The Role of Gravitation in Physics

Report from the 1957 Chapel Hill Conference

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Cécile M. DeWitt and Dean Rickles (eds.)

Communicated by

Jürgen Renn, Alexander Blum and Peter Damerow

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Communicated by Jürgen Renn, Alexander Blum and Peter Damerow
Copyedited by Beatrice Gabriel

Editors:

Cécile DeWitt-Morette
Jane and Roland Blumberg Centennial Professor Emerita
Department of Physics
University of Texas at Austin

Dean Rickles
Senior Research Fellow
Unit for History and Philosophy of Science
University of Sydney

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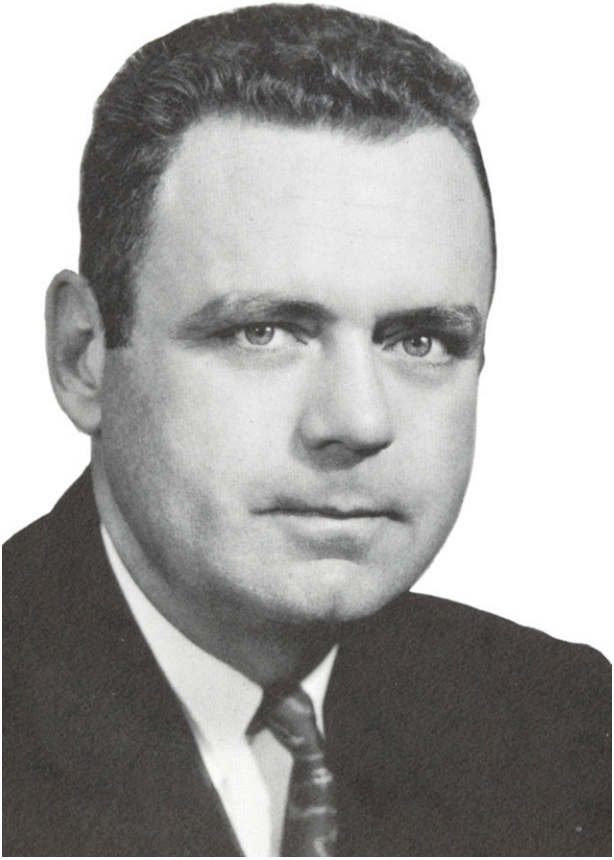
Dedicated to the memory of Bryce DeWitt



No one man lives long on this earth. It will take the concerted efforts of many men to pry forth one of the deepest and most obstinate, but one of the most important and potentially useful secrets of nature ... In this quest we are reaching for the stars, – and beyond.

Agnew H. Bahnson
The Stars Are Too High

Preface

This book contains the original report from the Conference on the Role of Gravitation in Physics, which took place at the University of North Carolina, Chapel Hill, over six days in 1957. The report was taken down by Cécile DeWitt and several other "reporters," as part of a conference funding agreement with the Wright Air Development Center, a U.S. Army (Air Force) funding body (the report's 'official' designation is: WADC Technical Report 57-216). Cécile DeWitt then edited the recorded material into its final form. The report, though publicly available as a government document, has not previously been published in book form, and there are not many copies of the report left in existence. Given the immense historical significance of the conference - giving gravitational research some much needed impetus at a time when it was in a state of dire neglect - we thought it was high time to produce a version of the report 'for the masses' as it were. The version presented here is almost entirely faithful to the original, and aside from the correction of a few spelling mistakes (and the possible *addition* of some entirely new typos!) features no substantive alterations or annotations. However, in order to make the document more navigable and more useful as a research tool, we have added an index (of both names and subjects) and also imposed a little more structure on the sessions, by setting some of the meatier contributions as chapters. This in no way interferes with the ordering, and simply amounts to the addition, in several places, of a title to the presentations and discussions that follow.

In addition to classic debates over cosmological models and the reality of gravitational waves, many of the still-pressing issues in quantum gravity were formulated in the discussion periods and interjections reproduced in the following pages. (Philosophers of physics might be interested to see Thomas Gold, p. 244, pressing those who assume that the gravitational field had to be quantized to *prove* it!) One can also find the roots of many current research programs in quantum gravity, as well as early intimations of what would become 'classic' thought experiments, offering some guidance to a subject without genuine experiments. We also find, fully staked out, three central approaches to the quantization of gravity: canonical (focusing on the observables and constraints of the *classical* theory[3]); the Feynman functional-integral approach; and the covariant perturbative approach. We also find what is, I believe, the first presentation (albeit very briefly: see p. 270) of Hugh Everett's relative-state interpretation of quantum mechanics - Feynman gives an explicitly 'many-worlds' characterization of Everett's approach (used, by Feynman, in fact, as a *reductio* of the interpretation).

As Cécile DeWitt notes in the foreword to the original report (also reproduced here), the document constitutes a somewhat incomplete representation of the actual proceedings in various ways, and does not amount to an *exact* transcription of all that went on at the conference.[4] However, the discussions that were captured are often very rich, and of such an interesting (and, I would say, still highly *relevant*) nature, that the report fully deserves its present resurrection if only to bring these discussions alone to a wider audience. However, the transcribed presentations also often reflect a research area on the cusp of various exciting discoveries, in astrophysics, cosmology, and quantum gravity. We are sure that both physicists and historians of physics will find much to interest them in the following pages.

By way of placing this report in the context of its time, an introductory chapter provides a brief account of how the conference came to be, for it is a rather remarkable story in itself. We then reproduce the original front matter from the report, followed by the report itself.

DR
Max Planck Institute for the History of Science, Berlin
September 2010

[3] As Peter Bergmann puts it, on p.192, 'once the classical problems are solved, quantization would be a "walk."'

[4] How could it? As Agnew Bahnson (about whom, see Chapter 1) noted in his post-conference report, there were some 57 half-hour tapes recorded in total.

Acknowledgements

We would like to express our thanks to all of those involved in the original Chapel Hill conference (or their estates) who very kindly gave their permission to reproduce their papers and comments, without which this book would not have been possible. We would also like to thank those involved in the smooth, speedy publication of the book, especially Beatrice Gabriel for her excellent proof reading.

DR is grateful to the the Dolph Briscoe Center for American History at the University of Texas at Austin for allowing him access to the Bryce DeWitt Papers; to Cécile DeWitt, firstly for allowing access to her own archive of papers and letters, from which much of the historical material in the introductory chapter was drawn, but also for allowing me to take part in this wonderful project; to Jürgen Renn, Max Planck Institute for the History of Science, and Don Salisbury, Austin College, for some much needed motivation, resulting in a significant speed up in the book's production; and to the Australian Research Council for their generous support of the History of Quantum Gravity Project (DP0984930) that the present book contributes to; and to the Max Planck Institute for the History of Science for hosting me as a visiting fellow during the books's completion. Finally, DR would like to thank his wife, Kirsty, and children, Sophie and Gaia, for putting up with his very many absences while this work was being completed.

Chapter 1
The Chapel Hill Conference in Context
Dean Rickles

The Conference on the Role of Gravitation in Physics was the (official) inaugural conference of the Institute of Field Physics [IOFP] which had only just been established at Chapel Hill, with Cécile and Bryce DeWitt at the helm.[1] The IOFP received its certificate of incorporation on September 7, 1955. In fact, it nearly had a very different name, "The Research Institute of the University of North Carolina", which, quite naturally, won the unanimous approval from the University (and Bryce DeWitt), but not, it transpires, the approval of the primary funder, Mr Agnew Bahnson. Bahnson was a wealthy North Carolinian industrialist with a passion for physics (especially gravitational); he made his fortune from industrial air conditioning systems.

The initial meeting between Bryce DeWitt and Bahnson took place on July 9, 1955, in Raleigh, N.C. They were joined by Clifford Beck, who was head of physics at State College in Raleigh - Bahnson originally planned to have the IOFP at State College since they had a nuclear reactor there, which, as Bryce DeWitt puts it, Bahnson felt "might be useful for anti-gravity" [3]. Curiously, at the same time, the Glenn L. Martin Company (now Lockheed-Martin) was setting up its own research institute that would do work for the U.S. National Defense Service[2] though of a

[1]An initial 'get to know each other' meeting of the IOFP was held June 8-10, 1956, at Roaring Gap in North Carolina, where Bahnson had a summer house. This was open to all members of the IOFP and a few select others, including Freeman Dyson and Lothar Nordheim, potential funders and a reporter from the *Winston-Salem Journal & Sentinel*. As Bahnson put it, in his "4th Memorandum" (of June 20, 1956) the purpose of the meeting was "to introduce members of the Institute and their guests to Mr and Mrs DeWitt" and "to define more clearly" the problems to be dealt with at the IOFP (with gravity as "the focal point of interest" (letter from Cécile DeWitt's own archive; henceforth, unless otherwise specified, references will be to documents contained in this archive).

[2]This would become RIAS, or the Research Institute for Advanced Study. It is possible that DeWitt's meeting was with the director of RIAS, Welcome Bender, who was in charge of recruitment, and who also attended the Roaring Gap conference on behalf

rather different sort, industry sponsored, and not ensconced in a university. DeWitt had a meeting with its vice-president, George Trimble (or Bender - see footnote 2), to discuss a potential project involving gravitational physics research (more on this below). DeWitt flew to his meeting with Bahnson immediately after visiting the Glenn Martin Company in Baltimore - indeed, the airplane was owned by a friend of Bahnson's, one Earl Slick of *Slick Airways*!

Bahnson had originally written to DeWitt on May 30, 1955. Just prior to this, DeWitt had already also been in correspondence with Roger Babson, another wealthy industrialist with a passion for gravity research. Babson had established the Gravity Research Foundation (GRF) in Salem, and had established an essay competition (governed by George Rideout) "for the best two thousand word essays on the possibilities of discovering some partial insulator, reflector or absorber of gravity waves" ([2], p. 344)! DeWitt won first prize in this competition (in 1953) with an essay dismissing the whole idea; or, as he put it: "[E]ssentially giving them hell for such a stupid - the way it had been phrased in those early years" [3]. DeWitt wrote the essay in a single evening: "... the quickest $1000 I ever earned!" (*ibid*).[3] Given that the essay led to the original exchange between Rideout and Bahnson over Bryce[4], it seems that this single night's work might in fact have earned him rather more than $1000!

Babson clearly saw in DeWitt one who could lift the respectability of the GRF. Indeed, this seemed to be the case, for whereas the prize was previously avoided by 'serious' physicists, after DeWitt won, the floodgates opened. The next year, 1954, saw Arnowitt and Deser take first prize.[5]

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of the Glenn Martin Company. However, there certainly was correspondence between DeWitt and Trimble.

[3]In a section entitled 'Purposes, Objects and Powers' of the document of incorporation of the Gravity Research Foundation formed by Babson, Clause (1) states that the purpose of the corporation is to "Observe the phenomena of nature and encourage, promote and support investigations in search of underlying knowledge of these phenomena. Conduct theoretical and experimental studies to discover the laws which affect them and evolve new technological concepts for the improvement and welfare of mankind" (Bryce DeWitt Archives, Center for American History, University of Texas at Austin). Babson had a curious view of gravity as a kind of 'natural evil' (a "dragon"), caused by his sister's and grandson's drowning by, as he saw it, the downward pull of gravity - see [1].

[4]Note that Bryce concluded his GRF essay with the words "External stimuli will be urgently needed in the near future to encourage young physicists to embark upon gravitational research in spite of the odds," which clearly resonated with both Babson and Bahnson.

[5]Much to the chagrin of Robert Oppenheimer who was supervising both of them at the Princeton Institute for Advanced Study. Oppenheimer thought that entering the

The GRF could never really gain the prestige it so desired. Babson held too much control over what lines of research were investigated. Since he was no scientist, these tended to be crankish - in one GRF bulletin the biblical miracle of Jesus walking on water was offered up as evidence in the possibility of antigravity shields, as was the ability of angels to defy gravity! The GRF stood no chance (at least not in this form). Martin Gardner famously mocked Babson's own gravity ideas in an article entitled "Sir Isaac Babson".[6]. Gardner called the GRF "perhaps the most useless scientific project of the twentieth century" ([6], p. 93). He was referring to the stated aims of the GRF and its essay competition; namely to discover some kind of gravity screen ('the right kind of alloy' - *cf.* [2], p. 343). Gardner rightly points out that the concept of a material that is opaque to gravitational interactions was made obsolete by the shift to the general theory of relativity.

Agnew Bahnson was a close friend of George Rideout, the president of Babson's GRF, and the one who had initially suggested the idea of a GRF to Babson. On the basis of how the initial organizational meetings went, Gardner's critique perhaps offered up a 'recipe' for a more successful venture. Whereas Babson promoted a vision of spectacular technologies as a result of its gravitational research, Bahnson adopted a more sober approach. Thus, in the foreword of an early draft (dated November 17, 1955) for the IOFP's promotional brochure Bahnson wrote (in stark contrast to Babson's statements):

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competition and accepting the prize brought the IAS into disrepute. He believed that Arnowitt and Deser exploited their positions at the institute (see [3]). Note that Pascual Jordan was placed in 5th position this same year, with his essay on his theory of a variable gravitational constant. James L. Anderson submitted an essay on the Measurability of Gravitational Fields, but was not placed at all - the same is true of Dennis Sciama who wrote on a possible method of shielding gravitation using Mach's principle. Frederik Belinfante took 1st prize in 1956 for his paper on gravitational absorbers and shields. In the 1957 competition, Thomas Gold took 1st prize for a paper on the gravitational interaction of matter and anti-matter, that he co-authored with Philip Morrison (John Wheeler took second prize, and Felix Pirani took 3rd).

[6] This is a reference to Babson's penchant for all things Newtonian. This penchant led to the establishment of one of the largest collections of "Newtonia" (as he calls it, [2] p. 340) in the world, on the campus of the Babson Institute in Wellesley, Massachusetts. One of the library's rooms is an actual room used by Newton while in his final years in London. This was purchased by Babson's wife when she discovered the building was being demolished, shipped over from the UK, and rebuilt on site as Newton would have used it "with the same walls, doors and even the identical shutters containing the hole through which he carried on his first experiments in connection with the diffusion of light" (*loc. cit.*, p. 340). David Kaiser has a useful discussion of this curious episode in his PhD thesis, *Making Theory: Producing Physics and Physicists in Postwar America* (Harvard University, 2000). See also Kaiser [9]

> In the minds of the public the subject of gravity is often associated with fantastic possibilities. From the standpoint of the institute no specific practical results of the studies can be foreseen at this time.

In many ways, then, Babson's GRF was used as a foil, highlighting things to adopt but (more importantly) things to avoid. A lengthy exchange of letters between Bahnson and several senior physicists (especially John Wheeler, whom Bahnson clearly admired a great deal) set to work on eradicating any aspects that might lead to claims that the institute was for crackpot research.

In 1955, G. S. Trimble, vice president of the Glenn Martin Company, wrote to Bryce DeWitt[7] that:

> During a recent conversation with Mr. George Rideout, president of Roger Babson's Gravity Research Foundation, we were commiserating on the unfortunate state of the affairs that knowledgable folks do not wish to get "mixed up" in the field of gravity research. During the course of the conversation he reviewed with me your suggestion that perhaps his Gravity Research Foundation might be transformed from its present function into an active center of research concentrating on the field of gravity. He also told me that the foundation was not able to undertake such an expansion. (Letter from G. S. Trimble to Bryce DeWitt, dated, June 10, 1955)

It seems that DeWitt had suggested to Babson something along the lines of the Institutes for Advanced Study - a model he was very familiar with since both he and his soon to be wife Cécile Morette, had both spent time in several of them. This model fitted with the Glenn Martin Company's plans. However, their goal was not pure research, but something grander, it seems - Trimble describes the proposed activity as an "industrial version of the Institute for Advanced Study".[8] The letter goes on:

---

[7]At this time senior research physicist, Radiation Laboratory, University of California, Berkeley and Livermore, then working on detonation hydrodynamics.

[8]Trimble makes an interesting remark concerning the tight relationship between scientific research and society: "[W]e feel morally obligated to push forward in the basic sciences and we believe as a dynamic industry we can provide the motivation for advances that can be obtained in no other way". In other words, for better or for worse, the pursuit of certain areas of basic research demand some kind of motivation beyond the search for deeper knowledge. Practical applications are one way to motivate such study.

> It occurred to us sometime ago that our industry was vitally concerned with gravity. As time goes on we become more and more concerned because we feel certain that sooner or later man will invade space and we see it as our job to do everything possible to speed this event. At least one category of the things one must study, when he desires to bring space flight to a reality, is the laws of nature surrounding the force of gravity. (ibid.)

One wonders what Roger Babson would have made of the fact that it was *Newton's* equations that got man into space![9] Trimble bemoans the fact that most of those working on gravitation are "mad men and quacks" - perhaps he has those connected with Babson's own endeavour in mind? Indeed, notes Trimble, any relevant work that they had done on space flight had been contracted out to German scientists working within Germany.

Louis Witten (the father of Ed Witten, and participant in the Chapel Hill conference)[10] was the first researcher hired by RIAS, of the Glenn Martin Company. He approached DeWitt in a bid to have him join RIAS, but DeWitt was then already being approached by Bahnson, offering more lucrative terms (both for him and Cécile): the promise of a position in a more traditionally academic environment, but with the freedom of an externally funded position, would ultimately win out. However, Trimble, and the Glenn Martin Company nonetheless played a role in helping the IOFP get off the ground. Not only did they purchase a 'Founder's Membership' for the Institute of Field Physics, for the considerable sum of $5000, they also offered their support to solicit further funding (letter from Trimble to Bahnson). The letter reproduced in Fig. 1.1, from Bahnson to Bryce and Cécile DeWitt, shows just how tightly bound industry, military, and research were at this time, and also how much influence a single individual could possess.

The plan of the IOFP was to house an institute within an academic institution, so as to avoid the conflict that physicists felt working in an industrial setting. The chosen location was the physics department at the University of North Carolina, Chapel Hill. In order to lend further prestige to the IOFP, Bahnson secured letters of comment from several of the most prominent physicists of the day, including Belinfante, Oppenheimer,

[9] Curiously, DeWitt later did a report on "The scientific uses of large space ships", for the Department of Defense (General atomic report GAMD 965, 1959).

[10] Bob Bass, who coauthored the paper with Witten in this conference report, was the first person to be hired by Witten, in 1956. Bass, together with Witten, would later manage to hire R. E. Kalman and Solomon Lefschetz, in 1956 and 1957 respectively.

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Dear Bryce and Cecile:

I enjoyed talking with Cecile earlier this week. I wanted to present a couple of questions to you which you can be thinking over and to which I will need a fairly prompt answer.

The Air Force has shown some interest in our work through a comment by Glenn Martin to the Baltimore office of the Air Force. Martin indicated that we were not interested in government funds at this time. I have talked the matter over with Mr. Morehead Patterson of American Machine and Foundry Company and with John Wheeler at Princeton. Both of them feel that there is no reason to shy away from government funds now that the ONR of the Navy and some division of the Air Force is setup to support theoretical research. John also intimated that it would be fine if we could get the two of you, a post doctoral assistant for about $4500.00 and two or three graduate assistants from $2100 to $2500.00 as well as one visitor from some outside university or even Europe to start in next fall. If we do this, I am pretty sure we will have to get help from the government and we will have to put the wheels in motion rapidly since most of these people will make up their minds by March as to where they will be next fall, at least the good ones. Therefore I think you had better advise me as soon as possible of your feelings in this matter so I can try to go to work. We seem to be running behind schedule a bit now as far as getting the support from industry into this year's tax deduction. It appears that I will have to go to New York to talk with American Machine & Foundry and TWA before they move. If we want the government help, however, I will have to start the ball rolling to see if we can get enough funds to locate and hire this panel of personnel that John has recommended. You had better start thinking about the personnel and advise me about the approach.

I look forward to seeing you both in the near future. It is possible that I will be going North around the 16 of January for a few days and Bryce may want to go up with me to New York and talk with Wheeler as well as to Boston and talk to Weisskopf. Best regards.

Sincerely,

Agnew

Agnew H. Bahnson, Jr.

AHBjr/s

Figure 1.1: Letter from Agnew Bahnson to the DeWitts from the early phase of the development of the Institute of Field Physics, December 29, 1955.

Dyson, Teller, Feynman, and Wheeler. Wheeler did much behind the scenes sculpting of the IOFP, and was, next to Bahnson, perhaps most responsible for the bringing about of the IOFP. For example, it was Wheeler who tempered Bahnson's own (somewhat Babsonesque) proposal entitled "The Glorious Quest", to make it more attractive to funding agencies: "Ebullient as you and I are, I suspect sober going may go further when it comes to getting money from a foundation" (Wheeler to Bahnson, August 29, 1955). Then, writing to the acting president of the University of North Carolina, Harris Purks, November 25, 1955, Wheeler writes of:

> the absolute necessity to avoid identification with so-called "anti-gravity research" that may be today's version of the last century's search for a perpetual motion machine. [...]
>
> Unfortunately, there are sensationalists only too willing to confuse in the public mind the distinction between so-called "anti-gravity research" ... and responsible, well informed attempts to understand field physics and gravitational theory at the level where it really is mysterious, on the scale of the universe and in the elementary particle domain.

He goes on to applaud the step (in fact suggested by Wheeler himself, earlier) of attaching to every piece of IOFP publicity a 'disclaimer' to the effect that the IOFP is in no way connected to anti-gravity research. This "Protection Clause" would be attached to each IOFP statement:

> The work in field physics and gravitation theory carried on at the University of North Carolina at Chapel Hill, and financed by the Institute of Field Physics, as fund raising agency, has no connection with so-called "anti-gravity research" of whatever kind and for whatever purposes. Its scientists, basing their investigations upon verifiable data, accept the Newton-Einstein analysis of gravity as free of a single established exception, and as the most comprehensive physical description we have today. They seek the implications of gravity and other fields of force at the level of the elementary particles. More generally, the Chapel Hill project is a modest attempt to learn more about the nature of matter and energy.

This expedient, Wheeler argued, is necessary to avoid discouraging both sponsors and scientists. The message that anti-gravity connotations must be avoided at all costs runs through much of the correspondence and foundation documents like a mantra. It clearly played a vital role (in the minds of physicists) in establishing the legitimacy of the enterprise.

Wheeler did not hold back on the need for the IOFP, though his claims were moderated somewhat by a knowledge that progress might well be very slow:

> It is hard to see how one can get to the bottom of the elementary particle problem - the central issue of modern physics - without coming to the very foundations of our physical world and the structure of space and time. Gravity, fields and particles must in the end be all one unity. The absence of any paradox or discrepancy in gravitation theory at the human and astronomical levels creates an obligation to apply Einstein's ideas down to smaller and smaller distances. One must check as one goes, until one has either a successful extension to the very smallest distances, or a definite contradiction or paradox that will demand revision. ... The challenge cannot be evaded. Exactly how to proceed is a matter of wisdom, skill, judgement, and a good idea. Nobody guarantees to have a good idea, but the DeWitts, fortunately, have a very sound plan of what to do while searching for a good idea. They propose to do something that has long needed doing - help make clear the fundamental facts and principles of general relativity so clearly and inescapably that every competent worker knows what is right and what is wrong. They can do much to clear away the debris of ruined theories from the rocklike solidity of Einstein's gravitation theory so its meaning and consequences will be clear to all. This is a great enterprise. Einstein's theory of the spacetime-gravitation field is even richer than Maxwell's theory of the electromagnetic field. That field has been investigated for many years, and now forms the foundation for a great science. One cannot feel physics has done its job until a similarly complete investigation has been made for the gravitational field. (John Wheeler, letter to Bahnson, November 25, 1955)

Though there is, of course, a good deal of colourful rhetoric in this passage, it nonetheless shows the importance in Wheeler's mind of the role that the IOFP (and the DeWitts) would play. (Note that Wheeler had only just recently had to intervene in a proposal of Samuel Goudschmidt's to impose an embargo on all papers dealing with general relativity and unified field theory from the pages of *Physical Review* - *cf.* [5], p. 414.)

The various letters of support (dating from between October 1955 and January 1956), for which the preceding letter from Wheeler to Purks provides a cover letter, highlight the recognition that general relativity and gravitational research had been unfairly neglected, and the need for a renewal of interest. Oppenheimer writes that he “shares with most physicists the impression that this field has been rather neglected by us”. Dyson seconds this (as does Nordheim), but adds some conditions for success, more or less reiterating what Wheeler had already said: that immediate results should not be expected, and that (“to avoid becoming isolated and sterile”) the institute should be settled as firmly as possible in “the framework of normal university life”. Edward Teller remarks in his letter that “a comprehensive examination of general relativity and high-energy physics, together with an investigation of the interaction between these two fields may very well lead to the essential advance for which we are all looking”.

Feynman too voiced the opinion that “the problem of the relation of gravitation to the rest of physics is one of the outstanding theoretical problems of our age”. However, he was less positive about the chances of the proposed institute in (what he thought was) its original form. Feynman was not convinced that an industrially funded institute, detached from a university, could possibly deliver the requisite flexibility to develop new fundamental knowledge: that required absolute freedom to bounce around between topics, as one chose. On learning that the institute was to be housed in a university, Feynman was unreservedly positive about the proposal (letter to Wheeler, dated December 2, 1955).

John Toll, head of physics at the University of Maryland, writes, directly discussing the other letters:

> Most of my colleagues have pointed out in their comments that the field of general relativity has not received the attention which it deserves and that it is particularly important to attempt to obtain some synthesis of the methods and concepts used in general relativity with the ideas now employed to discuss elementary particles. One reason for the neglect of general relativity has been the great difficulty of work in this field which challenges even the best theoretical physicists; solution of the major problems involved will probably require a determined program which may extend over many years. A second and related reason has been the difficulty of obtaining adequate support for this field; the problems are not of the

> type which are supported by federal agencies which finance so much of the research in physics in the United States by short term contracts, mostly in fields which appear to have more immediate applicability to defence problems. (Letter from J. S. Toll to John Wheeler, dated December 28, 1955.)

This was all written towards the end of 1955.[11] By 1957 the picture looked remarkably rosier. Whether it was due to some degree of influence of the IOFP[12] and/or RIAS (or the beginnings of the 'Space Race' and the Cold War), the Air Force and the Department of Defense in fact soon began to fund fundamental research in gravitational physics. Rather fortuitously, Joshua Goldberg was, at the time of the establishment of the IOFP and the conference, in charge of aspects of the Air Force's funding of general relativity, in the 'modern physics research branch' (based at the Aeronautical Research Laboratory at Wright Patterson Air Force Base). In addition to a \$5000 grant, one of the (very crucial) things he was able to do was secure MATS (Military Air Transport Service) transportation to and from the USA (initially just for Géhéniau, Rosen, and Laurent, but later this would include transportation for Behram Kursunoglu from Turkey and Ryoyu Utiyama from Japan, extending to 11 nations in all). In a letter to Bryce DeWitt (dated October 3, 1956), Goldberg notes that the suggestion to use Air Force transportation to bring over physicists

[11] December 7, 1955 saw DeWitt deliver a paper focusing on current research in gravitational physics to the American Astronautical Society (published as [4]). By this time he was able to give his position as 'Director of the IOFP'. The talk was clearly intended as a piece of propaganda for the IOFP. DeWitt opened by distancing his work from any foreseeable practical applications. He then notes the lack of serious research being carried out, counting just seven institutions with gravitation research projects: Syracuse, Princeton, Purdue, UNC, Cambridge, Paris, and Stockholm - with RIAS, Inc, on the industrial side. DeWitt mentions even at this early stage of quantum gravity history the problems that would plague the quantum geometrodynamical approach throughout its existence (until it transformed into loop quantum gravity): these are the problems of defining the energy and the quantities that are conserved with respect to it (i.e. the observables), and the factor ordering problem. (This problem refers to an issue caused by the straightforward canonical quantization of general relativity, based on the metric variables. According to the standard quantization algorithm, when one meets a momentum term, one substitutes a derivative. However, when this procedure is applied in general relativity, one faces situations were one has products, and so one has to multiply as well as differentiate. The order in which one does this matters for the form of the final wave equation.) The former was studied by Bergmann's group at Syracuse, while the latter problem was studied by DeWitt's own group at the IOFP.

[12] Not so far fetched as it might sound. Bahnson notes in a letter to Bryce and Cécile of December 29, 1956, that by then the Air Force had expressed interest in their work (this he heard directly from Glenn Martin) - see Fig.1.1.

from Europe had been Peter Bergmann's idea - Goldberg gives a personal account of his role in the Air Force's support of general relativity (and the possible reasons behind the military support of research in gravitation) in [7]. Such free transportation became commonplace for the IOFP at this time, and since many commentators who lived through this experience have suggested that the ability to be able to network, made possible by the availability of easy transportation, played a key role in the reemergence of gravitational physics. Later funding would also take the form of free computing time on IBM's best machines[13], and (in limited cases) free flights on TWA's line.

Securing additional funding for the conference was time-consuming. In May 1956, the DeWitts visited the National Science Foundation in Washington, to explain the nature of their project - a visit that met with success. The same week Bryce DeWitt gave a layman's talk to the Winston-Salem Rotary Club - at which various industrialists and wealthy interested parties were present - in which he described the various technological innovations that have emerged from 'pure research'. It seems (from a memorandum Bahnson sent to his fellow funders) that the Chapel Hill conference was virtually *entirely* externally funded (i.e. independent of the IOFP's own funds). This, he notes, is almost entirely thanks to the work of Cécile DeWitt (Bahnson, "Memorandum No. 9", May 7, 1957) - in a letter to Bahnson dated November 5, 1956, Bryce DeWitt notes that in the space of two weeks, Cécile composed 52 letters and placed 10 long distance calls, chasing potential funding for the conference.

Though not quite a cascade, the IOFP had enough funding in its heyday to attract several first-rate postdoctoral fellows. These included Peter Higgs, Heinz Pagels, and Ryoyu Utiyama. Among the first postdoctoral fellows at the institute was Felix Pirani, who had previously belonged (and would later return) to Hermann Bondi's Relativity and Gravitation group at King's College, London (Clive Kilmister was another long term member of this group).[14] Pirani received his (first) doctorate under Arthur Schild (who would later head the Center for Relativity) at The Carnegie

[13] This possibility was initially raised by IBM's representative, Dr John Greedstadt, at the Roaring Gap meeting, though apparently trying to define problems to run on the machines was not an easy task. It is, however, worth mentioning that Bryce DeWitt would later be a pioneer of numerical relativity; also, the issue of putting general relativity on a computer arises in the Chapel Hill report: p. 83.

[14] As Ezra Newman pointed out in his recollections of the early history of general relativity ([11], p. 379), Pirani completely abandoned physics for a life as an author of children's books. See also [14].

Institute of Technology - he received a second doctorate from Cambridge University, under Hermann Bondi.

Peter Higgs too had been part of Hermann Bondi's Relativity and Gravitation group at King's College, London, since 1956. It was Pirani who urged him to take more interest in quantum gravity, prompting him to take up the position at the IOFP. Though invited to the institute to study gravitation, Peter Higgs ruefully admits that he spent his time there working on symmetry breaking in quantum field theory.[15] Higgs first encountered Bryce DeWitt in 1959, in Royamont France. This was the second GRG conference[16], and it was shortly after that the International Committee on General Relativity and Gravitation was formed (see Kragh [10], p. 362). He met him again at the GRG3 conference in Warsaw, in 1962. After 1956, following Pirani's advice, Higgs began looking at quantum gravity - at the time he was working with Abdus Salam at Imperial College. Here he wrote on the constraints in general relativity [8]. This led, in 1964, to DeWitt's invitation to Higgs to spend a year at the institute, which he did, arriving in September 1965, after a year's postponement. Bahnson died tragically in an airplane crash the year prior on June 3, 1964 - a chair was established at UNC in his honour, to be occupied by Bryce DeWitt.

A follow up meeting focusing purely on 'Exploratory Research on the Quantization of the Gravitational Field' was held in Copenhagen, June 15 to July 15, the same year as the Chapel Hill conference. This meeting involved DeWitt, Deser, Klein, Laurent, Misner, and Møller. Again, MATS was utilised for this meeting, courtesy of Goldberg and the ARL. Møller and Laurent would return to Chapel Hill as visiting fellows for two months, starting in February. Their brief was to work on the role of gravitation in artificial Earth satellites and also atomic clocks (Bahnson, Memorandum #11, Feb. 3, 1958).[17]

There are similarities between the Chapel Hill conference and the 1955 conference to celebrate the 50th anniversary of Einstein's theory of special relativity, held in Berne. Einstein was, of course, to have attended

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[15] Letter to Cécile DeWitt.

[16] Or the third, depending on whether one counts the conference in Berne in 1955 to mark the Jubilee of Einstein's theory of special relativity. This conference is often referred to as GR0.

[17] Cold War paranoia can be clearly seen in this memorandum. Bahnson mentions a recent report (apparently reported in *American Aviation* magazine) of a "graviplane" about to be produced by the Russians, based on "the extension of Einstein's theories by Dr. Foch (sic.) of Leningrad" - though Bahnson admits it is likely a "propaganda trap."

but he fell ill during the planning and died just before the event took place, prompting Pauli to declare that "This important moment in history is a turning point in the history of the theory of relativity and therefore physics" ([12], p. 27). Pauli himself died not long after, in 1958. Somewhat surprisingly, this jubilee celebration was in fact the first ever international conference devoted *solely* to relativity. As it would turn out, the conference dealt almost exclusively with *general* relativity, special relativity being more or less a finished enterprise, formally, experimentally, and conceptually. The conference would later come to be known as "GR0", the zeroth conference in a series which continues to this day, and of which Chapel Hill was the first, GR1.[18]

The proceedings (replete with post-talk discussions) were quickly edited by André Mercier and Michel Kervaire. This proceedings volume, and the conference itself, played a central role in the future evolution of classical and quantum gravity. However, it was no Shelter Island. Whereas that conference had been driven by the younger generation of physicists - Feynman, Schwinger, Wheeler, and others - the Berne Jubilee conference was dominated by older, more established physicists. In his own report on the Chapel Hill conference, Bahnson noted that he had talked to a physicist (unnamed) who had also participated in the Berne conference, who had remarked that the Chapel Hill conference had "greater informality" and that "the younger participants contributed to more discussion and exchange of information". The Chapel Hill conference on the Role of Gravitation in Physics, that would happen just two years later, did

[18]Schweber traces the "reawakening of interest in the field [of GR]" ([13], p. 526) to the Berne conference, GR0, in 1955. He also notes that interest in quantum gravity was made "respectable" as a result of Feynman's course at Caltech between 1962 and 1963 (*loc. cit.*, p. 527). The Berne conference was important. But it was distinctly a European affair. In the United States, as we have seen, there were several converging lines of attack leading to a reawakening of interest. Indeed, I would argue that since the Berne conference consisted mostly of an older generation who had persistently thought about general relativity and quantum gravity for decades, the phrase 'reawakening of interest' is not really appropriate. Klein, Pauli, and Rosenfeld, for example, were veterans when it came to the study of both. In fact, there was an earlier conference in honour of Bohr to which many of the same people contributed, and gave very similar talks. Further, to trace the respectability of quantum gravity research to Feynman's course is over-stretching. Wheeler had been including material on quantum gravity from the time he began teaching his general relativity course at Princeton. One can even find quantum gravity problems posed within his earlier advanced quantum theory course. In addition to this, there were, as Schweber himself notes, several strong 'schools' concentrating on gravitation research by the end of the 1950s. That Feynman's course happened was as a result of the increased respectability already in operation - moreover, Feynman's interest was surely stimulated as a result of his participation in the Chapel Hill conference, though he seems to have already been investigating the subject by 1955.

for general relativity and gravitation what Shelter Island did for quantum electrodynamics. The Chapel Hill conference was a genuine break from the Berne conference, both in terms of its organization, its content, but more so its *spirit*.

