson loops one really needs tremendously large N values, if quenching is used. These limits may now be very hard to perform. So although these quenching methods give interesting relations between large N and large V effects, it is quite preposterous to suggest that the $N \to \infty$ theory in the continuum is made any easier this way[25].

As stated earlier, quantum chromodynamics is perhaps not a perfect theory, if universality does not hold. I would like to put this situation in a historical perspective.

1. In the beginning there was Lagrange field theory. Any Lagrangian was as good as any other, as far as tree diagrams were concerned. If the coupling constants are small enough then such a theory is reasonably accurate. An example is the 4-fermion theory for the weak interactions.

2. If the coupling is as large as 1/137 then higher order effects become worth while. Now we wish that such theories are renormalizable. Gauge theories for weak and electromagnetic interactions were indeed shown to satisfy this requirement. A very important result was obtained by Cornwall et all in 1973: all renormalizable field theories are of the gauge theory type[26].

3. At still stronger couplings one might worry of features such as the Landau ghost. At high energies accumulated interactions blow up and cease to make sense. This is why it is so important that some gauge theories, in particular QCD, are asymptotically free: at high energies the accumulated interactions cancel.

And now perhaps we reach stage 4: asymptotic freedom is still not good enough if we cannot prove universality. In that case the future may show "asymptotic universality" or "asymptotic convergence" meaning that the rate of convergence of perturbation expansion grows rapidly with energy.[27] Stage 5, still further in the future, may finally give us a convergent field theory. This is a field theory that is mathematically as rigorous as presently known "constructive field theories" in 2 or 3 space-time dimensions[28].

My own work now points towards a conjecture[27]: SU($N \to \infty$) gauge theories may be mathematically well defined. Universality holds in the $N \to \infty$ limit. As yet we claim the following result[29]: in a variant of SU(∞) gauge theory with massive gluons only (all masses $\geqslant m_0$) and weak enough coupling constants, $g^2N \leqslant (g^{\mathrm{crit}})^2$, the Dyson-Schwinger equations have a unique solution that can be obtained through an iteration procedure. So we do have a "constructive field theory in four dimensions" although it is still in an unphysical limit: the number of "colors" is strictly infinite. Two technical procedures are used:
one is the skeleton expansion. We write closed blobs representing the sum of diagrams for the dressed propagators, dressed irreducible 3-point vertices and dressed irreducible 4-point vertices (Figure 1) called elementary Green's functions.

