The Contributions of Bryce DeWitt to Classical General Relativity†

LARRY L SMARR

It is with great pleasure that I review the work of Bryce DeWitt. I had the honor of working with him during my graduate student days at the University of Texas. Indeed his support in early years made my career possible. I was one of the few colleagues Bryce has had that chose *not* to do quantum gravity. I suppose it is therefore appropriate that I attempt to summarize his work in classical gravity.

I think it becomes very clear as soon as you read any of Bryce's papers that his primary concern throughout his carrier has been with the *dynamics* of gravity. Certainly general relativity is the dynamical theory *par excellence*. Bryce's work in classical relativity can be divided into (1) dynamical theory of groups and fields, (2) gravitational radiation, (3) superspace, (4) numerical relativity, and (5) experimental relativity. I will survey each of these fields and then conclude with a few remarks about Bryce as a very dynamic leader.

1. DYNAMICAL THEORY OF GROUPS AND FIELDS

The Dynamical Theory of Groups and Fields is of course the title of a book; it was also a very long chapter in the 1963 Les Houches lectures. I think anyone who is aware of Bryce's work is familiar with this great opus. However, there was an earlier article in the Reviews of Modern Physics back in 1957 on 'Dynamical Theory in Curved Spaces'. That article really foreshadowed a great number of Bryce's later works.

There is a very nice quote in that paper, in which Bryce says:

> The existence of any fundamental theoretical structure which is far from having been pushed to its logical mathematical conclusions is a situation which may have great potentialities.
>
> DeWitt (1957a)

† Based on a talk given at an informal meeting held in Austin, Texas, on December 11 1982 to honor Bryce DeWitt's 60th birthday.

This statement is the secret to much of Bryce's success. Bryce always has been in love with formalism; he used to tell us: 'A formalism has a life of its own; it leads you, it gives you part of the answer'. I have never known anyone who has the ability to push a formalism to the limit the way that Bryce can. There is a lot to be said for this approach. The whole point to mathematical physics is that it is a self-contained kind of structure which can make its own internal predictions. As Wigner has emphasized, the most amazing thing about mathematical physics is that it describes the world at all—that's a miracle, something we don't understand. Bryce has always had great faith in the ability of mathematical formalism to lead us to greater truths about the world.

In these series of works Bryce laid out some very powerful tools, not only for him to use later on, but also for very many other scientists to use. In particular, anyone who has worked with Bryce at all understands what 'condensed notation' means. Bryce tackles problems which are too hard to deal with using the mathematical notation adopted by most people. Bryce got rid of all the things we usually worry about (subscripts, space–time dependencies, etc) in favor of one 'supersymbol'. One important result of this approach is that he was able to deal with many theories at once. Although at first it seems 'just like a formalism,' often this technique has led to new insights.

I might mention in passing that as I was preparing this lecture I went to the Physics Library at the University of Illinois to find *The Dynamical Theory of Groups and Fields*. It was not on the shelf, so I went to the librarian and asked, 'Where is this? Has some sort of fanatical relativist checked out the book?' She looked it up on the computer and, no, it was a graduate student in nuclear physics that currently has this book checked out and is working through it. The modern approach of gauge fields, of course, uses much of the formalism which Bryce developed in 1963. As a result, colleagues often remark: 'Bryce was many years ahead of his time when he wrote about gauge field theory.' This is not only true at the quantum level, but also at the classical level.

I believe that it was because Bryce got his PhD under Julian Schwinger, that he developed such a love of formalism and, in particular, of the action formalism. Much of Bryce's later research centers on the action and its uses. In *The Dynamical Theory of Groups and Fields*, Bryce develops very powerful variational tools and the Hamilton–Jacobi theory for deriving physics from an action. In addition, he laid out the theory of continuous groups, Lie groups, bi-tensors, and the theory of observation. This latter theory takes a situation which can be quite complicated and actually analyzes it mathematically into

(1) what is the instrument of observation

(2) what is the observed

(3) what is the coupling between them. This is a theory which is central in the development of not only quantum physics but also classical physics.

These early papers produced a theoretical framework. Now I want to describe how Bryce used that framework to solve a number of major problems in classical gravity. I will first discuss gravitational radiation.

2. GRAVITATIONAL RADIATION

Here there are two papers, one with Brehme in 1960 and one with Cécile in 1964 (DeWitt C M and DeWitt B S 1964). In addition, there are unpublished 1971 Stanford lecture notes. I was fortunate enough to be at Stanford when Bryce was a visiting professor there in 1971. He was lecturing on general relativity and produced a set of handwritten notes. Those notes are priceless, and I hope they can someday be published. Anyone who has ever seen Bryce's handwritten notes realizes that it is not necessary to type them; they are perfect—very meticulous, like calligraphy.

In the two papers, a large amount of mathematical formalism was developed which has turned out to be extremely useful. The major physics question posed was: if an electrical charge moves through curved space, does it radiate? After all, it's in some sense gravitationally accelerated, and accelerated charges should radiate. On the other hand, we are supposed to be able to locally transform away the gravitational field, so maybe it isn't really accelerated. There is a real paradox here. To solve this problem, Bryce used a curved space Green's function. Bryce developed the exact theory with Brehme and then made some very useful approximations to it with Cécile, for the case in which there is a weak gravitational field and a charged particle providing an electromagnetic field. One asks how the gravity makes linear perturbations of the electromagnetic field. Since it is linear, one can use Green's function techniques.

Now the reason I think that Bryce got so much out of this calculation and why so many people later on used the results of that calculation was that it was a *serious* calculation. The 1960 paper says:

> The calculation is patterned directly on Dirac's famous paper on the classical radiating electron. Just as Dirac's calculation was kept Lorentz invariant throughout, so the present calculation is maintained generally covariant throughout . . .
>
> DeWitt and Brehme (1960)

Bryce's reliance on the giants of mathematical physics, the people who made very clear calculations in mathematical physics, is another of the secrets to his success.

