Bernard Schutz: A statistical approach to radiation reaction.

I am glad that Jim Bardeen made the remark that questions about *I* do not necessarily belong to the domain of physics. Because of this, I dont have to begin as everyone else does, that is, by criticizing others. Toshi Futamasse sketched, in his talk, how one gets the quadrupole formula using our approach to the equations of motion. I will therefore restrict myself to points of principle.

The first thing is motivation. Really, all I wanted was the quadrupole formula! I did *not* want to look at the gravitational waves coming from the pulsars. I only wanted the radiation reaction and the formula for $\dot{P}$.

Two things about the "standard" approach bothered me. The first is that, in some derivations, infinite integrals occur even at orders that one might have thought to be important. I wanted to understand why a nice theory like general relativity led to such strange things. The second thing is that I never really understood radiation reaction. We are told that the binary system reacts to the radiation that it gives off. But you can not really say how much gravitational radiation is given off until you are way out in the wave zone. And, by that time, the stars have done whatever they do in reaction to the emission. This picture seems strange to me. The two bodies are creating some *local* field and are responding to that field. So, I thought that there should be an essentially local way of doing this problem. If it turns out that the reaction is accompanied by radiation going out to infinity, it would be nice; it would mean that Einstein's theory is conservative. But I did not want to *use* this conservative nature of the theory or a global approach because all I wanted was to know what was going on near the bodies.

In electrodynamics, one uses retarded integrals. It is pretty much considered a law of Nature. And, in Minkowski space, it is possible to consider it as a law (although even here it is not a verifiable law.) But when relativists tried to apply this thinking to general relativity, they ran in to trouble because they could not have all the properties of retarded radiation. First of all, the condition could be formulated only in the asymptotically flat space-times where one can hopefully get I . Even in this case, the requirement that there be no incoming radiation at I^- does not imply that there is only outgoing radiation at I^+ ; there is back-scattering. So, the nice symmetry of the retarded fields in Minkowski space can not be brought over to general relativity. Moreover, we only have one space-time and it does not have a I^-; it has a cosmological singularity instead. So, it can not be maintained that retarded potentials or their generalizations *must* be used in general relativity in radiation reaction problems. I am not saying that it is wrong to do that. But it is an idealization. And it has a lot of drawbacks because of global problems. Some of the criticisms of Thibaut's work are deeply rooted in the difficulty in solving global problems in general relativity. They are interesting problems and I would not say that he should stop working on them. But these problems are not particularly related to the formula for $\dot{P}$. That formula is a *local* formula. What should replace this idealization then ? Well, the binary pulsar is sitting in a bath of gravitational radiation. We all have the feeling that this radiation is random and is hitting the pulsar from all directions. (In fact, it is this feeling that justifies the application of no-incoming-radiation condition to the binary pulsar.) So, it seemed to me that one can get rid of all global problems by using the idea of averaging right from the start. Thus, the basic idea is to allow the radiation to come in all possible ways and obtain, not an exact evolution of the binary pulsar, but *an expected evolution of an ensemble of binary pulsars*. More precisely, the idea is the following. From observations, we can deduce the positions and velocities of the two bodies at an instant of time. But we do not know anything about the free part of the gravitational field at the initial instant. One way to face this problem is to say that there should be no incoming radiation. My way is to allow radiation but average over the free part of the gravitational field (i.e. free-data.) Of course, I am not trying to cover any physics by just saying that one should average; I can get any answer I want by averaging appropriately. But the point is that a very simple prescription of averaging gives the experimental result: one just requires that the ensemble-mean of the free-data should vanish at the initial instant of time.

Another important point is that if you want an approximation which is valid in the Newtonian limit, the approximation can not be uniform over all of space-time. For, if we let the binary pulsar go, ultimately the stars would spiral together and collide so that the situation would be very far from that predicted by Newtonian theory in which the stars can just go around forever. So, any approximation to such a system is not uniform in time. Consequently, for a particular system, I want to make predictions only for a finite time. Thus, I have only to consider a finite region to specify the initial data. This is where I do the averaging. But, as I make the system weaker and weaker and go towards the Newtonian limit, the time for which I can predict becomes longer and longer. This is the basis of what Toshi described. Let me recall that we can show that the Newtonian theory is an asymptotic approximation to general relativity. It seems that no one has bothered to show the sense in which it is an asymptotic approximation before. Finally we can get a formula for $\dot{P}$ and show that it is an asymptotic approximation to the period change of the binary pulsar.

