

Our visit is shorter than we should like because my Lorentz lectures come every Tuesday morning. We shall arrive at the Hotel Astoria Tuesday afternoon May 1, I shall go to the Institute that afternoon, and we shall return Sunday afternoon May 6. To Bohr 24 Apr '56

There are so many questions I should like to discuss with you that I shall hardly know where to begin if, as I hope, you have a little time free.

Constantly I puzzle over the issue you have so many times emphasized, how the energy of the several fields combines to give for the vacuum a zero mass density. You were kind enough to add some comments about this issue to my report at Palmer Laboratory in the fall of 1954 on consequences of the interaction between gravitation and electromagnetism. There my account was mainly at the classical level, concerned with the issue how to build up object with mass and charge from fields that have no mass and no charge. It was clear to what fantastic dimensions, 10^{11} cm, one had to go to justify a purely classical analysis, and equally clear that classical static, gravitational fields had nothing significant to contribute to the elementary particle problem. For the last year and a half I have been struggling to understand gravitational field fluctuations. This question is part of the wider and still more oppressive issue whether it obviously makes sense or nonsense to think of elementary particles as built of the fields of zero rest mass: gravitation, electromagnet and neutrinos. Not having been able to discover yet any sign that this what I conservative and invention-free approach is crazy, I struggle on stupidly like a freshman. He thinks he has in his textbook all the principles for the solution of his problem, and has the right numerical answer at the back of the book, and only has to juggle things together to come out with this answer. Fortunately I have had the help of some intelligent associates, Putnam, Weber and especially Misner, who has been very actively concerned with the problem of quantizing the gravitational field. If the answer at the back of the book is to come out, we have to take seriously fluctuations in the gravitational field down to dimensions of the order of $L_g = (hG/c^3)^{1/2} \approx 10^{-33}$ cm. Then changes in the metric of the order $\delta g_{ik} / L_g$, are great enough in comparison with unity to force consideration of multiply connected topologies of the space-time continuum, such as are necessary for the explanation of charge in a theory that does not start with the idea of charge. The flux through a "handle" or "worm hole" represents charge. Misner has been able to prove from Maxwell's equations in curved space that this flux - when treated on a classical level - remains constant in time in no matter what complicated way the metric changes with time. Also from the circumstances that quantum theory starts with a variational principle and that this principle is formulated in terms of electromagnetic potentials, not fields there follows a dissymmetry between electricity and magnetism that is reasonable

connected with the absence of magnetic charges in nature.

No reason at all is evident why the flux of lines of force through a typical worm hole should have the value e , and still less reason why it should be the same for all worm holes. Granted the freshman's philosophy, I see no escape from saying that the flux coming out of the end of a tunnel mouth is to be identified, not with the charge of an elementary particle, but with the charge of the "bare particle" as it is envisaged in quantum electrodynamics. In other words, the charge of the bare particle is regarded as of fluctuation theory origin. For a worm hole of effective dimension L the fluctuation field will be of the order $F \sim (hc)^{1/2}/L^2$ and the flux or charge will be of the order $Q \sim L^2 F \sim (hc)^{1/2}$. The electromagnetic part of the mass of a bare particle will be of the order $M_g \sim (F)^2 L^3/c^2 \sim h/Lc \sim (hc)^{1/2} \sim 10^{-5} \text{ gm}$.

If fluctuations of this type are accepted as reasonable in one region of space, they have to be accepted as occurring everywhere. In other words, the separation between worm holes will be of the same order of magnitude as the size of one of them. If one does not wish to give up the principles of electromagnetism and gravitation physics (plus neutrinos) and quantum theory, nor to add anything to them, then one seems forced to attribute to space time in the small this complicated foamlike character. Then it does not seem to make sense to start into physics by talking about the elementary particles - the first approximation after all to the description of the physical situation. Our freshman ought to start with the zeroth approximation, the vacuum itself, before he tries to worry about the first approximation. Thus back to the problem on which you have always laid so much stress! How does it come about that the vacuum has no mass density?