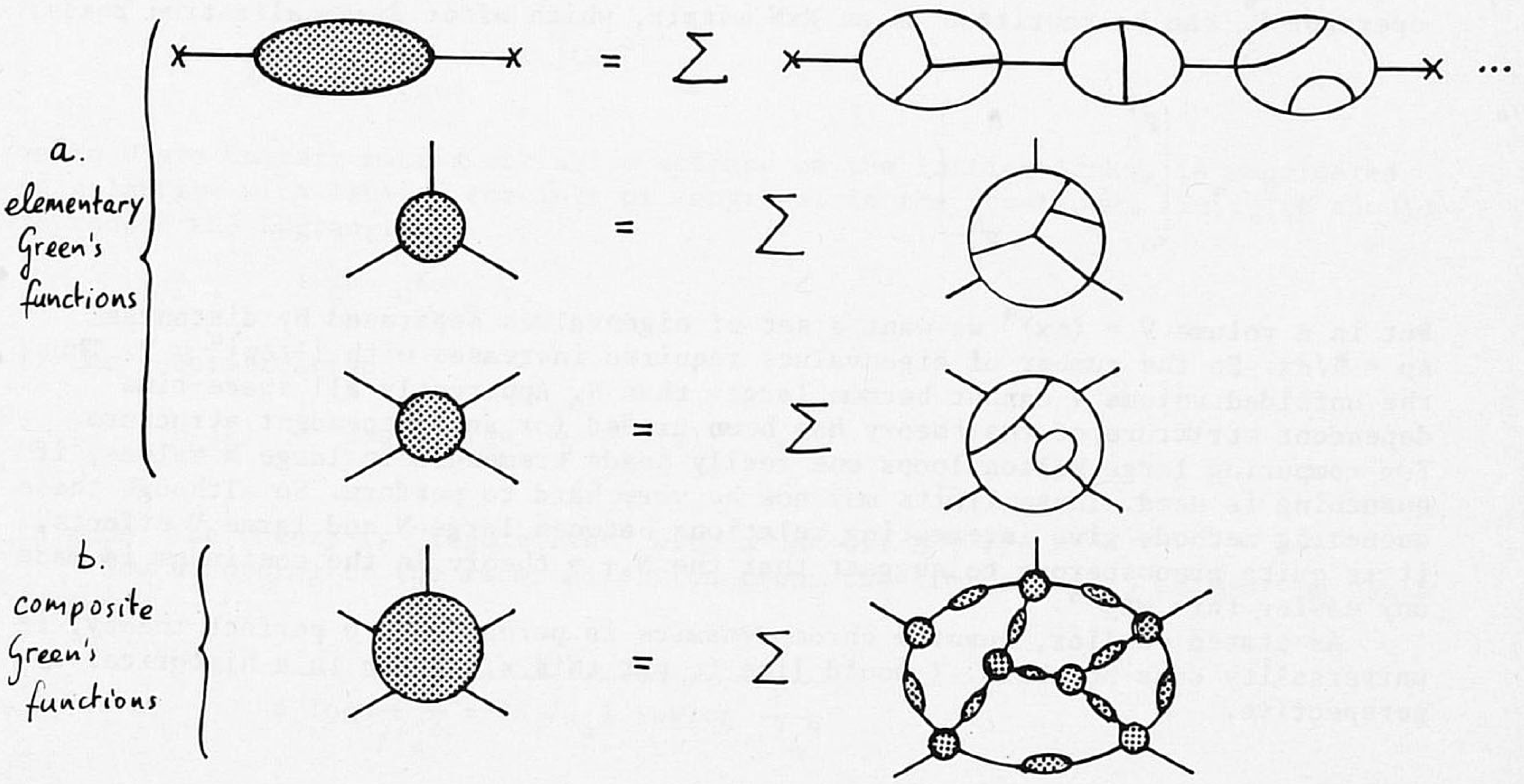

Figure 1. The skeleton expansion.

The higher ("composite") Green's functions such as the irreducible 5-point function, are expressed as sums of diagrams containing only dressed propagators, 3-point and 4-point vertices. Because $N \to \infty$ all these diagrams are planar. A crucial theorem now is that if the elementary Green's functions remain small enough ($g^2 N \leqslant (g^{crit})^2$) then the sum of the "skeleton graphs" for the composite 5 point function converges. Powerful mathematical theorems of ref.[30]) can be applied here. Secondly we use difference equations (Figure 2).