A follow-up occurs a sentence or two later. The relevant thing that Bryce discovered here is the importance of the 'tails' of the Green's function; that is, that some of the radiation does not lie on the light cone as one might expect in flat space. Again, Bryce says:

> As has been pointed out by Hadamard [in 1923], a plane or spherical sharp pulse of light, when propagating in a curved 4-dimensional hyperbolic Riemannian manifold, does not, in general, remain a sharp pulse, but gradually develops a 'tail'. It is this phenomenon which is responsible for the electrogravitic bremsstrahlung.
>
> DeWitt and Brehme (1960)

What Bryce realized here was that he could represent a well-posed physical problem, an electron falling in a gravitational field, by the classical mathematical results of a Green's function in the presence of curvature. So, he was able to go back to Hadamard and use some of his elegant mathematics to get the answer to the physics.

I will reproduce some of the mathematics from these two papers because it is very pretty. Consider the covariant D'Alembertian on a scalar field:

$$g^{\mu\nu}\phi_{,\mu\nu} = 0. \tag{2.1}$$

The 'dot' is DeWitt notation for the covariant derivative. If you want to know what the solution for the scalar field is, then you can instead write down the equation for the Green's function. Depending on boundary conditions, there are several Green's functions, retarded and advanced, etc. DeWitt and Brehme make extensive use of a symmetric one which satisfies

$$g^{\mu\nu}\bar{G}_{,\mu\nu} = -\bar{g}^{-1/2}\delta^{(4)}. \tag{2.2}$$

Once you have the solution for that Green's function, then from that by integration you can produce the field ϕ.

There are two parts to the symmetric Green's function: a direct part and a so-called tail part:

$$\bar{G} = \frac{1}{8\pi}[\underset{\text{direct}}{\Delta^{1/2}\delta(\sigma)} - \underset{\text{tail}}{v\theta(-\sigma)}]. \tag{2.3}$$

I reproduce figure 1 from DeWitt and Brehme (1960) to define the symbols used here. The fundamental variable σ is the square of the bi-scalar of geodetic interval $s(x, z)$. If you imagine going off from some event z to a point x which is space-like with respect to z, then you can calculate the arc length s and by squaring that get another bi-scalar σ. That bi-scalar, $\sigma = \pm\frac{1}{2}s^2$, you notice, depends on two events in space–time (z, x), and this is typical of bi-tensors (Synge's world function is the same thing). That σ is the argument in equation (2.3). You can see that the direct part of the Green's function says that there's a delta function of σ, that is, there is support only where $\sigma = 0$ on the light cone. This part of the Green's function makes sure that propagation occurs along the null cone, just as you would expect in flat space for electromagnetic radiation. But there is another term in equation (2.3) in which a Heaviside (step function) symbol occurs. Inside the light cone this term is non-zero, times an amplitude for the tail function v. The v part is the new part of the calculation. The coefficient Δ in the direct part is the 'scalarized van Vleck determinant':

$$\Delta = 1 - \tfrac{1}{6}R^{\alpha\beta}\sigma_{\cdot\alpha}\sigma_{\cdot\beta} + O(s^3) \tag{2.5}$$

and you can see that the Ricci curvature of the space–time comes in to that amplitude.

To do the electromagnetic case you simply write down the vector potential equation in curved space and you formally get a symmetric Green's function that looks just like the scalar with a direct part and a tail part:

$$g^{\nu\sigma}A_{\mu\cdot\nu\sigma} + R^{\nu}{}_{\mu}A_{\nu} = 0 \tag{2.6}$$

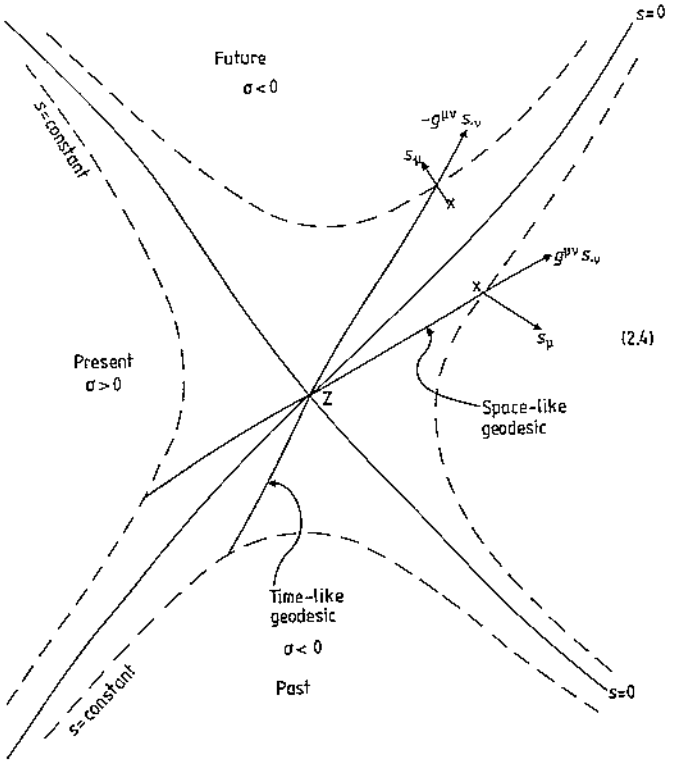


Figure 1 The geodetic structure of space–time which appears as figure 1 in DeWitt and Brehme (1960). Geodesics are drawn from a general event z to nearby events x.

$$\bar{G}_{\mu\alpha'} = \frac{1}{8\pi}\,[\underbrace{\Delta^{1/2}\bar{g}_{\mu\alpha'}\delta(\sigma)}_{\substack{\text{on light cone}\\ \text{bi-vector of}\\ \text{parallel}\\ \text{displacement}}} - \underbrace{v_{\mu\alpha'}\theta(-\sigma)}_{\substack{\text{inside light}\\ \text{cone}\\ \text{tail bi-vector}}}].$$

Here now you notice there are two indices on the Green's function and that these are evaluated in general at separate events, so again these are bi-tensors. The bi-vector of parallel displacement $\bar{g}_{\mu\alpha'}$ is very useful and comes up all the time. It transforms as the metric at each space–time point individually and can be used when you're looking at two tensors that are separated by a geodesic interval. The amplitude $v_{\mu\alpha'}$ in the symmetric Green's function physically tells you about the tails on the electromagnetic wave.