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Chapter 2
The Authors

In this chapter we provide brief biographical notes on the participants, focusing on those who made a contribution that is represented in the report. Many are, of course, very well-known figures, but others may not be familiar. For convenience we have arranged them according to their respective host countries (that is, the countries in which they were working at the time of the conference). Rather than provide any kind of extensive biography, we simply focus on key details and material that is relevant to the conference theme.

USA

J. L. Anderson James Anderson was born in Chicago in 1926. He received his PhD in 1952, under Peter Bergmann at Syracuse University, with a thesis entitled: "On the Quantization of Covariant Field Theories." He was Bergmann's research assistant between 1951 and 1952 and continued to work with him on the canonical approach to quantum gravity.

V. Bargmann Valentine Bargmann was born in 1908 in Berlin. He obtained his PhD in 1936 from the University of Zurich under the supervision of Gregor Wentzel, on the subject of electron scattering in crystals - "Über die durch Elektronenstrahlen in Kristallen angeregte Lichtemission". He had worked with Peter Bergmann, and was also Einstein's assistant, at the Institute for Advanced Study, working on the problem of motion in general relativity and unified field theory. He also did much important work on the application of group theory to physics in the years prior to the Chapel Hill conference.

R. Bass Robert Bass was born in 1930. He received his PhD in mathematics from Johns Hopkins University in 1955, under Aurel Wintner, on the subject: "On the Singularities of Certain Non-Linear Systems of Differential Equations." Following a postdoc at Princeton University, he was a staff scientist at RIAS [Research Institute for Advanced

Study] (a division of the Martin-Marietta Company) at the time of the Chapel Hill conference.

F. J. Belinfante Frederik Belinfante was born in the Hague, in the Netherlands, in 1913, and received his PhD under the supervision of Kramers at the University of Leiden. After World War II he moved to Purdue University (via the University of British Columbia). At the time of the Chapel Hill conference he was one of the few physicists pursuing an active research program in quantum gravity research, including the supervision of PhD students on this topic.

P. G. Bergmann Peter Bergmann was born in Berlin in 1915. He received his doctorate in 1936, under Philipp Frank, at the Deutsche Technische Hochschule in Prague, with a thesis entitled: "Der harmonische Oszillator im sphärischen Raum." He was Einstein's research assistant until 1941, working on unified field theory. He began working on the direct quantization of gravity in 1949, and soon established a group that specialized in the canonical quantization of gravity. By the time of the Chapel Hill conference he was responsible for one of the few 'schools of relativity,' based at Syracuse.

D. R. Brill Dieter Brill was born in 1933, and received his PhD from Princeton in 1959, on the subject of the positivity of mass in general relativity. He was a student of John Wheeler during the conference. He would later go on to write one of the earliest review articles on the quantization of general relativity, with his student Robert Gowdy.

M. J. Buckingham Michael Buckingham was born in Sydney in 1927. He gained his PhD in Liverpool, in the UK, under Herbert Fröhlich, on the subject of superconductivity. At the time of the Chapel Hill conference he was a postdoctoral fellow at nearby Duke University, also in North Carolina. He returned to Australia shortly after the conference.

W. R. Davis William Davis received his PhD from the University of Göttingen in 1956. He was hired by the University of North Carolina in 1957. He later became one of the world's foremost authorities on the works of Cornelius Lanczos, and is the editor of Lanczos' complete works.

B. S. DeWitt Bryce DeWitt was born in California in 1923. He received his PhD at Harvard under the supervision of Julian Schwinger in

1950. Entitled "I. The Theory of Gravitational Interactions. II. The Interaction of Gravitation with Light," it is one of the earliest examples of a thesis devoted to quantum gravity. After a string of research fellowships, including a Fullbright, he took up (with his wife Cécile) directorship of the Institute for Field Physics at the University of North Carolina.

C. M. DeWitt Cécile DeWitt-Morette was born in Paris in 1922. She completed her doctoral research at the Dublin Institute for Advanced Study, under Walter Heitler, in 1947, on the physics of mesons, entitled "Sur la Production des Mésons dans les Chocs entre Nucléons" - though *officially* she received her degree from the Université de Paris. She traveled to the Institute for Advanced Study at the request of Robert Oppenheimer were she first encountered Feynman's path integral theory, a subject she would specialize in. She founded the Les Houches summer school series in 1951, and directed the Institute of Field Physics with her husband Bryce DeWitt. She was the chief organizer of the Chapel Hill conference.

R. H. Dicke Robert Dicke was born in Missouri in 1916. He received his PhD from the University of Rochester in 1941, under Victor Weisskopf, with a thesis on the experimental investigation of the inelastic scattering of protons. He was based at Princeton in 1957, and was already gaining a reputation for his research on new experiments in the theory of gravitation as well as controversial interpretations of gravitational phenomena.

F. J. Ernst Frederick Ernst was born in 1932 near Manhattan. He was among those students to attend John Wheeler's first ever general relativity course at Princeton in 1953. He would later complete a thesis on geons under Wheeler's supervision (between 1951 and 1955), the contents of which were presented in his talk at the Chapel Hill conference. He gained his doctoral thesis in 1958 under Robert Sachs, on quantum field theory.

R. P. Feynman Richard Feynman was born in 1918 in New York City. He received his PhD under the supervision of John Wheeler, at Princeton, in 1942, with his thesis on "The Principle of Least Action in Quantum Mechanics." Even by 1955 Feynman claimed to have spent a great deal of effort on the problem of gravitation (letter from Bryce DeWitt to Agnew Bahnson, November 15, 1955).

T. Gold Thomas Gold was born in Austria in 1920. After a circuitous trajectory through engineering and the theory of hearing, Gold was, in 1947, elected to a Fellowship at Trinity College, Cambridge (without having obtained a PhD). Soon after he devised, with Hermann Bondi and Fred Hoyle, the idea of continuous creation of matter on the basis of a conflict between Hubble's time constant and empirical evidence. By the time of the Chapel Hill conference he was working on a range of problems in and around astronomy at Harvard.

J. N. Goldberg Joshua Goldberg was born in New York in 1925. He was a graduate student at Syracuse, where he received his PhD under Peter Bergmann in 1952. He took up a position as research physicist at the Wright-Patterson Air Force Base and, as part of his administrative duties at Wright-Patterson, was crucial in obtaining funding for the Chapel Hill conference.

A. E. Lilley Arthur Lilley was born in 1928. He received his PhD in 1956 in astronomy from Harvard University under the direction of Bart Bok. At the time of the conference he was a radio astronomer with the U.S. Naval Research Laboratory, though affiliated to Harvard. He was a pioneer in radio astronomy and its uses in the testing of relativity.

R. W. Lindquist Richard Lindquist was a graduate student of Wheeler's, and received his PhD under Wheeler's direction in 1962, with a thesis on "The Two Body Problem in Geometrodynamics." He later extended this work into numerical relativity in which he was one of the first to develop computer simulations of colliding black holes.

C. W. Misner Charles Misner was born in Michigan in 1932. He received his PhD under the supervision of John Wheeler the same year as the Chapel Hill conference, on the subject of the Feynman quantization of general relativity. He was already an instructor at Princeton from 1956, and would often give 'guest' seminars in John Wheeler's courses. He was very closely involved in Wheeler's 'geometrodynamical' approach at the time of the conference.

R. Mjolsness Raymond Mjolsness was a beginning graduate student of John Wheeler's at Princeton (on the NSF graduate fellowship) - though he later gained his PhD under Edward Freiman in 1963, with a thesis entitled: "A Study of the Stability of a Relativistic Particle Beam Passing Through a Plasma."

E. T. Newman Ezra Newman was born in New York in 1929. He was a student of Peter Bergmann at Syracuse and gained his PhD under his supervision in 1956 - Newman initially began working with Bergmann as an undergraduate in 1951.

A. Schild Alfred Schild was born in Istanbul in 1921. He was interned in Canada during World War II, and obtained his PhD there, from the University of Toronto, in 1946, under the supervision of Leopold Infeld, with a thesis entitled: "A New Approach to Kinematic Cosmology." He moved to the University of Austin, Texas, just after the Chapel Hill conference and established the very successful Center for Relativity Theory. Together with his student Felix Pirani, he had written one of the first papers on the canonical quantum of the gravitational field.

R. S. Schiller Ralph Schiller was a graduate student with Peter Bergmann at Syracuse and had worked on translating results from the canonical formulation of general relativity into the Lagrangian formulation. He later switched to biophysics after spending some time as David Bohm's research assistant in Brazil.

J. Weber Joseph Weber was born in New Jersey in 1919. His interests spanned laser physics and gravitational physics. He received his PhD in 1951 under Keith Laidler on the subject of "Microwave Technique in Chemical Kinetics" at the Catholic University of America. In 1955 he began investigating gravitational radiation, and accompanied John Wheeler to Leiden in 1956. Weber began the construction of the gravitational wave detector that bears his name the year following the Chapel Hill conference.

J. A. Wheeler John Wheeler was born in Florida in 1911. He obtained his PhD from Johns Hopkins University (under Karl Herzfeld) in 1933. He had a range of postdoctoral positions following this, including one under Bohr's supervision. He was a professor at Princeton when the Chapel Hill conference took place, and had only fairly recently, in the fall of 1952, begun to teach and learn general relativity. Many of his doctoral students were present at the conference. Just prior to the conference, Wheeler was Lorentz Professor in Leiden - he took three of his brightest doctoral students with him, including Charles Misner and Joseph Weber.

L. Witten Louis Witten was born in Baltimore in 1921. He received his PhD in 1951, from Johns Hopkins University, under the supervision of Theodore Berlin. His interests shifted to general relativity during postdoctoral stints at Princeton and Maryland. At the time of the conference he was working at RIAS.

UK

H. Bondi Hermann Bondi was born in Vienna in 1919. He did his PhD at Cambridge under Arthur Eddington. In 1948 Bondi had, together with Thomas Gold, published a theory of cosmology (the 'steady state theory') based on the 'perfect cosmological principle', which was still being debated at the time of the Chapel Hill conference. By 1957, Bondi had established (at King's College, London) one of the few schools of general relativity.

F. Pirani Felix Pirani was born in 1928. He received his first PhD under Arthur Schild, at the Carnegie Institute of Technology, in 1951, where he had worked (together with Schild) on the canonical quantization of the gravitational field. He obtained a second PhD under Hermann Bondi at Cambridge in 1956, on "The Relativistic Basis Of Mechanics." Just prior to the Chapel Hill conference he had proposed the geodesic deviation equation as a tool for designing a workable gravitational wave receiver. He later became an author of children's books, a shift which seems to have been triggered by his experience of racism in the United States during a postdoctoral fellowship at Chapel Hill in 1959.

L. Rosenfeld Léon Rosenfeld was born in Belgium in 1904. He received his PhD in 1926, at the University of Liège under Marcel Dehalu and Niels Bohr. Following this he undertook a series of fellowships, with de Broglie, Born, Pauli and Bohr. He had done pioneering work on constrained systems and the quantization of the gravitational field in the 1930s. He succeeded Hartree at Manchester in 1947 and was still based there at the time of the Chapel Hill conference.

D. Sciama Dennis Sciama was born in Manchester in 1926. He received his PhD from Cambridge under Dirac in 1953, with a thesis on Mach's Principle. He followed his PhD with postdoctoral fellowships at the Institute for Advanced Study and Harvard. He later returned to Trinity College in Cambridge on an earlier fellowship, before joining Bondi's group at King's College in 1958.

France

Y. Fourès Yvonne Fourès (née Bruhat; now Choquet-Bruhat) was born in Lille in 1923. She received her PhD ('Docteur des Sciences') from the Université de Paris in 1951 on the Cauchy problem for a system of second order partial differential equations (with application to general relativity). She spent 1951-1952 at the Institute for Advanced Study in Princeton. She was a member of the Faculté des Sciences de Marseille at the time of the Chapel Hill conference.

André Lichnerowicz Lichnerowicz was born in Bourbon l'Archambault in 1915. He received his PhD in 1939, on differential geometry in general relativity, under the supervision of Georges Darmois. He was based at the Collège de France, in Paris when the conference took place. He was a full professor of mathematics, rather than physics. As we have seen, at the time it was common for general relativists to be found in mathematics departments. Lichnerowicz had earlier been to the States, where he had spent some time as a visiting professor at Princeton. The same year as the conference, Lichnerowicz founded (along with John Wheeler and Vladimir Fock) the International Society for General Relativity and Gravitation.

M. A. Tonnelat Marie-Antoinette Tonnelat-Baudot was born in 1912 in Southern Burgundy. She gained her doctorate in 1939, under the supervision of de Broglie. In this work (which includes a little-known paper coauthored with de Broglie) she investigated the relationship between the wave mechanics of spin-2 particles (gravitons) and linearized general relativity. By the time of the conference she had a long history of research in general relativity and unified field theory and was holding a position at the Henri Poincaré Institute at the Université de Paris.

Germany

H. Salecker Helmut Salecker had been a research assistant of Eugene Wigner, and was reporting on their joint work at the Chapel Hill conference. He spent part of 1957 based in Princeton with Wigner, working on general relativistic invariance and quantum theory, and the work on quantum limitations of measurements of spacetime distances. Following this he took up a position at the Institut für Theoretische Physik at Freiburg.

Japan

R. Utiyama Ryōyū Utiyama was born in Shizuoka, Japan in 1916. He received his PhD from Osaka University under the supervision of Minoru Kobayashi in 1940. He had been a visiting fellow at the Institute for Advanced Study at Princeton between 1954 and 1956, where he was working on 'general gauge theory.' He had done earlier quite pioneering work on quantum gravity. He would be among the first postdoctoral fellows at the Institute for Field Physics at Chapel Hill.

Denmark

S. Deser Stanley Deser was born in 1931, in Poland. He obtained his PhD from Harvard in 1953, under the supervision of Julian Schwinger with a thesis on "Relativistic Two-Body Interactions." He had been an NSF Jewett Postdoctoral Fellow at the Institute of Advanced Study (IAS) between 1953 and 1955 under the supervision of Robert Oppenheimer - he was also associated with the 'Rad lab' at Berkeley during 1954. At the time of the conference he was a member of the Niels Bohr Institute in Copenhagen.

Sweden

B. Laurent Bertel Laurent had been Oskar Klein's last student. In the years immediately preceding the Chapel Hill conference he had been working on the first attempt at a Feynman-style quantization of general relativity.

Turkey

B. Kursunoglu Behram Kursunoglu was born in Turkey in 1922. He received his PhD from Cambridge, where he was part of a group including Kemmer, Salam, and Dirac - Kursunoglu also attended Dirac's quantum mechanics lectures. He was the chief scientific advisor to the Turkish general army staff at the time of the conference, and was working on a unified field theory.

The Original Chapel Hill Report

Foreword
Cécile DeWitt

A conference on The Role of Gravitation in Physics was held at the University of North Carolina, Chapel Hill, from January 18 to January 23, 1957. It was planned as a working session to discuss problems in the theory of gravitation which have recently received attention.

The present report was undertaken as a necessary requirement to obtain conference funds. However, as the conference progressed, it became more and more apparent that a report of the discussions would have a scientific interest, partly because of an increasing number of requests for a report from physicists unable to attend the conference, and partly because of the nature of the discussions.

Research in gravitational theory has been relatively neglected in the past two or three decades for several good reasons: (1) the lack of experimental guideposts, (2) the mathematical difficulties encountered in the study of non-linear fields, and (3) the experience of repeated early failures to extend general relativity theory in a permanently interesting fashion. A renewed interest in the subject has recently begun to develop, and the Chapel Hill conference gave an opportunity to the few physicists actively working in the field - some having kept up an interest in it in spite of its difficulties, others having lately engaged in its study, often from a new point of view - to discuss the preliminary results obtained and to present new lines of approach.

This situation gave rise to very lively discussions. Obviously, most of the material discussed is not ready for publication yet and - owing to the difficulty of the problems under consideration - may not be for a long time to come. That is why an informal report of the proceedings is valuable, but at the same time delicate to write up. An effort has been made to check with the authors the report of their contributions to the conference, and some have very kindly rewritten the draft proposed to them. However, the time allotted to the preparation of the report had to be limited, because the usefulness of such a report decreases with time more rapidly than its quality increases; moreover, the reporters should not be asked to spend an undue amount of time working on the report, which could be better spent on original research. **Consequently, no statement from this report can be quoted without explicit permission from the author.**

The papers which are ready for publication will appear in the July 1957 issue of *Reviews of Modern Physics*. A brief summary of the conference intended for non-specialists has been sent to *Science*.

It can hardly be said that the report gives a perfectly true picture of the conference. The report has been prepared from notes taken during the session, from material given by the authors, and from tape recordings. (The reporters had hoped to have a stenographic transcript available, but the cost of this transcript was beyond common sense.) Some contributions have been very appreciably abridged, some are reproduced practically verbatim, some are extended, and some have not been recorded, depending largely on the "communication" (both material and intellectual) between authors on the one hand and reporters and editors on the other.

At the end of the conference, the participants expressed - together with their belief in the importance of the subject matter - the wish to meet again, if possible in Europe in the summer of 1958. Such meetings should truly give an opportunity for discussions and for contacts between senior physicists who have a broad and thorough understanding of the subject and younger physicists of various backgrounds who have an interest in it. The need is also felt for discussions and contacts between workers in the field for periods of time longer than conferences.

Thanks are due to the sponsors, the members of the steering committee, and the members of the Physics Department of the University of North Carolina. The interest shown in the conference by Governor Luther H. Hodges and by the officers of the University was very gratifying. Special mention should be made of the cooperation of the University Extension Division and of the many individuals who assisted in hospitality arrangements for the conferees. To all who have made the Chapel Hill conference possible and have helped in its organization, this report is dedicated as a token of appreciation.

Cécile M. DeWitt

Chapel Hill, North Carolina
March 15, 1957

Abstract

From January 18-23, 1957, a group of physicists from several countries met at the University of North Carolina to discuss the role of gravitation in physics. The program was divided into two broad headings: Unquantized and quantized general relativity. Under the former came a review of classical relativity, its experimental tests, the initial value problem, gravitational radiation, equations of motion, and unified field theory. Under quantized general relativity came a discussion of the motivation for quantization, the problem of measurement, and the actual techniques for quantization. In both sections the relationship of general relativity to fundamental particles was discussed. In addition there was a session devoted to cosmological questions. A large part of the relevant discussions is reproduced in this report in a somewhat abridged form. A conference summary statement is presented by Professor P. G. Bergmann.

PROCEEDINGS
OF THE
CONFERENCE ON THE ROLE OF GRAVITATION IN PHYSICS

University of North Carolina, Chapel Hill, January 18-23, 1957

UNDER THE SPONSORSHIP
OF THE

International Union of Pure and Applied Physics, with financial support from
UNESCO
National Science Foundation
Wright Air Development Center, U.S. Air Force
Office of Ordnance Research, U.S. Army

This conference was an activity of the North Carolina Project of the Institute of Field Physics, established in 1956 in the Department of Physics of the University of North Carolina, Chapel Hill.

Its organization has been carried out through the Institute of Natural Science of the University of North Carolina, Chapel Hill.

STEERING COMMITTEE

Frederick J. Belinfante, Purdue University
Peter G. Bergmann, Syracuse University
Bryce S. DeWitt, University of North Carolina
Cécile M. DeWitt, University of North Carolina
Freeman J. Dyson, Institute for Advanced Study
John A. Wheeler, Princeton University

MARCH 1957[1]

Wright Air Development Center
Air Research and Development Command
United States Air Force
Wright-Patterson Air Force Base, Ohio

[1] Cécile M. DeWitt mailed this report to the Air Force on March 18; on March 19, she gave birth to her third daughter.

Reporters

W. A. Bowers
B. S. DeWitt
Cécile M. DeWitt
M. M. Duncan
J. M. Ging
J. R. Herring
E. M. Lynch
A. V. Masket
Eugen Merzbacher

Technical editor and typist

Ouida C. Taylor

Scientific editors

Cécile M. DeWitt
Bryce S. DeWitt

PARTICIPANTS IN THE CONFERENCE[2]

Anderson, J. L.	Stevens Institute of Technology
Bargmann, V.	Princeton University
Bass, R.W.	RIAS , Inc., Baltimore
Belinfante, F. J.	Purdue University
Bergmann, P. G.	Syracuse University
Bondi, H.	King's College, London
Brill, D. R.	Princeton University
Buckingham, M. J.	Duke University
Davis, W. R.	Oklahoma City, Oklahoma
Deser, S.	Institute for Theoretical Physics, Copenhagen
DeWitt, B. S.	University of North Carolina
DeWitt, C. M.	University of North Carolina
Dicke, R. H.	Princeton University
Ernst, F. J.	University of Wisconsin
Estabrook, F. B.	Office of Ordnance Research
Euwema, R. N.	Princeton University
Feynman, R. P.	California Institute of Technology
Fourès, Y.	University of Marseille
Gold, T.	Cornell University
Goldberg, I.	Syracuse University
Goldberg, J. N.	Wright Air Development Center
Havas, P.	Lehigh University
Janis, A.	Syracuse University
Kervaire, M.	Massachusetts Institute of Technology
Kursunoglu, B.	Maltepe, Ankara, Turkey

[2]Note that in the original report the front matter featured an index of (the more substantial) contributions of the various participants. In the present version, for convenience, this has been incorporated into a more general index. I have also included M. J. Buckingham who had been omitted in the original version.

Laurent, B.	Institute for Mechanics & Mathematical Physics, Stockholm
Lichnerowicz, A.	Collège de France, Paris
Lilley, A. E.	Naval Research Laboratory, Washington
Lindquist, R. W.	Princeton University
Mace, R.	Office of Ordnance Research
Misner, C. W.	Princeton University
Mjolsness, R.	Princeton University
Newman, E. T.	University of Pittsburgh
Pirani, F. A. E.	King's College, London
Rosen, N. (*in absentia*)	Israel Institute of Technology, Haifa
Rosenfeld, L.[3]	University of Manchester
Sachs, R.	Syracuse University
Salecker, H.	Institute of Theoretical Physics, Freiburg
Schild, A.	Westinghouse Research Laboratories, Pittsburgh
Schiller, R. S.	Stevens Institute of Technology
Sciama, D.	Trinity College, Cambridge
Tonnelat, M. A.	Henri Poincaré Institute, Paris
Utiyama, R.	University of Osaka, Japan
Weber, J.	University of Maryland
Wheeler, J. A.	Princeton University
Witten, L.	RIAS, Inc., Baltimore

[3]Rosenfeld had originally been denied a visa to attend the 1957 Chapel Hill conference by the US Consul in Manchester (FBI file January 24, 1957). Cécile DeWitt telephoned the US Attorney General and was able to convince him to reverse the Consul's decision.

Session I Unquantized General Relativity

Chairman: B. S. DeWitt

Chapter 3
The Present Position of Classical Relativity Theory and Some of its Problems

John Wheeler

We are here to consider an extraordinary topic, one that ranges from the infinitely large to the infinitely small. We want to find what general relativity and gravitation physics have to do with the description of nature. This task imposes a heavy burden of judgement and courage on us, for never before has theoretical physics had to face such wide subject matter, assisted by so comprehensive a theory but so little tested by experiment.

Our problems fall under three heads. First: What checks do we have today of Einstein's general relativity theory, and what can be done to improve our tests? Second: What ways can we see to draw new richness out of the theory? And third: What connection can we foresee between this theory and the quantum world of the elementary particles?

Without going over all the evidence from observational tests of general relativity, it is perhaps worthwhile to set down a few numbers relating to the three traditional tests of the theory.

Table 3.1: Precession in seconds of arc per century

Planet	Observed Value	Prediction from General Relativity
Mercury	42.56 ± 0.94	43.15
Earth	4.6 ± 2.7	3.84
Mars	—	1.35

Table 3.1 compares the predicted and observed values of the precession of the perihelion of Mercury and the Earth; for Mars there are not yet data to give a check. The predicted effects are well within the experimental error.

The second well-known test is the deflection of light in passing the sun, which shows itself as the difference between photographs of the star field taken when the sun is in its midst and those taken when the sun is elsewhere. The expected effect is 1.751 seconds of arc deflection for a ray which passes the sun at grazing incidence. Although there has been some disagreement among astronomers, the careful review of the evidence by McVittie leads to acceptance of Mitchell's result, based on the 1919, 1922, and 1929 eclipses, of $1.79 \pm .06$ seconds of arc, which value has since been confirmed by observations of the 1952 eclipse. The theoretical prediction is thus in agreement with the data.

The third observational test of general relativity, the gravitational red shift of spectral lines emitted on the surface of a massive star, presents insuperable difficulties in the case of the sun, owing to the fact that spectral shifts originating in atmospheric currents and turbulence completely mask the effect. On the other hand, white dwarf stars have mass to radius ratios large enough to make these Doppler effects negligible in comparison with the gravitational red shift. Observations are available on two stars, and are summarized in Table 3.2.

Table 3.2: Observed and Predicted Gravitational Red Shifts

	$\frac{\text{Mass}}{\text{SunMass}}$	$\frac{\text{Radius}}{\text{Sun Radius}}$	Red Shift Predicted	Red Shift Observed
40 Eri B	0.43 ± 0.04	0.016 ± 0.002	17 ± 3	21 ± 4
Sirius B	1	0.008	79	$60 - 80$

It appears from these three traditional tests that general relativity is not in disagreement with the observations.

The chief possibilities of improvement exist in the second and third effects - that is, the bending of light and the gravitational red shift. We hope to hear from Professor Dicke later what prospects there are for new experiments. In particular, the development of the "atomic clock" may begin to make feasible detection of gravitational red shift on the earth itself.

To go on to the second general problem, namely, how to draw new richness out of the theory on the classical level: there is a far-reaching analogy between the equations of general relativity on the one hand and those of Maxwell's theory on the other; they are both second order partial differential equations, and the future can be predicted from the knowledge of initial conditions. From Maxwell's equations, which are quite simple, a wealth of information can be obtained: dielectric constants, scattering of waves, index of refraction, and so on; the equations of general relativity have, however, scarcely been explored, and much remains to be done with them. Another task ahead of us is the construction of the curve giving the spectrum of gravitational radiation incident on the earth, analogous to the known curve of the electromagnetic radiation spectrum; or at least the determination of upper limits on it. Until something is known about this, we can scarcely be said to know what we are talking about.

We can, however, go on to explore some of these analogies on a deeper level. First among the problems is the "initial value problem." If I make a diagram similar to a shuffleboard court, with different squares corresponding to the various unsolved problems, and with scores attached to the various squares, we should assign a very high score - say 100 points - to this problem. We know that in electromagnetic theory, if $\underline{E}$ and $\underline{H}$ are given at time $t = t_0$, their values at later times can be predicted. However, they may not be specified arbitrarily on the initial time-like surface, but must obey certain conditions - namely the vanishing of their divergences. Through the work of Professor Lichnerowicz, we now know the analogous conditions in the case of relativity theory. They are non-linear conditions, rather than linear ones as in the Maxwell theory. Unfortunately, however, we do not know yet any "super-potentials," analogous to $\underline{A}$ in electromagnetic theory, which can be specified arbitrarily and whose derivatives give the field quantities while automatically satisfying the supplementary conditions. This is one of the most important problems of relativity theory, and one may well believe that it must be solved before further progress with quantization of the theory can be made. The further problem of the "time-symmetric" specification of initial conditions, which we know can be done in electromagnetic theory by specifying $\underline{H}$ on two time-like surfaces instead of specifying both $\underline{E}$ and $\underline{H}$ on one surface, and which is the appropriate form for quantization, is also yet to be solved in relativity theory.

Now this is a local condition; but boundary conditions in the large must be considered, especially when considering systems with closed topology - and to this problem we may assign a score of, say, 25.

A third problem is that of translating Mach's principle - that the distribution of matter in the universe should specify the inertial properties at any given point of space - into concrete, well defined form. We know from the work of Dr. Sciama that one can translate the differential equations of the Einstein theory into integral equations, and that one has an analogy between the solutions of the equations of static and radiative type, respectively, and the corresponding types of solutions of Maxwell's equations: namely, inverse square behavior of the static solutions, but inverse first power behavior at large distances, arising from the radiative terms. If one sums these interactions over all masses in the universe, one sees something of the connection between the inertia of one particle and the distribution of the rest of matter in space. This problem of spelling out Mach's principle in a better-defined way we may assign a score of, say, 25. It is of course related also to the problem of specification of conditions in the large, which we mentioned earlier; it is also related to another issue: that of uniqueness of solutions of the gravitational field equations, to which we may assign, say, 10 points.

To pass on from these issues, let us consider some further analogies with electromagnetic theory. We know that if we have in a given frame of reference a specified field, there exists another frame of reference moving with velocity

$$\frac{v}{c} = \frac{2(\underline{E} \times \underline{H})}{E^2 + H^2}$$

in which $\underline{E}$ and $\underline{H}$ have been reduced to parallelism. We then have a canonical situation in which, if we chose the z-axis as the direction of $\underline{E}$ and $\underline{H}$, any rotation in the x-y plane leaves the situation unchanged, and any Lorentz transformation in the z-t plane also leaves the situation unchanged. Thus what Schouten has called a "two-bladed" structure of the space-time continuum at any point is defined by the electromagnetic field. This is then a geometrical interpretation of what we have to deal with in the electromagnetic case, and we would like to understand what the analogous situation is in the gravitational case. Here we have to deal with the quantities R_{ijklm} that measure the curvature of space - geometrical quantities much more elaborate than the electromagnetic field tensor, of course. We would like to understand what kinds of invariance this quantity defines. We have the remarkable work of Géhéniau and Debever on this problem, but there are still issues to be defined in giving a clear understanding of what geometrical quantities are specified by the existence of the gravitational field. I would attach a score of, say, 15 to this problem of the geometrical interpretation of the gravitational field quantities.

This in turn leads us to another issue. Einstein has taught us to think in terms of closed continua, rather than the open continua such as Euclidean space; but since the differential equations are purely local and say nothing about the topology in the large, we are obliged to consider what the possibilities are. Recently we have become aware, for example, of one way of understanding, in terms of topological notions, such a concept as that of electric charge. Figure 3.1 below shows one intriguing kind of topological connectedness in the small.

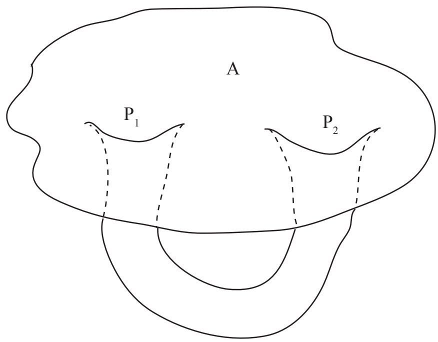

Figure 3.1

If one thinks of the upper region A as a two-dimensional space, there exists the possibility of connecting two regions P_1 and P_2 by a "wormhole" so that for instance an ant coming to P_1, would emerge at P_2 without ever having left the two-dimensional surface on which he started out. Now ordinarily, if we have electric lines of force converging on some point in space, we think of two possibilities: either Maxwell's equations break down near the point, or there exists a mysterious entity called electric charge at the point. Now a third possibility emerges: the topology in the neighborhood of the point may be such as to give a space-like binding to the lines of force; they may enter the "wormhole" at P_1 and emerge at P_2 (see Fig. 3.2). Misner has shown, in fact, that the flux through such a "wormhole" is invariant, giving one a possibility of identifying this flux with the notion of charge. Actually, there are strong reasons why this cannot be identified with an elementary charge; our purpose is to classify and try to understand the topological possibilities of the classical theory. I would give this problem a rather high score, say 50 points.

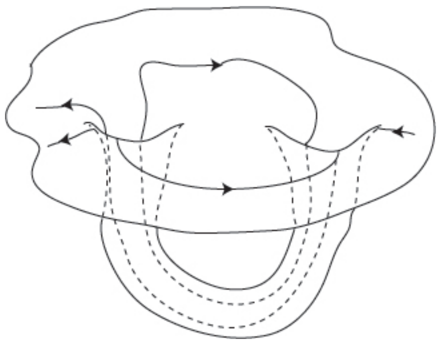

Figure 3.2

An objection one hears raised against the general theory of relativity is that the equations are non-linear, and hence too difficult to correspond to reality. I would like to apply that argument to hydrodynamics - rivers cannot possibly flow in North America because the hydrodynamical equations are non-linear and hence much too difficult to correspond to nature! We need to explore the analogies between the gravitational and the hydrodynamic equations; are there gravitational counterparts of such hydrodynamic phenomena as shock waves, cavitation, and so on? A fairly high score, say 40 points, should be given to this problem of the physics of non-linear fields, and analogies with hydrodynamics.

The non-linear couplings between gravitation and electromagnetism also give rise to new structures such as "geons," which should be studied further to enrich one's understanding on the physical side.

The last topic to be covered under the classical aspects of general relativity is the topic of unified field theory - namely, what can one find in the way of a description of electromagnetism which has the same purely geometrical character as that which Einstein gave to gravity? Although the various attempts to modify Einstein's theory in such a direction are well known, it does not seem to be so well known that within the framework of Einstein's theory one can construct, so to speak, an "already unified" theory, as Misner and earlier Rainich have shown, based on the role of the electromagnetic field as a source of the gravitational field through its stress-energy tensor.

The result is that one can write the equations of both electromagnetism and gravitation in a form in which only purely metric quantities enter, as in Einstein's theory. It is a system of fourth order equations which contain all the content of the two coupled second order equations of electromagnetism and gravity. Further investigation of theories of this kind deserve a score of, say, 25 points on our "shuffleboard" court.

In summarizing, let me remark that we have here, in the "already unified theory," electromagnetism without electromagnetism, just as we have, in geons, "mass without mass," and in connection with multiply connected topologies, "charge without charge." This then is just one man's start at listing some of the problems with which we will be occupied in the coming days.

Chapter 4

The Experimental Basis of Einstein's Theory

R. H. Dicke

It is unfortunate to note that the situation with respect to the experimental checks of general relativity theory is not much better than it was a few years after the theory was discovered - say in 1920. This is in striking contrast to the situation with respect to quantum theory, where we have literally thousands of experimental checks. Relativity seems almost to be a purely mathematical formalism, bearing little relation to phenomena observed in the laboratory. It is a great challenge to the experimental physicist to try to improve this situation; to try to devise new experiments and refine old ones to give new checks on the theory. We have been accustomed to thinking that gravity can play no role in laboratory-scale experiments; that the gradients are too small, and that all gravitational effects are equivalent to a change of frame of reference. Recently I have been changing my views about this. The assumption of the experimentalist that he can isolate his apparatus from the rest of the universe is not necessarily so; and he must remember also that the "private" world which he carries around within himself is not necessarily identical with the "external" world he believes in. Particularly in the case of relativity theory, where one thinks one knows the results of certain experiments, it is all the more important to *perform* these crucial experiments, the null experiments. For example, the Eötvös experiment has not been repeated, in spite of the tremendous improvement in experimental techniques now available. An improvement by a factor of 1,000 in the accuracy of this experiment should be possible, and it is a disgrace to experimental physics that this has not yet been done. It is a measure of our strong belief in the foundations of relativity - the principle of equivalence - that this has not been done; but an excellent example that our beliefs are not always correct was provided for us by the events of last week, when it was discovered that parity is not conserved in beta decay! This experiment could have been done much sooner, if people had thought it was worth doing.