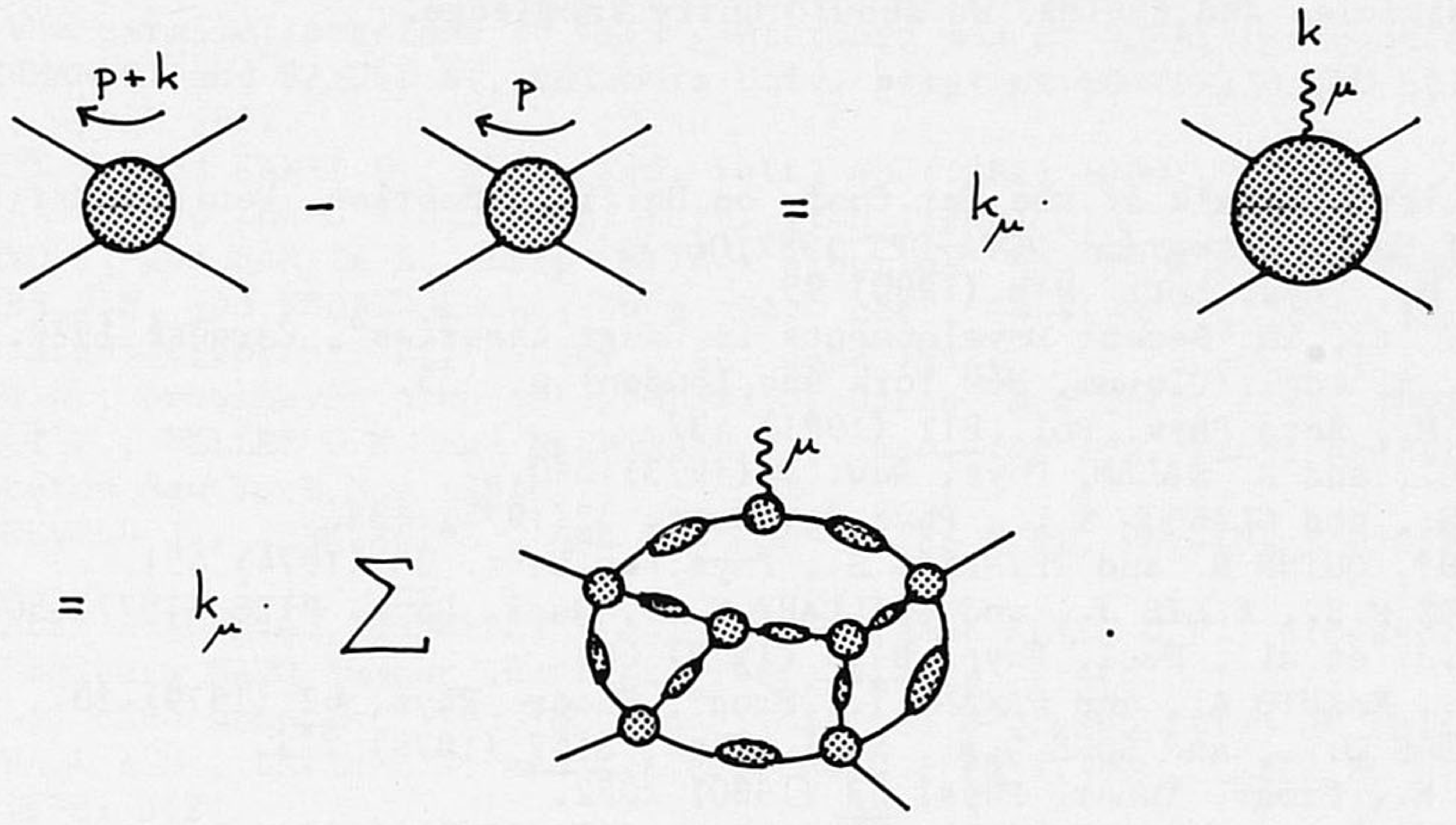

Figure 2. Difference equations

The difference between the values for elementary Green functions at different external momenta is given by a Green function with one extra external leg. After differentiating a few times this way we get five legs and for those diagrams again the skeleton expansion can be written down.

The set (difference equations + skeleton expansions) determines the Green functions completely, up to "integration constants" which replace the bare parameters of the theory. No bare parameters occur explicitly. One can solve these equations by an iteration procedure which under the conditions I mentioned can be shown to converge! It was important to use difference equations, not differential equations in order to avoid some bothersome infrared problems. At some point the difference equation looks like the ordinary renormalization group equation in Gell-Mann-Low form:

$$\frac{d}{d|p|}\, g^2(|p|) = \frac{1}{|p|}\, \beta(g^2)$$

It is instructive to observe that if in this equation we iteratively determine $\beta(g^2)$ from $g^2(|p|)$ and then integrate to reobtain $g^2(|p|)$, then this iteration converges if we start within certain limits. We should mention that the above derivations are certainly related to, or rather mathematically precise implementations of, more intuitive and practical arguments presented in ref[31].