The final physical answer, about whether the charged particle follows the geodesic or not, is stated very clearly by DeWitt and Brehme (1960):

> The charged particle tries its best to satisfy the equivalence principle, and on a local basis, in fact, does so. [That is, the deviation caused by the electric field scattering off the curvature is non-local.] In the absence of an externally applied electromagnetic field, the motion of the particle deviates from geodetic motion only because of the unavoidable tail in the propagation function for the electromagnetic field . . . Physically, the tail may be pictured as arising from a sort of scatter process, with the 'bumps' in space–time playing the role of scatterers.
>
> DeWitt and Brehme (1960)

There is a very elegant way of seeing that precisely. DeWitt and Brehme (1960) derived the Ponderomotive equation, the classical equation for a charged particle, in the presence of curvature:

$$m\ddot{z}^{\alpha} = \underbrace{\frac{e}{c}F^{\alpha}{}_{\beta}\dot{z}^{\beta}}_{\substack{\text{Lorentz Force}\\ \text{local}}} + \underbrace{\frac{2}{3}\frac{e^2}{c^3}\left(\dddot{z}^{\alpha} - \frac{1}{c^2}\dot{z}^{\alpha}\ddot{z}^{2}\right)}_{\substack{\text{classical radiation damping}\\ \text{local}}} + \underbrace{\frac{e^2}{c}\dot{z}^{\beta}\int_{-\infty}^{\tau} g^{\alpha\delta}(v_{\delta\gamma'\cdot\beta} - v_{\beta\gamma'\cdot\delta})\dot{z}^{\gamma'}\,d\tau'}_{\substack{\text{tail induced radiation damping}\\ \text{non-local}}}. \tag{2.7}$$

The amazing thing is that it comes out just as Dirac discovered: the acceleration of the charged particle is just equal to the Lorentz force, if there is an externally applied electromagnetic field, and the classical radiation damping term. The third derivative term is the one that gives problems with runaway solutions in flat space. But in addition to these, there is the term in which you notice the curl of the function v, i.e. the tail bi-tensor $v_{\mu\alpha'}$ which was in the Green's function in equation (2.6). Its curl acts as the source of the deviation of the electron's worldline from being a geodesic. And this is integrated over the entire past history of the charged particle's trajectory.

Another way of seeing what goes on here is to assume that there's no external field ($F_{\alpha\beta} = 0$) and we're in flat space, so that the tail goes away, then what you'd get is that the particle falls along a geodesic, as you'd expect in flat space.

No EM field + flat space	$\rightarrow \ddot{z}^{\alpha} = 0$	geodesic motion	
No EM field + curved space	$\rightarrow \ddot{z}^{\alpha} \neq 0$	non-geodesic motion electrogravitic brems.	(2.8)

However, if you go to curved space but leave the electromagnetic field off, then what you see is that the tail term now forces the particle away from a geodesic and causes radiation which finally feeds back as damping. Again, this is an extremely elegant solution to this problem.

Now of course a person doesn't have an impact on a field just by doing a calculation. He really only has an impact if he provides something in that calculation that allows later scientists to do new calculations. I want to emphasize this point

repeatedly in discussing Bryce's work. The calculation described above was a thought experiment, not in gravitational radiation, but in electromagnetic radiation. But what this formalism allowed was for Kip Thorne and his co-workers, in the late 70s, to calculate the full *gravitational* scattering problem of two massive particles. They included the bodies' finite gravitational self-energies and obtained an answer to the question of what kind of gravitational bremsstrahlung occurs for arbitrary mass ratios and arbitrary velocities. I include here a couple of quotes from the classic paper of Kip and his colleagues, which is one of the most beautiful calculations in relativity theory we've had in quite a while.

The first paper of the series is by Thorne and Kovács (1977). They say:

> The 'guts' of this paper, in terms of complex calculations, reside in the Green's function manipulations of Section IVc. Our particular way of handling the Green's functions is motivated in Appendix A, and has been influenced by the following papers: DeWitt and Brehme (1960), . . . Robaschik (1963), . . . John (1973a, b), Bird (1974), and especially Peters (1966).
>
> Thorne and Kovács (1977)

In the second paper in that series by Crowley and Thorne (1977), they say:

> Central to the development of their formalism is the determination of an appropriate approximation to the well-known Green's function for the scalar wave equation in curved space–time developed by DeWitt and Brehme (1960). The purpose of this paper is threefold: first, to show that an approximate form of the exact Green's function developed previously by DeWitt and DeWitt (1964) has exactly the same mathematical content (within the constraints of the approximations used) as that constructed by Thorne and Kovács.
>
> Crowley and Thorne (1977)

It is clear that Kip and his co-workers were only able to carry out this classic problem in general relativity by basing it on the tool which had been developed some 17 years earlier by Bryce in his calculation.

I will discuss briefly several other topics in gravitational radiation. If you pick up Misner *et al.* (1973) (MTW) and turn to the chapter on gravitational radiation, you'll find a number of references to Bryce, in particular, the famous Stanford lecture series (DeWitt 1971). In those lectures, I remember Bryce developing the theory of gravitational radiation, one reason being William Fairbank's experiments with the bars at Stanford. Bryce obviously wanted to describe the complete theory of how one detects gravitational radiation.

One of the important things one wants is to evaluate the energy content of a gravitational wave as it passes by some apparatus. Since that energy is not well defined locally, you have to average it over several wavelengths. Isaacson, using what was called Brill–Hartle averaging, developed the Isaacson formalism for averaging over gravitational waves and then allowing those waves to act back as the source of the gravitational field. If you look in MTW (equation (35.73′)) for the formula for how you do that averaging,

$$\langle E_{\alpha\beta}(x)\rangle \equiv \int g_{\alpha}{}^{\mu'}(x,x')g_{\beta}{}^{\nu'}(x,x')E_{\mu'\nu'}(x')f(x,x')\sqrt{-g(x')}\,\mathrm{d}^4x' \tag{2.9}$$

you'll find that it requires the bi-vectors of parallel displacement $g_{\alpha}{}^{\mu'}$; metrics which are defined with their two indices at different events. MTW refer to Bryce's work in the paper with Brehme for the formalism of the bi-vectors.

The important use of Isaacson formalism is to show that the background gravitational field can have as its source ripples of the gravitational field averaged over large numbers of wavelengths.

$$G_{\mu\nu}^{(\mathrm{B})} = 8\pi T_{\mu\nu}^{(\mathrm{GW})} \tag{2.10}$$

Wheeler's geons are an extreme example, where the gravitational field provides its own mass energy which then traps the high-frequency gravitational waves into going around in a circle; it's a bootstrap approach.