Professor Wheeler has already discussed the three famous checks of general relativity; this is really very flimsy evidence on which to hang a theory. To go a little further back: the Eötvös experiment is in some ways very powerful; it is really remarkable that such wide ranging conclusions should be drawn from a single experiment. It measures the ratio of mass to weight for an object, and sees whether this varies with the atomic weight of the element involved, the binding energy of the atoms, and things of this kind. One finds that, to about one part in 10^8; the ratio is independent of the constitution of the matter. It does not prove exact equality of mass and weight, contrary to the assertion in one well-known text on relativity; it proves it to one part in 10^8, which is quite a different thing! To considerable accuracy, it also shows that electromagnetic energy has the same mass-weight ratio as the "intrinsic" mass of nucleons, since in heavy nuclei the electromagnetic contribution is an appreciable fraction of the total binding energy. On the other hand, gravitational binding energy is generally negligible and one cannot infer its constancy from the Eötvös experiment.

I would like now to refer to the table below, which may indicate where some of the troubles lie.

PHYSICAL AND ASTROPHYSICAL CONSTANTS

(Units of $\hbar$, c, m)

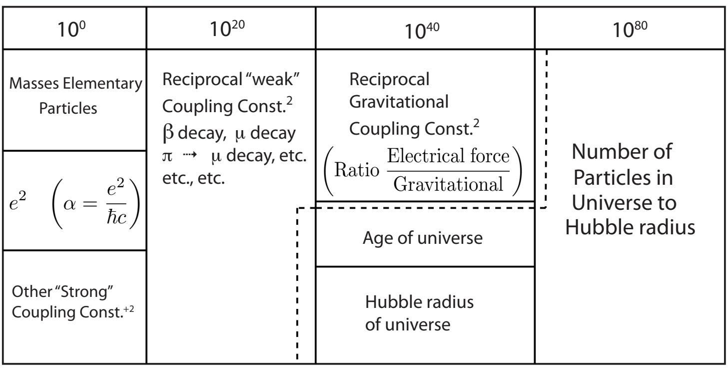

Figure 4.1: Physical and Astrophysical Constants (Units of $\hbar$, c, m)

These are the famous dimensionless numbers - and of course everyone will say, "He's fallen into *that* trap!" I've written in dimensionless form the important atomic and astrophysical constants, and arranged them according to order of magnitudes; we see that they group themselves in the remarkable way indicated. The masses of elementary particles, the fine structure constant, and other "strong" coupling constants are all of order unity (using the term "order of magnitude" rather loosely!). Then the "weak" coupling constants have reciprocals squared all of order 10^{20}. At 10^{40} we have the gravitational coupling constant; everything so far has been an "atomic constant." But also at 10^{40} we have the constants associated with the universe as a whole; the age and the Hubble radius; while at 10^{80} we have the number of particles out to the Hubble radius.

What is queer about these? The atomic numbers are queer because if we think nature is orderly and not capricious, then we would expect some day to have a theory from which these numbers would come out, but to expect a number of the order of 10^{40} to turn up as the root of our equation is not reasonable. The other thing we notice is the pattern: 10^{40} being 10^{20} squared, and 10^{80} being 10^{40} squared; and finally the strange equality of the "universal" constants - age of the universe, Hubble radius - to the gravitational coupling constant, which is "atomic" in nature.

What explanations exist for these regularities? First, and what ninety percent of physicists probably believe, is that it is all accidental; approximations have been made anyway, irregularities smoothed out, and there is really nothing to explain; nature is capricious. Second, we have Eddington's view, which I may describe by saying that if we make the mathematics complicated enough, we can expect to make things fit. Third, there is the view of Dirac and others, that this pattern indicates some connections not understood as yet. On this view, there is really only *one* "accidental" number, namely, the age of the universe; all the others derive from it.

The last of these appeals to me; but we see immediately that this explanation gets into trouble with relativity theory, because it would imply that the gravitational coupling constant varies with time. Hence it might also well vary with position; hence gravitational energy might contribute to weight in a different way from other energy, and the principle of equivalence might be violated, or at least be only approximately true. However, it is just at this point that the Eötvös experiment is not accurate enough to say anything; it says the "strong interactions" are all right (as regards the principle of equivalence), but it is the "weak interactions" we are questioning. I would like therefore to run through the indirect evidence we have. There would be many implications of a variation with time of the gravitational constant, and we must see what the evidence is regarding them.

Assuming that the gravitational binding energy of a body contributes anomalously to its weight (e.g., does not contribute or contributes too much), a large body would have a gravitational acceleration different from that of a small one. A first possible effect is the slight difference between the effective weight of an object when it is on the side of the earth toward the sun and when it is on the side away from the sun. This would arise from the slightly different acceleration toward the *sun* of a large object (the earth) and the small object. If we estimate his effect, it turns out to be of the order of one part in 10^{13} on g, which I think there is no hope of detecting, since tidal effects are of the order of two parts in 10^8. The mechanism of the distortion of the earth by tidal forces would have to be completely understood in order to get at such a small effect.

Another way of getting at this same effect would be to look at the period and orbit radius of Jupiter, and compare with the earth, to see if there is any anomaly. Here the effect would be of the order of one part in 10^8, which is on the verge of being measurable. Possibly by taking averages over a long period of time one can get at it; I haven't talked with astronomers and am not sure.

Another interesting question is this: are there effects associated with motion of the earth relative to the rest of the matter of the universe? All the classical "ether drift" experiments were electromagnetic; what about gravitational interactions? Could their strength depend on our velocity relative to the co-moving coordinate system? We can say something about this, assuming the effect to be of the order of β^2, where β is the ratio of earth velocity to light velocity. We know the velocity of the sun relative to the local galactic group; but we know nothing of the velocity of the galactic group relative to the rest of the universe, except that it is not likely to be much greater than 100 km/sec. Supposing it unlikely that the motion of the local group would be such as to just cancel the motion of the sun relative to it, we can say that the velocity of the sun relative to the rest of the universe is perhaps of the order of 100 km/sec. Then the annual variation in β^2, owing to the fact that the earth's velocity at one time of the year adds to and at another time subtracts from the sun's velocity, amounts to about one part in 10^7. This could conceivably give rise to an annual variation in "g" which could be detected, for example, by a pendulum clock. Now the best pendulum clocks are not quite up to this; but an improvement of a factor of ten would make this effect detectable, if it exists.

There is, however, an indirect way of getting at this effect: any annual variation of the gravitational interaction would give rise to an annual variation of the earth's radius, and hence of the earth's rotation rate. Now such an annual change is in fact observed. The earth runs slightly slow in the spring. The effect is, within a factor two, roughly what you might expect from assuming 100 km/sec. and putting in the known compressibility of the earth. However, it is possible to explain it also purely on the basis of effects connected with the earth itself, such as variations of air currents with seasons. Also the irregular character of the variation indicates at least some contribution from such factors.

GOLD: It varies from one year to the next a bit, so it is certainly not to be attributed to relativity.

DICKE: It's not too clear. Certainly part of it varies, but what the variation amounts to is not too obvious because its measurement depends on crystal clocks and they haven't been too good until recently so we can't go back far in time.

Now there is some other evidence on the question of a long period change in the gravitational interaction as we go back into the past. If gravity was stronger in the past, the sun would have been hotter, and we ask whether we can then account for the formation of rocks and creation of life. The accompanying figure (4.2) shows what we can say about the temperature of the earth.

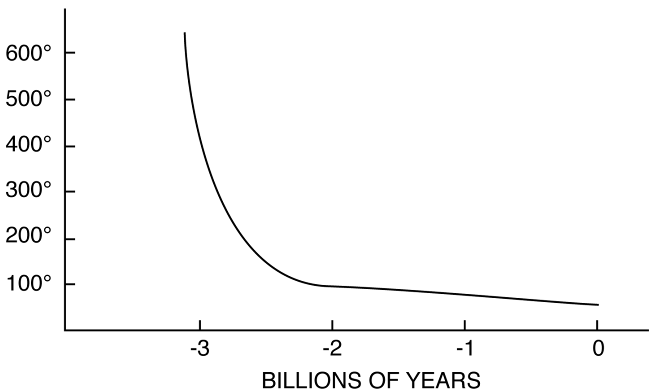

Figure 4.2

This figure gives the temperature of the earth as a function of time as we go into the past, "0" on the scale representing the present. I have assumed that as the temperature rises, the amount of water vapor present in the atmosphere increases, and the sky becomes completely overcast. Then if we use the known albedo of clouds (.8), and the black body radiation in the infra-red, we get the curve shown for the temperature, assuming the age of the universe to be 6.5 billion years. The temperature rises slowly to about 50° C one billion years ago. Evidence for life as we know it exists back to about 1.0 billion years. Life could have been present back 1.7 billion years, when the temperature would have been about 100° C. According to biologists, algae from hot springs are known capable of living at such temperatures, so life could have existed then. There is evidence for a fairly sharp cut-off on the existence of sedimentary rocks at about 2.7 billion years, even though the solar system is about 4 billion years old. This agrees with our curve, since at the temperature corresponding to that age, about half the water would be in the atmosphere, and the pressure would be so high that its boiling point would be about 300° C. There would still be enough liquid water to form sedimentary deposits. However, at slightly higher temperatures, the critical temperature of water would be exceeded, only vapor would exist, and sedimentary rocks would not have been formed.

I would say that the evidence does not rule out the possibility of the sun's having been hotter in the past.

There is some additional indirect evidence arising from the problem of the formation of the moon. The moon has a density so low that there are only two possible explanations: first, different composition from the earth; second, a phase change in the earth that leads to a very dense core. The second is, however, hard to believe, because if the core is assumed to be liquid iron, the total amount of iron in the earth is in agreement with what we believe to be the abundance of iron in the solar system as a whole. If we say the composition is different, we notice that it seems to be about the same as that of the earth's mantle. This in turn suggests Darwin's old explanation, that the moon flew out of the earth. The earth was formed first and was rotating with a period of about four hours, when as a result of tidal interaction with the sun, a large tidal wave was set up which split the earth into two parts; then as a result of tidal interaction between moon and earth, the moon gradually moved out to its present position. The latter part of this account is probably right, as we know from the evidence on slowing down of the rotation rate as deduced from comparison of Babylonian eclipse records with modern ones. However, if the earth was rigid then as it is now, the natural frequency of oscillation would have been too high for the first part of the account to be correct.

With a liquid earth, the period is more nearly right, but Jeffreys' objection, that turbulent dampening would prevent the buildup of a wave to the point of producing fission, arises. However, Professor Wheeler has pointed out recently that this objection may be met by including the effect of the magnetic field of the earth in damping the turbulence. Now the only difficulty is keeping the earth liquid, since the rate of radiation from a liquid earth is so great that it would freeze up in a hundred years - nowhere near a long enough time to set up the oscillation required. One thing which could keep it liquid would be a hotter sun!

There is another bit of evidence on this: the moon is distorted in such a way that it has been called a "frozen tide"; it corresponds somewhat to the shape it would have had if it had frozen when at about a quarter of its present distance from the earth.

GOLD: This is not correct; the present shape is not at all what it would have been at any closer distance.

DICKE: This is true, but there is the possibility that the moon was somewhat plastic at the time of freezing so that the biggest distortions would have subsided somewhat to give you dimensions compatible with what is now seen.

GOLD: If the moon were formed by a lot of lumps falling together, it would have an effect on how strong the moon is, and what disturbance it could bear; then it could have three unequal axes, as it does.

DICKE: Yes; this is the other explanation for the moon's shape, that big meteorites piled into it and made it lopsided.

Another piece of indirect evidence on this is connected with the problem of heat flow out of the earth. There is evidence for the earth's core being in convective equilibrium; and heat flowing out of the earth's core seems to be the only reasonable mechanism at present for a convective core. The question is how that heat gets out. It may be that there is radioactive material at the center which is the source of the heat flowing out; but potassium would be expected to be the biggest source of heat, and it is so active chemically that we expect to find it in the mantle only. If, however, gravity gets weaker with time, there would be a shift along the melting point curve of the mantle which would lead to a slow lowering of the temperature of the core, and heat flowing at such a rate as to keep the mantle near the melting point. If we put in a reasonable melting point curve for the mantle, we find this contribution to the heat flowing out of the earth is of the order of one-third of the total heat. So the evidence is not incompatible with the idea that gravity could be weaker with time, even though radioactive materials are of sufficient importance so that one would not attempt to account for the heat flow out of the earth *solely* on this basis.

There is some small bit of evidence that there may be something wrong with beta decay: the beta coupling constant may be varying with time. This comes from the evidence on Rubidium dating of rocks. The geologists on the basis of their dating of rocks assign a half life for Rb 87 decay of about 5×10^{10} years. There has been quite a series of laboratory measurements giving values of about 6×10^{10} years; on the other hand one particular group has consistently got a value lower than five. So this is up in the air; but I think that before long we will have a definitive laboratory value for the half-life.

Finally, l may just mention last week's discovery that parity is not conserved in beta decay. What the explanation for this is no one knows, but it could conceivably indicate some interaction with the universe as a whole.

DE WITT called for discussion.

BERGMANN: What is the status of the experiments which it is rumored are being done at Princeton?

DICKE: There are two experiments being started now. One is an improved measurement of "g" to detect possible annual variations. This is coming nicely, and I think we can improve earlier work by a factor of ten. This is done by using a very short pendulum, without knife edges, just suspended by a quartz fiber, oscillating at a high rate of around 30 cycles/sec. instead of the long slow pendulum. The other experiment is a repetition of the Eötvös experiment. We put the whole system in a vacuum to get rid of Brownian motion disturbances; we use better geometry than Eötvös used; and instead of looking for deflections, the apparatus would be in an automatic feed-back loop such that the position is held fixed by feeding in external torque to balance the gravitational torque. This leads to rapid damping, and allows you to divide time up so that you don't need to average over long time intervals, but can look at each separate interval of time. This is being instrumented; we are worrying about such questions as temperature control of the room right now, because we'd like stability of the temperature to a thousandth of a degree, which is a bit difficult for the whole room.

BARGMANN: About three years ago Clemence discussed the comparison between atomic and gravitational time.

DICKE: We have been working on an atomic clock, with which we will be able to measure variations in the moon's rotation rate. Astronomical observations are accurate enough so that, with a good atomic clock, it should be possible in three years' time to detect variations in "g" of the size of the effects we have been considering. We are working on a rubidium clock, which we hope may be good to one part in 10^{10}.

BONDI: One mildly disturbing fact is this: the direction to the center of the galaxy also lies in the plane of the ecliptic. Why should this be so? I see no relation to the effects you have been discussing, but this might be included in the list of slightly curious and unexplained facts about the universe.

ANDERSON: There has been some discussion of the possibility that elementary particles have no gravitational field - or that there exist both "active" and "passive" gravitational mass.

GOLD: I would strongly advise the Eötvös experiment with a lump of anti-matter!

DICKE: One should also try seeing whether positronium falls in a gravitational field - but how can one do it?

GOLD: I think a more relevant experiment is to do it with anti-protons or anti-neutrons rather than positronium. Wilson at Cornell thinks it could be done - for a few million dollars.

DICKE: Is there some reason why it is better to do it with anti-protons rather than positrons?

GOLD: A scheme is possible according to which - not that I believe it - anti-gravity would be possessed only by particles of a specific gravitational charge; then it would not be possible to have more than one type of such charge, and you would have to assign it to the nucleon.

WITTEN: We are starting at our laboratory some experiments to measure precisely the time dilation effect predicted by the special theory of relativity. Two sets of observations exist regarding this prediction. One is the famous experiment of Ives and Stilwell who observed the effect by measuring the shifts of spectral lines omitted by moving atoms; the other is the various measurements on the lifetime of the μ-meson in different frames of reference. Neither method has so far been carried to its highest attainable accuracy.

We are going to do the experiment using basically the techniques of Ives and Stilwell with improvements. We shall observe lines emitted or absorbed by hydrogen, helium, or lithium atoms or ions. The expected accuracy at velocities comparable to those used by Ives and Stilwell should be a factor of about 100 greater than theirs. By going to higher velocities the relative accuracy should be greater. We hope to measure the angular dependence of the effect; it has been measured so far only in one direction. lves and Stilwell[1] have observed in their experiment a tendency towards disagreement with theory at high velocities and have suggested a possible source of experimental error that leads to this tendency. This point should be investigated. We shall eventually extend the measurements to v/c large enough to detect effects of the fourth order $(v/c)^4$. We hope to develop techniques sufficiently well to do a precise measurement of the effect at highly relativistic velocities. Another goal is to make the time dilation measurement for ions moving in a magnetic field and being accelerated. The purpose here is to see if there is any shift in the spectral line of a kinematic origin due solely to the acceleration of the clock with respect to the observer.

[1] *J. Opt. Soc. Am.* **31**, 369 (1941)

Session II Unquantized General Relativity, Continued

Chairman: P. G. Bergmann

BERGMANN, who opened the session, proposed that the following topics should be discussed together in this session: the integration of the partial differential equations of general relativity, the properties of their solutions, the invariants of the theory, and various topological problems.

LICHNEROWICZ was the first speaker. For details, including those of notation and terminology, the reader is referred to his book, *Théories Relativistes de la Gravitation et de l'Électromagnétisme*, Paris, 1955.

Chapter 5
On the Integration of the Einstein Equations

André Lichnerowicz

This is a survey of the main results on the integration of the Einstein equations and the problems which remain open.

5.1 The Space-Time Manifold

In any relativistic theory of the gravitational field, the main element is a differentiable manifold of four dimensions, the space-time V_4. To be precise: I assume that this differentiable structure is "C^2, piecewise C^4." This means that in the intersection of the domains of two admissible systems of local coordinates the local coordinates of a point for one system are functions of class C^2, i.e., possess continuous derivatives up to second order, with non-vanishing Jacobian, of the coordinates of the point in the other system. Third and fourth derivatives also exist, but are only piecewise continuous.

On V_4 we have a hyperbolic normal Riemannian metric

$$ds^2 = g_{\alpha\beta} dx^\alpha dx^\beta \tag{5.1}$$

which is everywhere "C^1, piecewise C^3." This metric is said to be *regular* on V_4. Note that the manifolds which admit hyperbolic metrics are precisely those on which there exist vector fields without zeros. Such a manifold admits global systems of time-like curves, but generally does not admit global systems of space-like hypersurfaces.

We suppose that no further specification of the differentiable structure of the manifold or of the metric has any physical meaning.

For the study of global problems assumptions belonging to the differential geometry in the large are often needed. Frequently V_4 is identified with a topological product $V_3 \times \mathbb{R}$, $\mathbb{R}$ being the real numbers, where straight lines are time-like curves and thus trajectories of a unitary vector field u. The space sections defined by V_3 are then generally either closed manifolds or complete Riemannian manifolds for the metric defined on them by

$$ds^{*2} = (g_{\alpha\beta} - u_\alpha u_\beta) dx^\alpha dx^\beta \tag{5.2}$$

moreover, in this case V_4 admits a Minkowskian asymptotic behavior at infinity.

The problem whether these assumptions are necessary or sufficient is open.

5.2 The Cauchy Problem for the Gravitational Field

In general relativity the metric ds^2 satisfies the Einstein equations:

$$S_{\alpha\beta} \equiv R_{\alpha\beta} - \frac{1}{2} g_{\alpha\beta} R = \chi T_{\alpha\beta} (\alpha, \beta = 0,1,2,3) \tag{5.3}$$

where the energy-momentum tensor $T_{\alpha\beta}$ is piecewise continuous. It defines the sources of the field. In the regions where $T_{\alpha\beta} = 0$ we have the “exterior case.”

Let Σ be a local hypersurface of V_4 which is not tangent to the cones $ds^2 = 0$. If $x^0 = 0$ defines Σ locally, we have $g^{00} \neq 0$. The Einstein equations can then be written as[1]

$$R_{ij} \equiv \frac{1}{2} g^{00} \partial_{00} g_{ij} + F_{ij} = \chi (T_{ij} - g_{ij} T) \quad (i, j = 1,2,3) \tag{5.4}$$

$$S^0_\alpha \equiv G_\alpha = \chi T_\alpha \tag{5.5}$$

where F_{ij} and G_α are known functions on Σ if we know the values of the potentials $g_{\alpha\beta}$ and their first derivatives (“Cauchy data”).

Eq. (5.5) links the Cauchy data to the sources. Eq. (5.4) gives the values on Σ of the six second derivatives $\partial_{00} g_{ij}$ which I have called the *significant derivatives*. The four derivatives remain unknown and may be discontinuous at Σ. But, according to our differentiable structure, these discontinuities have no physical or geometrical meaning. We grasp here the connection between the covariance of the formalism and the assumptions on the structure.

From Eq.(5.4) one sees that the significant derivatives for Σ can have discontinuities only if Σ is tangent to the fundamental cones, or if the energy-momentum tensor is discontinuous at Σ. Gravitational waves are defined as the hypersurfaces $f(x^\alpha) = 0$, tangent to the fundamental cone, i.e., such that

[1] We use the notation $\partial_\alpha = \frac{\partial}{\partial x^\alpha}$, $\partial_{\alpha\beta} = \frac{\partial^2}{\partial x^\alpha \partial x^\beta}$.

$$\Delta_1 f \equiv g^{\alpha\beta} \partial_\alpha f \partial_\beta f = 0. \tag{5.6}$$

(WHEELER pointed out, as a matter of terminology that what is called "gravitational wave" by Lichnerowicz is a purely geometrical concept, like the eikonal in optics, and not the same as the physical "gravitational waves." The question whether gravitational *radiation* does or does not exist was aired thoroughly in later sessions.) Gravitational rays are defined as the characteristics of $\Delta_1 f = 0$, or as the singular geodesics of the metric.

In the hydrodynamic interior case the exceptional hypersurfaces are (1) the gravitational waves, (2) the manifolds generated by streamlines, and (3) the hydrodynamic waves.

5.3 Local Solutions for the Exterior Case

In the exterior case, $T_{\alpha\beta} = 0$, the system of Einstein equations has the involution property: If a metric satisfies Eq. (5.5) and, on Σ only, the equations $S^0_\alpha = 0$, then it satisfies these equations also outside of Σ. This is a trivial consequence of the conservation identities. Thus, for a space-like hypersurface the Cauchy problem leads to two different problems:

1. The search for Cauchy data satisfying the system $S^\beta_\alpha \partial_\beta f = 0$ on $\Sigma(f = 0)$. This is the *problem of the initial values.*

2. The *evolutionary problem* of integrating Eq. (5.4) subject to these Cauchy data.

Assuming only differentiability, Mme. Fourès has recently solved this second problem locally by considering a difficult system of partial differential equations. She uses isothermal coordinates (for which $\Delta_x \rho = 0$) and thus proves the local existence and physical uniqueness theorem for the solution of the Cauchy problem of the Einstein equations. The characteristic conoid generated by the singular geodesics which issue from a point x plays the essential part, and it is clear that the values of the solution in x depend only on the values of the Cauchy data in the part of Σ interior to the characteristic conoid. We obtain thus all the results of a classical wave propagation theory.

5.4 The Problem of the Initial Values

This, most interesting, problem is simple if Σ is a minimal hypersurface. If

$$ds^2 = V^2(dx^0)^2 + g_{ij}dx^i dx^j \quad (i,j = 1,2,3) \tag{5.7}$$

we take

$$\Omega_{ij} = \frac{1}{2}\partial_0 g_{ij}, \quad K = \Omega_i^i, \quad H^2 = \Omega_{ij}\Omega^{ij} \tag{5.8}$$

and we obtain for the exterior case

$$K = 0, \quad \nabla_j \Omega^{ij} = 0, \quad \overline{R} + H^2 = 0 \tag{5.9}$$

where ∇ is the operator of covariant differentiation, and $\overline{R}$ the scalar curvature of Σ. If we set

$$g_{ij} = \exp(2\theta)\, g^*_{ij} \tag{5.10}$$

where we assume the metric $ds^{*2} = g^*_{ij}dx^i dx^j$ to be known on Σ, then it is possible to introduce the functions

$$\varphi^2 = e^\theta, \quad \Pi^{ij} = e^{5\theta}\Omega^{ij}, \quad L^2 = g^*_{ik}g^*_{jl}\Pi^{ij}\Pi^{kl}. \tag{5.11}$$

For these functions a very simple system of equations holds:

$$g^*_{ij}\Pi^{ij} = 0 \qquad \nabla^*_j \Pi^{ij} = 0 \tag{5.12}$$

and the elliptic equation:

$$-8\Delta^*\varphi + R^*\varphi = -\frac{L^2}{\varphi^7}. \tag{5.13}$$

The Dirichlet problem for Eq.(5.5) admits solutions if the domain of Σ is sufficiently small or if $R^* \leq 0$ and if L^2 is sufficiently small. This study can be extended to some hydrodynamic interior cases, and it is thus possible to construct examples of Cauchy data corresponding to the motion of n bodies. For the two-body problem, Newton's law is obtained approximately.

Recently Mme. Fourès has extended my method to the general case.

It would be important to obtain new global theorems for this problem, currently perhaps the most important problem of the theory.

5.5 The Cauchy Problem for the Asymmetric Theory

The Cauchy problem of the asymmetric theory was studied in collaboration with Mme. Maurer.

On the differentiable manifold V_4 we have:

1. An asymmetric tensor field $g_{\alpha\beta}$ of class "C^1, piecewise C^3," with a non-vanishing determinant g, the associated quadratic form defined by $h_{\alpha\beta} = g_{(\alpha\beta)}$ being hyperbolic normal.

2. An affine connection of class "C^0, piecewise C^2;" S_α is the torsion vector of the connection.

If we substitute into this connection the connection L without torsion vector, which admits the same parallelism, the field equations deduced from the classical variational principle for the first connection give two partial systems. According to Mme. Tonnelat and Hlavatý, the first system gives the connection L from $g_{\alpha\beta}$ and the first derivatives of the tensor. The field is now defined by $g_{\alpha\beta}, S_\alpha$ which satisfy the equations

$$R_{\alpha\beta} - \frac{2}{3}(\partial_\alpha S_\beta - \partial_\beta S_\alpha) = 0, \quad \partial_\rho (g^{[\rho\beta]}\sqrt{|g|}) = 0 \tag{5.14}$$

where $R_{\alpha\beta}$ is the Ricci tensor of L. In addition we have a normalization condition for S_α:

$$\partial_\alpha (g^{(\rho\beta)}) S_\beta \sqrt{|g|}) = 0. \tag{5.15}$$

The Cauchy problem can now be investigated. The system obtained still possesses the involution property. The field waves are hypersurfaces tangent to one of the following two cones:

(a) If $l^{\alpha\beta} = g^{(\alpha\beta)}$ we have the cone

$$(C_1) \;\; l_{\alpha\beta} dx^\alpha dx^\beta = 0 \tag{5.16}$$

where $l_{\alpha\beta}$ is dual to $l^{\alpha\beta}$.

(b) If $\gamma^{\alpha\beta} = \frac{2h}{g} h^{\alpha\beta} - l^{\alpha\beta}$ ($h = \det h_{\alpha\beta}$) we have the cone

$$(C_2) \;\; \gamma_{\alpha\beta} dx^\alpha dx^\beta = 0 \tag{5.17}$$

where $\gamma_{\alpha\beta}$ is dual to $\gamma^{\alpha\beta}$.

If the skewsymmetric part of $g_{\alpha\beta}$ is small compared to the symmetric part, then C_2 contains C_1. $h_{\alpha\beta}$ itself has no wave properties in the unified field theory.

The evolutionary problem of the asymmetric theory remains unsolved.

5.6 Global Solutions and Universes

I now return to general relativity. The main question of the theory is the following: When is a gravitation problem effectively solved?

A model of the universe - or shortly, "a universe" - is a V_4 with a regular metric satisfying the Einstein equations and certain asymptotic conditions. When $T_{\alpha\beta}$ is discontinuous through a hypersurface $\Sigma(f=0)$ we assume that ds^2 is always "C^1, piecewise C^3" in the neighborhood of Σ. The joining of the different interior material fields with the same field causes the interdependence of the motions, and the classical equations of motion are due to the continuity through Σ of the four quantities

$$V_\alpha = S_\alpha^\beta \partial_\beta f. \tag{5.18}$$

In this view the main problem is to construct and study universes. This being a hyperbolic non-linear global problem, it is very difficult to do this. Clearly global solutions of the problem of the initial values would be very helpful here.

Another approach might be the study of some elementary global solutions of the Einstein equations. It appears that such solutions are connected with the solutions with singularities introduced by various authors.

5.7 Global Problems

This view of the universe leads us to the following question: Is it possible to introduce in a universe new energy distributions, the interior fields of which are consistent with the exterior field of the universe? Furthermore, if a universe model is defined by a purely exterior field, regular everywhere, is the universe empty and thus locally flat?

The regularity problem is the study of the exterior fields regular everywhere under some general topological or geometrical assumptions. Good results are known only in the stationary case. We assume $V_4 = V_3 \times \mathbb{R}$, and we use the explicit assumptions made at the beginning of this survey. Then we have the following results:

1. An exterior stationary field which is regular everywhere is locally flat

(a) if V_3 is compact, or

(b) if V_3 is complete and has a Minkowskian asymptotic behavior.

2. In a stationary universe the exterior field extended through continuity of the second derivative into the interior of the bodies is singular in the interior.

3. A stationary universe which admits a domain surrounding infinity and which in this domain has a Minkowskian asymptotic behavior and for which the streamlines are time lines, is static. There exist space sections orthogonal to the time lines. This is of interest in the Schwarzschild theory.

Much less is known in the non-stationary case, which is of course more interesting. Concerning the regularity problem, some counterexamples have been constructed, especially by Racine, Taub and Bonner. But for these examples either the existence of the solutions is certain only in a finite interval of time, or the constructed solutions do not behave Minkowskian asymptotically. The general regularity problem is still an open one.

However, the results on the problem of the initial values give new theorems under the following assumptions:

(a) It is possible to diagonalize the matrix Ω_{ij} which defines the second fundamental quadratic form of Σ and there exist foliations of Σ by the corresponding system of curves, that is to say by curvature lines.

(b) ds^{*2} has a flat asymptotic behavior.

(c) The tensor Ω_{ij} has a summable square on Σ.

If these assumptions are satisfied on $\Sigma(x^0 = 0)$ and also on the sections corresponding to arbitrarily small x^0, the universe is static and thus locally flat.

***

MISNER first stressed the physical ideas in which he and Wheeler were interested and which had led them to inquire into the details of the Einstein-Maxwell equations, particularly whether or not they are singular.

"This comes about because, according to work of Rainich,[2] the Einstein-Maxwell equations can be interpreted as an *already unified* field theory. Explicit mention of the electromagnetic field is not necessary even while

[2] G. Y. Rainich. *Trans. Am. Math. Soc.* **27** 106 (1925).

working with a system equivalent in all cases, except the null electromagnetic field, to the Einstein-Maxwell set of equations. One aim of unified field theory has always been the notion that fields are more fundamental than particles, and that it should be possible to construct all particles from the purely geometrical concept of the field. To allow singularities in the fields to represent the particles would be a delusion, since then the stress tensor is not merely the electromagnetic one but includes mass terms, even though idealized to delta functions. Since we wish to be careful about singularities the results of Mme. Fourès and Lichnerowicz[3] have been important to us. They assure us that if we specify certain initial conditions we shall have non-singular solutions at least for a short time. We are interested in finding solutions to these initial value equations and seeing what they lead to, whether they indicate any possibility of constructing particles from these electromagnetic and gravitational fields. Notice that even though one speaks of the electromagnetic field here, one does not really have to, since one can use the ideas of Rainich to give a complete description of the electromagnetic field in terms of metric quantities alone. The influence of the electromagnetic stress energy tensor upon the gravitational field is sufficiently specific so that from the curvature quantities one can work backwards to the electromagnetic field and have a purely metric description of both electromagnetism and gravitation.

"We have studied a certain class of solutions to the problem of initial values. These solutions are global but by no means the most general. They serve as examples. These solutions are always continuous and instantaneously static. We find that there exist exact solutions of the Einstein-Maxwell equations for which the fields on a certain surface $t = 0$ are given by:

$$ds^2 = -dt^2 + (\chi^2 - \Phi^2)^2(dx^2 + dy^2 + dz^2) \tag{5.19}$$

$$F_{i0} = 2(\chi^2 - \Phi^2)^{-1}(\Phi\frac{\partial\chi}{\partial x^i} - \chi\frac{\partial\Phi}{\partial x^i}) \tag{5.20}$$

$$F_{ij} = 0 \tag{5.21}$$

$$\frac{\partial g_{\mu\nu}}{\partial t} = 0 \tag{5.22}$$

where χ, Φ satisfy

[3] A. Lichnerowicz, *loc. cit.*, p. 50.

$$\nabla^2\chi = \nabla^2\Phi = 0, \qquad \nabla^2 = \frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2} + \frac{\partial^2}{\partial z^2}. \tag{5.23}$$

This metric together with the electric field F_{i0} gives a solution of the initial value equations. In the neighborhood of the surface $t = 0$ we have a solution of the time-dependent equation but do not know what it is. Perhaps one should use a high speed computing machine. The regularity conditions of Lichnerowicz, that the three-dimensional manifold should be either compact or complete but asymptotically flat, lead to fields which are completely free of singularities. The functions χ and Φ can only have the form:

$$\chi = 1 + \sum_a \frac{\alpha_a}{\gamma_a} \qquad \Phi = \sum \frac{\beta_a}{\gamma_a} \tag{5.24}$$

where

$$\gamma_a = |\overrightarrow{\gamma} - \overrightarrow{a}| \qquad \alpha_a \geqslant |\beta_a| \qquad \gamma = \sqrt{x^2 + y^2 + z^2}. \tag{5.25}$$

These functions appear to contain singularities, but they really do not. This brings us into the study of the topology of this situation. The point at which the metric seems to become singular can be thrown away. This is consistent with the requirement of completeness, but would not be consistent if the point which is thrown away were one at which the metric did not become singular. A picture can be drawn to indicate what the space so obtained looks like. Near the point $\overrightarrow{a}$ the space, which is flat further away, curves and flares out and goes over into another roughly flat portion like this:

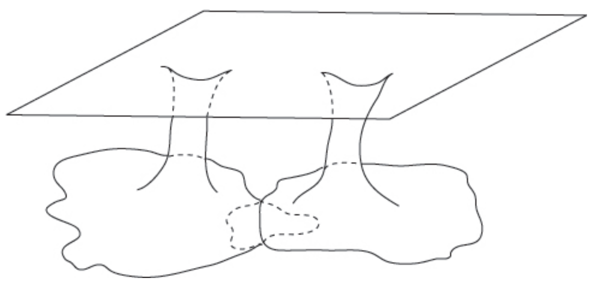

Figure 5.1

We have here as many regions where the space is asymptotically flat as we have particles. The next problem is to put these portions together. One should then try to construct the initial values which would give rise to the wormhole picture of Wheeler (see Session 1). All these things are possible; they just have to be worked out.