Science Fiction

The author speculated that the only theory for the world that is indeed mathematically unique, with predictive power, is SU(N) with $N \to \infty$ as the energy $E \to \infty$. One can well imagine such a system. At low energies we have SU(3) of color. At some energy scale this turns out to be SU(4) broken down to SU(3) by some Higgs mechanism. The story repeats endlessly at higher energies. This way one can hope to obtain an 1/N expansion that is "asymptotically free", with N increasing linearly with energy. In that case there are some hopes that the 1/N expansion becomes Borel summable. Indeed such a theory would have an immensely complex structure at higher energies so it would represent option d.

Needless to say that these were phantasies more likely to be wrong than right. The main message is that we should try to find better model building methods than just "take a gauge theory...", and indeed there should be compelling mathematical arguments for them. Presently proposed theories are often based just on arguments of esthetics and those have historically always shown to be misleading. Only on hindsight, yes, successful theories of Nature always turn out to be beautiful, but often in some unpredictable way. In conclusion[32]: we should not primarily attempt to unify particles and fields. We should unify knowledge.

REFERENCES

1. GATTO R.,invited talk at the Int.Conf. on Unified Theories, Venice, March 1981, Univ. of Genève preprint UGVA-DPT 1982/06-351.
2. VELTMAN M., Phys. Lett. 91B (1980) 95.
3. HOOFT G. 't., in "Recent Developments in Gauge theories", Cargèse 1979, G. 't Hooft et al eds. (Plenum, New York and London) p. 135.
4. VELTMAN M., Acta Phys. Pol. B12 (1981) 437.
5. PATI J.C., and A. SALAM, Phys. Rev. D8(1973)1240.
   GEORGI H., and GLASHOW S.L., Phys.Rev.Lett. 32(1974) 438.
   GEORGI H., QUINN H. and WEINBERG S., Phys.Rev.Lett. 33 (1974) 451.
   CHANOVITZ M.S., ELLIS J., and GAILLARD M.K., Nucl. Phys. B128 (1977) 506.
   BURAS A.J. et al., Nucl. Phys. B135 (1978) 66.
   INOUE K., KAKUTO A., and NAKANO Y., Progr. Theor. Phys. 62 (1979) 307.
   NANOPOULOS D.V., and ROSS D.A., Nucl. Phys. B157 (1979) 273.
   KOMATSU H., Progr. Theor. Phys. 63 (1980) 2052.
   DIMOPOULOS S., and SUSSKIND L., Phys. Rev. D18 (1978) 4500.
   For more recent references see the sessions on GUTs at this conference, and: "The second Workshop on Grand Unification, Univ. of Michigan, Ann Arbor, April 24-26, 1981, LEVEILLE J.P. et al, eds. Birkhäuser, Boston , Basel, Stuttgart, 1981.
6. DIMOPOULOS S. and GEORGI H., Harvard preprint HUTP-82/AO 46, July 1982.
   INOUE K. et al, Kyushu preprint 82-HE-5, Fukuoka 812, Japan, May 1982.
   INAMI T. and LIM C.S., Tokyo preprint INS-Rep-451, June 1982.
   TODA O.E., Purdue preprint, Purd.-TH-82-10.
7. DIMOPOULOS S. and SUSSKIND L., Nucl. Phys. B155 (1979) 237.
   DAVIS A.C., "Composite Higgs, Tumbling and vacuum alignment": preprint TH3347, CERN: talk given at the V Warsaw Symposium, Kazimierz, May 1982.
8. NIELSEN H.B., report given at this conference.
9. FRISHMAN Y. et al., Nucl. Phys. B177 (1981) 151.
   BAUKS T. et al., Phys. Lett. 96B (1980) 67.
   RABY S. et al., Nucl. Phys. B169 (1980) 373.
   FARRAR G. Phys. Lett. B96 (1980) 273.
   COLEMAN S. and WITTEN E., Phys. Rev. Lett. 45 (1980) 100.
   BARS I and YANKIELOWICZ S., Phys. Lett. 101B (1981) 159.
10. PRESKILL J. and WEINBERG S., Phys. Rev. D24 (1981) 1059.
11. ALBRIGHT C.H., Phys.Rev. D24 (1981) 1969.
    ALBRIGHT C.H., SCHREMPP B., and SCHREMPP F., Phys.Lett. 113B (1982) 225, and NAL preprint Fermilab-Pub-82/14-THY, Jan. 1982.
12. STROMINGER A.E., Inst. of Adv. Study preprint, Princeton, May 1982.
13. LA RUE G.S., FAIRBANKS W.M. and PHILLIPS J.D., Phys. Lett. 42 (1979) 142.
14. CABRERA B., Phys. Rev. Lett. 48 (1982) 1378.
15. RUBAKOV V.A., JETP Lett. 33 (1981) 644.
    CALLAN C.G. Jr. Phys. Rev. D25 (1982) 2141.
    CALLAN C.G. Jr. "Monopole Catalysis of Baryon decay" Princeton University preprint 1982.
    BAIS F.A. et al., CERN preprint TH3380.
16. WITTEN E., "An SU(2) anomaly", Princeton University preprint 1982.
17. ABERS E.S. and LEE B.W., Phys. Repts. 9C (1973) 1. (keeping in mind constraints raised by the anomaly cancellation requirement and the restriction imposed by ref. 15).