Now, to the negative side. There are some philosophical objections against the averaging idea. Secondly, the averaging that we do is rather naive. Finally, as in other methods, the errors are not estimated. There are two kinds of errors. Asymptotwo bodies are creating some *local* field and are responding to that field. So, I thought that there should be an essentially local way of doing this problem. If it turns out that the reaction is accompanied by radiation going out to infinity, it would be nice; it would mean that Einstein's theory is conservative. But I did not want to *use* this conservative nature of the theory or a global approach because all I wanted was to know what was going on near the bodies.

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Now, to the negative side. There are some philosophical objections against the averaging idea. Secondly, the averaging that we do is rather naive. Finally, as in other methods, the errors are not estimated. There are two kinds of errors.Asymptotic approximation means that I can always choose a system weak enough so that my Newtonian approximations are good to whatever accuracy you want to choose. But I should be permitted to make the system weakly relativistic in that sense. But the binary pulsar is just one system and you can not apply a limiting procedure to a single system. This is the first source of errors. The other is that the ensemble variance does matter. The size of the radiation that I take here is important. So, we have to check this using the observed stochastic backgrounds and things like that. But I should say that no one has come as close to estimating errors so far. We *can* write down the error terms. The estimates are, however, too complicated at the moment.

Discussion:

Unruh: Your averaging is done at a particular time. Doesn't this imply that the situation is time-symmetric? If so, does it mean that the pulsar evolves from some compact system in the past?

Schutz: Yes and no. We are familiar with the same paradox in statistical mechanics. In a naive picture, if you observe a thermodynamic system which is not in equilibrium and has an absurdly low entropy, then the most probable thing is that you observed a fluctuation; the entropy is increasing both forward and backward in time. That is a statistical result. And the same thing will be true here. To be more philosophical, one is asking only for future predictions. That is, one is asking: If you have this amount of information now, what is the sensible prediction for future? One is not asking for past-evolution. If you dont like the physical underpinning of this, then you can use any of the other approaches with their drawbacks.What averaging gets you,is a local problem.

Kates: You didn't mention in your list of drawbacks the issue of compact objects.It seems to me that for a compact object such as the binary pulsar, the post-Newtonian expansion is not really valid.Thibaut's work as well as the three zone matching have an advantage in this respect. But I also think that compact objects could be incorporated in your scheme with some more work.

Schutz: I completely agree. It is entirely possible that our error terms are bigger than those in other approaches.

Several participants: Eventually, someone is going to have to look at what is happening to the fluid motion, the pressure etc. In the post-Newtonian discussions, where pressure and fluid velocities are small, people have looked at these things for perfect fluids. But for compact objects, no one has really discussed this problem. Numerical relativity may be the only way to do it.

Schutz: That's right. As Jim said, there is physics and there is mathematics. I am convinced from all the work that has been done so far that, for radiation reaction, the physics is okay. So, the important issues are not the mathematical issues in the relativity theory, but issues like the ones you raised.

William Shaw: I think there have been at least three unfair attacks on I and mathematics ! The I -structure is guaranteed to exist only in those situations which *are* physically interesting. If the system was radiating intensely for infinite time, the properties of I would be destroyed and you wont be able to use even the mathematics of I . So, I is giving sensible boundary conditions.

Schutz: I agree.

Bardeen: The point is that, for our universe, I does not exist.

Ashtekar:That point is not significant. By the same argument you can throw away the entire scattering theory in quantum mechanics and high energy physics. We also dont have a Minkowski space in our universe! I is certainly an idealization. The notion of isolated systems is an idealization. And it is not peculiar to general relativity. It is made in other branches of physics too.