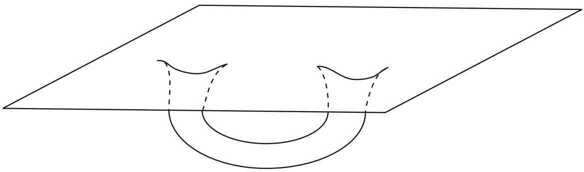

Figure 5.2: “Wormhole”

“One additional advantage of the solutions exhibited here over introducing point singularities into the theory is that by using Lichnerowicz’s regularity conditions you eliminate automatically all negative masses, mass dipoles and higher multipoles. It also, unfortunately, implies that for any particle or cluster of particles described by these deformities the total charge and mass have a ratio $e/m \leq 1$, while for an electron in these units, where $G = c = 1$, $e/m = 2.04 \times 10^{21}$. The reason for this too large mass, for a given electron charge, is that you don’t have enough of a cut-off for the electromagnetic mass. Perhaps one might hope that quantum mechanics might help here. In any event, we have here strong indications that the Einstein-Maxwell theory is a good model for a unified field theory. It can tell us what a unified field theory may have to say about physics.”

In the discussion, PIRANI asked for the definition of “completeness.” MISNER said that a manifold is complete if all geodesics can be continued to infinite length.

BERGMANN compared the present topology with the symmetric one of Einstein and Rosen.[4]
DE WITT noted the similarity between Misner’s model and the Schwarzschild solution for a charged mass particle. MISNER agreed and said that he had been led to his solutions by the observation that the space part of the Schwarzschild metric can be written in the form

[4]A. Einstein and N. Rosen. *Phys. Rev.* **48** (73) (1935).

$$[(1+\frac{m}{2r})^2 - \frac{e^2}{4r^2}]^2(dx^2+dy^2+dz^2). \tag{5.26}$$

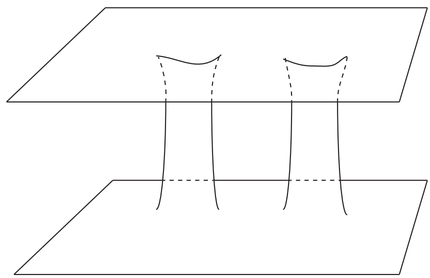

Figure 5.3: Einstein-Rosen Topology

MlSNER then added some remarks on global solutions.

Chapter 6
Remarks on Global Solutions
C. W. Misner

"Suppose we consider the following set of equations:

$$E^i_{;i} = 0 \quad (\operatorname{div} \overrightarrow{E} = 0) \tag{6.1}$$

$$H^i_{;i} = 0 \quad (\operatorname{div} \overrightarrow{H} = 0) \tag{6.2}$$

$$\varphi_i{}^j{}_{;j} = -\sqrt{g}\,\varepsilon_{ijk} E^j H^k \equiv S_i \quad \text{(Poynting vector)} \tag{6.3}$$

$$\frac{1}{2}\varphi^2 - \varphi^{ij}\varphi_{ij} + \frac{1}{4}R^{(3)} = \frac{1}{2}(E^2 + H^2) \tag{6.4}$$

where $\varphi = \varphi^i_i$ and $\varphi_{ij} = 1/4(\chi_{ij} - g_{ij}\chi)$

$$\chi_{ij} = \frac{\partial g_{ij}}{\partial t}.$$

We study these equations in order of difficulty. Eq. (6.1) has as its most general solution:

$$E^i = \frac{1}{\sqrt{g}} \frac{\partial}{\partial x^j}(\sqrt{g} A^{ij}) + E^i_{\text{Coulomb}} \tag{6.5}$$

where E^i_{Coulomb} has vanishing divergence and also vanishing curl:

$$E^{i,j} - E^{j,i} = 0 \tag{6.6}$$

but

$$Q \equiv \frac{1}{8\pi} \int \sqrt{g}\varepsilon_{ijk} E^i dx^j dx^k \neq 0 \ \text{ for some closed surface.} \tag{6.7}$$

This is only possible if "wormholes" are present. There are as many independent solutions as there are wormholes, and one can specify arbitrarily the charge of each. Eq. (6.3) can now be regarded as an inhomogeneous linear equation. A solution of the inhomogeneous equation exists on a compact manifold if and only if

$$\int K_i S^i \sqrt{g} d^3x = 0 \tag{6.8}$$

for every Killing vector K_i (generating a group of motions) satisfying

$$K_{i;j} + K_{j;i} = 0. \tag{6.9}$$

Whether this has any significant applications I do not know. About Eq. (6.4) I can say nothing.

“l conjecture that the homogeneous equation could be solved by the use of potentials, provided we use a rather broad idea of a potential. By this I mean a solution which somehow involves the use of an arbitrary function. To illustrate the idea I give a particular class of solutions to the equation $\varphi^{ij}_{;j} = 0$, namely:

$$\varphi_i^j = [\nabla_i \nabla^j - \delta_i^j \nabla_k \nabla^k - R_i^{(3)j}] f'(R^{(3)}) + \frac{1}{2} \delta_i^j f(R^{(3)}) \tag{6.10}$$

where f is an arbitrary function of the scalar curvature on the surface, and f' is the derivative of f. ∇_i is the covariant derivative.

“Besides the problems of topology in the small associated with “flares” and wormholes, or with the symmetric Einstein-Rosen picture, there are the problems of topology in the large. These are familiar problems: Is the universe closed and spherical in a reasonable approximation, or is it open? Other topological problems are the problems of classifying three-dimensional compact manifolds. One can at present write out a list containing all three-dimensional compact manifolds, but one has no way of telling whether two are the same or different by a finite algorithm, since the list may contain redundant cases. Most mathematicians seem to believe that the solution to this problem is not in sight.

“Another point is this: Once you take the course indicated here and decide to make a unified field theory to avoid singularities and you bring in the possibility of non-Euclidean topology, then you would like to know the limitations on the number of topologies that are likely to appear. The problem is not one of finding ingenious enough topologies to do what you want them to do, but rather to find a reason why a host of topologies will never appear. One must examine the 4-dimensional manifold. Professor Lichnerowicz has mentioned that for most manifolds which are not of the type $V_3 \times \mathbb{R}$ there will not exist reasonable space-like surfaces. However, the limitation $V_4 = V_3 \times \mathbb{R}$ might be too restrictive. In other words: Can the number of particles in a universe with wormholes change? For instance, one might ask if a wormhole can disappear in time. Can it happen that starting from a wormhole topology at $t = 0$ you arrive at a Euclidean space at $t = 1$?

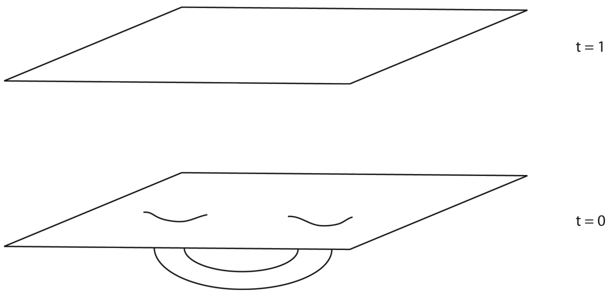

Figure 6.1

From the work of Thom[1] one can show that there are four-dimensional manifolds which have these two surfaces as boundaries. The question whether a continuous metric of Lorentz signature can be put on such manifolds can, I believe, be answered. In such a metric can you find solutions to any hyperbolic equations, or better, Maxwell's equations? Anyone trying to study this would be much aided by certain qualitative classifications of metrics. I conjecture that if you divide the metrics which it is possible to put on a given manifold whose two boundaries are space-like, into homotopy equivalence classes, then ordinarily there will not be more than one of these classes which allows any solutions to a hyperbolic equation; i.e., most homotopy classes are inconsistent with hyperbolic differential equations."

MISNER concluded with a few remarks about local invariants, such as those given by Géhéniau[2] and others: "Instead of the string of components of the Ricci tensor, one looks at all the invariants which can be constructed from these. One possible use of these might be as variables in discussing the quantization of general relativity, since quantization seems to handle

[1] Comment *Math. Helv.* **28**, 17-86 (1954), *Theorem* IV, 13. Also see B. A. Rokhlin, *Doklady Akad. Nauk.* SSSR **81**, 355 (1951).

[2] J. Géhéniau and R. Debever, *Bull. Cl. des Sci., Acad. roy. Belg.*, 5^e Sér., **42**, (1956).

invariant quantities better than tensor components. Also it would be interesting to see *Gedankenexperiment* to measure each of these curvature invariants. This would be a beginning for making simple quantum mechanical arguments about measurability in general relativity. Perhaps Pirani could give a characterization of pure gravitational radiation in terms of these invariants?"

BERGMANN pointed out that in the study of the equations of general relativity today one seems to have a choice of obtaining either general local properties or special global theorems.

MME. FOURES then gave her solution of the problem of the initial values using Cartan's exterior differential calculus:

Chapter 7

Solving The Initial Value Problem Using Cartan Calculus

Y. Fourès

I write for the space-time manifold

$$ds^2 = \sum_{\alpha=0}^{3} \omega_\alpha^2 \tag{7.1}$$

where I take as the basis in my four-dimensional manifold one vector orthogonal to the initial hypersurface, so that

$$\omega_0 = V dx^0 \tag{7.2}$$
$$\omega_i = a_{ij} dx^j + \lambda_i dx^0 \quad (i, j = 1, 2, 3). \tag{7.3}$$

Then the differential relations for the field can be written in the form

$$S_{0k} = \nabla_h P_{hk} - \nabla_k P \tag{7.4}$$
$$S_{00} = \overline{R} + H^2 - P^2 \ , \ P = P_{hh} \ , \ H^2 = P_{hk} P_{hk} \tag{7.5}$$

where ∇_h is the covariant derivative, and P_{hk} is what Lichnerowicz in the orthogonal calls Ω_{hk}. (For further notation see Section 5.4 of the talk by Lichnerowicz earlier in this session.) If the coordinates were orthogonal we would have $\lambda_i = 0$, and hence

$$P_{hk} = \frac{1}{2v} \partial_0 g_{hk}. \tag{7.6}$$

The coefficients P_{hk} are the coefficients of the second fundamental quadratic form of the hypersurface Σ.

We are dealing with the purely gravitational case. To solve the differential equations I choose on the hypersurface three particular vectors, the eigenvector of the tensor P_{hk}, that is to say we will have

$$P_{hk} = 0 \quad \text{for} \quad h \neq k. \tag{7.7}$$

P_{11}, P_{22}, P_{33} are unknowns. The metric of the hypersurface Σ is

$$d\bar{s}^2 = \sum_i \overline{\omega}_i^2. \tag{7.8}$$

If I set $u_1 = P_{22} + P_{33}$, *et cycl.*, and if I use Lichnerowicz's idea to take the metric on the hypersurface conformal to a given metric, denoted by an asterisk:

$$d\bar{s}^2 = e^{2\theta} ds^{\star 2} \tag{7.9}$$

then I obtain the following system of equations:

$$\partial_1^\star u_1 - \partial_1^\star \theta (u_2 + u_3 - 2u_1) - j_{1kk}^\star u_1 = 0 \quad \text{etc.} \tag{7.10}$$
$$-4\triangle_2^\star \theta - 2(\partial_i^\star \theta)^2 + Le^{2\theta} + R^\star = 0 \tag{7.11}$$

where $\triangle_2$ is the Beltrami operator of second order, and where

$$L = H^2 - P^2\ , \quad P = P_{11} + P_{22} + P_{33}\ , \quad H^2 = P_{hk}P_{hk}. \tag{7.12}$$

"It is possible to solve this system of equations which are linear with respect to the highest derivatives. For instance, I can solve them by giving the values of the unknown u in my three-dimensional space Σ on a two-dimensional variety S, and I can solve then the Cauchy problem for the system which contains θ. A general iteration method can be used to solve the set of equations by writing them in the form of integral equations using Green's functions. If I give the value of θ on the boundary, and the values of the u on S, then I find the general solution of these equations. This is a rigorous mathematical treatment of the problem of initial values."

WHEELER pointed out the analogy with the electromagnetic case where the equation $div \vec{E} = 0$ yields $\vec{E}$ in all space if it is known over a two-dimensional surface. However, one would still like to have a potential which gives the field automatically without having to do any integration.

MME. FOURES, answering a question of MISNER, pointed out that the statement that the metric is conformal to a Euclidean one is an assertion, not an assumption, and that her method gives general solutions for an open space.

DE WITT asked if the Cauchy problem is now understood sufficiently to be put on an electronic computer for actual calculation. "Do we now know enough about constraints and initial conditions to do this at least for certain symmetrical cases?"

MISNER replied that you could take some of the particular solutions which he has given, and which start out as instantaneously static fields, or the interesting example of two particles which start to rotate about each other, and you could perform actual calculations. "You start out with a Euclidean metric and would use the method of Mme. Fourès' proof.[1] On the surface $z = 0$ you put

$$e^{\frac{\theta}{2}} = 1 + \frac{m}{r_1} + \frac{m}{r_2} \tag{7.13}$$

r_1 and r_2 being distances measured from two points in space. Then you assign initial conditions for the eigenvalues u. You want to specify u in such a way that the velocities of the two particles are initially like that:

$$\Downarrow \quad \Uparrow$$

Then you try to use the same method which was used for the general proofs to find particular solutions not just on $z = 0$ but through all space. These can be interpreted as two particles which are non-singular, or they can be thought of as the kind of $1/r$ type singularity of which one ordinarily thinks 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.

DE WITT 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."

[1] *J. Rat. Mech. Anal.* **2**, 951 (1956).

BERGMANN remarked on the general discussion of the afternoon that six years ago it was discovered that one can find a closed form for the Hamiltonian of the Einstein equations, and it was thought then that all troubles with the Cauchy problem for gravitational theory were over. "This is not so, for the simple reason that there are eight constraints for the twenty canonical variables. Of the remaining twelve variables eight have nothing to do with the problem, they correspond to an arbitrariness in the choice of coordinates, so that one is left with four canonical variables. Can you introduce new variables such that these variables could be set up arbitrarily? This is essentially a search for potentials in the general sense in which that term has been used here." BERGMANN is personally convinced that the search for locally defined potentials is probably hopeless, because they probably do not exist. "This is only a belief, of course."

ANDERSON commented on the problem of two-surface boundary conditions. He suggested that since it is possible to put the gravitational Lagrangian into a form in which the time derivatives of $g_{0\mu}$ do not appear, just as the time derivative of the scalar potential does not appear in the electromagnetic Lagrangian, it may be possible to break up g_{ij} in a fashion analogous to the separation of the vector potential in the electromagnetic case into transverse and longitudinal parts. Only one of these parts appears in those equations of motion which are free from second time derivatives. One could then initially and finally specify the part which does not appear in these equations, and obtain a complete solution. There may be one catch to this. There is reason to believe that one of the field equations, which do not depend on second time derivatives, is really the Hamiltonian of the theory. If that is the case one would not be allowed to specify independent initial and final values for the quantities just discussed.

Discussion of these points, which are closely related with the problem of quantization, was postponed until later.

WHEELER returned to the central problem of the afternoon, the initial value problem. He asked whether, leaving technical details aside, there is a point of view which would permit us to find our way around some of these subtleties.

"I would like to raise in this connection the question of the use of the variational principle itself. The variational principle in general relativity has a character rather different from that in electromagnetic theory. In the latter we have the square of the field variables, whereas in the gravitational case we have the curvature, i.e., the second derivatives of the potentials entering. This has always puzzled us: Why is the variational formulation of the gravitational field so different? I would like to emphasize that the convenience which one so often has sought in transforming by partial integration the second derivatives into first derivatives has concealed what the equations are trying to tell us. I would like to illustrate this by a trivial example. Take the free particle of ordinary mechanics. The variational principle is:

$$\delta \int_1^2 \frac{1}{2}\dot{x}^2 dt = 0. \tag{7.14}$$

How do we tell what to fix at the endpoints of the time interval? We do this by performing an integration by parts:

$$= \dot{x}\delta x|_1^2 - \int_1^2 \ddot{x}\delta x dt. \tag{7.15}$$

From this we draw two separate conclusions:

1. $\ddot{x} = 0$.

2. $\delta x = 0$ at the two endpoints of the motion, i.e., we must specify the coordinate x at the times 1 and 2. This is a time-symmetric specification. One is so accustomed to this pattern of thinking that when general relativity presents us with a different pattern we are not prepared for that. Therefore, let me introduce a problem which does have the pattern characteristic of general relativity, and let us see how we would face up to it. Consider this:

$$\delta \int_1^2 x\ddot{x} dt = 0. \tag{7.16}$$

   Traditionally one abhors these second derivatives in the problem, so one quickly transforms it into the previous problem. I propose to deal rather directly with the new problem like this:

$$= (x\delta\dot{x} - \dot{x}\delta x)|_1^2 + 2\int \ddot{x}\delta x dt. \tag{7.17}$$

The conclusions would now be:

1. $\ddot{x} = 0$.

2. $\delta\left(\frac{\dot{x}}{x}\right) = 0$, i.e., we are to specify the value of $\frac{\dot{x}}{x}$ at the two endpoints of the motion.

"In other words, we are led here to a different kind of specification of the problem. I merely want to point out that the variational problem itself has the property if we don't monkey with it, to tell us what it is that we should deal with."

"In the case of electromagnetism we can also let the variational principle tell us what the quantity is that we are concerned with. Note the equation:[2]

$$\delta \int_1^2 (E^2 - H^2)\, d\Omega = 0 = \left[\int \vec{E} \cdot \delta \vec{A}\, dS \right] |_1^2 - \text{volume integral} \tag{7.18}$$

The volume integral gives us the usual field equations. The boundary term tells us that we should specify $A_{\text{transverse}}$ at the limits. If you specify A itself, rather than its transverse part, you would be taking too seriously the demand that the boundary term must vanish. Clearly you don't have to be that hard on A. Analogous considerations can and have been applied to the general relativity by Misner and Fletcher."

MISNER outlined his present, tentative results on this question. He noted that one has not yet been able to specify the gravitational variables which are analogous to $A_{\text{transverse}}$ in the sense mentioned by Wheeler. "With arguments based on the normalization in Feynman integration one can single out the invariant variational principle:

$$\delta \int R\sqrt{-g}\, d\Omega = 0. \tag{7.19}$$

From this one gets the surface term of form:

$$\begin{aligned}
&\int [\gamma^{ij}\delta\chi_{ij} + \frac{1}{2}\chi_{ij}\delta\gamma^{ij}]\sqrt{-g}\; d^3x \\
&g_{ij}\gamma^{jk} = \delta_i^k \\
&\chi_{ij} = \frac{\partial g_{ij}}{\partial t} \qquad \text{if Gaussian coordinates are used.} \\
&\qquad\qquad\qquad \text{Otherwise the formula for } \chi_{\text{ij}} \text{ is long.}
\end{aligned}$$

The suggestion is that if you look at the surface term carefully you will find what the correct variables to use are, and what you can specify on the initial and final surface."

[2] We are using the notation of Landau and Lifshitz, *Classical Theory of Fields.*

MISNER summarized the discussion of this session: “First we assume that you have a computing machine better than anything we have now, and many programmers and a lot of money, and you want to look at a nice pretty solution of the Einstein equations. The computer wants to know from you what are the values of $g_{\mu\nu}$ and $\frac{\partial g_{\mu\nu}}{\partial t}$ at some initial surface, say at $t = 0$. Now, if you don't watch out when you specify these initial conditions, then either the programmer will shoot himself or the machine will blow up. In order to avoid this calamity you must make sure that the initial conditions which you prescribe are in accord with certain differential equations in their dependence on x, y, z at the initial time. These are what are called the “constraints.” They are the equations analogous to but much more complicated than $\operatorname{div} \overrightarrow{E} = 0$. They are the equations to which we have been finding particular solutions; and on the other hand, Mme. Fourès has shown the existence of more general kinds of solutions. Mme. Fourès has told us that to get these initial conditions you must specify something else on a two-dimensional surface and hand over that problem, the *problem of the initial values*, to a smaller computer first, before you start on what Lichnerowicz called the *evolutionary problem.* The small computer would prepare the initial conditions for the big one. Then the theory, while not guaranteeing solutions for the whole future, says that it will be some finite time before anything blows up.”

LICHNEROWICZ emphasized that one seeks to answer the question whether V_4 is compact and whether V_4 is orientable.

Chapter 8
Some Remarks on Cosmological Models

R. W. Bass and L. Witten

It is unusual to hypothesize that the four-dimensional space-time universe of general relativity is compact (i.e., "finite"). But in such a case several interesting conclusions can be drawn. In the first place, if the mass distribution is assumed to be continuous, so that the metric tensor has no singularities, then the Euler-Poincaré characteristic of the universe must be zero [6]. This implies, for example, that *the universe cannot be a four-dimensional sphere.* It also implies that *a finite universe cannot be simply-connected*, in the sense that the first Betti number cannot vanish. This is reminiscent of Professor J.A. Wheeler's non-simply connected models.)

In the second place, it seems to be generally known that *in a finite cosmology there must exist a closed curve in space-time whose tangent vector at every point is time-like.* Professor L. Markus has indicated a proof to us. Let V_4 denote the 4-manifold of the universe. Now, on V_4 construct a continuous, nowhere vanishing field of time-like vectors ([6], pp. 6-7; *cf.*[7], p. 207). By Birkhoff's fundamental theorem on the existence of recurrent orbits in compact dynamical systems [3], there must exist either an orbit of the type sought or else an "almost-closed" time-like orbit which can serve for the construction of such a closed orbit by an obvious procedure.

A more standard hypothesis, however, is that the universe V_4 is not compact, but is the topological product of the infinite real line (a time axis) with a 3-manifold V_3. The manifold V_3 is often assumed to be compact, and any local (hence experimentally verifiable) condition which implies compactness is of much interest. For example, if V_3 has constant curvature K then V_3 is compact if, and only if, K is positive ([4], pp. 84 and 203), and in this case is a 3-sphere if its first Betti number vanishes, and in general admits the 3-sphere as a covering space.

We wish to point out a new method for studying the topology of manifolds such as V_3 and V_4. This method consists of the construction of a continuous, nowhere vanishing, irrotational vector field on the manifold under consideration. Once such vector field has been constructed, we can assert that either the manifold is non-compact (i.e., open or "infinite"), or that it cannot be simply-connected.

We shall prove a slight generalization of this theorem; but first, let us note that a similar, but more restrictive and less easily applicable condition is a trivial consequence of Hodge's well-known theorem that the number of linearly independent harmonic vector fields on a compact Riemannian manifold is equal to its first Betti number. For if after constructing on our manifold an irrotational vector field (which is non-trivial but may vanish at more than one point), we then ascertain that it is also solenoidal (i.e., of vanishing divergence), then the vector field must be harmonic ([9], p. 56).

Theorem 1 (Hodge): Let V_n be an n-dimensional Riemannian manifold (with positive definite metric tensor), and let F denote a non-trivial class C^2 vector field defined on V_n. Suppose that the curl and the divergence of F both vanish identically; or equivalently, suppose that the field F satisfies the generalized Laplace equation for harmonic vector fields. Then, if V_n is compact, its first Betti number is not zero.

Corollary (Bochner-Myers): If V_n is orientable and has positive definite Ricci curvature throughout, then its first Betti number vanishes. ([9], p. 37).

Recall that the curl tensor of a vector field is independent of the metric tensor, and so is a non-metric notion. Accordingly, *the following theorem applies equally well to* V_4 *with its indefinite hyperbolic metric as to* V_4 *with its positive definite Riemannian metric.*

Theorem 2: Let V_n be an n-dimensional differentiable manifold, and let F be a continuous, class C^1 vector field defined on V_n. Suppose that F vanishes at most once and that its curl vanishes identically on V_n. Then either V_n is non-compact, or V_n is compact and its first Betti number does not vanish. In either case, of course, if F actually vanishes nowhere, the Euler-Poincaré characteristic of V_n is zero.

For non-vanishing F this theorem is a consequence of a more general theorem [1] which applies, for example, to manifolds with boundary. In fact, by a generalization to arbitrary flows of a theorem proved by Lichnerowicz for a very special class of flows ([5], p. 79), we can prove [2] that V_n is homeomorphic to the product of the real line with an $(n-1)$-dimensional space which is a connected subset of a V_{n-1}. But in the present case, because we are dealing with a manifold, there is a much simpler proof. We wish to thank Professor Kervaire for pointing out to us this simpler proof during the *Conference on the Role of Gravitation in Physics*. The proof runs as follows. If V_n is simply connected, then the generalized Stokes Theorem assures us that there exists on V_n a single-valued scalar potential function of which F is the gradient field. (See the survey of vector analysis in [8].) But if V_n is compact, this potential function must assume both its maximum and minimum values on M, and at these extreme points the gradient must vanish. This contradicts the hypothesis that F has at most one zero on V_n, and so proves the theorem.

It is possible that Theorems 1 and 2 have applications to the study of specific cosmological models. In fact, there are many ways of constructing on V_3, or on V_4 continuous vector fields which are unique once the indefinite metric (or set of gravitational potentials) for V_4 has been specified.

Professor J.A. Wheeler has pointed out to us an application of Theorem 2 to V_4.

Theorem 3: Consider the combined Einstein-Maxwell field theory on V_4. If the vector field

$$u_i = \varepsilon_{ijkl} R_m^{j;l} \frac{R^{mk}}{R_{pq}} R^{pq}$$

is defined everywhere and of class C^1 on V_4, and if u_i does not vanish more than once, then the universe V_4 cannot be compact.

The vector field u_i, which was defined by Dr. C. W. Misner, is essentially the gradient of the ratio (in a certain coordinate system) of the electric to the magnetic field strength. Dr. Misner has shown that Maxwell's equations imply that $u_{i;j} - u_{j;i} \equiv 0$. Hence, Theorem 3 follows from Theorem 2.

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Session III Unquantized General Relativity, Continued

Chairman: H. Bondi

Introductory remarks to the session on gravitational radiation were made by BONDI.

There are two quite different attitudes one can take toward the general theory of relativity. First, one can regard it principally as a theory of gravitation. Then one knows that it must be an open theory into which knowledge gained in other fields can be fitted. Secondly, one can take the attitude that relativity is more than other theories in that it says more than just something about gravitation; and then one supposes that there are somehow some wonderful clues hidden in it. The latter attitude, which is what the speakers of yesterday put forward, is one which Bondi cannot accept.

The theory of relativity is based on two principles: (1) the principle of equivalence, and (2) the general principle of relativity. The principle of equivalence, we all agree, is profound. The principle of general relativity, on the other hand, actually says nothing physical at all; it is purely a mathematical challenge, which has been used successfully in gravitational theory as a heuristic principle. On the basis of the first ideology, BONDI proceeded to discuss some of the principal questions in gravitational radiation.

The analogy between electromagnetic and gravitational waves has often been made, but doesn't go very far, holding only to the very questionable extent to which the equations are similar. The cardinal feature of electromagnetic radiation is that when radiation is produced the radiator loses an amount of energy which is independent of the location of the absorbers. With gravitational radiation, on the other hand, we still do not know whether a gravitational radiator transmits energy whether there is a near receiver or not.

Gravitational radiation, by definition, must transmit information; and this information must be something new. The picture of a gravitational transmitter Bondi has is a finite region of space, inside of which something is going on and outside of which space is empty. An example of a gravitational transmitter is a person sitting very quietly holding two dumbbells, who suddenly, unpredictably, starts taking exercise with them. What we want to know is what is the effect of his motion? Does it transmit information to other regions of space of what the person taking exercise is doing, and does it transmit energy? In connection with these questions, BONDI reports on some work of L. MARDER carried out at King's College. The following is a summary of Marder's results:

Chapter 9
Gravitational Waves
L. Marder, Presented by H. Bondi

9.1 Static Cylinder

The general external solution may be reduced to

$$ds^2 = r^{2c}dt^2 - r^{2(1-c)}d\varphi^2 - A^2r^{-2c(1-c)}(dr^2 + dz^2). \tag{9.1}$$

This is equivalent to Wilson's result (1920), who finds $c \cong 2M$ by considerations of geodesic motion.

Consider solutions inside the cylinder (radius $r = a$) corresponding to metrics of the type (not the most general)

$$ds^2 = e^{2\psi}dt^2 - r^2e^{-2\psi}d\psi^2 - e^{2\gamma-2\psi}(dr^2 + dz^2), \quad \text{(functions } r \text{ only)} \tag{9.2}$$

of which (9.1) is a particular case.

Suppose we take $\psi' = Xr$, $\gamma' = Yr^m$, where X and Y are constants determined by boundary conditions. Field equations give

$$\begin{aligned} p_1 = -p_3 &\cong \frac{C^2}{8\pi a^2A^2}u^2(1-u^{m-3}) \qquad u = r/a \\ p_2 &\cong \frac{C^2}{8\pi a^2A^2}u^2(1+mu^{m-3}) \quad x^1 = r, \quad x^2 = \varphi, \quad x^3 = z, \quad x^4 = t \ . \\ \rho &\cong \frac{C}{2\pi a^2A^2} \end{aligned}$$

C is small ($<< 10^{-10}$, say) since $C \cong 2M$. Pressures are therefore small compared with density which is positive and approximately constant. p_2 is larger than other pressures and balances gravitational attraction. Integrating ρ gives $M = \frac{C}{2} + O(C^2)$ where $O(C^2)$ depends on distribution. Considerations of periodicity of φ show $A = 1 + O(C^2)$. Other solutions for ψ', γ' give alternative but less interesting solutions. Geodesic motion shows that free particles outside the cylinder have, in general, an acceleration in the z-direction.

9.2 Periodic Waves

Take metric of form

$$ds^2 = e^{2\gamma - 2\psi}(dt^2 - dr^2) - e^{-2\psi}r^2 d\varphi^2 - e^{2\psi + 2\mu}dz^2, \tag{9.3}$$

with γ, ψ, μ functions r, t (not to be confused with previous γ, ψ). Rosen solves the case $\mu = 0$ [field equations $\psi_{rr} + (1/r)\psi_r - \psi_{tt} = 0$, $\gamma_t = 2r\psi_r\psi_t$, $\gamma_r = r(\psi_r^2 + \psi_t^2)$] in free space, and takes as periodic wave solution (denoted by $\overline{\psi}$, 8 on detailed paper).

$$\psi = AJ_0(wr)\cos wt + AN_0(wr)\sin wt \quad \text{(do not confuse } A \text{ with previous)}$$

and a corresponding γ which has a factor A^2 and contains an aperiodic term, $\frac{-2}{\pi}A^2wt$. We have superimposed the static exterior solution (transformed) on to Rosen'ssolution and extended to within the cylinder obtaining a system which is initially physical, for A of order C^2. The aperiodic term makes the system unphysical by changing the sign of the density at $r = O$. This is the first unphysical behavior of the system. The mass of the cylinder/unit length is initially $M \cong \frac{C}{2} - \frac{A^2}{2\pi}wt$, showing that M vanishes at $t = \frac{\pi C}{A^2 w}$. The density changes sign at $r = 0$ at $t = \frac{\pi C}{3A^2 w}$.

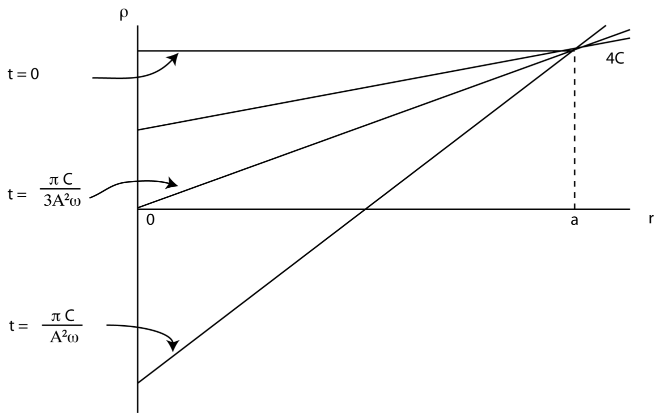

Figure 9.1

9.3 Pulse Waves

A solution of field equations relating to (9.3) is

$$\psi = \frac{1}{2\pi}\int_{-\infty}^{t-r} \frac{f(\beta)d\beta}{[(t-\beta)^2 - r^2]^{1/2}}$$

where $f(t)$ represents the source of the pulse.

If (1) $f(t)$ is of finite duration

or (2) $f(t)$ is of infinite duration with a finite number of sign changes,

$$\text{and } \int_{-\infty}^{\infty} f(t)dt \text{ is convergent}$$

or (3) $f(t)$ is of infinite duration with an infinite number of sign changes,

$$\text{and } \int_{-\infty}^{\infty} |f(t)|dt \text{ and } \int_{-\infty}^{\infty} |f'(t)|dt \text{ are both convergent}$$

then ψ, ψ_r, $\psi_t \to 0$, $t \to \infty$, for fixed r, showing, by boundary conditions, etc., that when the pulse has travelled to infinity the mass of the cylinder reverts to its value prior to the pulse. Considerations of a particular form of pulse show that the mass does change during early motion of the pulse. It is clear that ψ and derivatives are not of such a form as to allow the mass to return to its original value whatever the form of $f(t)$, as such functions as $f(t) = cos\, wt$ do not correspond to true pulses, but to travelling waves such as those considered above, which clearly radiate mass.

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DE WITT asked, "If the mass eventually returns to its original state, are there any waves left out in space?"

BONDI replied that the waves travel out toward infinity. They carry no energy with them. If one integrates the energy momentum tensor through the interior during the time the motion is going on, then while the wave is being sent out the mass is decreasing, but as the wave dies down the mass returns to its original value.

In answer to a question as to why it was necessary for the process of emission of gravitational waves to be so unpredictable, BONDI replied that then and only then is information being transmitted.

DE WITT added, “In other words, if I know at an initial time that I am going to give you a yes answer, then the field already contains it.”

BELINFANTE asked if there were any incoming waves. BONDI replied that there were no incoming waves in this example at all. He then continued with the analogy between electromagnetic and gravitational field: “To my mind the electromagnetic field is like money spent. I do not get it back unless someone is very charitable. The gravitational field is more like my breathlessness when I do my exercise. When I stop, I regain my breath. If I do not stop (as in the periodic case) I will collapse. In the finite case, which to my mind is the more physical one, no irreversible change has taken place.”

BELINFANTE asked if one would find, although one starts with no fields, as in this case, that at a certain time the energy ceases to go outward and starts to go inward. Bondi replied that he has not so far examined this, so he does not have the answer to the questions.

WHEELER remarked, “How one could think that a cylindrically symmetric system could radiate is a surprise to me. There seems to be a far-reaching analogy between this case and the problem of emission of electromagnetic radiation from a zero-zero transmission in an atom or nucleus. The charge can oscillate spherically symmetrically, but the system doesn’t radiate. However, if we have an electron in the neighborhood, internal conversion can take place, with still no electromagnetic radiation emitted. This would correspond to the uptake of energy of the gravitational disturbance created by the ‘cylindrical symmetric’ exercise of yours.”

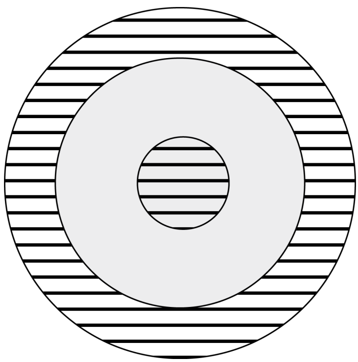

Figure 9.2

BONDI replied that he has had suspicions on that side also. To put it crudely, what stops the emission of electromagnetic radiation in the atom is the law of conservation of charge, and what stops gravitational radiation from taking place is the conservation of mass and of momentum. But he does not think there is necessarily anything against radiation of cylindrical symmetry. However, he hopes to be able to demonstrate some day that if one has a cylinder surrounded by a shell of matter one can transfer energy from the cylinder to the shell by means of cylindrically symmetric motions. This, he agrees, is required to complete the problem.