You can derive equation (2.10) starting with the Lagrangian for the linearized gravitational field and by performing appropriate variations. Bryce did this derivation in his Stanford notes (1971). Now MTW suggest doing this as exercise 35.20, however they add: 'WARNING: the amount of algebra in this exercise is enormous'. And any of us who have sat through Bryce's derivations realize that. What MTW mean is, if you're not Bryce DeWitt, go on to the next exercise.

Let me turn finally to one last application of the Isaacson formalism. When Bryce was at Stanford, MTW were in the process of finalizing their book and they wanted to have the formula for the amount of angular momentum that is carried off by a gravitational wave. Now that's a very delicate and very, very long calculation. Peters in 1964 had gone through the calculation, but there were questions about some factors involved and whether some terms were correct. I remember Kip asking Bryce whether he'd ever worked that out and Bryce said something like, 'Well, it can't be *that* difficult—it's *only* linearized theory'. And so, some weeks of lecture later we got to the end of the calculation. One can take the reduced quadrupole moment I_{km} and by taking second and third time derivatives in appropriate combinations get exactly the density of angular momentum radiated. Then, by integrating over a two sphere find out the amount of angular momentum carried away from an isolated system by gravitational radiation (MTW equations (36.24) and (36.25)):

$$\begin{aligned} \mathrm{d}J_i/\mathrm{d}t &= -\int J_i r^2\,\mathrm{d}\Omega \\ J_i &= \frac{1}{8\pi r^2}\,\varepsilon_i{}^{jk}\langle -6n_j\ddot{I}_{km}\dddot{I}_{mp}n_p + 9n_j\ddot{I}_{km}n_m n_p\dddot{I}_{pq}n_q\rangle \end{aligned} \tag{2.11}$$

where the brackets $\langle\ \rangle$ are as defined in equation (2.9).

Now you notice that the above equations (2.9)–(2.11) are fundamental equations. Any process involving gravitational radiation from an isolated system will use these a thousand years from now. That's a trademark of Bryce's—he goes right to the heart of the matter and when he's finished with a problem, it's finished.

3. SUPERSPACE

I want to move on to an area which I think is not finished yet and is quite difficult, and that's the area of superspace. Again, you'll begin to notice a pattern here. In each of these major fields of classical gravity, there are usually no more than two papers by Bryce. And yet out of those a good fraction of that entire subfield usually comes. Bryce is not prolific in the number of his publications, and I think some of us might remember that these days. But the two papers he did write were very influential in this field. The first paper was in the first volume of the trilogy, *The Quantum Theory of Gravity* (DeWitt 1967), which dealt with the canonical theory. Chris Isham (in this volume) will describe the physics in the other two volumes and perhaps the quantum part of this one as well.

As John Wheeler, who really started the idea of superspace, has emphasized so many times over the last twenty years, the dynamics of the gravitational field is carried in the 3-geometry. We'll come back to this when we deal with numerical relativity below. The geometry of 3-space, the sequence of those geometries changing in time, is what makes up a space–time. And if you want to look for the state of the gravitational field, you look to the 3-geometry. Well, that realization was a long time in coming, and there were many, many people who worked on it. I'll give you a brief list of some of the names which came up while I was preparing this paper: Dirac, Pirani, Schild, Bergmann, Arnowitt, Deser, Misner, Higgs, Wheeler, DeWitt . . . .

Things were quite confused until, I would say, 1955 to 1960. There were questions being asked both from the quantum side, such as what is the appropriate state functional for quantum gravity, and also from the classical side, such as how to sort out the dynamics of gravitation. Bryce contributed to this sorting out process in a Stevens Institute of Technology talk in 1958 (unpublished). Bryce did some very important calculations which showed that the state functional had to be independent of the time–time and time–space components of the 4-metric. We would now say that the lapse and shift are not relevant, they are coordinate setting devices, and that the dynamics lies in the 3-metric. In particular, as Bryce and Wheeler kept emphasizing, it is not in the 3-metric either, but it is in the 3-geometry represented by that 3-metric.

Now as I said, Wheeler, in these wonderful articles of his for so many years, kept telling us to concentrate on the 3-geometry which had to have a place to live, superspace—the space of all possible 3-geometries. The 1963 Les Houches lectures was where superspace first started, but I believe that it first really caught fire in the Battelle Rencontres volume in 1968. In that volume, as John Wheeler (1968) begins to introduce the concept of superspace, he says, 'One climbs up to the concept of geometry only to find a new height beyond: superspace.' The only references Wheeler gives for superspace are Wheeler's (1963) own lectures at Les Houches and DeWitt's (1967) quantum theory of gravity article. So it's clear that by 1968, when the concept of superspace really came into its own, that John Wheeler certainly regarded Bryce as a sort of co-architect of that space. I remind you that one elegant formal way of defining superspace is simply (DeWitt 1970, equation (3*b*)):

$$S(M) \equiv \frac{\mathrm{Riem}(M)}{\mathrm{Diff}(M)}. \tag{3.1}$$

If you take a compact, Hausdorff 3-manifold M, such as a closed cosmology, and you imagine all possible 3-dimensional positive-definite metrics on it, you obtain the space Riem (M). Then you mod out the diffeomorphisms, Diff(M), the possible coordinate transformations or relabeling of points in the same 3-geometry, which leaves you with the points in superspace $S(M)$. Arthur Fischer (1970) gave a beautiful mathematical analysis of the structure of superspace and showed that it really wasn't a manifold, it was a stratified union of manifolds. This problem came up largely because of the existence of isometries in space–times which provided boundaries, from where trajectories in superspace could reflect. And so Bryce, in his article, suggested extending superspace, as defined here, to 'extended superspace', which would smooth out the kinks and curves in superspace so that one could talk about those more fruitfully.