18. HOOFT G. 't and VELTMAN M., "DIAGRAMMAR", CERN report 73-9, reprinted in "Particle Interactions at Very High Energies", NATO Adv. Study Inst. Series, Sect. B, vol. 4b, p. 177; Nucl. Phys. B50 (1972) 318.
19. BERG B, BILLOIRE A. and REBBI C., Brookhaven preprint BNL 30826.
ISHIKAWA K. et al., DESY 82-41, preprint July 1982.
BERNARD C., DRAPER T. and OLYNYK K., UCLA preprint UCLA/82/TEP/10, June 1982.
20. SHIFMAN M.A., VAINSHTEIN A.I., ZAKHAROV V.I., Nucl. Phys. B147 (1979) 385; 448.
BELYAEV V.M. and IOFFE B.L., preprint ITEP-59, Moscow 1982.
21. See the parallel sessions at this conference and C. REBBI's rapporteur's talk.
22. GROSSMAN B. and SAMUEL S., Columbia Univ. preprint CU-TP-230 (RU 82/B/25) New York, April 1982.
23. EGUCHI T. and KAWAI H., Phys. Rev. Lett. 48 (1982) 1063.
PARISI G. and ZHANG Y.C., Phys. Lett. 114B (1982) 319.
ALFARO J. and SAKITA B., City College preprint, New York CCNY-HEP-82/8.
HELLER U.M. and NEUBERGER H., Phys. Rev. Lett. 49 (1982) 621 and Inst. for Adv. Study preprint, Princeton, N.J., June 1982.
OKAWA M., Brookhaven preprint BNL31330, Upton, New York, April 1982.
BHANOT G., HELLER U.M. and NEUBERGER H., Inst. for Adv. Study preprint, Princeton New York, May 1982.
24. GROENEVELD J., JURKIEWICZ J. and KORTHALS ALTES C.P., Physica Scripta 23 (1981) 1022.
GONZALEZ-ARROYO A., JURKIEWICZ J., and KORTHALS ALTES C.P., proceedings of the Freiburg NATO Summer Institute 1981, Plenùm Press.
25. BARS I, CERN preprint TH3318, June 1982, and a presentation at this conference.
26. CORNWALL J.M., LEVIN D.N. and TIKTOPOULOS G., Phys. Rev. Lett. 30 (1973) 1268; 31 (1973) 572.
LLEWELLYN SMITH C.H., Phys. Lett. B46 (1973) 233.
27. HOOFT G. 't., Phys. Lett. 109B (1982) 474.
28. Constructive Quantum Field Theory , 1973 "Ettore Majorana" Int. School of Mathematical Physics. VELO G. and WIGHTMAN A., eds., Lecture Notes in Physics Vol. 25 (and references there in), Springer, Berlin, Heidelberg, New York 1973.
29. HOOFT G. 't., "On the Convergence of Planar diagram expansions", Commun. Math. Phys.; to be publ.
30. Calan C. de and RIVASSEAU V., Commun. Math. Phys. 82 (1981) 69.
31. STEVENSON P.M., Phys. Rev. D23 (1981) 2916.; Nucl. Phys. B203 (1982) 472; CERN preprint TH3358, 12 July 1982.
32. HOOFT G. 't, rapporteur's talk given at the EPS Int.Conf. on High Energy Physics, Palermo, Sicily, June 1975.