BONDI then reported on some of his own research. He takes a finite three-dimensional region in which there is mass, and considers the gravitational field at large distances from the mass. In this region the metric has the approximate form

$$g_{\mu\nu} = \delta_{\mu\nu} + \begin{pmatrix} t & -r \\ \theta_1 & \varphi \end{pmatrix} r^{-1} + \begin{pmatrix} & \\ & \end{pmatrix} r^{-2} + \cdots$$

The coefficients are functions of the variables in the brackets. He investigates what one gets if one proceeds to the approximation which includes the r^{-1} term. His strong impression is that this coefficient is independent of time. If we again consider the transmitter, only this time a much more general one (the system is three-dimensional) - but suppose the system to be spherically symmetrical both before and after emission - the "m" of the two Schwarzschild solutions is then the same: hence no energy will have been lost.

The next speaker was WEBER, who made some remarks on cylindrical waves, using the pseudo-tensor formalism. He considered the Einstein-Rosen metric, and its relevant solution. Because of the linear nature of the equation one can construct a pulse which, for example, can implode from large distances. This cylindrical pulse will have a metric which is well behaved at large distances and this implies immediately that the energy per unit length of this wave will be zero. This "horse sense" argument is bolstered by an exact calculation of the pseudo-tensor. If one calculates t_0^0 and t_1^0, he finds that they vanish everywhere. This has the consequence that energy cannot be transferred around as long as one has this type of symmetry and the above metric.

BONDI: "Where did you feed in the condition that the solution is well behaved at infinity?"

WEBER replied that he did not make use of this at all, but if it is well behaved at infinity, then nothing can happen. He continued: If one has a wave with energy per unit length which is zero, a particle put in the system will move in some fashion. Since this seems nonsensical, one thinks of some alternative. A possible one is that the total energy of the wave is not zero. He has shown that the energy of the entire disturbance is zero. He has also obtained an approximate solution of the Hamilton-Jacobi equation of a particle which interacts with this wave. The approximate solution says that if the particle is initially at rest, it will be at rest also after the disturbance has passed over it. The wave interacts with the particle in this respect like a conservative field - it takes no energy from the wave. In connection with the question of whether or not these solutions are trivial, he has calculated all of the components of the Riemann tensor, and has found that not all of them vanish.

BONDI remarked that it is vital, in this confusing subject, to make sure that one can physically detect what one is talking about. Also a single particle is a very poor absorber of any sort of energy.

WEBER replied that he thinks one could carry this sort of calculation out for a couple of particles.

BERGMANN remarked that if we try to bring in decent conditions at infinity, we are licked before we start. Also if we have a spatially limited area of disturbance, we cannot assume too low forms of symmetry because we are limited by four conservation laws, which have rigorous significance.

The next speaker was PIRANI. An attempt was made to formulate a definition of gravitational radiation in an invariant way. The definition was arrived at by making two assumptions: (1) gravitational radiation is characterized by the Riemann tensor, and (2) radiation must be propagated along the null cone. From this point of view, on a wave front one could expect to find a discontinuity in the Riemann tensor. One takes a space-time in which Lichnerowicz's conditions hold, and calculates the permitted discontinuity across the wave front in the Riemann tensor. To make a physical interpretation one introduces a vierbein at any space-time event; its interpretation is that the time-like vector is the observer's four-velocity, and the three space-like vectors are the Cartesian coordinate axes which he happens to be using at that time. In order to write down the permissible discontinuities, it is convenient to introduce the six-dimensional formalism. One re-labels the physical components $H_{\alpha\beta}$ of a skew tensor, regarding it as a six dimensional vector. The curvature tensor $R_{\alpha\beta\gamma\delta}$ can be similarly cast in the six dimensional formalism, appearing as a symmetric 6-tensor. If one imposes the empty space-time field equations one finds that its form must be

$$[R_{(\alpha\beta)(\gamma\delta)}] = \begin{bmatrix} P & Q \\ Q & -P \end{bmatrix}$$

where P and Q are symmetric 3×3 arrays. The spurs of both P and Q are zero. Lichnerowicz's continuity conditions permit the following array of discontinuities (taking the discontinuity across the surface $t - x = 0$):

$$\Delta P = \begin{bmatrix} \cdot & \cdot & \cdot \\ \cdot & -\sigma & -\Phi \\ \cdot & -\Phi & \sigma \end{bmatrix}, \quad \Delta Q = \begin{bmatrix} \cdot & \cdot & \cdot \\ \cdot & -\Phi & \sigma \\ \cdot & \sigma & \Phi \end{bmatrix} \quad ;$$

σ and Φ are independent numbers. This form is obtained by taking a particular choice of x and t axes. Here Φ can be made to vanish by a suitable choice of y and z axes. The above array, using the preceding definition, characterizes a gravitational wave front (or shock wave). The remarks from here on refer only to the pure gravitational field, which is analogous to the pure electromagnetic field.

In the electromagnetic case, one can define an invariant Poynting vector

$$P_\rho = (\delta_\rho^\nu - v_\rho v^\nu) T_{\mu\nu} v^\mu .$$

If the field is not of the self conjugate type, then an observer following the field can make the Poynting vector vanish by acquiring a suitable 4-velocity. If it *is* self-conjugate, then the field contains pure radiation, and the Poynting vector vanishes only if the observer acquires the velocity of propagation of the wave.

Similarly, in the gravitational case one can define a certain timelike eigenvector, in terms of the Riemann tensor. This vector is interpreted as the 4-velocity of an observer following the field, and if for some field this vector collapses onto the null cone, one has radiation. The eigenvectors of the gravitational field are defined with the aid of Petrov's classification of empty space-time Riemann tensors into three canonical types. These are:

$$\mathbf{I}: \quad P = \begin{bmatrix} \alpha_1 & \cdot & \cdot \\ \cdot & \alpha_2 & \cdot \\ \cdot & \cdot & \alpha_3 \end{bmatrix} \qquad Q = \begin{bmatrix} \beta_1 & \cdot & \cdot \\ \cdot & \beta_2 & \cdot \\ \cdot & \cdot & \beta_3 \end{bmatrix} \qquad \Sigma_\alpha = \Sigma_\beta = 0$$

$$\mathbf{II}: \quad P = \begin{bmatrix} -2\alpha & \cdot & \cdot \\ \cdot & \alpha - \sigma & \cdot \\ \cdot & \cdot & \alpha + \sigma \end{bmatrix} \qquad Q = \begin{bmatrix} -2\beta & \cdot & \cdot \\ \cdot & \beta & \sigma \\ \cdot & \sigma & \beta \end{bmatrix}$$

$$\mathbf{III}: \quad P = \begin{bmatrix} \cdot & -\alpha & \cdot \\ -\alpha & \cdot & \cdot \\ \cdot & \cdot & \cdot \end{bmatrix} \qquad Q = \begin{bmatrix} \cdot & \cdot & \sigma \\ \cdot & \cdot & \cdot \\ \sigma & \cdot & \cdot \end{bmatrix}$$

The α's and β's are independent scalar invariants of the Riemann tensor. For example, in the Schwarzschild solution, which is of the first type,

$$-\frac{1}{2}\alpha_1 = \alpha_2 = \alpha_3 = \frac{m}{r^3}, \qquad \beta_r = 0.$$

The Einstein-Rosen metric gives a Riemann tensor of the second type. Using these forms one may work out the eigen 6-vectors of the Riemann tensor, and if one takes the intersections of the corresponding 2-planes in pairs, one obtains 4-vectors, the Riemann principal vectors. The definition of gravitational radiation is that if the Riemann tensor is of the second or third type, one has radiation, and if it is of the first type, one does not have radiation. It can be shown that the difference between the non-radiation type and one of the radiation types can be made to correspond to the discontinuity in the Riemann tensor across a wave front permitted by Lichnerowicz's conditions. (Details of this work are to be published in the *Physical Review*, February 1957.)

Next, SCHILD presented some remarks on an apparent possibility to get indirect information on gravitational radiation by looking for gravitational radiation reaction force. The idea is built on an analogy to electromagnetic radiation reaction force. In the electromagnetic case, the equations of motion of a particle are

$$m\ddot{x}^\mu - e\dot{x}^\nu \overset{\text{ext}}{F^\mu_\nu} = \frac{2}{3}e^2(\dddot{x}^\mu - \dot{x}^\mu \ddot{x}^\nu \ddot{x}_\nu).$$

One proceeds by analogy and attempts to write the geodesic equations in the gravitational case as

$$m\frac{d^2x^\mu}{ds^2}+m\left\{\begin{array}{c}\mu\\ \alpha\,\beta\end{array}\right\}\frac{dx^\alpha}{ds}\frac{dx^\beta}{ds}=m^2A^\mu$$

where, in analogy with the electromagnetic case

$$A^\mu=\alpha\left(\frac{d^3x^\mu}{ds^3}-\text{ tangential terms}\right).$$

A procedure like this does not work in principle, and this for the following reasons: For an equation like this to make sense, one must go to a limiting background field, letting the mass tend to zero. That is, what one really is considering is the limit of the field containing mass as the mass goes to zero:

$$g_{\mu\nu}(x;m)-g_{\mu\nu}(x;0)=\underset{0}{g_{\mu\nu}}$$

$$ds_0^2=\underset{0}{g_{\mu\nu}}\,dx^\mu\,dx^\nu\;.$$

This means that we are considering different Riemannian 4-spaces, with no natural one-to-one correspondence between the points of any two spaces. Thus we must demand invariance under independent transformations in different spaces:

$$x'^{\,\mu}=F^\mu(x^1,x^2,x^3,x^4\;;\;m).$$

Since the equations are tensorial under

$$x'^{\,\mu}=F^\mu(x^1,x^2,x^3,x^4)$$

it remains to demand (to order in m considered) invariance under

$$x'^{\,\mu}=x^\mu+mf^\mu(x).$$

However, the equations are not invariant under such a transformation. It appears from an investigation still in progress that any right hand side m^2A^μ can be wiped out by a suitable choice of such a transformation.

Geometrically one may picture this as follows:

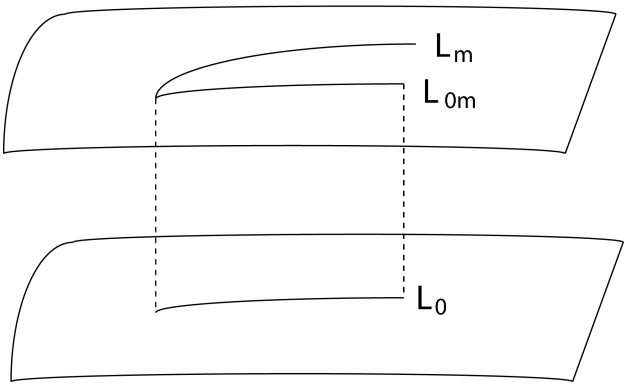

Figure 9.3

If one changes coordinate systems in R_m (or equivalently, a one-to-one correspondence $R_m \leftrightarrow R_0$) one can move L_m into coincidence with L_{0m}.

PIRANI reported the following communication from ROSEN.

Chapter 10

Gravitational Field of an Axially Symmetric System

N. Rosen and H. Shamir, Presented by F. Pirani

In order to investigate the possibility of a physical system radiating gravitational waves, it seems desirable to choose a simple system, one with axial symmetry. If the field of such a system is described by means of a spherical polar coordinate system, $(x^1, x^2, x^3, x^4) \equiv (r, \theta, \varphi, t)$, then by a suitable choice of coordinates one can satisfy two conditions:

(a) The metric tensor $g_{\mu\nu}$ is independent of the angle φ.
(b) It is diagonal.

If one writes down the field equations in the empty space surrounding the system

$$G_{\mu\nu} \equiv R_{\mu\nu} - \frac{1}{2} g_{\mu\nu} R = 0,$$

one obtains a set of 7 equations (since $G_{\mu\nu}$ vanishes identically if one index is equal to 3) for the 4 diagonal components of $g_{\mu\nu}$. Among these there exist 3 identities (the Bianchi identities, except for the one with index 3).

The field equations are non-linear and difficult to solve. It is proposed to investigate them by the method of successive approximations. As a beginning, the first approximation can be calculated. Let us write the line element in the form

$$ds^2 = -(1+\rho)dr^2 - r^2(1+\sigma)d\theta^2 - r^2 \sin^2\theta d\varphi^2 + (1+\mu)dt^2$$

where ρ, σ, τ, and μ are regarded as small of the first order. The linear approximation of the field equations has the following form (indexes denoting partial differentiation):

$$\begin{aligned}
\sigma_{44}+\tau_{44}-\tfrac{1}{r^2}(\tau_{22}+\mu_{22})+\tfrac{\cot\theta}{r^2}(\sigma_2-2\tau_2-\mu_2)-\tfrac{1}{r}(\sigma_1+\tau_1+2\mu_1)&\\
+\tfrac{2}{r^2}(\rho-\sigma)&=0,\\
-\tau_{11}-\mu_{11}+\rho_{44}+\tau_{44}-\tfrac{\cot\theta}{r^2}(\rho_2+\mu_2)+\tfrac{1}{r}(\sigma_1-2\tau_1-\mu_1)&=0,\\
-\sigma_{11}-\mu_{11}+\rho_{44}+\sigma_{44}-\tfrac{1}{r^2}(\rho_{22}+\mu_{22})+\tfrac{1}{r}(\rho_1-2\sigma_1-\mu_1)&=0,\\
\sigma_{11}+\tau_{11}+\tfrac{1}{r^2}(\rho_{22}+\tau_{22})+\tfrac{\cot\theta}{r^2}(\rho_2-\sigma_2+2\tau_2)-\tfrac{1}{r}(2\rho_1-3\sigma_1-3\tau_1)&\\
-\tfrac{2}{r^2}(\rho-\sigma)&=0,\\
\tau_{12}+\mu_{12}-\tfrac{1}{r}(\rho_2+\mu_2)-\cot\theta(\sigma_1-\tau_1)&=0,\\
\sigma_{14}+\tau_{14}-\tfrac{1}{r}(2\rho_4-\sigma_4-\tau_4)&=0,\\
\rho_{24}+\tau_{24}-\cot\theta(\sigma_4-\tau_4)&=0.
\end{aligned}$$

From these equations it is possible to derive the wave equation for ρ,

$$\rho_{11}+\frac{2}{r}\rho_1+\frac{1}{r^2}\rho_{22}+\frac{\cot\theta}{r^2}\rho_2-\rho_{44}=0,$$

and to express the other unknowns in terms of the solution for ρ.

If we represent the radiating system by a quadrupole mass moment ρ, then the solution describing outgoing monochromatic waves can be written in terms of the frequency ω and the amplitude of the moment p_0 ($p=p_0\cos\omega t$) as follows:

$$\begin{aligned}
\rho&=-\frac{\omega^3}{6\pi}p_0[j_2(x)\sin\omega t+j_{-2}(x)\cos\omega t]P_2(\cos\theta),\\
\sigma&=-\frac{\omega^3}{12\pi}p_0\{[(3k_2(x)+j_2(x))\sin\omega t+(3k_{-2}(x)+j_{-2}(x))\cos\omega t]P_2(\cos\theta)\\
&\qquad-(k_2(x)+j_2(x))\sin\omega t-(k_{-2}(x)+j_{-2}(x))\cos\omega t\},\\
\tau&=-\frac{\omega^3}{12\pi}p_0\{[(k_2(x)-j_2(x))\sin\omega t+(k_{-2}(x)-j_{-2}(x))\cos\omega t]P_2(\cos\theta)\\
&\qquad+(k_2(x)+j_2(x))\sin\omega t+(k_{-2}(x)+j_{-2}(x))\cos\omega t\},\\
\mu&=-\frac{\omega^3}{6\pi}p_0[(2q_2(x)+j_2(x))\sin\omega t+(2q_{-2}(x)+j_{-2}(x))\cos\omega t]P_2\cos\theta].
\end{aligned}$$

Here

$$
\begin{aligned}
x &= \omega r\,, \\
j_n(x) &= \left(\frac{\pi}{2}\right)^{1/2} x^{-1/2} J_{n+1/2}(x), \\
k_n(x) &= \frac{1}{x}\int_{\infty}^{x} j_n(u)du, \\
q_n(x) &= x\int_{\infty}^{x} u^{-2} j_n(u)du.
\end{aligned}
$$

It is planned to use this solution as the starting point for a more accurate calculation. The interesting question, of course, is whether the exact equations have a solution going over into the above for sufficiently weak fields.

***

J. N. GOLDBERG presented an approximation method for high velocities. The obvious one is a mass expansion of the gravitational field. In the first approximation there is no acceleration of the particles, which are represented by singularities. This situation differs from the corresponding case in electromagnetic theory. The reason for this is that the electromagnetic field is described by a vector potential whereas the gravitational field is tensor in character. In the E.I.H. method, the motion of the particles is determined by certain consistency conditions in the form of surface integrals each of which contains two terms. One of these is the time derivative of an expression which is linear in the first derivatives of the potentials, the other is quadratic in the first derivatives of the potentials. Therefore, in each order the surface integrals place restrictions on the solutions of lower order. These restrictions may be relaxed by adding appropriate poles and dipoles in the lower orders. The requirement that the sum of the dipoles added must vanish leads to the equations of motion. However, since we do not discriminate against the time in the mass expansion, the surface integral conditions must be satisfied in the same order as the corresponding field equations. This situation is very unsatisfactory. The surface integrals, however, are related to the Bianchi identities. Instead of the surface integral conditions, one can require that the Bianchi identities be satisfied everywhere, even across the singularities. In this manner one can obtain higher order equations of motion from the lower order field equations both in the E.I.H. method and in the mass expansion.

TONNELAT: The starting point of the non-symmetric unified field theory is usually the invariant density:

$$L = G^{\mu\nu} R_{\mu\nu}$$

in which $R_{\mu\nu}$ is the Ricci tensor built from an arbitrary affine connection $\Gamma^{\lambda}_{\rho\sigma}$. The variations $\delta\Gamma^{\lambda}_{\rho\sigma}$, $\delta G^{\mu\nu}$ lead to the field equations. However, if one tries to apply the Einstein, Infeld, Hoffmann method to these equations, one obtains merely the results of general relativity, and one does not obtain the equations for a charged particle. This result stems from the condition

$$\partial_\rho G^{\underset{\smile}{\mu\rho}} = 0$$

which is imposed by the theory.

To avoid this situation, one can start from an affine connection $\Gamma^{\rho}_{\mu\nu}$ with vanishing torque

$$L_\rho \equiv L^{\sigma}_{\underset{\smile}{\rho\sigma}} = 0.$$

Lagrange multipliers are needed in the variations of $\delta L^{\rho}_{\mu\nu}$ because the 64 $L^{\rho}_{\mu\nu}$ are not independent and, moreover, the vector A_ρ related to the torque is introduced. In this case, one obtains

$$\partial_\rho G^{\underset{\smile}{\mu\rho}} \neq 0.$$

Introducing the metric

$$\sqrt{-a}\, a^{\mu\rho} = G^{\underline{\mu\rho}}$$

one obtains

$$\nabla_\rho \varphi^{\rho}_{\mu} = F_{\mu\rho} J^{\rho}$$

with

$$S_{\mu\rho} = R_{\underline{\mu\sigma}} - \frac{1}{2} a_{\mu\rho} R_{\underline{\rho\sigma}} a^{\rho\sigma}.$$

Setting

$$R_{\underline{\mu\nu}} \equiv G_{\mu\nu}(\{\ \}_a) + V_{\underline{\mu\nu}}$$

defines a tensor $V_{\underline{\mu\nu}}$ whose divergence gives a Lorentz force. This tensor plays in the field equations the role of a Maxwell tensor and leads, with the use of an extension of the Einstein, Infeld, Hoffmann method, to a Coulomb force.

Chapter 11
The Dynamics of a Lattice Universe

R. W. Lindquist

One considers a cosmological universe with the cosmological constant set equal to zero and asks whether it is possible to construct a closed universe which contains a finite number of mass singularities. In dealing with this problem, one works with what is essentially the Wigner-Seitz approximation carried over bodily from solid state physics. One begins by taking a space-like hypersurface or zone of influence surrounding a particle. These zones are assumed to be spheres and for the aggregate of particles each sphere is tangent to all of its nearest neighbors. Next one makes the somewhat more drastic assumption that inside each zone of influence one has a pure Schwarzschild solution. These singularities can then be fitted onto the surface of a hypersphere as shown in the figure to the left. The requirement that each zone touches its nearest neighbor at only one point requires, in view of the very high symmetry of the problem, that one takes the number of particles in the problem to be equal to the number of vertices in a regular hyperpolyhedron, i.e., $N = [5, 8, 16, 24, 120, 600]$. One must now set up the tangency condition, and require that the Schwarzschild funnels shall be tangent to the hypersphere.

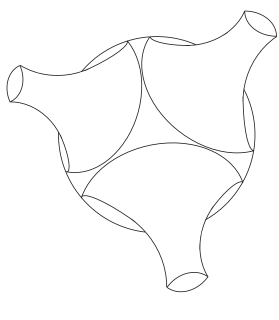

Figure 11.1

This can be done in an invariant manner. One must also find how to define the hypersurfaces to begin with. This can also be done. This, with the tangency condition, gives the motion of the hypersphere. One gets from this model an expanding and contracting universe, with a cycloidal behavior.

BONDI commented that this is interesting from the point of view of the Eddington universe, admitting that this is the case with the cosmological constant not equal to zero. One can ask, if one allows for the granular structure of mass, how many granules can one allow? Of course it is your number 600.

LINDQUIST agreed and continued: In order to get an idea of how good an approximation the Schwarzschild singularities are, one can compute the maximum radius of the tangent hypersphere, for the various number of particles.

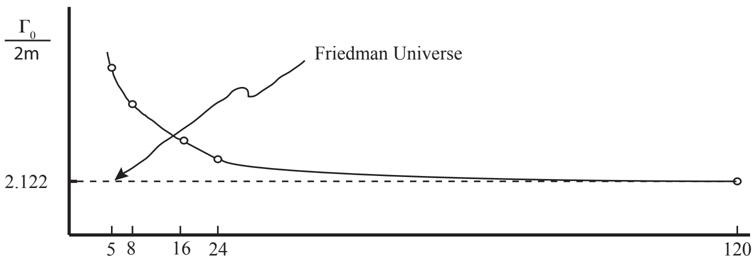

Figure 11.2

This is shown in the above graph, in which Γ_0 is the maximum radius of expansion. We have also computed the invariant distance between two Schwarzschild singularities (at the radius of maximum expansion). This has been done both rigorously - as an initial value problem - and with the above approximation. One finds the values given below:

	Rigorous	Approx.
$d/2m$	.9994	1.18

The session was concluded by WHEELER, who summarized the results presented by the various speakers.

First, from what PIRANI has said, we have gained some insight into how we may define what the measurability properties are locally of the gravitational field. The tensors and invariants he has described are at the heart of the matter. Second, as concerns the radiation problem, we would like to know what is the highest degree of symmetry one can have in a problem, and still have interesting radiation. This leads one to the question of whether, even in the cases where there is no symmetry, one has reason to expect radiation. On this score, it would be well to recall an important physical fact: that the gravitational field of a point charge has close analogies to the electric field. One knows that there is a certain linear approximation to the field equations similar in nature to the electromagnetic equations, so that if a mass is accelerating, one finds it produces radiation similar to the electromagnetic radiation of an accelerating charge. On this account, one expects gravitational radiation. Using this analogy, Einstein was able to calculate the rate of radiation from a double star. The issue which SCHILD brought up is one quite different from the double star radiation, in that the system is time-nonsymmetric. He invites us to consider the radiation produced in a gravitational scattering process. The acceleration of the particle as it zooms by the stationary one will give radiation at large distances, but the recoil acceleration of the particle which was initially at rest will produce a wave of nearly the opposite phase of the first, so that one expects the rate of radiation to be proportional to something analogous to the moment of inertia of the double star system. This means that the damping of this system depends on some complicated geometric concept like mr^2. Therefore, if one hopes to get a proper answer to the question of how to describe the "frictional force" between the two masses, one must find some quite general way of describing invariantly that region of space in which the particle undergoes its acceleration.

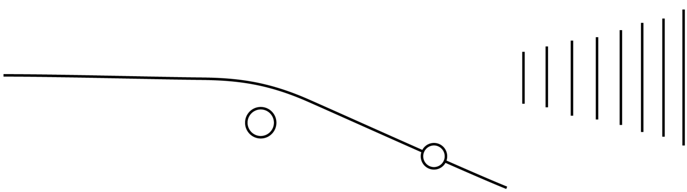

Figure 11.3

BONDI has reminded us that if one looks for radiation pressure on a particle in gravitational waves, he must take into account the radiation produced by the motion of the particle itself. The situation here is analogous to an electromagnetic wave passing over a particle. To the lowest approximation, the particle only feels the electric field and oscillates with it. If one improves his approximation, he finds that the particle begins to respond to the magnetic field, and moves in a figure eight. Still there is no radiation pressure. It is only when one includes the radiation that the particle itself gives out that one gets radiation pressure. That is, it is only when one allows for the radiation damping force that one finds the particle moving forward. Similarly, in the case of gravitational radiation, one faces similar problems.

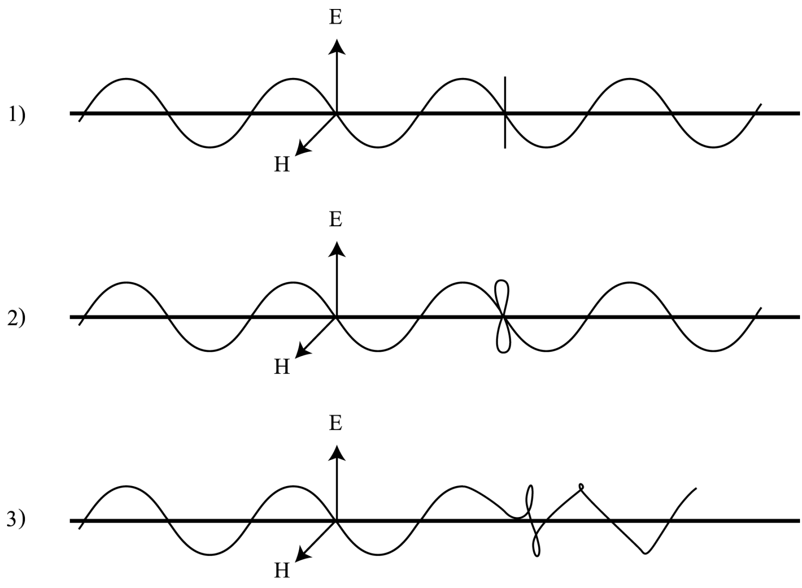

Figure 11.4

As WEBER brought out, in the case of the cylindrical wave, a gravitational metric charge passing over a particle leaves the particle with its initial energy after the disturbance has left. At first sight, one might believe that there is no observable consequence of the action of the wave on the particle. However, the electromagnetic analogy suggests that if one were to go further, one might expect to find radiation pressure.

One has also to consider the nature of the one-sidedness of gravitational radiation. Here one faces the problem of what is to be meant by the difference between retarded and advanced waves. If one employs the absorption theory of radiation damping in treating the above problem, one must employ the use of advanced and retarded waves. In flat space the concepts of advanced or retarded waves are easily understood. However, with gravitational waves, space is curved, and this has the consequence that it is difficult to distinguish between retarded and advanced waves. This is due to the fact that a pulse sent out by a source gets defracted by the curvature of space and secondary waves are thereby generated, which are in turn scattered. In this way, one may ultimately get contributions to an incoming wave, so that the distinction between retarded and advanced waves is lost.

Session IV Invited Reports on Cosmology

Chairman: F. J. Belinfante

Chapter 12
Measurable Quantities that May Enable Questions of Cosmology to be Answered

Thomas Gold

12.1 Introduction

A discussion of a number of cosmological problems was presented including radio and optical astronomy as important observational tests of cosmological theory. The point of view adopted was that, while locally discovered laws of physics may possibly suffice to describe observations on the large scale, it is conceivable that this may not be true on the very large scale requiring extreme extrapolation. Cosmological models should therefore be constructed that are simple and can be subjected to observational tests. With respect to the distribution of matter, it is necessary to discuss not only the geometrical and kinematical aspects but also physical characteristics such as the abundance of the elements, galaxies, and galactic clusters. Models must be capable of allowing for such evolution. We cannot be sure that we know all the laws of physics required for a real understanding of the situation, and we should consider seriously any model which is simple and presents the greatest variety of experimentally verifiable conclusions.

12.2 Measurements and Observations

The observations which are most easily interpretable as of a cosmological nature with respect to galaxies are the following quantities:

1. distance (brightness)
2. red shift
3. number of galaxies
4. distribution of θ and φ (on the celestial sphere)
5. color and other physical characteristics

6. mean density of matter in the world
7. distribution and abundance of elements

In the above list, 1 to 4 would represent a distribution in phase space. These would be expected to be predicted (as well as the other measurements) by a cosmological model.

12.3 Cosmological Models

A. The "Explosion" Model

The present state of the universe is the result of an explosion of a previous dense state of matter at a time of the order *Hubble's constant* years ago.

B. The "Oscillatory" Models

These are similar to the explosion model, and we are supposedly now on an expansion phase of the "oscillation" which should be followed by a "contraction" of the universe to a repetition of the cycle.

C. The "Steady State" Model

In this model the observational quantities on the average do not change with time. It eliminates the effects of local evolution on all statistical observations and avoids therefore a great difficulty in testing the model in contrast to other models. All observations on distant masses are past history of those distant masses. On the steady state model the average of such observations is to be the same as for nearby masses. Unless we know how to calculate the changes in distant galaxies with time as a function of a wide range of detailed information, we would not know how to interpret the data. The virtue of the steady state model is that it bypasses this problem and that it could therefore be disproved by the observations as they are all crucial.

12.4 Observational Data

A. Distance vs. Red Shift ($C\ \Delta\lambda/\lambda$)

The best summary has been given by Sandage on the work of Sandage and Humason. According to the Cosmological Principle, at a suitable moment in time all the localities, suitably chosen, look alike.

The data is to be summarized by the relations:

$$C\ (\Delta\lambda/\lambda) = H \cdot D$$

where

C = speed of light,
H = Hubble's constant,
D = distance,

$\Delta\lambda/\lambda$ = fractional shift in wave length;

$$\frac{1}{R}\frac{dR}{dt} = H_0$$

defines the present observational magnitude of Hubble's constant;

$$\frac{1}{R}\frac{d^2R}{dt^2} = -q_0 H_0^2$$

where q_0 is a quantity to be discovered by the observations.

The observations to be made during the next few years should give us the required kinematical information. The predictions of the different models in regard to q_0 are not resolvable by present observations when one compares the red shift vs. bolometric magnitude. It appears that the newly-developed photoelectric image multiplier technique used at Mt. Palomar by Baum should extend the data sufficiently to make possible the resolution between models in regard to the red shift data.

In regard to the quantity q, it is to have the following cosmological significance:

q	Type of Universe
$q > 1/2$	closed spherical universe
$q = 1/2$	flat
$0 \leq q < 1/2$	open (hyperbolic)
$q = -1$	steady state

B. Red Shift and Number of Galaxies

This correlation would be an especially attractive result. However, insufficient data exist because of the laborious effort involved in examining hundreds of spectrograms of distant galaxies to obtain number counts and spectral measurements. It would be helpful to have this information obtained by mechanical means.

C. Color vs. Distance (Red Shift)

The absence of color effect as a function of distance (defined by red shift) as now observed by Stebbins and Whitford can be interpreted to mean there is no large space absorption and (at great distances) there are no large evolutionary effects in times of the order of one quarter of Hubble's constant. This would agree with the steady state model.

D. Radio Counts of Galactic Number (Number vs. Brightness (distance))

All number counts are subject to a type of error which must be avoided. In a homogeneous system the law of number (N) versus intensity (I) should be

$$\ln N \approx -3/2 \ln I$$

If the error in I is symmetric and equal at all intensities, it does not affect the gradient of the relationship which is 3/2 in magnitude but will shift the curve parallel to itself. When the error is greater at small intensities, the effect is to change the gradient and is then very strongly dependent on the extent of the tail of the error distribution (the small number of large errors). The effect is to steepen the gradient, and can be calculated from the radio source data if an independent measure of the error exists.

The radio counts are not able to produce a cosmological answer at this time except if it were found unambiguously that the gradient was greater than 3/2 or that it was found possible to observe independently the distance of sources rather than just their brightness, for in the case that they follow the 3/2 law they can be assumed to be too near to have any cosmological significance.

E. Mean Density of Matter in the Universe

Another type of radio observation of cosmological significance is the mean density of matter. This is involved in the relation (according to some cosmological theories)

$$4\pi G\rho\tau^2 \approx 6$$

where

$G =$ universal gravitational constant,

$\rho =$ mean density $\sim 10^{-28} gm/cm^3$,

$\tau =$ Hubble's constant (as a time).

The radio observations on neutral hydrogen (21 cm wavelength) in distant galactic clusters indicate appreciable addition of neutral hydrogen so that much of the matter of the universe can no longer be considered luminous. This is to the effect that ρ will always tend to increase as methods are found to detect matter. It would be nice to know the amount of matter in intergalactic space. It is presumably largely ionized because the recombination time is about 10^{10} years. (Dust is exceedingly rare.) Enough dust to affect the recombination time would make intergalactic space opaque to visible radiation.

F. Origin of the Elements and Abundance

The recent developments (the work of Fowler, Hoyle, Burbidge, and Cameron on element generation in stars) are to be regarded as a great triumph. According to this theory, elements are being generated at present in stars that are seen at present by calculable nuclear processes. The massive stars have a shorter life and scatter themselves. They throw out their matter containing elements up to the Fe-group. This matter is caught in other stars and in turn captures neutrons (because of the larger cross-section) from light element reactions within the stars. In this way a small proportion of heavy elements will be built up. It has been possible to give explanation for some of the details in the nuclear abundance diagram - an extremely complicated picture. A particular triumph within this theoretical picture of element buildup in stars is the prediction by Hoyle of a C^{12} nuclear energy level (which was verified to exist). A cosmological requirement thus served as a prediction of a nuclear physics experiment.

According to the steady state model, there is no need to imagine a particular state of matter which was different in the past. As the formation of the elements is proceeding continuously, the overwhelming abundance of hydrogen now implies that the bulk of material is young enough not to have undergone these processes. In other models it is necessary to assume also that the material is mainly young, or that it has been in a non-reactive state until recently. To suppose that some process undid the nuclear combinations imagined as having taken place previously is very difficult and requires circumstances so far from those we know that such speculation seems unprofitable.

Discussion

BONDI remarked that q is not a purely geometric quantity. The first three values given (page 123) involve the use of the field equations of general relativity and the conservation of matter. The steady state model is flat in the large.

BELINFANTE inquired about the loss of number counts because of absorption of intergalactic matter.

BONDI replied that this could almost be ruled out because one could see so very far and the photoelectric measurements of Baum indicate there cannot be too much absorption out to the limits of observation.

DICKE observed that one could not see anything beyond that point and raised the question of the significance of number counts without going into red shift.

GOLD stated that there exist no good number counts since 15 years ago, and they do not distinguish between the models.