Superspace, again, is the set of 3-geometries. Imagine you start with some 3-geometry, then a little bit later it's slightly bent and that's another point in superspace, and the next time it's somewhere else, and so you make a curve through superspace and that represents a space–time. One needs an equation for the dynamics in superspace and this is the Hamilton–Jacobi equation. The reason that, again, we arrive back at the Hamilton–Jacobi equation, something that Bryce has always stressed from 1957 on, is that the Hamilton–Jacobi equation is the proper formalism in which one is able to simultaneously do classical and quantum physics. This is because one can go back and forth very easily using the Hamilton–Jacobi functional. This allows WKB approximations to be done very nicely here. The equation one wants is for the Hamilton–Jacobi functional W in superspace (DeWitt 1970, equation (15)):

$$G_{ijkl}\left(\frac{\delta W}{\delta \gamma_{ij}}\right)\left(\frac{\delta W}{\delta \gamma_{kl}}\right) - \gamma^{1/2}\,{}^{(3)}R = 0. \tag{3.2}$$

There is a set of coefficients G_{ijkl} coupling two first-order variations of the functional W with respect to the 3-metric γ_{ij}, minus the scalar Ricci 3-curvature. Peres, in 1962, was the one who first put forth this formula. There is a companion equation (DeWitt 1970, equation (17)):

$$\left(\frac{\delta W}{\delta \gamma_{ij}}\right)_{.j} = 0 \tag{3.3}$$

where the 'dot' denotes covariant derivation with respect to the 3-metric. One can easily see that equations (3.2) and (3.3) are just formally the Hamiltonian and the supermomentum constraints in general relativity. One of the key ideas behind superspace is that one can get the dynamics of general relativity just out of the constraints. A pretty derivation in Bryce's quantum theory of gravity article starts with the formally-defined equations (3.2) and (3.3) and in a couple of lines recovers the familiar Einstein equation in the $3+1$ language, such as are used in numerical relativity; that is, you end up with equations that say that the second time derivative of the 3-metric is equal to a bunch of forcing terms.

The quantum version of equation (3.2) has been referred to by Kuchar as the 'Wheeler–DeWitt equation.' So Bryce's name is intimately hooked together with this

equation both in the classical regime and in the quantum regime. The set of coefficients G_{ikjl} was considered to be just a set of coefficients until Bryce's (1967) quantum theory of gravity article. In that article he pointed out that G_{ikjl} actually is the supermetric on superspace

$$G_{ikjl} = \frac{1}{2\gamma^{\frac{1}{2}}}(\gamma_{ik}\gamma_{jl} + \gamma_{il}\gamma_{jk} - \gamma_{ij}\gamma_{kl}). \tag{3.4}$$

Because of its construction, it is an indefinite metric with one minus and five pluses. Thus the 3-metrics enter into the definition of the supermetric on superspace. Now, pointing out that G_{ijkl} is a supermetric may seem like a minor detail, until one reads from the beautiful article by Charles Misner (1972) in the Festschrift for John Wheeler. Misner wrote a review article on minisuperspace, in particular the Hamiltonian cosmologies that Misner and many others worked on in the late 60s and early 70s. While giving examples of the various 'toy models' using anisotropic homogeneous cosmologies that they had worked on, he remarks that they kept finding that the metric on superspace continually arose. Then Misner notes:

> Of course we had known of DeWitt's work pointing out the existence of a metric in superspace, but paid insufficient attention to the essential role this metric plays until we repeatedly found that it was the clue to solving specific simple examples in quantum cosmology.
>
> Misner (1972)

Another example of Bryce's deep insight into superspace is the beautiful result which Bryce found: one can consider space–times as geodesics in superspace, particularly if the extended superspace is used. That is, the geodesic equation on superspace gives a curve in superspace which represents a space–time. If it's a geodesic in superspace with the right parameterization, it turns out that the resulting space–time solves Einstein's equations, which is an amazing result. Then Bryce asked the reverse question: how is a given space–time represented in superspace? If you imagine taking a space–time and time-slicing it one way, you get a set of 3-geometries; if you time-slice it another way, using a different lapse function, you get a different sequence of 3-geometries. And yet glued together they are the same space–time. But they are different sequences of 3-geometries and therefore they are different curves in superspace. Therefore Bryce pointed out that one must consider, because of the many fingered time, a sheaf of geodesics in superspace as representing one space–time.

Again, Misner (1972) states the importance of

> ... the beautiful theorem [due to DeWitt] that solutions of Einstein's equations are geodesics in superspace which conveys succinctly the main thrust of all nonquantum study of superspace.
>
> Misner (1972)

Misner goes on to sum up, at the end of his article, the history of superspace. He said that:

> The next big step in seeing Einstein's theory as a superspace structure was due to Bergmann, who pointed out that Peres was using approaches to the initial value problem of general relativity which allowed one to write the Hamilton–Jacobi equation for general relativity [equation (3.2)]. DeWitt, in private communications, made Wheeler's group aware of the importance of the Einstein–Hamilton–Jacobi equation and pointed out that it provided a basis for defining a metric in superspace. Published results [including Bryce's] appeared in due course as these ideas developed. The word 'superspace' seems to appear first in Wheeler's 1963 Les Houches lectures, but its formal baptism is not evident until the 1967 *Battelle Rencontres.*
>
> Misner (1972)

Misner then finally ends this whole paper on minisuperspace by saying

> The first minisuperspace calculations were DeWitt's for the Robinson–Walker universes.
>
> Misner (1972)

I think it's clear that all the way through the study of superspace, which is one of the major areas of classical gravity, Bryce has shown the way over and over again. But more to the point, I think that it speaks extremely well of his work that people as bright as Charles Misner and as productive as John Wheeler felt compelled to continually point out how Bryce had made such important contributions to their understanding; they were some of the main pioneers of the subject itself.

4. NUMERICAL RELATIVITY

In the last section we saw how the dynamics of gravity can be represented as a formal structure in superspace; it's a completely formal structure. I think Bryce was never happy with just a formal structure unless it was also good for calculating particular examples. This is what led him to invent numerical relativity.

Numerical Relativity is a method for calculating, in principle, an arbitrary solution of Einstein's equations. Again there are only two published works by DeWitt on the subject, one on the maximal slicing of a black hole by Bryce and co-workers (Estabrook *et al* 1973) and one on the collision of two black holes (Smarr *et al* 1976). There is also an unpublished transcript, one which I have always found a tremendous source of inspiration, which contains Bryce's remarks at the Chapel Hill Conference on Gravitation in 1957 (DeWitt 1957c). To understand these remarks one must remember that numerical relativity requires solving for both the evolution of the matter and of the gravitational field. I will start with the former topic and show how it influenced the latter.