Thorne: I dont understand the point that errors are not estimated. Again, I am just a poor physicist. But I would have said that the errors are of the order of 10^{-6}, i.e., going up one more order in that expansion. And, I would have thought that I could even calculate the post-Newtonian corrections to this, if you permit me that certain things at higher order are not sharply varying functions.

Schutz: Wait. There is more to it. In a different theory of gravity, that estimate would be all wrong. It is only the equivalence principle that allows us to say that we can forget about the 10^{-1} factor inside the neutron star.

Thorne: I saying that, I meant to assume the equivalence principle. I think one *could* calculate the next order corrections to this somewhat easily if you permit the leaps of faith that I made at the end of my talk.So, why do you say that the errors are not estimated ?

Walker: You can not write inequalities.

Damour: Yes. The errors would be of the order of magnitude Kip said. It is a matter of rogour only.

Horowitz: In the light of this question of error estimates, I might just mention that there is now work going on with colleagues in which one gets an *exact*formula for the energy that is radiated,in terms of sources. It is an exact formula for solutions to Einstein's equation with sources. One would be able to use it to derive an approximate formula using any approximation technique you choose. Since one would start from a formula valid in the exact theory, it is,at least in principle, easy to estimate the errors. The exact formula is derived using the Witten spinors and the arguments that went in to the positive energy theorems.

Concluding remarks:

I hope that the session served its purpose.As an outsider, I can say for myself that now I have a much better feeling for the subtleties involved in the two approaches. The two approaches are indeed very complimentary. The Paris group emphasizes the post-Minkowski approximation and uses what may be called global methods. In particular, they wish to impose the no-incoming-radiation condition. The Cardiff group, on the other hand, follows the post-Newtonian scheme and uses local methods. In particular, they wish to average out the free-data for gravitational waves at an initial instant of time and then let the system go. The Paris group is interested in the exact evolution of an isolated binary system, while the Cardiff group analyses the expected evolution of an ensemble of binary systems. It is indeed very interesting to see that such diverse methods address the same physical problem.

Both approaches have some puzzling aspects, primarily, I suppose, because both methods are somewhat unconventional. In the Paris work, the use of analytical continuation and Riesz functions makes the analysis somewhat mysterious. Why should this mathematical technique of replacing the physical sources by complex tensor fields, solving the problem for these fields and then taking analytic continuation,yield a physically sensible answer to such a complicated problem? How does this method "know" about the physical boundary conditions? The final solution has an unique asymptotic expansion near the world-lines. For non-rotating sources, the expected approximate spherical symmetry near the world-lines implies that the solution should resemble the Schwarzschild metric up to some order, say in the expansion in r. And the analytic continuation method fulfils this expectation in a precise sense. But for rotating sources, one would expect the uniqueness to fail; apriori, even in low orders in expansion, the differences between various rotating sources should show up. Yet, analytical continuation apparently leads to an unique answer! What is the distinguished axi-symmetric metric approached asymptotically by this solution? Is it the Kerr metric? That would be very interesting. But it may also mean that the method is geared to black holes and not to general neutron stars.In the Cardiff approach, a special status is given to the "initial" time at which the ensemble is so to say "prepared". For example, it is only at this initial instant that the average value of the free-data vanishes. This special status also seems a bit mysterious. One can of course say that the same problem arises also in other applications of statistical mechanics. But that hardly removes the mystery. Secondly, although the initial goal in this approach was somewhat limited, one would like to know if it can now be made more complete, perhaps even as complete as the Paris work.

These questions, I feel, are interesting in their own right. I agree that their analysis can hardly change the status of the formula for $\dot{P}$. But, having p in so much effort to construct a conceptual framework, it seems to me that one should not be satisfied just with a formula, however significant it may be. As is often the case, the insights that one may gain by answering such questions may we be of crucial help elsewhere, not just in general relativity but also in other branches of mathematical physics.

Finally, let me say that there are of course other groups which are working, or have worked, extensively on equations of motion. Not everything that is interesting could be included in the program today partly because of time limitation and partly because not all groups are represented at this School.So, the discussi is far from being exhaustive.