BONDI stated the astronomers say they cannot get such numbers easily because of the great accuracy required in brightness measurements and the steepness of the curve relating number and brightness.

GOLD remarked that the question of absorption is important but in a way the observations indicate it does not exist in fact to a significant extent.

BONDI inquired what fraction of the mass of galactic clusters is estimated to be neutral hydrogen.

GOLD replied several times the stellar masses.

DE WITT inquired about what happens to the energy-stress tensor which is not conserved in the steady-state theory.

BONDI replied that there is no clear-cut mathematical analysis. The assumption of the steady state theory is a physical hypothesis from which it is possible to make predictions, verifiable by observation, without going into the tensor calculus. McCrea suggests there is a zero point tension in the vacuum which on expansion produces outgoing energy.

DE WITT asked why protons should be chosen as the entities which are "created out of nothing." Since they are already complicated structures, why not go all the way and use heavier nucleii - such as Fe - as well? Or is the theory to be considered purely phenomenological?

GOLD said that if any more basic heavy particles were to be made they would end up as protons and hence hydrogen gas. To assume the creation of Fe would seem very artificial and is in fact unnecessary.

BONDI pointed out that the evolutionary history of any galaxy required the pre-existence of matter and in the steady state model this means cold hydrogen gas is the simplest hypothesis and reasonable.

GOLD remarked that one is forced to the supposition of the coming into being of the matter anyway, as it could not have been around for an indefinite period without going into heavier elements.

DE WITT said that the creation of matter required the existence of some unknown dynamical process as a precursor to the creation which forces the production of protons rather than something else.

SCIAMA remarked that any future detailed formulation of the theory would have to account for the production of protons.

BERGMANN emphasized that the principle of equivalence of the general theory carried with it certain detailed consequences which includes the conservation of the stress tensor which follows from the covariance and action principles.

BONDI stated that what one has is something which is interpreted as having components measurable in the form of stress.

BERGMANN replied that whatever it is which is conserved is stress by definition. If expansion and creation occur simultaneously, stress can be conserved only by having influx of matter along with efflux so that the concept of creation is superfluous. The steady state hypothesis requires true creation which is a violation of stress conservation. It is conceivable that the principle of equivalence and the action principle may require modification from the other point of view.

PIRANI proposed that one envisage a Dirac type distribution of states which contributed to the energy tensor and condensed into the protons at the required rate.

BERGMANN said this could not lead to steady state.

PIRANI said it could.

GOLD pointed out that if creation is denied currently, it cannot be maintained for the past; and this issue cannot be avoided on any theory.

BERGMANN stated that he did not favor the explosion hypothesis either.

SCIAMA felt that the conservation equations as now known are interpreted too seriously. One could devise a more complicated theory with conservation equations arising from identities. The extra variable in such a theory could allow for creation.

WHEELER took the point of view that one should not give up accepted ideas of wide applicability such as general relativity but should investigate them completely. The two things left in question are the expansion and the creation of elements. The first does not appear to cause any difficulty in general relativity theory. Hydrogen can be created out of heavy elements by the reaction of matter in stars. This process continued sufficiently far will get one down to the absolute zero of temperature. There is a well defined state of absolute zero and, in the beginning, for not too great a mass, one has iron. If the mass is too great, the composition of the interior is pushed into neutrons. A neutron core star undergoing explosions will throw large quantities of neutron-rich matter into space which yields hydrogen.

GOLD questioned whether such a process could yield 99% hydrogen and helium.

WHEELER replied that no detailed calculation had been made but would not say that it could not be explained within the present framework of accepted ideas.

BERGMANN inquired about the thermodynamic analysis of the neutron core model leading to the hydrogen clouds from the point of view of entropy.

WHEELER said that initially it is a neutron situation at absolute zero under high pressure.

BONDI commented on the fact that Einstein's proposal of the Cosmological Principle 40 years ago occurred at a time without knowledge of the galaxies. The extension of the range of observation by radio and optical means has shown that a homogeneous system obeying the Cosmological Principle demands the distance vs. recession-velocity relation according to McCrea and Milne. The body of evidence in support of the Cosmological Principle makes it an astounding prediction, more striking, in a sense, than those of general relativity

WHEELER remarked that Einstein said also that symmetric motions say nothing about laws of motion. What is simple in physics are the laws of motion and not the motions themselves as, for example the situation in hydrodynamics with turbulence. The Cosmological Principle is not in the same category as other physical laws because it is not a fundamental law.

BONDI said its fundamental character may be debatable but not its success.

ERNST questioned if a steady-state theorist would drop the generalized Cosmological Principle if observations did not fit the steady-state theory.

BONDI replied, “Like a hot brick.”

ERNST continued with the statement that this would perforce leave only the stress-tensor and asked where could one go from there.

BONDI said he would drop the subject.

GOLD remarked that the steady-state theory is certainly successful in the sifting of observational data.

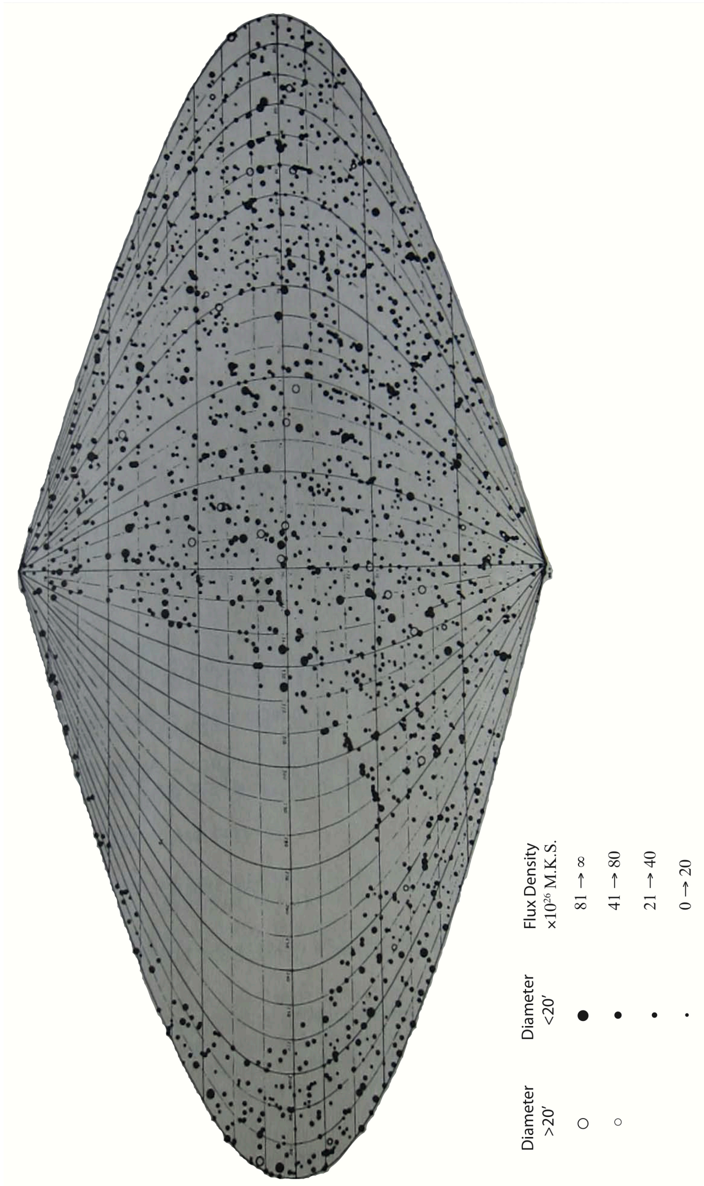

Figure 12.1: Map showing the distribution of radio sources in galactic coordinates. The open circles represent the sources of large angular diameter, and in both cases the sizes indicate the flux density of the sources

Chapter 13
Radio Astronomical Measurements of Interest to Cosmology

A. E. Lilley

With the discovery of radio radiation of galactic origin in 1932 by Karl Jansky, this new science of radio astronomy has rapidly matured and already has produced measurements which are of interest to cosmologists. This afternoon we shall take up several of these, and in particular discuss those radio astronomical measurements which have or may produce data of quantitative significance for cosmologists; however, before examining these measurements in detail, let us review briefly the appearance of the radio sky as evidenced by the radio data. We may conveniently divide the radio radiation into two components: that originating from an apparently continuously distributed source and that originating from discrete sources, the so-called radio stars. The continuous radiation field displays a distribution on the celestial sphere which concentrates itself toward the plane of the Milky Way. It reaches maximum intensity in the Sagittarius region, which contains the nucleus of our galaxy. The variation of intensity with wavelength is such that the longer the wavelength the greater the observed intensity. This radiation component therefore is of non-thermal origin. Superimposed on the continuous radiation field are the radio stars. It is the analyses of the radio star studies which have been made to date which we shall consider first as one example of radio astronomical measurement of cosmological interest.

Extensive surveys have been made of radio stars, and these surveys continue currently. The survey data produce positions and apparent intensities of the radio stars. Approximately 2,000 discrete sources have been observed with current instruments. This number will undoubtedly increase as instrumentation improves.

If we assume that the radio sources have a constant luminosity, or, obey some regular luminosity function which does not vary with distance, the distribution of radio sources throughout the observable universe may be inferred from the survey data. We may write down how the number of observed radio sources per unit solid angle will increase with decreasing apparent intensity if one is viewing a universe having an isotropic distribution. The number of observed sources having an intensity greater than I is plotted against intensity and the resulting curve is compared with the curve which would result from an isotropic distribution. On a $\log N$ versus $\log I$ plot, an isotropic distribution would produce a straight line having a slope of $-3/2$.

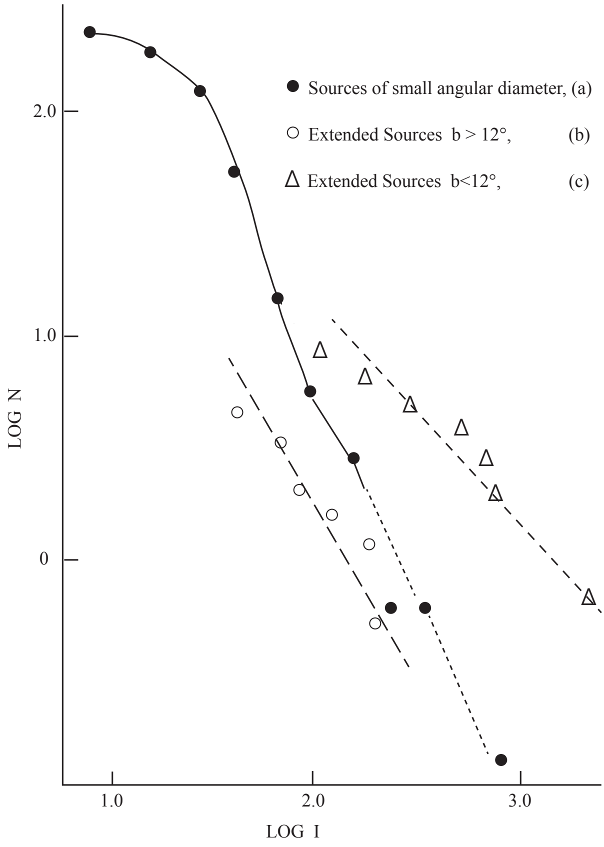

Figure 13.1: Curves of $\log N$ against $\log I$, where N is the number of sources per unit solid angle having a flux density greater than I.

Ryle and Scheuer of Cambridge analyzed the first such survey data which involved a statistically significant sample of radio sources. The slide shows the observed distribution of radio sources over the sky displayed in galactic coordinates. The circular region in which no radio stars appear is simply the region inaccessible to observation. The next slide which is, as the first, taken from the work of the Cambridge group, displays the $\log N$ versus $\log I$ behavior of the data. You will note that as the data proceed toward decreasing intensity, the actual plot shows that there is an apparent accumulation of radio stars which is more rapid that one would expect from an isotropic distribution. The interpretation placed upon the observed curve by the Cambridge group suggests that there is an increasing density of radio stars with increasing distance from the neighborhood of our galaxy.

This will suggest departures from an isotropic and uniform universe and the results, if valid, are not consistent with a steady-state universe. However, the interpretation of this curve has been discussed by Bolton, who has suggested that when one has observational errors which increase with decreasing intensity, even an isotropic distribution can produce a curve of the form shown in the slide.

In addition to the interpretative difficulty pointed out by Bolton, observational conflict now exists. A similar observational survey has been conducted by Mills in Australia with a different type of antenna system; and this system also possesses a greater sensitivity than the Cambridge instrumentation. Where the surveys of the Australian and Cambridge workers overlap, and where a detailed intercomparison can be made, the agreement between the separate surveys has been disappointing. In addition, the $\log N$ versus $\log I$ analysis by Mills shows no significant departure from the $-3/2$ curve until the approximate sensitivity limit is reached where the curve does display some increase; however, this faint limit increase is very suggestive of the effect pointed out by Bolton.

Thus, the analysis of radio star data in quest of their distribution throughout space has resulted in observational conflict. Since this topic is of considerable interest to the next speaker, Dr. Gold, I will leave its further discussion to him.

Let us now discuss several possible measurements which can be made by employing microwave spectral lines which originate in the gases which compose the interstellar medium. Although others are expected, only one such line has been successfully detected and studied to date - the hyperfine transition originating in the ground-state of atomic hydrogen. This transition occurs in the microwave domain near a wave length of 21 cm.

As the first surveys of this radiation in our galaxy were nearing completion, consideration was given to the possible behavior of the spectral line profile in directions which contain radio stars. Although the line predominantly appears in emission distributed around the galactic plane, the first observations in radio star directions revealed the line in absorption.

An analysis of the absorption effect shows the absorption studies to be extremely high resolution investigations of the interstellar gas. Minimal distances to radio stars and observations of small-scale turbulent structure of the interstellar medium were early consequences of the absorption studies. The ability of the absorption effect to make extremely high resolution studies of the interstellar medium will probably prove more valuable than utilization of the data for measurement of radio star distances.

The absorption lines produced in the continuum of radio stars are sensitive to the size of the antenna which views the gaseous assembly. By employing larger antennas and looking for absorption lines in the spectra of the radio stars, we ultimately hope to observe other gaseous components of the interstellar medium. A transition in deuterium at a frequency of 327 mc and in the hydroxyl radical at a frequency of 1667 mc are examples of new lines which may ultimately be detected by larger radio telescopes. With the development of a new type of microwave receiver of vastly improved sensitivity, the so-called solid-state maser, we may confidently expect detection of other gaseous components in the interstellar medium. When such lines are detected, radio astronomy will provide a few numbers in the tables of cosmic abundance.

Of considerable interest to cosmologists is the size of the observable sample universe. Let us briefly compare the size of the universe available to radio astronomical measurements with the size available for optical examination. Two billion light years may be taken as a measure of the limiting distance at which objects are detectable by the Hale $200''$ telescope. This is a measure of the limiting distance without electronic aids. We may compare the optical 2 billion light year figure with a hypothetical radio case. Restricting our attention to the detection of radio flux (neglecting for the moment red shift corrections) a $150'$ diameter parabolicradio telescope equipped with a conventional microwave receiver could detect a radio star of the Cygnus A type at a distance of 8 billion light years. If the $150'$ antenna were equipped with a solid-state maser, the maser would overcome some of the red-shift flux reduction and fruitful measurements could be made at distances significantly beyond the range of 2 billion light years.

Another topic of interest to cosmologists and of interest to workers employing the hydrogen line is the possible existence of hydrogen gas in the intergalactic medium. Unfortunately, the intergalactic gas is probably ionized, and the time for recombination is expressed in billions of years. However, it is of interest to ask what density could be detected in the intergalactic medium if it were neutral form. We would search for an absorption line in the spectrum of an extragalactic radio source. The minimum detectable density is of the order

$$\overline{\rho}_{igm} \sim 3 \times 10^{-11} \frac{T_S \Delta T_{min} H}{T_A}$$

where H is Hubble's constant, ΔT_{min} is the sensitivity limit of the microwave radiometer, T_S is the state temperature of the intergalactic gas and T_A is the antennatemperature of the extragalactic source. Taking as an example the $150'$ antenna equipped with a solid state maser and using the Cygnus A source as a test object, the corresponding minimum detectable density is about $3 \times 10^{-33}\, T_S$ gm/cm^3. This number refers only to the unionized component (if any) in the intergalactic medium.

It also refers to average regions of intergalactic space which are well removed from rich clusters of galaxies. The density of material distribution in the confines of rich clusters of galaxies could not be regarded as indicative of the average mean density throughout intergalactic space. However, the contribution to the average density by the clusters is most important, and measures of the cluster masses are required.

Heeschen has succeeded in detecting emission from hydrogen gas in the Coma cluster of galaxies. This is presumably gaseous emission from the "cluster medium." The emission results indicate that the gaseous mass of the cluster medium is of the order 1/3 the total mass of the cluster. Measurements of 21 cm emission from clusters of galaxies may revise and strengthen our estimates of the mean density of material in the universe.

Finally, we shall take up the measurement of the red shifts of extragalactic objects by radio techniques. The identification by Baade and Minkowski of the Cygnus A radio star as a pair of galaxies in collision produce a strong out-pouring of radio energy. The radio intensity of the galaxies in collision is enhanced by a factor of the order 10^8 compared to the normal radio intensity of isolated galaxies. In such colliding galaxies, it is possible that peripheral gases not yet involved in the collision contain atomic hydrogen gas. This gas would absorb part of the radio continuum originating in the collisional zone thereby producing an absorption line in the continuous spectrum. The optical studies of Baade and Minkowski showed that the Cygnus A system has a red shift indicating a recessional velocity of approximately 17,000 km/sec. The corresponding shift of the hydrogen line would be a decrease of about 81 mc from the rest frequency of 1420 mc. This line was sought and found by investigators at the Naval Research Laboratory in Washington, who used a 50' antenna. Although this first measurement was crude, it revealed that the microwave Doppler displacement was, within the limits imposed by experimental error, identical for optical and microwave determinations. This is what the result must be if the universe is expanding.

With the increasing distances available for observation in radio astronomy, the possibility of making red shift measurements on Cygnus A type systems well beyond the two billion light year range suggests itself. Measurements such as this may ultimately prove of considerable value in determining the exact shape of the red shift curve with distance which, as you will hear in Dr. Gold's talk, is a means for testing models of the universe.

Although radio astronomy is in its first stage of development, it is already evident that this new endeavor can produce measurements of considerable interest to cosmology.

Session V Unquantized General Relativity, Concluded

Chairman: A. Lichnerowicz

WHEELER opened the session with a presentation of some conclusions reached by members of his group. Komar has investigated the interpretation of Mach's Principle and has found great difficulties, which are ascribed to the lack of understanding of the initial value problem. The stability of a Schwarzschild singularity in empty space to small disturbances was investigated with T. Regge. It was concluded that the Schwarzschild singularity is, to the extent investigated, stable against small disturbances away from its normal spherical form. Thermal geons were investigated with E. Power. A self-consistent problem was set up for the motion of photons of thermal radiation in a spherically symmetric gravitational field produced by these photons. It was found that a region existed in which photons would be trapped; there is a stable circular orbit in this region, and the other orbits consist of oscillations about the circular orbit. The limiting orbit is a spiral which is asymptotic to the outer boundary.

Ambiguity in a Gravitational Stress Energy Pseudotensor Interpreted in Terms of Arbitrariness in a 'Base Line Coordinate System'

W. R. Davis

In order to avoid the non-localizability to transform the stress energy pseudotensor, Kohler attempts to transform the stress energy pseudotensor in the given coordinate system, introducing in addition to the g_{ik} another set of quantities, $\overline{g}_{ik}$, such that the artificial metric described by the $\overline{g}_{ik}$'s possesses a zero curvature. He then shows that the *difference* between the gravitational pseudotensor defined by the g_{ik} and $\overline{g}_{ik}$ itself transforms like a tensor under coordinate changes. This result might seem to give a certain uniqueness to the definition of a gravitational stress energy tensor if one could imagine that the quantities, $\overline{g}_{ik}$, were unique. However, a simple investigation shows that the $\overline{g}_{ik}$'s are highly arbitrary. In fact, for every coordinate system, x^i, that is asymptotically straight at infinity one can introduce a set of $\overline{g}_{ik}$'s which have the normal Lorentz values everywhere. By this procedure the $\overline{g}_{ik}$'s are of course defined in any other coordinate system. The ambiguity in the choice of the $\overline{g}_{ik}$'s in some specific coordinate system is therefore as great as the ambiguity in the choice of the original coordinate system which becomes asymptotically flat - an ambiguity can be regarded as a base line. Motions with respect to this base line can be thought of as due to "gravitational" forces. These gravitational forces define a gravitational stress energy tensor. This point of view has the following usefulness: One is in this way easily able to extend the discussions of the equivalence principle from the domain of equivalence between gravitation and acceleration at a point to the question of equivalence in the large. At a point the magnitude and direction of the acceleration are arbitrary. For discussing the equivalence principle in the large, *the choice of the base coordinate system is arbitrary.* Once one has selected this base coordinate system all else is uniquely determined.

Chapter 14
Measurement of Classical Gravitation Fields
Felix Pirani

Because of the principle of equivalence, one cannot ascribe a direct physical interpretation to the gravitational field insofar as it is characterized by Christoffel symbols $\Gamma^{\mu}_{\nu\rho}$. One can, however, give an invariant interpretation to the variations of the gravitational field. These variations are described by the Riemann tensor; therefore, measurements of the relative acceleration of neighboring free particles, which yield information about the variation of the field, will also yield information about the Riemann tensor.

Now the relative motion of free particles is given by the equation of geodesic deviation

$$\frac{\partial^2 \eta^\mu}{\partial \tau^2} + R^{\mu}_{\nu\rho\sigma} v^\nu \eta^\rho v^\sigma = 0 \quad (\mu, \nu, \rho, \sigma = 1, 2, 3, 4) \tag{14.1}$$

Here η^μ is the infinitesimal orthogonal displacement from the (geodesic) worldline ζ of a free particle to that of a neighboring similar particle. v^ν is the 4-velocity of the first particle, and τ the proper time along ζ. If now one introduces an orthonormal frame on ζ, v^μ being the timelike vector of the frame, and assumes that the frame is parallelly propagated along ζ (which insures that an observer using this frame will see things in as Newtonian a way as possible) then the equation of geodesic deviation (14.1) becomes

$$\frac{\partial^2 \eta^a}{\partial \tau^2} + R^{a}_{0b0} \eta^b = 0 \quad (a, b = 1, 2, 3,) \tag{14.2}$$

Here η^a are the physical components of the infinitesimal displacement and R^{a}_{0b0} some of the physical components of the Riemann tensor, referred to the orthonormal frame.

By measurements of the relative accelerations of several different pairs of particles, one may obtain full details about the Riemann tensor. One can thus very easily imagine an experiment for measuring the physical components of the Riemann tensor.

Now the Newtonian equation corresponding to (14.2) is

$$\frac{\partial^2 \eta^a}{\partial \tau^2} + \frac{\partial^2 v}{\partial x^a \partial x^b} \eta^b = 0 \tag{14.3}$$

It is interesting that the empty-space field equations in the Newtonian and general relativity theories take the same form when one recognizes the correspondence $R^a_{0b0} \sim \frac{\partial^2 v}{\partial x^a \partial x^b}$ between equations (14.2) and (14.3), for the respective empty-space equations may be written $R^a_{0a0} = 0$ and $\frac{\partial^2 v}{\partial x^a \partial x^b} = 0$. (Details of this work are in the course of publication in *Acta Physica Polonica.*)

BONDI: Can one construct in this way an absorber for gravitational energy by inserting a $\frac{d\eta}{d\tau}$ term, to learn what part of the Riemann tensor would be the energy producing one, because it is that part that we want to isolate to study gravitational waves?

PIRANI: I have not put in an absorption term, but I have put in a "spring." You can invent a system with such a term quite easily.

LICHNEROWICZ: Is it possible to study stability problems for η?

PIRANI: It is the same as the stability problem in classical mechanics, but I haven't tried to see for which kind of Riemann tensor it would blow up.

Interaction of Neutrinos with the Gravitational Field
D. Brill

The wave equation of a neutrino in a centrally symmetric gravitational field was derived, using the formalism of Schrödinger and Bargmann. In order to construct a neutrino geon, one must also find the gravitational field produced by a statistical distribution of neutrinos among available states of such a character that the resulting stress energy tensor is spherically symmetric. The stress energy tensor was worked out from a variational principle. The equations which must be solved self-consistently were also worked out for the case of a spherically symmetric neutrino distribution.

BERGMANN: What is the present motivation for the geon research?

WHEELER: The motivation is simply to understand more about how one deals with non-linear field equations. The idea is not that the geon has the slightest to do with an elementary particle, nor with astronomical objects. The interesting things that come out, in the case of the electromagnetic geon, are: if they have any stability at all, and the nature of the radial wave equation for the electromagnetic field. In the neutrino case: first, there is a similarity to the electromagnetic case; second, the radial equation contains a spin orbit coupling term which does not appear in the electromagnetic case, which may help to understand the dynamics of neutrinos.

DE WITT: In this work the neutrinos are not quantized; they are not real neutrinos, are they?

WHEELER: One puts into each neutrino state just one neutrino; this includes all of the results of second quantization.

DE WITT: Might not the gravitational field induce transitions into negative energy states?

WHEELER: This already comes up in the electromagnetic case; what should one use for the stress energy tensor? One counts as gravitation producing only the excitations above the vacuum state, in order to avoid infinities. Here the only neutrino states one considers are the positive energy states; the negative energy states are all filled.

BELINFANTE: You consider a complete absence of antineutrinos; no holes in negative energy states.

WHEELER. Yes, just exactly as one does in the Dirac theory of the electron.

Linear and Toroidal Geons

F. J. Ernst

Tolman has shown that there is no gravitational attraction between parallel light rays and that the attraction increases with the angle between the rays, until they are antiparallel. For this reason, a spherical electromagnetic geon is expected to be less stable than a toroidal one. However, the mathematical difficulties in this case are great, and thus as a preliminary step we consider a linear geon, in which the major radius of the toroid has become infinite; we consider light rays travelling along a central axis in both directions. We look for the solution of the coupled equations of general relativity and electromagnetism with a 4-vector potential of a form deduced by analogy with the modes of excitation of a circular wave guide. The equations can then be written down and numerically integrated. One obtains a complete solution of the field equation which contains no singularities. This solution contains an arbitrary constant through which it can be related to the toroidal solution. When the linear geon is bent into torus, this constant is proportional to the major radius.[1]

Unified, Non-Symmetrical Field Theory
M. Tonnelat

The unified and non-symmetrical field theory starts with a principle which extends the principle of the Born-Infeld electrodynamics. It consists in reducing the sources of the electromagnetic and gravitational field to the field itself.

In order to avoid a point singularity, it is necessary in the static case to define an electric field which is finite at the origin. The formalism of the unified and non-symmetrical field theory immediately introduces the characteristic expressions of Born-Infeld theory.

By writing the fundamental tensor as the sum of two parts, we can relate it to the invariant Lagrangian that appears in the Born-Infeld electrodynamics and the two invariants of the Maxwell theory.

On the other hand, the Einstein theory introduces the contravariant tensor, which may also be written as a sum of symmetric and skew symmetric parts, which shall be called inductions. The relations foreseen partly by the Born-Infeld theory appear between the field and the inductions after making appropriate definitions. The definitions of metric, field and inductions may be chosen in two ways. These definitions depend only on the properties of determinants and on the choice of field and of metric; they do not depend on the Lagrangian of the theory.

We apply the equations of the field to the calculations of a static and spherically symmetric solution of the form chosen by Papapetrou. We can then deduce the radial component of the electrostatic field, p^{14}. Now introducing like Born-Infeld a field $b = e/r_0^2$, we set $E = bp^{14}$. Then, when r approaches 0, E approaches b, a finite value. The originality of this conclusion comes from the fact that the values of the field and of the metric are not given *a priori*, but result from the field equations in the particular case of the Schwarzschild solution. We are now led to define a current following the principles of the Born-Infeld theory. We find the charge density, after substitution of the values obtained in the Schwarzschild solution, and upon integrating this over all space, we find the finite value e. So from the very principles of the unitary and non-symmetric theory, the

[1]For full discussion, see F. J. Ernst, *Phys. Rev.* **105** (1957).

distribution of charge is characterized by a "free density" which leads to a finite expression when integrated over the whole space. It is remarkable that this conclusion which results from the Born-Infeld electrodynamics, is connected here with a choice of the fundamental tensor and with an extension of the Schwarzschild solution.

FEYNMAN: How big is r_0 for an electron?

TONNELAT: It is exactly like in the Born-Infeld theory; it depends on the choice of b; r_0 is a fundamental length. There are two constants of integration which correspond to r_0 and mass, but there is a further arbitrariness in the choice of b, so that the charge e is not determined by the choice of r_0 and m.

DAVIS: I understand that b can be regarded as the ratio of the ordinary electromagnetic field to the natural electromagnetic field. It appears that b, as in the Born-Infeld theory could depend on the sign of the charge. Is this correct?

TONNELAT: Yes, b appears like the quotient of the physical field and the mathematical field.

Chapter 15
Correspondence in the Generalized Theory of Gravitation

Behram Kursunoglu

It is hoped that a reconciliation of quantized gravitational fields, the matter field, and electromagnetic field might induce a further structure on the elementary particles, and might result in softening the divergences, or eliminating them altogether. It is well known that the gravitational field has a non-local character, so it probably would not be possible by quantizing the gravitational field to find a 4-vector momentum giving a particle property to gravity. One tries the approach of generalizing Einstein's theory, making it more complicated than it is, hoping to get some more physics out of it. Since there is no physical basis in unified field theory, as there is in general relativity, one proceeds in a more mathematical fashion. Nature must be described by a complete tensor with symmetric and antisymmetric fields. One then constructs all of the quantities recognized in general relativity and obtains, for example, Bianchi identities and Maxwell identities. In order to compare the theory with general relativity, we have a conservation law in the absence of charges and this introduces a fundamental length r_0. The real motivation for this procedure is the development of a correspondence argument in order to identify the formalism with a theory. The question arises what happens if we assign no structure to the theory and r_0 approaches 0. We obtain the equations of general relativity with the electromagnetic field as the source of gravitation plus Maxwell's equations for charge-free fields. The existence of charges is linked up with a non-vanishing fundamental length just as the existence of spin arises from a finite h.

Now, the metrics depends on length. We have calculated the propagation of light in the presence of very strong background fields; the result is in agreement with those of Schrödinger from non-linear optics. One may also change the sign of r_0, but we cannot find a way of measuring negative length. The concept of negative length may be linked up with negative mass (or antimatter).

DE WITT: Can you state any conclusions about matter and antimatter?

KURSUNOGLU: I cannot state any conclusions because I must solve the equations and show that solutions involving a negative and a positive length will give repulsion.

TONNELAT: It is not necessary to introduce the factor $1/r_0$ into the Lagrangian itself in order to obtain $E \to D$, $B \to H$ when $r_0 \to 0$, because it is a conclusion of the Born-Infeld theory. However, it is necessary to have this factor $1/r_0$ or something else, in order to obtain equations of motion.

KURSUNOGLU: Firstly, I don't believe in the Born-Infeld theory; secondly, equations of motion are a difficult concept in this theory because one has to define what a particle is first. Usually the field variables are singular at the point of a particle; here what can I let move if I don't have a particle yet?

SALECKER: Can you not quantize this theory to get the results for elementary forces?

KURSUNOGLU: One develops a unified theory in order not to do anything with quantum theory. If there are any quantum effects, they must be contained in the non-linearity which is caused by the finite value that we have introduced for the electromagnetic field. Quantization is out of the question.

LICHNEROWICZ: It seems to me that many physicists do not like this type of unified field theory. We have a very good theory of propagation, but it is difficult to find physical interpretations, because the theory is in a sense too unified. We have a good interpretation for the metric or the gravitational part, but it is difficult to obtain a good interpretation for the electromagnetic part. The first problem is not to obtain the equations of motion; the first problem is to obtain by a process of approximation a good identification.

BERGMANN: I believe that we have the following principal problems: Firstly, the identification, I agree with you. Secondly, the actual finding of the solutions which are free of singularities. If one believes in the unified field theory approach, he must face the question: does the theory lead to solutions which can be interpreted as particles? The equations of motion are a subsidiary problem. If there are such solutions, are they stable?

WHEELER: Perhaps the first question to be asked about unified field theory is "why?". We have non-singular solutions in geons in the present theory.

BERGMANN: All of the geons involve stochastic methods. A rigorous solution would be one in which no averaging operation were performed, where you can tie down a single solution.

WHEELER: A geon has exactly the property of being only an approximate solution; or rather, an accurate solution which is not fully stable with time - it leaks energy. Thus it is not in agreement with one's preconceived idea that there should be a particle-like solution that is fully stable; but aren't we being very brash if we say that the world isn't built that way? How do we know that the leakage of energy is not a property of high mass particles which leak energy and drop down to lower mass particles? Perhaps the stability of the particles we know is due to some intrinsically quantum character, which we cannot expect to show up before we have gone to the quantum level.

BERGMANN: The original motivation of unified field theory is get a theory of elementary particles, which includes electrons and not only hyperfragments, and furthermore to obviate the need for quantization which would result from the intrinsic non-linearity.

FEYNMANN: Historically, when the unified field theory was first tackled, we had only gravitation, electrodynamics, and a few facts about quantization, so it was natural to try to write down equations which would only unite the Maxwell equations with the gravity equations and leave out of account the strange quantum effects, with the hope that the non-linearity would produce this business. In the meantime, the rest of physics has developed, but still no attempt starts out looking for the quantum effects. There is no clue that a unified field theory will give quantum effects.

ROSENFELD: We know that there are forces which are certainly not reducible to electromagnetic and gravitational forces; forces of finite range. It seems that this unification program is anachronistic. I think that it is an illusion to hope that the particle can be reduced to some distinct distribution, either singular or non-singular, for the particle must be regarded as a field itself in quantum theory.

LICHNEROWICZ: We have here a theory which has a definite mathematical meaning. In quantum theory, there are many good experiments, but many parts of quantum theory are not good theories. There are various difficulties with mathematical consistency in a large part of the quantum theory.

BONDI: We should regard general relativity not as a supertheory, but as the best theory of gravitation we have got, which describes a certain set of phenomena better than any other theory we have.

KURSUNOGLU: In unified field theory, one tries to make particles, sort of mass, out of the electromagnetic field. This is also the case in quantum electrodynamics; the field creates mass, the electron-positron pairs.

FEYNMAN: Let's ask ourselves the question: If we looked at things on a large scale, and saw them gravitating, we should discuss the character of the singularities; but, gentlemen, look at one of the singularities: we're sitting on it. Look at the degree of complexity that it has; the lights and the colors, and the trees, and the particles that it's made out of. Can you believe that in the fact that they gravitate there lies the key to all this confusion? Wouldn't it be more reasonable that gravity were a result of something on the small scale?

ANDERSON: To describe elementary particles, we must adopt a space-time description; you must say something about the metric. Relativity is not only a theory of gravity, but also a theory of space structure.

WITTEN: There may be another reason to look for a unified field theory, as a step toward quantization. Now we quantize a lot of fields, and it might be easier to quantize just one unified field.

BONDI: One important point: gravitational forces are not self-compensating and that is why they predominate on the large scale. I think that's a side that should be looked at more, in the large.

Chapter 16
Presentation of Work by T. Taniuchi

Ryoyu Utiyama

UTIYAMA: I am going to talk about an article of my colleague, Dr. T. Taniuchi.