At Jim Wilson's Festschrift meeting held at the University of Illinois in October 1982, Bryce discussed his early 1950s days at Livermore. Most people don't realize that Bryce was one of the inventors of multidimensional Lagrangian hydrodynamics. In Lagrangian hydrodynamics, one fixes the coordinate grid on the particles of the fluid, and as the fluid moves around, it carries the grid with it, warping it as it goes.

Then one finite-differences the partial differential equations of hydrodynamics on that moving grid. Of course, the squares get bent into very peculiar diamonds after a while, and this causes a lot of trouble. In one dimension it's actually a pretty good method because, for example, in a spherical star the Lagrangian markers are just the mass shells. As the star becomes compressed, the markers get closer to each other, or if the star blows off a supernova envelope, the markers can get far apart, but they all stay in order; just the distance between the grid points changes.

By 1953, one-dimensional Lagrangian hydrodynamics had been developed quite well, but how to go successfully to higher dimensions was a mystery. Because Bryce knew about Jacobians and general coordinate transformations, which were quite foreign to most people at the lab, he was able immediately to write down the generalization for two dimensions (or n dimensions). Edward Teller, who was running the lab at that time, got so enthusiastic he immediately called a lab-wide meeting and Bryce became the instant lab expert on Lagrangian hydrodynamics.

The reason I relate this story is that until I prepared this lecture I didn't appreciate how much the mind-set of doing Lagrangian hydrodynamics had affected numerical relativity. If you go back to the summary of remarks made at the Chapel Hill conference in 1957, you find the following:

> Bryce DeWitt pointed out some difficulties encountered in high-speed computational techniques. 'Singularities are, of course, difficult to handle. Secondly, any non-linear hydrodynamic calculations are always done in so-called Lagrangian coordinates, so that the mesh points move with the material instead of being fixed in space. Similar problems would arise in applying computers to gravitational radiation, since you don't want the radiation to move quickly out of the range of your computer'.
>
> DeWitt (1957c)

An example of Bryce's concern is that if you are trying to solve a star collapse which bounces, in which you must follow the star inward for several orders of magnitude in radius, all your grid points are down in the center of the star when it bounces. Since gravitational radiation goes out at the speed of light, there are no grid points left outside of the star for the radiation to propagate on. Now Bryce saw that clearly in 1957—I find that amazing, because the conceptual problems in simultaneously worrying about

(1) the computer algorithm
(2) the structure of space–time
(3) the coordinate system

were things really sorted out only in the last ten years. And yet the problem was crystal clear thirty years ago to Bryce.

Although Bryce had these worries about Lagrangian hydrodynamics, people were even more afraid of Eulerian hydrodynamics—where you leave the coordinates fixed in space and then let the fluid move through them. Therefore, almost all of the early attempts to do numerical relativity were Lagrangian (for a review see Smarr *et al* 1980): all of the great work of Taub, in which he developed the formulae of general relativity in a Lagrangian coordinate system; the early codes of Misner and Sharp

and of May and White, which were spherical star collapse codes; and Wilson's early work on supernovae. Until Wilson came along in the early 70s and started doing Eulerian hydrodynamics, there was this enormous mental roadblock of how one could calculate non-spherical general relativity, which is what you needed to get gravitational radiation, without having to use Lagrangian techniques for the hydrodynamics, in which case you were going to get all snarled up in your coordinates.

Bryce DeWitt and Charles Misner very clearly understood what solving Einstein's equations was going to be like in 1957, some twenty years before the first successful two-dimensional calculation. Again, from the Chapel Hill conference:

> Bryce DeWitt asked if the Cauchy problem is now understood sufficiently to be put on an electronic computer for actual calculation. 'Do we know enough about constraints and initial conditions to do this at least for certain symmetrical cases?'
>
> DeWitt (1972)

Charles Misner, who always seems to be where Bryce is, answered that he happened to have some initial conditions for two throats and

> '. . . these can be interpreted as two particles which are non-singular, or they could be thought of as the kind of $1/r$ type singularities which one ordinarily thinks of in gravitational theory. These partial differential equations, although very difficult, can then in principle be put on a computer'. Misner thinks that one can now give initial conditions so that one would expect to get gravitational radiation, and computers could be used for this.
>
> DeWitt (1957c)

Those two Einstein–Rosen bridges were, in fact, put on the computer in the early 60s, when the general relativistic two-body problem was first attempted numerically on a computer by Hahn and Lindquist (1964). So it was really quite rapidly from these remarks in 1957 that we moved to the beginnings of two-dimensional numerical relativity. Unfortunately what was lacking at that time was the concept of black holes. We hadn't yet understood that these weren't just throats, but that they could be considered as physical objects—black holes. The work of Wheeler, Hawking, and of other people who elucidated the concept of black holes, allowed Bryce to start the project back up again in 1969 as 'the two black hole collision' problem. Bryce tried to get students to solve it to actually see what would happen when the two holes collided and coalesced.

Bryce started out with Frank Zerilli and then with Andrej Cadez on this problem in 1969. After the Einstein equations were put on the computer, DeWitt and Cadez ran immediately into the problem of the lapse—that the time-slicing was absolutely crucial. Now Bryce understood this, as you have seen, early on; this is again the idea that the many fingered time is central to general relativity. What Bryce decided was that we had to come up with a better way of slicing space–time. Figure 2 shows the Kruskal diagram from the 1972 article on maximally slicing a single black hole (Estabrook *et al* 1973). If you would have used what people first tried, namely