Discussion

E. BREZIN (Saclay).- The large-N limit always assumes that N goes to infinity faster than all other parameters such as the volume. This takes place for instance in the O(N) -σ-model in the master field approach, however it does yield the correct continuum solution.

G. 'T HOOFT.- The question refers to my comment on the Eguchi-Kawai theorem. I entirely agree. I simply conclude that this fact implies that when large Wilson loops are considered one has to choose between large N or large V so that the E-K theorem may be not so powerful in practice for computational purposes.

C. CALLAN (Princeton Univ.) .- Some people (Tomboulis, in particular) have proposed to cure the sicknesses of quantum gravity by adding an R^2 term to the Einstein action thereby improving ultraviolet convergence. What do you think of these schemes ?

G. 'T HOOFT.- This is one of many theories in which space-time is kept continuous. My problem is then always the same : what does the system look like at distance scales much smaller than the Planck mass inverse ? It does not seem to make sense. Perturbation expansion may be convergent term by term but diverges when summed.

C. ITZYKSON (Saclay).- This is a question, not a comment. Would your conjecture or theorem of the consistency of the large N-gauge theory imply that finite N-gauge theories are inconsistent ?

G. 'T HOOFT.- I wish I knew. The problem is that the skeleton expansion for the 5-point function is guaranteed to diverge. Either one will find Borel and instanton methods to improve perturbation theory or prove that it cannot be done.

J. ILIOPOULOS (ENS).- How are the 2-, 3- and 4- point bubbles to be determined ? Are they arbitrary parameters of the theory ?

G. 'T HOOFT.- The arbitrary parameters come in the boundary conditions at infinite (Euclidean) energy. The 2-, 3- and 4- point functions come from iteratively integrating the renormalization group-like equations.

M. CAHILL (Univ. of New Mexico).- Are you pessimistic about supergravity ? And if so, why ?

G. 'T HOOFT.- The answer is the same as I gave to Callan. Perturbation expansion may be finite term by term, but diverges beyond the Planck-length.

E. BREZIN (Saclay).- Are your integral equations the large N-limit of the Migdal-Polyakov bootstrap ?

G. 'T HOOFT.- They are similar but I try to prove rigorously that they converge (under limited conditions). They seem to rely on Padé techniques.

L. MAIANI (Univ. of Roma).- There are indications that some extended super-symmetric theories are finite. Could you make some comment whether these theories could provide an alternative to the $SU(N=\infty)$ theory ?

G. 'T HOOFT.- They would be finite order by order. For perturbation expansion itself to converge you need planarity. In the equations I use all integrals converge, but the indications you mentioned (Mandelstam's talk at this conference) could imply that the $N\to\infty$ limit of such theories perhaps converges within some radius of convergence without requiring, as I do, asymptotic freedom.