A classical worm hole of radius L violates the field equations locally to give a mass-energy discrepancy of the order

$$M_g \sim \sigma^2 L/0,$$

analogous to a surface-tension effect. For distances of the order L_g this quantity is of the order of magnitude of what is required to compensate the electromagnetic mass. Even the name compensation of course needs modification when one has to deal with substantial departures from flat space, as at distances of the order L_g . Then the stress energy tensor for the free electromagnetic field does not have a conserved integral, due to its interaction with the gravitational-metric field. Only the sum of the energies of the zero rest mass fields is well defined. Would that it were not so difficult to see how to calculate this sum! I should like to learn more from you and Möller and Källen of the possibilities of understanding the compensation problem along these lines.

Nobody can be very happy about postponing the elementary particle issue by words like "only a first approximation; do the zeroth approximation first". How could anyone in his senses conceive of objects with characteristic dimensions like 10^{-11} cm or 10^{-13} cm , and masses like 10^{-27} gm or 10^{-24} gm , being related to worm holes with dimensions and masses like 10^{-33} cm and 10^{-5} gm . One seems to be off by a factor of 10^{22} . To this question I have no answer except like the freshman to fumble stupidly ahead from step to step hoping that the veil will be lifted little by little. I can only think of another question - why should one

IT ISN'T POSSIBLE TO GET A BETTER IDEA OF THE SITUATION THAN BY TALKING WITH THE PEOPLE WHO ARE WORKING ON IT. I DON'T THINK THERE'S ANYTHING THAT CAN BE DONE TO STOP THIS. I DON'T THINK IT'S POSSIBLE TO GET A BETTER IDEA OF THE SITUATION THAN BY TALKING WITH THE PEOPLE WHO ARE WORKING ON IT. I DON'T THINK THERE'S ANYTHING THAT CAN BE DONE TO STOP THIS.

imagine that superconductivity can ever be explained by atomic theory? The characteristic distance of 10^{-5} cm and energy of $kT_{\text{crit}} = 10^{-4}$ ev equally obviously has nothing to do with atomic distances of 10^{-8} cm and energies of 1 ev!

I give this thumbnail sketch of the questions that worry me both because I want to discuss them with you, and because a practical question arises about these discussions. Is it really appropriate to make a talk along such unconventional lines as response to Möller's kind invitation to speak? Would not people at the Institute feel happier if I spoke about something more conventional and more acceptable, and would it not be better to reserve the points I have just mentioned for private discussions? I could for example speak about a topic like "Potentials, Phase Shifts and Scattering Cross Section Related Semi-Classically" and then I would not feel responsible for distracting people with ideas that might turn out to be quite mistaken. I say this only to give you and Möller and Aage the opportunity to arrange things as you think best. You will understand that the topic that I mentioned to Möller, "Problems and Properties of a Universe Built of Fields of Zero Rest Mass" is very much closer to my heart than any other question. It is constantly with me, and I shall be so grateful for any illumination which my friends in Copenhagen can give me. So, I will wait to hear when I arrive whether Fields of Zero Rest Mass should be the topic of my seminar talk or should be reserved for private discussions.

I look forward very much to talking with Aage and Kottelson and others about nuclear problems.

Also I would be appreciative of comments by you and Aage Peterson about the work of Everett. I am mailing today to Aage Peterson the second draft of the thesis of Everett, which Everett gave me 10 days ago in Princeton. The title itself, "Wave Mechanics without Probability", like so many of the ideas in it, need further analysis and rephrasing, as I know Everett would be the first to say. But I am more concerned with your reaction to the more fundamental question, whether there is any escape from a formalism like Everett's when one wants to deal with a situation where several observers are at work, and wants to include the observers themselves in the system that is to receive mathematical analysis.

Because of Everett's thesis and especially because the practical question of seminar topic concerns others at Copenhagen, I am taking the liberty to send carbon copies of all but the first paragraph of this letter to Möller, Aage, Kälen and Aage Petersen.