In quantizing the gravitational field, or any type of non-linear fields, particular attention should be paid to the following points.

In general a disturbance of non-linear fields in Minkowski-space propagates with a super-light velocity in some cases and with a sub-light velocity in some other cases. Namely, the velocity of non-linear fields usually depends on the strength of fields. This means that it is not so clear as in the case of linear fields whether any couple of field-quantities on a space-like surface is commutable or not. Another remarkable feature of non-linear fields is the following. Suppose that some initial value is given which has no discontinuity on the initial spacelike surface. In some cases, however, some discontinuity of the field strength (shock wave) may be expected to occur after the time passed. On the contrary, even though the initial value has some discontinuity, at a later time such a discontinuity will be expected to be smeared out owing to the non-linearity. In general the occurrence of shock waves depends not only on the type of non-linear fields but also on the type of initial values.

Owing to these situations mentioned above, it seems necessary to investigate the character of propagation of the gravitational field before making quantization of this field.

However, since the gravitational field is too complicated to deal with, we considered the following two cases as our training, the Born-type field with the Lagrangian density [1]

$$L = l^2\sqrt{1+l^{-2}(\Phi_{,x}^2-\Phi_{,t}^2)}$$

$$l = \text{constant with a dimension (length)}^{-2},$$

and the Landau-Khalatnikow-type field [2, 3]

$$L = \frac{1}{4}\{\Phi_{,x}^2 - \Phi_{,t}^2\}^2.$$

These two types of non-linear field were adopted by Heisenberg and Landau respectively in order to explain the phenomena of multiple meson-productions.

Our method of solving these equations is completely similar to that of Courant and Friedrichs stated in their textbook [4]. We are also planning to investigate the equation of gravitational field in some simple case.

(1) Case of Born-type field: The Lagrangian density is

$$L = l^2\sqrt{1 + l^{-2}(\Phi_{,x}^2 - \Phi_{,t}^2)}.$$

Put $\quad u = \Phi_{,x}\ ,\ \ v = -\Phi_{,t}.$

Then the field equation is

$$(v^2 - l^2)u_x + (u_t - v_x)uv - (u^2 + l^2)v_t = 0,$$
$$u_t + v_x = 0.$$

In this case the two kinds of characteristics c^+-curves and c^--curves consist of two sets of parallel straight lines respectively and shock waves do not occur.

Consider the following initial condition:

at $t = 0$

$$u(x) \quad = 0 \qquad \text{for all } x$$
$$v(x) \quad \begin{cases} = 0 & \text{for } |x| > a \\ = \text{const.} = \alpha & \text{for } |x| < a \end{cases}$$

The solution in this case is shown in Figure 16.1, where

$$\overline{OP} = a/\{1 - (\alpha/l)^2\}^{1/2}.$$

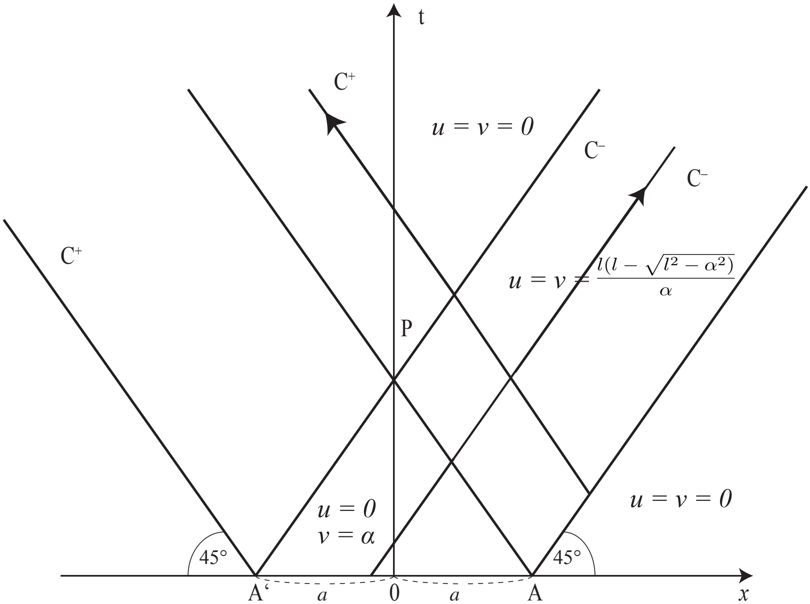

Figure 16.1: Solution of Born-type field

The initial discontinuity at A propagates with the light velocity. The feature of propagation is quite similar to that of linear fields, because this particular non-linear field becomesidentical with the linear field in case of an extremely weak field.

(2) Case of Landau-Khalatnikow-type field: The Lagrangian density is

$$L = l^2 \sqrt{1 + l^{-2}(\Phi_{,x}^2 - \Phi_{,t}^2)}.$$

$$\text{Put} \qquad u = \Phi_{,x} \;,\; v = -\Phi_{,t}.$$

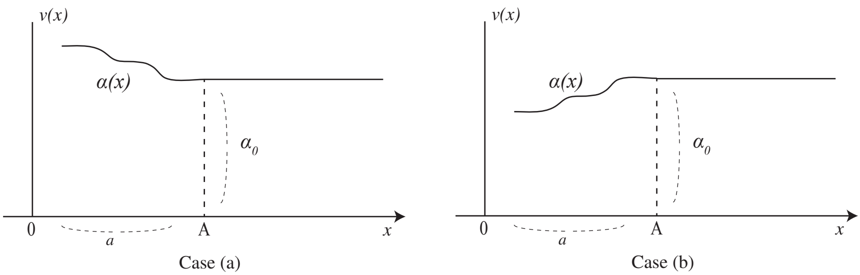

Figure 16.2: Initial value for L-K-type field

Let us consider the two kinds of initial values shown in Figure 16.2. In the case of (a), namely $\alpha(x) > \alpha_0$, the c^+-characteristics intersect with each other and the shock wave occurs in spite of the continuous initial value (see Figure 16.3). On the other hand, in the case of (b), namely $\alpha(x) < \alpha_0$, the c^+-characteristics diverge from each other, that is, the rarefaction wave occurs (see Figure 16.4).

Let us also consider the same initial condition as that of Born-type field. The solution is illustrated in Figure 16.5. In this case the discontinuity of v at A is smeared out, and u and v at any later time $t > 0$ continuously vanish on the light-cone $\overline{AB}$. Contrary to linear fields, the disturbance of the field soaks into the region CPC'.

From these results we can see that the latter type of field is more adequate to the explanation of the phenomena of multiple meson-production.

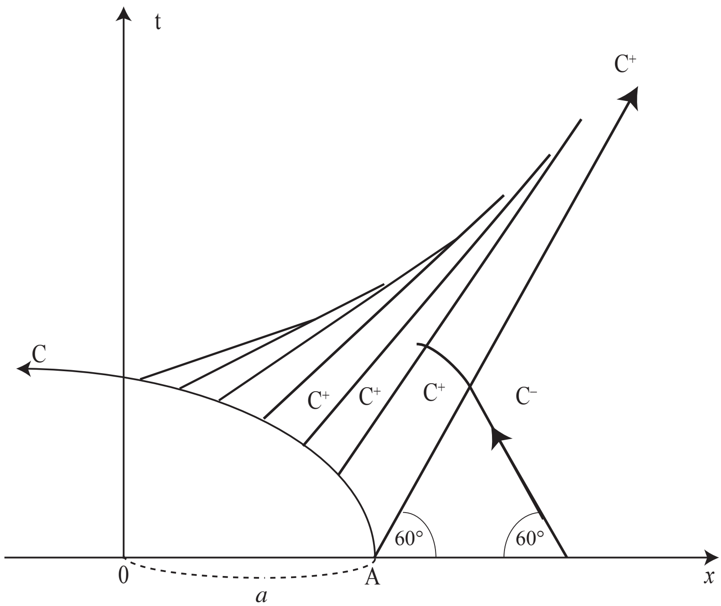

Figure 16.3: Characteristics for (a)

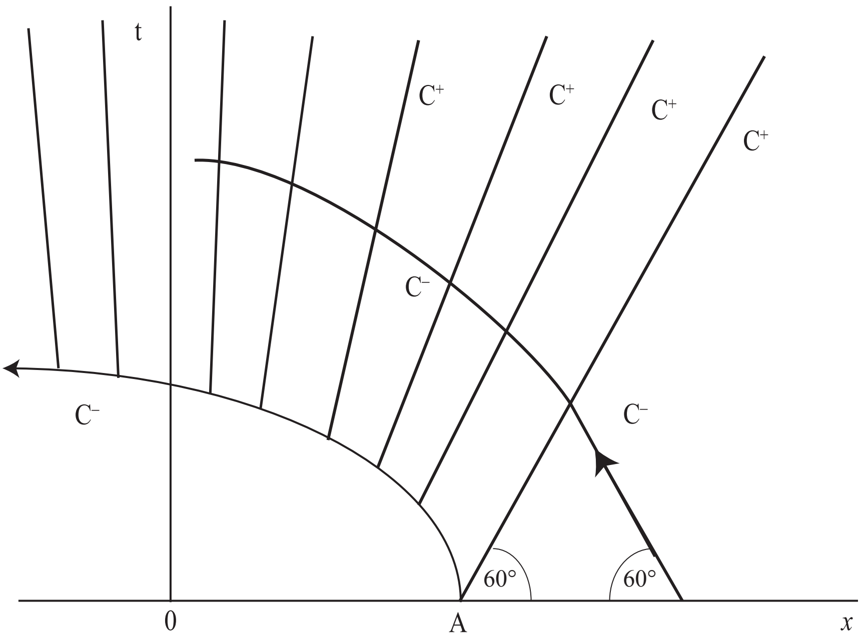

Figure 16.4: Characteristics for (b)

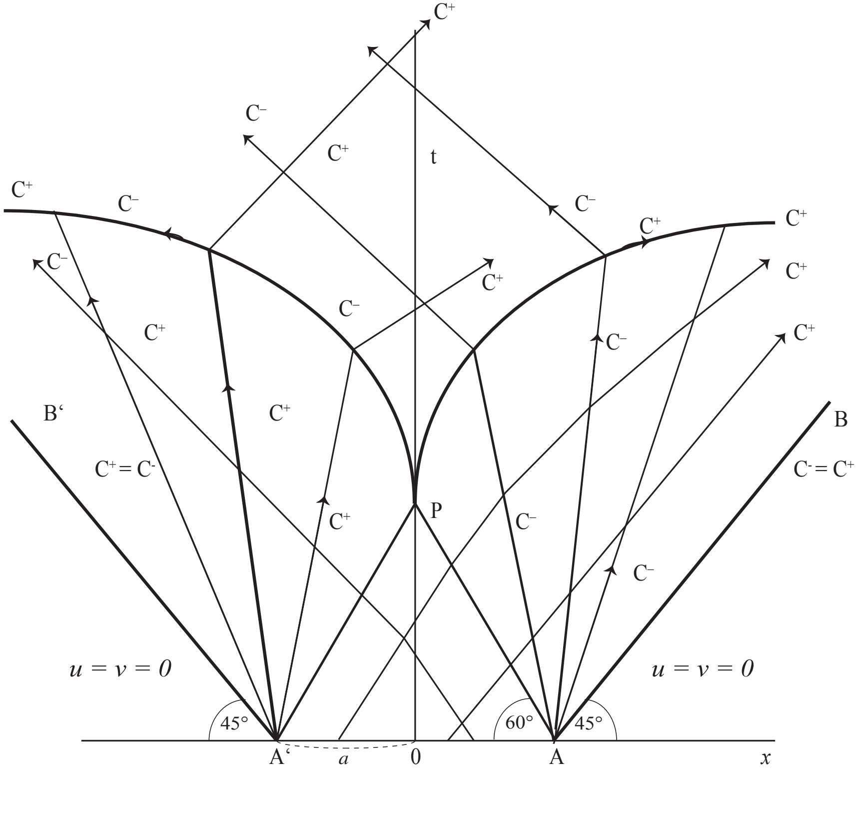

Figure 16.5: Solution of L-K-type field

DE WITT: Does Professor Lichnerowicz know whether gravitational waves form shocks? Do the characteristics cross?

LICHNEROWICZ: Not for a regular model.

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Chapter 17
Negative Mass in General Relativity
Hermann Bondi

In general relativity there is no obvious reason why masses shouldn't be negative, so we will investigate this concept. We can get three definitions of mass: inertial mass, passive gravitational mass on which the field acts and active gravitational mass which is the source of the field. In Newtonian theory, due to action and reaction, passive and active gravitational masses must be the same. The fact that passive and inertial masses are the same is the consequence of experiments. In general relativity, passive and inertial masses are the same due to the principle of equivalence. The first place where active gravitational mass occurs is the m in the Schwarzschild solution. There is no action and reaction principle, and we know that active and passive masses may not be equal. In Newtonian theory, the most obvious notion of a negative mass is similar to charge; like masses attract and unlike repel. In general relativity, a negative mass repels all masses, a positive attracts all. If we have a system with one mass of each type, the system will uniformly accelerate. I have constructed an exact solution of the field equations which shows these properties. The solution fills only one-fourth of space-time. I have found a method of continuing the solution into the other regions.

BERGMANN: I think one can show that within the general theory, it is not possible to have the active and passive masses different.

BONDI: This is a question of conservation of momentum, which means integrals over extended regions of space. But what do you say about the Schwarzschild interior solution where the m is certainly not just the integral of the g_{00}?

GOLD: What happens if one attaches a negative and positive mass pair to the rim of a wheel? This is incompatible with general relativity, for the device gets more massive.

BONDI: The purpose of this is to show that negative mass is incompatible, but I haven't got there yet.

PIRANI: I want to reply about active and passive mass. One can show simply that they are different and that the density of one is got by taking the density of the other and subtracting the principal stresses.

A Dynamic Instability of Expanding Universes
R. Mjolsness

Small deviations of the metric from exact sphericity are considered in order to investigate the stability of the standard Friedman solution. An oscillating cosmological model is used with positive curvature and zero cosmological constant. In order to solve the problem exactly, a relation between the pressure and the density is needed. Two cases are considered for which this relation is known; the dust-filled and the radiation-filled universe. This problem is similar to one done by Lifschitz in 1946. He wished to discover whether one can account for the formation of nebulae in an expanding universe of negative curvature. He found that the perturbations do not grow sufficiently rapidly. Here, the question is reopened, for the perturbations do grow sufficiently rapidly. Deviations of the metric from spherical symmetry are expanded in terms of tensors formed from hyperspherical harmonics. Two of the four types of tensors formed give no contribution to a change in the density, but two of them do. These two result in two coupled ordinary differential equations. The case of the dust-filled universe is completely solvable and unstable. Work on the radiation-filled case has been started. Here it appears that there is a solution in which the perturbations grow. There is a close analogy with the problem of an oscillating underwater bubble which is well known to show Rayleigh-Taylor instability against small departures from a spherical form.

BONDI: Have you tried this for the Newtonian cosmological models of Milne and McCrae?

MJOLSNESS: No.

BONDI: They have an extremely close analogy; on the cosmological and galactic scale we get the same equations. I have played about with that, and I think that you always get that while the relative density fluctuations increases, the density goes down.

WEBER: In the topological models of Wheeler, one forms charges out of fields. The question arises how one can measure fields without charges. I will outline one method. If a neutral body is constructed out of fields and placed in an external uniform field, the body will become polarized and appear to have a dipole moment. If two such neutral bodies are placed in a field, one can determine the existence of an external field from their motion. In addition, the fact that a field is capable of polarizing a neutral body implies that a neutral body should repel a charged body. This may have some cosmological significance, but the forces are very small.

SCIAMA: Instead of studying all of the complexities that exist in the symmetric theory of gravitation, I want to propose the possibility that the theory of the pure gravitational field should be based on a non-symmetric potential if the sources have spin. When one tries to define spinors in such a scheme, certain definite statements about elementary particles may be made, which can be checked within a year or two. The original motivation for introducing a non-symmetric potential is heuristic. From special relativistic field theory, we know that the energy momentum tensor is non-symmetric if the system has spin. Belinfante has shown how this tensor can be symmetrized. This procedure constructs a complicated energy momentum tensor whose moment contains the spin; but the spin is not fundamentally a moment, as orbital angular momentum is.

As a purely heuristic argument, it might be less artificial to keep the energy momentum tensor non-symmetric, so that it is apparent that the system possesses spin. In that case, one is forced to construct a theory of gravitation with a non-symmetric potential. However, we make an exception of fields with zero rest mass, for in this case we cannot make a distinction between spin and orbital angular momentum. We construct a theory whose mathematics is similar to Einstein-Schrödinger type theories, but has nothing to do with the electromagnetic field. We find that the orbital angular moment in the special relativistic limit is the moment of the canonical stress energy tensor. The quantity $g^{\underline{ij}}$ plays the same role in the equations of motion of a scalar test particle as g_{ij} in the symmetric theory. To introduce spinors in the non-symmetric theory, we use a Hermitian metric with symmetric real part and non-symmetric imaginary part. This allows us to introduce complex vierbeine, so we can perform unitary transformations rather than rotations and this leads to a Hermitian requirement for the energy momentum tensor. To get something analogous to spinors, we must discuss quantities that transform irreducibly with respect to the unitary group. There are two main differences between this group and the Lorentz group. First, the Lorentz group is composed of four disconnected parts, while the unitary group envelops all continuously. This suggests that parity must be conserved in such a scheme. These representations are complex, and thus describe charged particles; strictly neutral particles must be described by real wave functions and thus are subject only to the Lorentz subgroup and will have symmetric energy momentum tensors. Neutral particles may not conserve parity. Also, neutral particles must have zero spin, zero rest mass, or both. The second difference relates to topological properties; the topological properties of the unitary group are more complicated, and this allows one more freedom in constructing representations. A phase transformation is included under the unitary group. If one has only one field, the phase transformation cannot be determined; but if several fields interact, this leads to selection rules. The question now is can we arrange these rules to forbid certain interactions which are known not to occur in nature, which are consistent with the present selection rules. This work is not yet complete; but one can presumably find rules which will prevent heavy particles from decaying into light particles, and thus understand the conservation of heavy particle number.

KURSUNOGLU: In this theory with a complex representation of field variables, isn't the velocity of light greater than its normal value as it is with Einstein's λ invariance?

SCIAMA: I presume that the motion of light will be given by the same quantity that governs the equations of motion.

BERGMANN: This is a different theory, you don't have λ invariance.

A discussion of the recent discoveries regarding parity followed. Professor ROSENFELD spoke of the contents of a recent note from Landau in which he suggested that in weak interactions, the coupling is not invariant for parity, or charge conjugation, but for the product of the two. By introducing this combined parity, one is left with invariant couplings; one can't have one of the transformations without the other.

Session VI Quantized General Relativity

Chairman: J. A. Wheeler

Chapter 18
The Problems of Quantizing the Gravitational Field
P. G. Bergmann

This session opened the second half of the conference, devoted to discussion of the problems of *quantizing* the gravitational field, previous sessions having been restricted to the classical domain. The first contribution was an introduction by P. G. BERGMANN outlining the present position of the infant subject of quantum gravidynamics, indicating why one is interested in it in the first place, and stating some of its outstanding problems. The following is a summary of BERGMANN's introduction:

BERGMANN first asked the question, "Why quantize?" His reply was that physical nature is an organic whole, and that various parts of physical theory must not be expected to endure in "peaceful coexistence." An attempt should be made to force separate branches of theory together to see if they can be made to merge, and if they cannot be united, to try to understand why they clash. Furthermore, a study should be made of the extent to which arguments based on the uncertainty principle force one to the conclusion that the gravitational field must be subject to quantum laws: (a) Can quantized elementary particles serve as sources for a classical field? (b) If the metric is unquantized, would this not in principle allow a precise determination of *both* the positions and velocities of the Schwarzschild singularities of these particles?

These aims have not yet been achieved, but BERGMANN expressed certain hopes as to the results of such a program: Quantization of the gravitational field is likely to have a profound effect on our notions of space and time, making all distance and volume concepts subject to uncertainty relations. Thus, in spite of the quantitatively negligible character of the gravitational forces between elementary particles, it is conceivable that (a) the gravitational field may help to eliminate the divergences of quantum field theory (which result from the compounded effect of singularities in particle propagators) by smearing out the light cone, and (b) it may somehow contribute to the structure of elementary particles. In regard to the latter point, however, BERGMANN expressed his opinion that one cannot hope to get the complete structure of the elementary particles from any quantized unified field theory that is principally motivated by the desire to unify *just* the gravitational and electromagnetic fields.

BERGMANN emphasized the formidable nature of the problem of quantizing generally covariant theories and expressed the conviction that one will first have to have a thorough clarification of the underlying conceptual problems in the classical theory. He then went on to outline the principal methods of approach to the purely technical problems of quantization which we now possess: (1) The canonical Hamiltonian method (Dirac); (2) The Lagrangian method (Schwinger); (3) The path-summation method (Feynman). It appears that the need to identify the so-called "true observables," or coordinate-invariant quantities, arises in all three schemes. Furthermore, this identification will be intimately related with the structure of the transformation groups under which the classical theory remains invariant.

In the quantum theory the state vector of a generally covariant system will be subject to various constraints which must, of necessity, be imposed owing to the existence of the invariant transformation groups. A "true observable" will be described by an operator which, when applied to the state vector, produces another vector which satisfies the same constraints as the original vector. A reduced Hilbert space is envisaged in which the only canonical transformations which are physically meaningful are those generated by true observables. The constraint operators themselves qualify as true observables under this definition, but they are trivial, being simply "null generators." BERGMANN believes that it is immaterial whether the Lagrangian or Hamiltonian approach is used to discover the nontrivial true observables; the results will be the same in either case.

As remaining problems which must eventually be looked at, BERGMANN gave the following partial list:

1. The hyperquantized particle field vs. the treatment of particles as singularities in a quantized gravitational field.
2. The interaction of the gravitational field with fermions.
3. The interaction of the gravitational field with other quantized fields.
4. The relation of elementary particle theory to unitary field theories.
5. The relation of the law of conservation of energy and momentum arising from the coordinate transformation invariance of general relativity to other *strong* conservative laws of physics with their associated invariance groups.

Discussion then turned to the problems of measurement of the gravitational field. This item was placed first on the agenda in an attempt to keep physical concepts as much as possible in the foreground in a subject which can otherwise be quickly flooded by masses of detail and which suffers from lack of experimental guideposts. The question was asked: What are the limitations imposed by the quantum theory on the measurements of space-time distances and curvature? Since the curvature is supposed to be affected by the presence of gravitating matter, an equivalent question is to ask: What are the quantum limitations imposed on the measurement of the gravitational mass of a material body, and, in particular, can the principle of equivalence be extended to elementary particles? (In the interest of clarifying the importance of some of the following discussion, the editors would like to point out at least one reason, which was not given sufficient attention at the conference, why the answer to the latter question is not a simple matter of dimensional arguments. In quantum gravidynamics there exists a natural unit of mass, namely $\sqrt{c\hbar/G} \approx 10^{-5}$ g. One might be tempted to suppose that this represents a lower limit on the mass of a particle whose gravitational effect can in principle be measured. That this conclusion is wrong can be shown by the following arguments:

Consider just a classical test particle of mass m which is initially at rest at the point x_0 (we restrict ourselves to one dimension) in a gravitational potential φ. At the time t the position of the particle will be

$$x = x_0 - \frac{1}{2}\frac{\partial\varphi}{\partial x}t^2.$$

The gravitational field strength is given by $\partial\varphi/\partial x$ and can immediately be determined by a measurement of t and x. If however the particle is subject to quantum laws its initial position and velocity are subject to (root mean square) uncertainties related by

$$\Delta v_0 = \frac{\hbar}{2m\Delta x_0}$$

leading to an uncertainty in position at time $t > 2m\Delta x_0^2/\hbar = \hbar/2m\Delta v_0^2$ of amount

$$\Delta x = \Delta v_0 t = \frac{\hbar t}{2m\Delta x_0}.$$

The initial position measurement may be made by a photon of momentum uncertainty $\Delta p = m\Delta v_0$ (or, in principle, by a graviton having the same momentum uncertainty if one prefers to deal with absolute electrically neutral test particles!). The resulting uncertainty in the initial time may be ignored since it is given by $\Delta t_0 = \Delta x_0/c = \hbar/2mc\Delta v_0 << t$ (assuming $\Delta v_0 << c$). The final position and time measurements may be made with arbitrarily high precision, using energetic photons, since the experiment is then over.

The gravitational field can now be determined from the classical equation if

$$\left| \frac{\partial\varphi}{\partial x} \right| t^2 >> \frac{\hbar t}{m\Delta x_0} >> \frac{\hbar}{mc}.$$

However, one must be able to choose t small enough so that $|\partial\varphi/\partial x|$ does not change appreciably during the course of the motion. This imposes a condition on the gravitational field, namely

$$\left| \frac{\partial^2\varphi}{\partial x^2} \right| \left| \frac{\partial\varphi}{\partial x} \right| t^2 << \left| \frac{\partial\varphi}{\partial x} \right|$$

or

$$\frac{\left| \frac{\partial\varphi}{\partial x} \right|}{\left| \frac{\partial^2\varphi}{\partial x^2} \right|} >>>> \frac{\hbar}{mc}.$$

If the gravitational field is produced by a point mass M then $\varphi = -GM/x$, $\partial\varphi/\partial x = GM/x^2$, $\partial^2\varphi/\partial x^2 = -2GM/x^3$, and the required condition becomes $x >>>> \hbar/mc$ which can always be satisfied. Evidently the gravitational field of any mass is measurable in principle because of the long "tail" in the Newtonian force law. In fact, the long tail is what permits us to "measure" the gravitational mass of, for example, a nucleon, by measuring the gravitational mass of a large ball of lead and dividing by the known number of nucleons in it.

It is, however, still a matter of basic interest to determine whether or not the measurement could be carried out on a single elementary particle. Here the main practical problem concerns the measurement of time. One has the restriction $t >> (h/mc)^{1/2}/|\partial\varphi/\partial x|^{1/2}$ or

$$t >> \sqrt{\frac{\hbar x^2}{GMmc}} >>>> \sqrt{\frac{\hbar^3}{GMm^3c^3}}.$$

If both the test particle and source has protonic mass then

$$\sqrt{\frac{\hbar^3}{GM^4c^3}} \approx 10^{-5}\,\text{sec.},$$

and all conditions may seemingly be satisfied by choosing t to have the not unreasonable value $t = 1$ sec. Moreover, no complications should arise due to the nonlinear character of the gravitational field (the linear Newtonian approximation should be masses as small as a proton).

Nevertheless, a number of subtleties enter the problem at this point, chiefly concerning the nature of the recording apparatus, or clock, which measures the requisite time intervals (independently of the photons which interact with the test particle) and how the presence of the clock's mass in the neighborhood may influence results of the experiment.)

Chapter 19
Conceptual Clock Models
H. Salecker

H. SALECKER opened the discussion by reporting on some conceptual clock models which he has analyzed in collaboration with E. P. Wigner. He first made some general comments on the fundamental nature of space-time distance measurements: In ordinary quantum mechanics the space-time point is specified by its four coordinates, but no prescription is given as to how these coordinates are to be measured. This, however, is in conflict with the principles of the general theory of relativity, according to which coordinates have no meaning independent of observation. A coordinate system can be defined only if space-time distance measurements can be carried out in principle without restrictions. SALECKER then went on to point out that the use of clocks alone is sufficient to measure both space-time and time-like distances. This is important since measuring rods, in contrast to clocks, are essentially macro-physical objects which will strongly influence other objects during the measuring procedure through their gravitational fields. Moreover, the measurement of distances between space-time points is considerably more complicated with measuring rods than clocks because of the Lorentz contraction of rods.

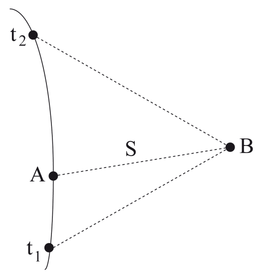

Figure 19.1

The method by which a clock can be used to determine a space-like distance between two events A and B is as follows: Let the geodesic world line of the clock pass through the event A at time t_A, and suppose that the clock emits a continuously modulated light signal, e.g., monotonically changing color.[1] Let this signal be reflected at event B by a briefly exposed mirror. The time of emission t_1 of the reflected portion of the signal will be determined by inspection of the color of this portion when it eventually returns to the clock at time t_2. The invariant distance S between A and B is then given by

$$S^2 = c^2(t_2 - t_A)(t_A - t_1)$$

provided S is small compared to the radius of curvature of the space-time neighborhood of the two events.

A time-like distance can of course be measured by the obvious method of letting the clock pass along a geodesic between the two events.

SALECKER then described the results of considering the clock itself, and the process of reading it, in greater detail. Although his actual exposition suffered from lack of time in which to present the material, he very kindly gave the editors access to the manuscript of a paper which he will shortly publish on the subject. The following is a resume of portions of his paper. Certain simplifications have been made, leading to slight changes in numerical coefficients, for which the editors are entirely responsible.

One must first ask the question: What is the accuracy with which a clock can be *read*, independently of inherent inaccuracies of the clock itself? At the beginning of a time interval an observer may emit a light signal to read the clock. If the length of the signal's train is l and if the pulse is properly shaped, the root mean square uncertainty in the initial time measurement will be

$$\Delta t_l = l/2c.$$

The clock may simultaneously be made to emit a photon to compensate for recoil. However, an inevitable uncertainty in the clock's momentum will remain, of amount

$$\Delta p_l = \hbar/l = \hbar/2c\Delta t_l$$

corresponding to an energy uncertainty in the light signal of amount

[1] SALECKER let the position of event B be simply determined by the time of emission t_1 of a pulsed signal. The editors have inserted the idea of color modulation in order to avoid objections based on the principle of causality.

$$\Delta E_l = c\Delta p_l = \hbar/2\Delta t_l.$$

The clock will generally have also an initial position uncertainty Δx giving rise to a momentum uncertainty

$$\Delta p_x = \hbar/2\,\Delta x.$$

The total momentum uncertainty after the first reading is therefore

$$\Delta p = \Delta p_x + \Delta p_l = \hbar\left(\frac{1}{2\Delta x} + \frac{1}{l}\right)$$

corresponding to a velocity uncertainty of amount

$$\Delta v = \frac{\Delta p}{M} = \frac{\hbar}{M}\left(\frac{1}{2\Delta x} + \frac{1}{l}\right)$$

where M the mass of the clock. (We neglect here certain relativistic corrections which SALECKER considered.) If x is the mean distance from the observer to the clock then the time at which the observer receives the initial reading from the clock will be

$$t_1 = (x \pm \Delta x)/c \pm \Delta t_l.$$

The time at which the observer receives a second reading at the end of a (clock's) time interval t will be

$$t_2 = (x \pm \Delta x \pm \Delta vt)/c \pm \Delta t_l + t,$$

giving for the total inaccuracy in the reading of the time interval

$$\begin{aligned}\Delta t &= |t_2 - t_1 - t|_{max} = 2\,\Delta x/c + 2\,\Delta t_l + \Delta vt/c \\ &= \frac{1}{c}(2\Delta x + l) + \frac{\hbar t}{Mc}(\frac{1}{2\Delta x} + \frac{1}{l}).\end{aligned}$$

The minimum uncertainty is achieved by choosing $2\Delta x = l = (\hbar t/M)^{1/2}$, which yields

$$\Delta t_{min} = 4(t_c t)^{1/2} \qquad t_c = \frac{\hbar}{Mc^2}.$$

Evidently the accuracy of reading is greater the larger the mass of the clock. On the other hand, the gravitational field of the clock will be disturbing if its mass is made too large. One has therefore to consider the problem of how to construct a clock which shall be as light and as accurate as possible. One must at this point take into account the inherent inaccuracies in the clock itself, by considering its atomic structure.

A single atom by itself represents to a very high degree of accuracy an oscillator, but in spite of this it is not possible to take a single atom emitting radiation as a clock. Before an oscillator can be considered to be a clock it must be possible to register its information; i.e., the atom must be coupled with a device to count the number of times the emitted electromagnetic field strength reaches a certain value. Such a counting device would be in contradiction with the principles of quantum mechanics.

As his first model, therefore, SALECKER considered a *statistical* clock, composed of a certain number N_0 of elementary systems initially in an excited state. The systems were assumed to go over directly to a ground state and to be sufficiently well separated so as not to reexcite one another. If the decay rate is λ, then the probability of finding $\mathscr{N}$ systems remaining in the excited state after a time t is

$$P(N,t) = \frac{N_0!}{N!(N-N_0)!}\omega^N(1-\omega)^{N_0-N}$$

where

$$\omega = e^{-\lambda t}.$$

The average value of N and the root mean square deviation at time t are given respectively by

$$\overline{N} = N_0\omega$$
$$\Delta\overline{N} = \sqrt{N_0\omega(1-\omega)}.$$

The registering device in the clock has only to count the number of atoms remaining in the excited state (or, alternatively, the number of systems which have decayed) in order to record a statistical time given by

$$t_{stat} = \frac{1}{\lambda}\ln\frac{N_0}{N}$$

which has an uncertainty of amount

$$\Delta t_{stat} = |\Delta \frac{1}{\lambda} \ln \frac{N_0}{N}| = \frac{1}{\lambda} \frac{\Delta \overline{N}}{N} = \frac{(e^{\lambda t} - 1)^{1/2}}{\lambda N_0^{1/2}}.$$

The minimum uncertainty is achieved by choosing $\lambda = 1.6t$, which yields

$$\Delta t_{stat\ min} = 1.2 \frac{t}{N_0^{1/2}}.$$

A final uncertainty arises from the fact that the registering device must distinguish between excited and unexcited systems, and for this purpose a time Δt_m at least as great as $h/(E_1 - E_0)$ is required, where E_1 and E_0 are respectively the elementary excited and ground state energy levels. In the most favorable case the decaying systems would undergo complete disintegration with, for example, the emission of two photons in opposite directions so as to eliminate recoil. The registering device might then be a counter to detect the photons, but however constructed it would have an absolute minimum time inaccuracy of amount $\Delta t_m = \hbar/mc^2$, corresponding to photon energies of order mc^2 where m is the mass of a decaying elementary component of the clock.

The total inaccuracy in the measurement of a time interval t by means of a statistical clock is therefore at least

$$\begin{aligned} \Delta t_{tot} &= \Delta t_{min} + \Delta t_{stat\ min} + \Delta t_m \\ &= 4(t_c t)^{1/2} + 1.2 \frac{t}{N_0^{1/2}} + (1 - \frac{\mu}{M})^{-1} N_0 t_c \end{aligned}$$

where μ is the mass of the registering device plus the framework of the clock, and $M = N_0 m + \mu$ is the total clock mass. If one is clever enough in the construction of the clock the number $(1 - \frac{\mu}{M})^{-1}$ may be kept from being excessively large. The minimum total inaccuracy for a given total mass M is achieved by choosing

$$N_0^{3/2} = 0.6(1 - \frac{\mu}{M}) \frac{t}{t_c}$$

which yields

$$\Delta t_{tot\ min} = 4(t_c t)^{1/2} + 2.1(1 - \frac{\mu}{M})^{-1/3} (t_c t^2)^{1/3}.$$

SALECKER considered also another type of statistical model, with essentially the same results. It should be pointed out immediately that for a given M and t (e.g., suitable for a certain experiment) it may be utterly impossible to choose the optimum value of N_0, because of the very limited range of elementary-system masses m occurring in nature. SALECKER therefore considered, as a third model, a very hypothetical type of clock, consisting of a single elementary harmonic oscillator, but operating at sufficiently high quantum numbers for it to be "read" like a classical device. Permitting himself to imagine an elementary oscillator which could function even at relativistic energies, he found for the minimum total uncertainty

$$\Delta_{tot\ min} \approx \frac{\hbar}{m'c^2} + \sqrt{\frac{\hbar t}{m'c^2}}$$

where $m' = m + E/c^2$, m being the rest mass of the oscillator and E is its excitation energy.