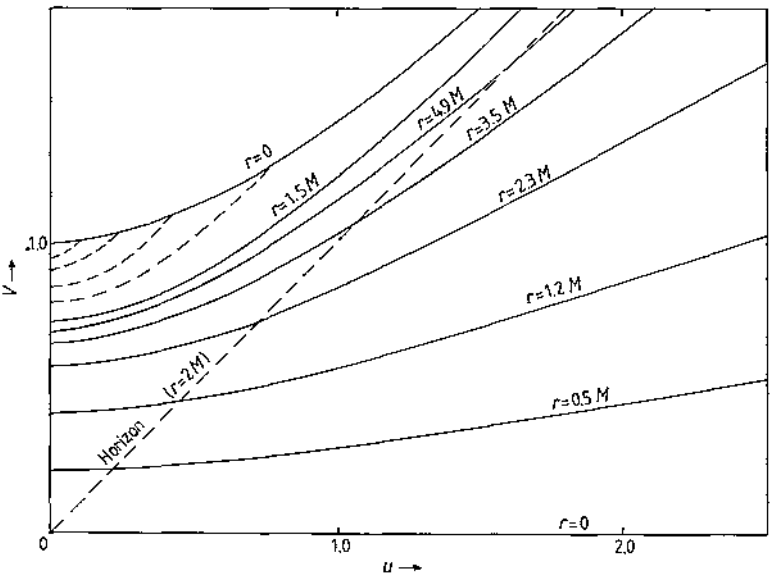


Figure 2 The Kruskal diagram for Schwarzschild space–time with a maximal time-slicing. This appeared as figure 1 in Estabrook *et al* (1973). The maximal time slices are labeled by their asymptotic Schwarzschild time coordinate values. Notice that at $t \to \infty$, the slices wrap up around $r = 3M/2$, thus avoiding the singularity at $r = 0$.

geodesic slicing, you would start off from the $t = 0$ slice and you would move forward equal increments of proper time until your slices hit the singularity in a time of the order of 3 M. If you were trying to let a star collapse, and let gravity waves be generated, and propagate out to the weak field zone, which might take 50 or 100 M, of course you'd be out of luck.

So Bryce invented the idea of using maximal time-slicing on the computer. Now Lichnerowicz already, during the War in 1944, had pointed out that one could use maximal slicing as a coordinate condition and that this would be like having an incompressible fluid for coordinates. But Bryce was the first one to really decide this was what we needed practically to build space–times. He could forsee what has now come to be known as singularity avoidance, the most important property of maximal slicing. What happens when you use maximal slicing is that as the slicing begins to approach the singularity at $r = 0$, the lapse function falls exponentially fast to zero in the strong field region. This allows the slices to wrap up around the singularity while always staying a finite distance away from it. Bryce used simple reasoning to tell us why this singularity avoidance occurs. He would tell his students: 'It's because the slice is maximal; maximal means maximal volume, and if you try to get near a singularity, the volume's going to zero, so the slice avoids that region'. Eardley and I, as well as Choquet-Bruhat, Marsden, Tipler and York, were able to prove theorems later on, which demonstrated in detail how this happens.

Steve Christensen, Elaine Tsiang, and I were young and impressionable graduate students at the time and although it sounded crazy to us, we needed a PhD so we started working on it. I think a lot of people believe that Bryce had Steve, Elaine and me slaving away on writing the codes while he would just stand up and say that maximal slicing is the way you do it. I'd like to show you that that is not the case, by publishing an artifact (figure 3). This is from the maximal hypersurface code for slicing the Schwarzschild black hole, done at the University of Texas in 1972. This is the original FORTRAN coding sheet; you can recognize the handwriting as being Bryce's because it's so neat. I remember Bryce was always infuriated with how stupid FORTRAN was as a language, in fact many people still feel that way today. So you'll notice that there's some very important comments: 'adjustable dimension subversion program'. This was to get around the fact that you might want to change your array sizes and didn't want to have to rewrite the whole program every time you did it. And further down the page: 'main program disguised as a subroutine'. This is another example of DeWitt extended FORTRAN.

Just as Bryce, Elaine, Steve and I had finished our numerical calculation, we received a letter from Frank Estabrook and Hugo Wahlquist of JPL. Independently from us, they had solved the maximal slicing of one black hole analytically! We were all disappointed because we didn't have the discovery to ourselves, but happy because we could check that we had, in fact, correctly solved the problem numerically. Many scientists, faced with this problem, would have rushed to publish their own work. This never seemed to occur to Bryce. He graciously asked Frank and Hugo to join us in publishing jointly; furthermore, he made them first authors. This professional style was a model for many of the young people in relativity and shows the true measure of Bryce.

The maximal slicing worked for one black hole and has now become the industry-wide standard for doing numerical relativity; using it Eppley and I were able to finish the two black hole collision. One of the things I always wondered about was why Bryce chose the two black hole collision problem as the GR laboratory to work in—why not a star collapse? I believe that his worries about how you were going to get around Lagrangian hydrodynamics might have led him to think: 'Well, fortunately in general relativity you don't need matter as a source for the field, so let's do a vacuum space–time and we'll worry about the matter later'. In fact, I think that was a brilliant insight, because there was plenty of work just to understand how to solve Einstein equations. In the meantime, Jim Wilson came along and developed his beautiful techniques for doing Eulerian hydrodynamics. Finally, these two methods were fused together so that today we have codes capable of solving the full matter plus Einstein's equations. I must say that program was very much influenced by Bryce, who always kept a very strong Texas–Livermore connection.

5. EXPERIMENTAL RELATIVITY

Of course, it doesn't do any good to study the dynamics of general relativity unless ultimately relativistic gravity really exists. Now Newton knew weak gravity existed

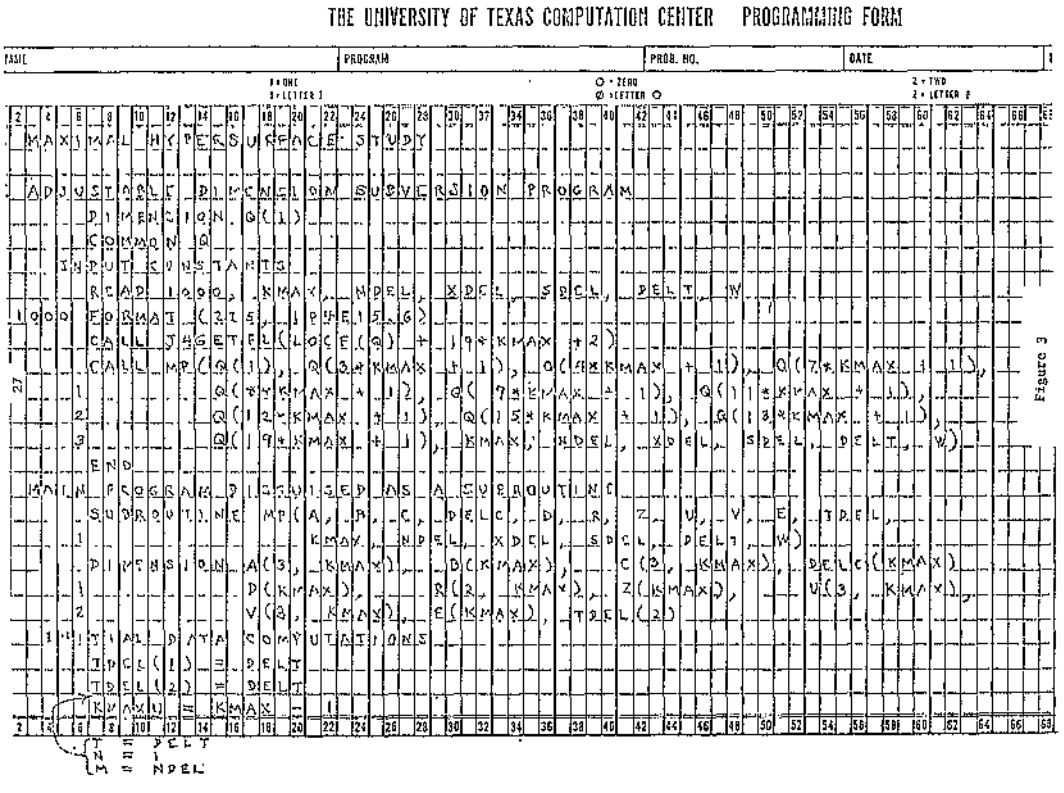


Figure 3 The original hand-coding sheet for the maximal slicing of one black hole. This artifact demonstrates that Bryce wrote the first FORTRAN code for the vacuum numerical relativity.

and he was able to make some very nice calculations on tides; as a result of which the British took over the world. So I've often wondered what Bryce's idea was about 'applied gravity'. I want to share with you Bryce's attitude about experimental relativity from 1957. It's in the Journal of Astronautical Sciences, from an address that Bryce gave at a meeting in 1956.

> Those of you who read newspapers will be aware that there has been a recent renewed upsurge of interest in the phenomenon of gravitation. What you may not be aware of is that the picture given by the press is by and large incorrect. The ensemble of news items to date tends to give the impression that there is a large scale program feverishly under way to 'harness the force of gravity'. This is simply not so, for the simple reason that not the slightest plausible method is known for developing practical applications of the phenomenon of gravitation . . .
>
> DeWitt (1957b)

Nonetheless, Bryce did go on to make a number of attempts to measure gravity. Bryce was always interested with how you would go about measuring something. He would always start by writing down a Lagrangian for a given apparatus. In fact, there's one famous mimeographed homework problem that he gave out to us in which you were supposed to write down the Lagrangian for a little cart that had an electric motor on it that powered a belt that went over and drove a paddle wheel through a vat of viscous fluid (apparently this problem originated with Wheeler). Well, Bryce expected us to solve this as a homework problem.

The Lense–Thirring dragging-of-inertial-frames effect was a longtime love of Bryce's. In the 1957 Journal of Astronomical Sciences article, Bryce wrote:

> There are other effects which possibly could be measured in a satellite . . . For example, Einstein's theory predicts certain induction effects. Just like electrodynamics predicts induction effects . . . Suppose the satellite is in 'free fall', and suppose a very large, heavy disc or flywheel is turning around at a high speed inside. If you take a little test particle and let it fall towards the disc, the rate of fall will be very slow, because these are relatively small objects, and the attraction of gravity is not very large, so you have a lot of time over which you can make your experiment. Now, after a while you will find that instead of falling directly toward the center of the disc, the test particle will be dragged along a little bit in the direction of the rotation of the disc. This is just an inductive effect. If the disc were motionless and not rotating, the particle would fall straight toward the center. These are things that would be very interesting to measure some day.
>
> DeWitt (1957b)

Ten years later in 1966, Bryce wrote a Physical Review Letter on *Superconductors and Gravitational Drag.* (DeWitt 1966) In it he proposed a new effect in superconductors of the Lense–Thirring term. Finally, in 1972 at Stanford (DeWitt 1972), he lectured on the detailed analysis of the Schiff gyroscope experiment which was to be orbited

around the earth. Hopefully, that space experiment will be flown before the 30th year anniversary of Bryce's remarks above.

The other major experiment Bryce was involved in was the Texas Mauritania Eclipse Expedition (DeWitt *et al* 1974, DeWitt 1975, 1979, Texas Mauritanian Eclipse Team 1976) of which he was the leader. This was a grand effort to update the original 1919 observations of Sir Arthur Eddington. It was a team effort with an enormous amount of work and endurance required. Because of a dust storm during the eclipse, the accuracy was not as great as was hoped for. Nonetheless, it was a great adventure and a remarkable effort by a man known for his work in quantum gravity!

6. DYNAMIC LEADER

I come finally to the last dynamical area, namely that of Bryce himself. I think anyone who worked with Bryce is aware of the tremendous leadership quality that he has. This is of course shown up in the fact that he has been Director of the Center of Relativity at Austin since 1972; he was the Director of three of the Les Houches programs that Cécile has run for many years—the 1963, 1972, and 1983 Les Houches summer schools; he was group leader twice at Santa Barbara in 1980 and 1981; and leader of the eclipse expedition in 1973. In addition, he has always been a leader in the field of quantum gravity.

But those are just the official recognitions of what is actually the character of Bryce. Bryce fears nothing, and that attitude pervades his everyday outlook; he's always very cheerful because there's nothing he's going to come up against he can't deal with. That spirit pervades anyone who works with Bryce. I remember when we were trying to invent numerical relativity, I would come up against the situation where we didn't have any idea what was going on with the mathematical equation we were trying to solve, much less have any idea of what would be the numerical approach for it, and whether it would be stable or unstable. If you'd go to Bryce and say, 'Well, Bryce I don't know—it might be unstable, or it might not be accurate', Bryce would blow you out of the office and say, 'Don't bother me with these details. Look, these are equations, right? You can difference equations. Go difference them. Just do it!' I think that the idea of 'just do it!' comes through in everything that Bryce has ever attempted. This is the reason why many of us feel that he has had such an enormous impact on shaping not only our professional careers but our personal lives as well.

ACKNOWLEDGMENT

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