The quantity $\Delta_{tot\ min}$ is seen generally to depend on the time interval itself. When t becomes of the order of $\Delta_{tot\ min}$ then the time interval can no longer be measured. In every case the smallest time interval which can be measured is roughly

$$t_{min} \approx \frac{\hbar}{mc^2}.$$

That is, the time associated with the mass of one of the elementary systems out of which the clock is constructed represents an absolute minimum for measurable time intervals. This minimum arises, of course, from the coupling between the registering device and the elementary systems, and would exist even if the registering device were removed from the immediate vicinity of the clock proper. (For example, the registering device might be the observer himself, but then, in the case of the statistical clocks, the observer would have to send out a large number of photons so as to "read" each elementary system separately, thus raising the reading error from $4(\hbar t/Mc^2)^{1/2}$ to $4(\hbar t/mc^2)^{1/2}$.) Since, for existing elementary systems, $m \approx 10^{-24}$ g, the corresponding time $t_{min} \approx 10^{-23}$sec. may, presently at least, be regarded as an absolute lower limit to measurable time intervals.

SALECKER finished his discussion by reporting on the results of applying such clocks to the measurement of gravitational fields. This part of his work has not yet been written up, so we can only record the final results which he quoted. Three typical measurements were considered by him:

1. The direct measurement of $g_{\mu\nu}$.
2. The measurement of $\{{}^{\sigma}_{\mu\nu}\}$ through the observation of geodesics.
3. The measurement of the scalar curvature of a three-dimensional space-like cross section of a quasi-static region of space-time. This is defined by

$$K = \lim_{a\to 0} \frac{1}{a}\left(\sum_{i=1}^{3}\alpha_i - \pi\right)$$

where a is the area of a small triangle (or prism, in space-time) around which a test body is passed, and the α_i are its angles. (He also proposed that a direct measurement of the Riemann tensor should be carried out, by the method suggested in a previous session by Pirani. $\left(\frac{\partial^2\eta^a}{\partial t^2} = R^a_{0b0}\eta^b.\right)$ He had not been aware of this possibility prior to the conference.)

Carrying out these conceptual measurements on the gravitational field of a particle of protonic mass, using a particle of electronic mass as a test particle, and choosing the most favorable clock model to serve as measuring apparatus, SALECKER found for the uncertainty in the resultant measurement of the mass producing the field

$$\Delta M \approx 10^{-24}\mathrm{g}\ .$$

That is, the uncertainty is of the same order as the mass itself. From this, he concluded that *the gravitational mass of a single proton is not strictly an observable quantity.*

FEYNMAN asked SALECKER if he could write down a formula for ΔM in terms of fundamental constants, omitting dimensionless factors. SALECKER replied that this would be quite difficult. The difficulty seems to be that one has to account for the perturbing effect of the clock and, in effect, solve a two-body problem. The uncertainty in the measurement of time increases proportionally to some fractional power ($t^{1/2}$ or $t^{2/3}$) of the interval being measured, and decreases with increasing clock mass. On the other hand the bigger the clock the greater its perturbing (or masking) effect. One has to carry out a minimizing procedure in which the interval t, the protonic mass under observation, the clock mass M, and, in the case of statistical clocks, the number N_0 are all involved. In the resulting numerical computations the fundamental constants get mixed up in a very complicated way. SALECKER did say, however, that ΔM is proportional to the mass being measured, the factor of proportionality depending on the ratio of the gravitational radius of this mass to that of the clock.

(The editors would like to suggest that, in view of the long range character of the gravitational force, discussed previously, the gravitational constant may, in the last analysis, not enter into the factor of proportionality. Furthermore, the value actually found for ΔM may in some way reflect the fundamental limitation in the clock, viz. that it cannot be constructed out of elementary systems having masses smaller than those found in nature, i.e., protonic.)

FEYNMAN remained unconvinced of Salecker's result. He suggested the use of hypothetical atoms in which only two forces are operative: (1) the gravitational force and (2) some force which does not have an inverse square character. The relative phases of different time dependent states will then differ for atoms of different masses, and by waiting a sufficiently long time (e.g., 100 times the age of the universe!) one could measure the phase differences and hence infer the gravitational masses involved to as high a degree of precision as may be desired.

SALECKER replied that Feynman would still have to take into account the apparatus which measured this very long interval of time. Moreover, he pointed out that his own result did not take into account the possibility of pair production arising from necessity of measuring space-like intervals having dimensions less than the electronic Compton wavelength.

ROSENFELD asked how the expression for ΔM depends on $\hbar$.

SALECKER replied that he was unable to say, since his computations were started from the beginning with numerical values (e.g. 10^{-24} g for the proton mass, etc.).

ROSENFELD then took the floor to make some remarks about the difficulties of trying to extend the theory of measurement to the gravitational field in the manner in which he and Bohr had done for the electromagnetic field. He first said that he had tried to see how far one can get, following Salecker, in determining the gravitational potentials by metrical measurements rather than by dynamical measurements. (By dynamical measurements one can get only the derivatives of the potentials, metrical measurements give the potentials themselves, apart from a gauge factor.)

Roughly speaking, the inaccuracy in the measurement of a gravitational potential - say g_{00} - will be proportional to the inaccuracy in the determination of lengths. Therefore, if you can determine lengths with any accuracy, then you can also determine the potential with any accuracy. However, quantum considerations tell you that if the position of the measuring rod or clock is known to an accuracy Δx then its momentum is uncertain by an amount $\Delta p > \hbar/\Delta x$. This gives rise to an uncertainty in the value of the gravitational field produced by the measuring instrument.

The factors which saved Bohr and Rosenfeld in the electromagnetic case were:

1. Because of the existence of both negative and positive charge the perturbing field of the measuring instrument could be reduced to a dipole field.

2. The charge to mass ratio of the measuring instrument could be controlled.

Therefore, Bohr and Rosenfeld came to the conclusion that the measurement of any component of the electromagnetic field could be carried out with arbitrarily high precision in spite of the quantum restrictions.

These saving features are not present in the gravitational case. Whether one takes the measuring instrument heavy or light its perturbing effect will be roughly the same (proportional to Δp). Therefore ROSENFELD agreed with Salecker that there will be a fundamental limitation on the accuracy with which one can measure the gravitational field, although he could give no numerical estimate of this limitation.

WHEELER suggested that perhaps one should simply forget about the measurement problem and proceed with other aspects of theory. The history of electrodynamics shows that it is always a ticklish business to conclude too early that there are certain limitations on a measurement. He would propose rather to emphasize the organic unity of nature, to develop the theory (i.e., quantum gravidynamics) first and then to return later to the measurement problem. He suggested that this was particularly appropriate when we don't even understand too much about the classical measurement process! We don't know yet exactly what it is that one should measure on two space-like surfaces, i.e. the specification of the initial value problem.

He then went on to imagine what sort of ideas scientists might come up with if they were "put under torture" to develop a theory that would explain all the elementary particles and their interactions solely in terms of gravitation and electromagnetism alone! He first took a look a magnitudes and dimensions. In the Feynman quantization method one must "sum over histories" an amplitude which, in the combined electromagnetic-gravitational case has roughly the form

$$\exp \frac{i}{\hbar c}\left[\int E^2 d^4x + \frac{c^4}{G}\int\left(\frac{\partial g}{\partial x}\right)^2 d^4x\right].$$

Therefore if one is making measurements in a space-time region of volume L^4, contributions to this sum will be more or less in phase until variations in the electromagnetic and gravitational field amplitudes from their classical values become as large as

$$\Delta E \approx \frac{\sqrt{\hbar c}}{L^2}\,, \quad \Delta g \approx \frac{\frac{\sqrt{\hbar G}}{c^3}}{L} \approx \frac{10^{-33}\mathrm{cm}}{L}.$$

These represent the quantum fluctuations of the electromagnetic and gravitational fields. In the gravitational case, owing to the nonlinearity of the field equations truly new effects come into play at distances as small as 10^{-33}cm where Δg becomes of the order of unity. WHEELER envisages a "foam-like structure" for the vacuum, arising from these fluctuations of the metric. He compared our observation of the vacuum with the view of an aviator flying over the ocean. At high altitudes the ocean looks smooth, but begins to show roughness as the aviator descends. In the ease of the vacuum, WHEELER believes that if we look at it on a sufficiently small scale it may even change its topological connectedness, thus:

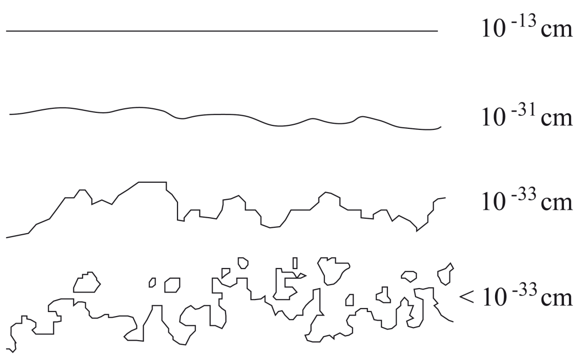

Figure 19.2

In this way he has been led to the concept of "wormhole" in space. A two-dimensional analog of a wormhole would look like this:

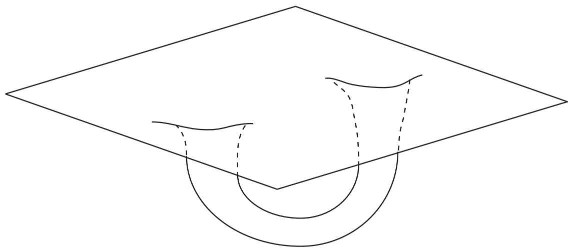

Figure 19.3

WHEELER pointed out another way in which a dimension of the order of 10^{-33}cm could be arrived at, by considering only the fluctuations in the electromagnetic field. A fluctuation of amount ΔE would correspond to an energy fluctuation of amount $\Delta E^2 L^3 = \hbar c/L$ in a region of dimension L. The gravitational energy produced in this region by this fluctuation would be of the order of $G(\hbar c/L)^2/c^4 L = \hbar^2 G/c^2 L^3$. The two energies become comparable when $L = \sqrt{\hbar G/c^3} \approx 10^{-33}$cm. In WHEELER's "dream of the tortured scientists" the wormholes may serve as sources or sinks of electric lines of force, no charge being actually involved since the lines of force may pass continuously through the wormholes. A surface integral of the lines of force over a region of dimension L would yield a value of the order of $\Delta E L^2 = \sqrt{\hbar c}$ which would represent a rough average of the apparent charge associated with each wormhole. No quantization of charge is implied here. In fact the wormholes themselves have nothing directly to do with elementary particles. WHEELER envisaged an elementary particle as a vast structure (10^{-13}cm) compared to a wormhole. However, he left open the possibility that elementary particles might somehow be constructed out of wormholes. He compared the wormholes to "undressed particles," their continual formation corresponding to pair production which is going on in the vacuum at all times. The electromagnetic mass associated with each wormhole is of the order of $\Delta E^2 L^3/c^2 = \sqrt{c\hbar/G} = 10^{-5}$g, but this huge mass is almost entirely compensated by an equivalent amount of negative gravitational energy.

(Editor's Note: No one at the conference thought to ask Wheeler why wormholes corresponding to magnetic poles would not be just as likely to occur as those corresponding to charges.)

WHEELER outlined five new concepts which have either already arisen or may yet arise as a result of pushing the ideas of general relativity to the limit:

(1) Electromagnetism without electromagnetism. By this, WHEELER was referring to MISNER's work which shows that the electromagnetic field can be completely specified by its stress tensor alone. But the stress tensor is in turn specified by certain derivatives of the metric. Hence, merely by looking at the metric one can tell all about the electromagnetic field. One might call this the "unified imprint theory," since the electromagnetic field leaves its characteristic imprint on the structure of space-time.

(2) Mass without mass. This concept is illustrated by *geons*, of which several varieties are currently under consideration:

 a) Electromagnetic geons.

 b) Geons built out of neutrinos.

 c) Geons built out of pure gravitational radiation alone.

(3) Charge without charge. Wormholes.

(4) Spin without spin.

(5) Elementary particles without elementary particles.

As for the last two ideas, WHEELER stated that so far the "tortured scientists" have been unable to come up with anything plausible, except for some vague suggestions that may temporarily stave off the punishment which awaits them if they fail to produce an answer. He thought that spin might arise in a Feynman quantization procedure owing to a possible double-valuedness in the sign of $\sqrt{-g}$ which occurs in the action.

FEYNMAN objected at this point, saying that as long as $-g$ is positive, $\sqrt{-g}$ is always of the same sign. A much more serious problem, he felt, would be to determine what happens if $-g$ changes sign.

BELINFANTE pointed out that the square root does not occur in the Lagrangian if the metric field is described in terms of "vierbeine".

WHEELER emphasized that he was not trying to give answers but merely pointing out what one might try to do if motivated by the overriding idea that "physics is geometry" and that everything can be derived from a master metric field.

WEBER wondered how one could get some quantities possessing spin and others which are spinless out of such a picture.

As for “elementary particles without elementary particles,” WHEELER suggested that one must first study the vacuum. The vacuum is in such turmoil, according to his picture, that it would be foolish to study elementary particles without first trying to understand the vacuum. He drew the following schematic dispersion curve for a wave disturbance (e.g., a photon or a graviton) propagating through the vacuum:

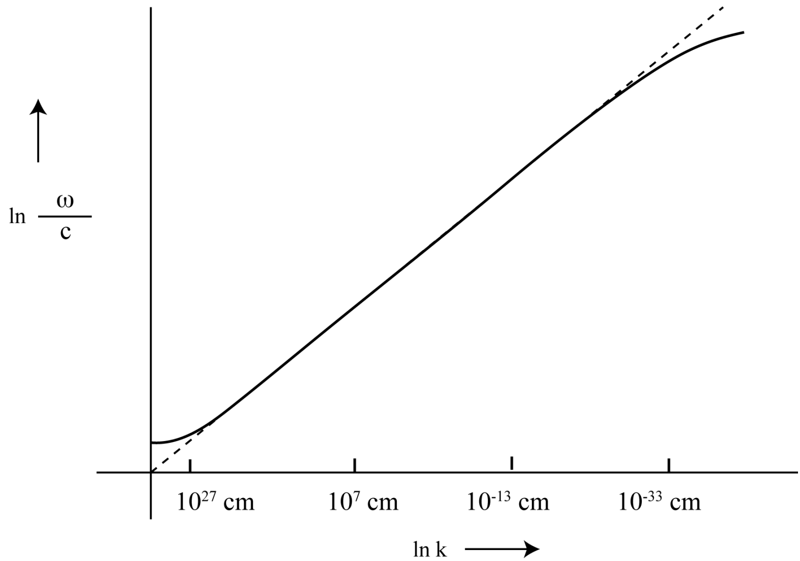

Figure 19.4

Over a tremendous range the wave satisfies the simple relation $\omega = ck$. When the wavelength becomes of the order of the size of the universe ($\approx 10^{28}$cm), however, the phase velocity is greater than c, as has been shown long ago by Schrödinger (*Papal Acad.*). On the other hand, when the wavelength becomes of the order of 10^{-33}cm the disturbance will be slowed down by the foam-like structure of the vacuum (and also by having always to climb over the metric bump which it is itself continuously creating due to its large energy concentration - Ed.). WHEELER suggested wedding the ultramacroscopic to the ultramicroscopic by finding the point of inflection on the dispersion curve. This would be the point at which disturbances would tend to hold themselves together. He also drew an analogy with superconductivity by suggesting that elementary particles might be the product of long range collective motions of the vacuum "foam," the scale of elementary particles being much larger than the scale of the wormholes, just as the scale of superconducting regions is much larger than that of the elementary structures which give rise to it. In view of possibilities like these, WHEELER concluded finally that one could not absolutely deny the possibility of explaining all things in terms of metric.

GOLD suggested that this was an "answer without an answer."

WEBER spoke for many by saying that he didn't understand how such a large number of wormholes ($\approx 10^{60}$) could give rise to such a well-defined quantity as the observed charge on an electron, except possibly by some statistical means.

FEYNMAN suggested that perhaps there was a unique stable ground state for a wormhole and that this would represent a sharply quantized "bare charge".

WHEELER pointed out that the discrepancy between the charge $\approx \sqrt{\hbar c}$ associated with an undressed particle and the observed charge $\sqrt{\hbar c/137}$ on an electron could be accounted for by vacuum polarization.

ROSENFELD then took the floor to express some second thoughts which he had had on the question of measurability. It seemed to him that, in principle, one could determine the mass of an arbitrarily small body to an arbitrarily high degree of precision by putting it on an ordinary spring balance and waiting long enough. Therefore he thought there was perhaps some doubt after all about Salecker's limitations on the measurability of the gravitational mass of elementary particles.

FEYNMAN pointed out however that Salecker was trying to measure the gravitational field produced by a small mass whereas Rosenfeld was here considering the response of that mass to a given gravitational field.

ROSENFELD said, nevertheless, that perhaps his original pessimism in regard to the measurability of gravitational fields, as compared to the electromagnetic case, might be unjustified. For example, it might happen that the uncertainties Δx and Δp in the measuring instrument affect different components of the gravitational potential, leading to reciprocal relations of the form

$$\Delta g_{00} \sim \Delta x \qquad \Delta g_{0i} \sim \Delta p$$
$$\Delta g_{00} \cdot \Delta g_{0i} \sim \hbar$$

which would not, however, prevent the precise determination of the value of a given component.

ANDERSON raised the question of the propriety of "measuring" quantities such as $\{{}^{\sigma}_{\mu\nu}\}$ which have neither invariance nor tensor properties.

BARGMANN replied that the insistence on invariance has often been overdone. An invariant can always be defined provided one simply introduces the measuring apparatus into the mathematical description.

ANDERSON said that "gadgets" then evidently introduce "preferred" situations into the scheme of things and permit one to measure non-invariant quantities directly.

BERGMANN objected to this point of view.

PIRANI remarked that, in any case, if you are clever enough you should be able to construct true invariants or tensors (which don't depend on your gadgets) from the results of reading your gadgets.

This concluded the discussion of the problems of measurement. Attention next turned to the technical aspects of the purely formal problems which arise when the attempt is made to apply mathematical quantization procedures to the gravitational field.

Chapter 20
The Three-Field Problem
F. J. Belinfante

F. J. BELINFANTE opened with an examination of the three-field problem: gravitational and electromagnetic fields plus the Dirac electron field. He considered first the "classical" theory, defined by him as the limit of the quantum theory as $\hbar \to 0$ so that gravitational and electromagnetic fields commute and spinor fields purely anticommute. The notational developments which he then proceeded to outline go something like this:

He defines a "modernized" Poisson bracket $(A, B') = (-1)^{u_A u_B + 1}(B', A)$ with $u = 1$ for Fermi-Dirac fields and $u = 0$ for Bose-Einstein fields. He also distinguishes $\partial^p f/\partial q = (df)(dq)^{-1}$ and $\partial^A f/\partial q = (dq)^{-1}(df)$, which is a refinement necessitated by factor ordering difficulties.

He then uses vierbeine $h^{\mu}_{(\alpha)}$ to describe the gravitational field, and introduces local $\gamma^{(\alpha)}$ components independent of x for the spinor fields so that

$$\gamma^{\mu}(x) = h^{\mu}_{(\alpha)}(x)\gamma^{(\alpha)}.$$

The canonical formalism can be developed following the method of Dirac[1] There are 11 first-class primary constraints Φ, Φ_{μ}, $\Phi^{(\alpha)(\beta)}$ arising from gauge, coordinate, and vierbein-rotational invariance of the theory, which lead to five first-class secondary constraints χ, χ_{μ}. In addition there are eight second-class constraints

$$\theta \equiv \eta \equiv \pi - \partial^p L/\partial \overline{\psi}_{,0} \qquad \overline{\theta} \equiv \overline{\eta} \equiv \overline{\pi} - \partial^p L/\partial \psi_{,0}$$

arising from redundancy in the variables used to describe the spinor field.

[1] *Can. J. Math.* **2**, 129 (1950).

One finds

$$
\begin{aligned}
\chi\ &= P^s_{,s} + ie\overline{\psi}\,\gamma^{(\alpha)}\,\psi\, h^0_{(\alpha)}\,\sqrt{-g} \\
\chi_0 &= \mathfrak{H} + [p_0^{(\alpha)}\, h^r_{(\alpha)} - P^r A_0]_{,r} + C\,\mathfrak{g}^{rs}_{,rs} \\
\chi_s &= \mathfrak{G}_s + [p_s^{(\alpha)}\, h^r_{(\alpha)} - P^r A_s]_{,r} - C\mathfrak{g}^{0r}_{,rs}
\end{aligned}
$$

where $C = c^4/16\pi G$, and P^μ is canonically conjugate to A_μ, while $\mathfrak{H}$ with $\mathfrak{G}_s$ is integrated over space to give the energy-momentum (free-) fourvector, if one assumes vanishing fields and flat space-time at infinity.

In passing to the quantum theory one makes use of the modified Poisson bracket due to Dirac, which is defined by

$$
\begin{aligned}
(A,B')_{\text{modif}} \equiv (A,B') - \frac{1}{2}\int d^3\underline{x}'' \int d^3\underline{x}'''(A,\theta_i'')c^{i\gamma}(\underline{x}'',\underline{x}''')(\theta_\gamma''',B') \\
+ \frac{1}{2}(-1)^{u_A u_B}\int d^3\underline{x}''(B',\theta_\gamma''')c^{\gamma i}(\underline{x}'',\underline{x}''')(\theta_i'',A)
\end{aligned}
$$

with

$$c_{\gamma i}(\underline{x}',\underline{x}) \equiv (\theta_i',\theta_\gamma)$$

and

$$\int d^3\underline{x}'''\, c^{i\gamma}(\underline{x}'',\underline{x}''')c_{\gamma k}(\underline{x}''',\underline{x}') = \delta^i_k\delta_3(\underline{x}''-\underline{x}').$$

One sets

$$AB' - (-1)^{u_A u_B}B'A = i\hbar c(A,B')_{\text{modif}}\,.$$

The dynamical equations then become

$$F_{,O} \equiv (F,\mathscr{H}_0)_{\text{modif}} + \underbrace{O^i\frac{\partial^A F}{\partial p^i}}$$

where the symbol $\underbrace{\qquad}$ means place the O^i factor where p_i is taken out. The O^i factor is given by

$$O^i \equiv \eta^i_{,O} - \mathfrak{G}^i$$

where the $\eta^i \equiv p^i - \partial^p L_0/\partial q_{i,O}$.

$$\mathfrak{G}^i = \frac{\partial^p L}{\partial q_i} - \left(\frac{\partial^p L_0}{\partial q_{i,\mu}}\right)_{,\mu}.$$

Also

$$\mathscr{H}_0 \equiv \int (p^i q_{i,O} - L_0) d^3x$$
$$\equiv \int (\theta_i \mu^i + \Phi^\gamma \beta_\gamma + O_2 + \mathfrak{H}) d^3\underline{x}$$

the μ^i and β_γ being certain coefficients and $\mathfrak{H}$ a function of the p's, q's, and their space derivatives only. O_2 is quadratic in the η. The constraint expressions θ and Φ hereare meant as functions of the q and p and therefore are expressions in the η partially linear in them. (Note that the constraints are identities when expressed in terms of q and $\partial L_0/\partial q_{,O}$, but through the η vanish only weakly when expressed in the q and p as we do.) If one works only with modified Poissonbrackets the θ_i may be set to zero, and then

$$F_{,O} = \int \left[[F, \mathfrak{H}]_{\text{modif}} + (F, \Phi^i)_{\text{modif}}\, \beta_i \right] d^3\underline{x}.$$

Kennedy has verified the consistency of this scheme by direct computation of the modified Poisson brackets $(\Phi^i, \Phi^{\gamma'})$, $(\Phi^i, \chi^{\gamma'})$, $(\chi^i, \chi^{\gamma'})$. They all reduce to linear combinations of the Φ's and χ's alone.

The modified Poisson brackets for the spinor variables lead to some unwanted peculiarities which can be removed by redefining the spinor variables according to

$$\tilde{\psi} = \frac{(-g)^{1/4}[\sqrt{-g^{00}} - \gamma^{(0)}\gamma^0]}{\sqrt{2(\sqrt{-g^{00} + h^0_{(0)}})}}$$
$$\tilde{p}^i = \eta^i + \frac{\partial^p L_0}{\partial q_{i,O}}$$

One then finds

$$(q_i, p'^\gamma)_{\text{modif}} = \delta_i^\gamma \delta_3(\underline{x} - \underline{x}')\ ,\ \ (\tilde{q}_i, \tilde{q}'_\gamma)_{\text{modif}} = 0\ ,\ \ (p^i, \tilde{p}'^\gamma)_{\text{modif}} = 0.$$

Thence, modified Poisson brackets defined by partial derivatives with respect to q and p equal ordinary (though modernized) Poisson brackets defined by partial derivatives with respect to q and p. Thus working with

L_0 in terms of the q and $q_{,\mu}$ from the beginning makes it possible to quantize without ever mentioning modified Poisson brackets.

BELINFANTE pointed out that in its present form his theory seems to be incovariant. This is related to the fact that the O^i do not vanish in the strict sense. (They only vanish "weakly," in Dirac's terminology.) However he proposed simply to bypass this problem for the time being, and, for the sake of being able to make practical computations, pass over to what he calls a "muddified theory," i.e., a theory obtained by throwing in "mud." In electrodynamics this is just Fermi's procedure of adding a non-gauge invariant quantity to the Lagrangian. The first-class constraints then disappear and one must replace them by auxiliary conditions (e.g. the Lorentz condition). There is a certain arbitrariness here, since the forms of the auxiliary conditions depend on the precise form of the "mud" which has been thrown in. However, if the $\dot{q}$'s are replaced by their expressions in terms of the p's then the auxiliary conditions must reduce to the original constraints. BELINFANTE has made certain special choices for these conditions (e.g., De Donder condition), based on convenience, and he hopes he can then do meaningful practical calculations, just as the Fermi theory was long used for practical calculations in electrodynamics before all the mathematical subtleties of various constraints were precisely understood. BELINFANTE has shown by explicit computation that the constraints of his "muddified" theory are conserved, and has calculated explicitly the $\dot{q}(p)$.

With the "muddified" theory the commutation relations are covariant. However, the equations of motion are no longer covariant. This means that the q's and p's at time t_2 will depend not only on the q's and p's at a time t_1, but also on the coordinate system chosen to link t_1 and t_2. On the other hand, differences in results of going from t_1 to t_2 are merely accumulated mud; so, if a "true" theory is later developed in which the mud can be made identically zero by altering the commutation relations as proposed by Bergmann and Goldberg, the covariance will be restored. (In the muddified theory, covariance will exist "weakly" unless the positions of the weakly vanishing factors in products due to noncommutativities would cause troubles.)

One can introduce annihilation and creation operators (for photons, gravitons, etc.), although the Fourier transformation procedure on which they are based is a non-covariant procedure.

BELINFANTE has gone on to see if he can find the unitary transformations which will separate the gravitational and electromagnetic fields into their so-called "true" and "untrue" parts (e.g., transverse and longitudinal parts). This is rather difficult, particularly when spinor fields are present. But there is also a more serious problem, connected with the χ-constraints (above). "True observables" have been defined by Bergmann and Goldberg as those which commute with the first-class constraints as well as with the canonical conjugates to the first-class constraints. BELINFANTE prefers to call them true "variables." If the constraints are only linear in the momenta it is not difficult to find the "true variables." However, the constraint χ_0 involves the quantity $\mathfrak{H}$ which is quadratic in the momenta, being essentially the energy density of the combined fields. This means that the only "true variables" which will be easy to find are the constants of the motion. (Since χ_0 differs from $\mathfrak{H}$ by a divergence one might at first sight conclude, by integrating χ_0 over all space, that the total energy must always be zero. However, the surface integrals cannot be ignored here – Ed.)

BELINFANTE concluded by suggesting that a theory in which only "true variables" appear may be mathematically nice but somewhat impractical. From the point of view of a scattering calculation, for example, there may be some truth even in an almost true "untrue variable." In any event the "true" theory still eludes us at present.

Following BELINFANTE's remarks, there was considerable discussion as to whether or not all true observables are necessarily constants of the motion in a generally covariant theory. No progress was made on this question, however, and the answer is still up in the air as of this moment.

NEWMAN next reported on some work he has been doing to try to obtain the true observables by an approximation procedure. Instead of dealing directly with the gravitational field he considered a "particle" Lagrangian of the form

$$L = \frac{1}{2} g_{ij} \dot{q}^i \dot{q}^j + a_i \dot{q}^i - v \quad (i, j = 1, \ldots, n)$$

for which the equations of motion are nonlinear but invariant under a transformation group analogous to the gauge group of electrodynamics or the coordinate transformation group of general relativity. The gravitational field is embraced by this example when n becomes transfinite. In a linear theory the true observables are easy to find. In NEWMAN's approximation procedure the search for the next higher order terms (with respect to some expansion parameter) in the true observables is no worse than finding the exact expression in the linear case, and can actually be carried out, even when some of the constraints are quadratic in the momenta. In the case of the gravitational field the true observables evidently become more and more nonlocal (i.e., involving higher order multiple integrals) at each higher level of approximation. NEWMAN could say nothing about the convergence of his expansion procedure.

BERGMANN remarked that NEWMAN's procedure was quite different from the more common differential-geometric approach which is specifically tailored to the gravitational case. He mentioned in this connection the work of Komar and Géhéniau on metric invariants constructed out of the curvature tensor. It is thought that these invariants have some close connection with the "true observables."

MISNER advocated at this point that one simply forget about the true observables, at least as far as quantization is concerned. He suggested starting with a metricless, field-less space, defined simply in terms of quadruples of real numbers, then performing certain formal mathematical operations and finding the true observables later, if one desires them.

SCHILLER outlined still another way of looking at the problem of finding the true observables: The field equations of Einstein are not of the Cauchy-Kowalewski type. That is, the metric field variables and their time derivatives cannot all be specified on an initial space-like surface. However, Einstein's equations can be replaced by a Cauchy-Kowalewski set provided one imposes coordinate conditions. A standard canonical formalism can then be set up in which the coordinates and momenta are expressed as functions of $2n$ constants of the motion c and the time $t : q_i = q_i(c,t)\ p_i = p_i(c,t)$ (n is transfinite). In the canonical formalism the coordinate conditions take the form $Z_\alpha(p,\alpha,t) = 0$ for certain functions Z_α. These three sets of equations can then (in principle) be solved to eliminate some of the constants c from the theory. The remaining constants, when, reexpressed as functions of the p's, q's and t, will be the true observables. This, of course, assumes that the true observables are constants of the motion.

WHEELER remarked that all of these discussions lead to the conclusion that the problems we face are problems of the classical theory.

BERGMANN agreed, and expressed his conviction that once the classical problems are solved, quantization would be a "walk."

WHEELER, however, still felt that in the Feynman quantization procedure the whole problem is already solved in advance.

GOLDBERG and ANDERSON concluded the afternoon session with a discussion of "Schwinger quantization," that is, the procedure which uses a q-number Lagrangian as a starting point. In making variations of such a Lagrangian one must pay careful attention to the ordering of factors.

GOLDBERG outlined a problem of trying to find the unitary operators which generate various invariant transformations, within the subspace defined by the constraints.

ANDERSON pointed out some difficulties, connected with the factor ordering problem, of defining a unique q-number Lagrangian.

BELINFANTE suggested that, if an interaction representation could be found, at least some of the factor ordering problems might be avoided with the use of "Wick brackets" which reorder the annihilation and creation operators, even though these brackets, being defined with respect to a flat space, would have no generally covariant significance.

Session VII Quantized General Relativity, Continued

Chairman: A. Schild

Chapter 21
Quantum Gravidynamics
Bryce DeWitt

DE WITT opened the session by expressing the hope that one would soon be able to compute something in quantum gravidynamics. He felt that formal matters should be settled as soon as possible so that one could get down to physics. One of the most pressing problems to his mind was the question of what "measure" to use in the quantization of a non-linear theory, particularly in quantization by the Feynman method. In previous conversations with other conferees he had become aware of some differences of opinion on this point. In his view the appropriate measure or "metric" was in every case (including that of the gravitational field) already given by the Lagrangian of the system. As an illustration he considered a system described by a Lagrangian function of the form

$$L = \frac{1}{2} g_{ij} \dot{q}^i \dot{q}^j,$$

where the g_{ij} may be functions of the q's and the indices i, j run from 1 to n. The fact that n may be nondenumerably infinite is ignored. For an actual system the Lagrangian may possess other terms, but the essential difficulties are contained in the term considered. Inclusion of the other terms modifies the following discussion in no essential way.

If (g_{ij}) is a nonsingular matrix with inverse given by (g^{ij}) then the system possesses a Hamiltonian function given by

$$H = \frac{1}{2} g^{ij} p_i p_j,$$

and the action

$$S(q'', t'' | q', t') = \int_{q',t'}^{q'',t''} L \, dt$$

satisfies the Hamilton-Jacobi equations

$$
\begin{aligned}
\frac{\partial S}{\partial t''} + \frac{1}{2} g^{ij}(q'') \frac{\partial S}{\partial q''^i} \frac{\partial S}{\partial q''^j} &= 0, \\
-\frac{\partial S}{\partial t'} + \frac{1}{2} g^{ij}(q') \frac{\partial S}{\partial q'^i} \frac{\partial S}{\partial q'^j} &= 0.
\end{aligned}
$$

According to DE WITT, the structure g_{ij}, which is already contained in the Lagrangian to be taken as the metric for the space of the q's and provides the appropriate "measure" for a Feynman summation. Following Pauli[1] one may introduce a "classical kernel" of the form

$$
\langle q'', t'' | q', t' \rangle_c = (2\pi i\hbar)^{-n/2} g^{-1/4}(q'') D^{1/2}(q'', t'' | q', t') g^{-1/4}(q') e^{\frac{i}{\hbar} S(q'', t'' | q', t')}
$$

where $g \equiv |g_{ij}|$, and where the quantity D is a determinant originally introduced by Van Vleck in an attempt to extend the WKB method to systems in more than one dimension.[2]

$$
D \equiv |D_{ji}|, \qquad D_{ji} \equiv -\frac{\partial^2 S}{\partial q''^j \partial q'^i}
$$

and satisfies an important conservation law:

$$
\frac{\partial D}{\partial t''} + \frac{\partial}{\partial q''^i} [D g^{ij}(q'') \frac{\partial S}{\partial q''^j}] = 0
$$

which can be obtained by differentiating the first Hamilton-Jacobi equation with respect to q'^i and q''^j and multiplying by the inverse matrix $D^{-1\,ij}$. From the quantum viewpoint this law expresses *conservation of probability.*

It is easy to see that $\langle q'', t'' | q', t' \rangle_c$ is an invariant under point transformations of the q's. With the aid of the Hamilton-Jacobi equations one may show that it satisfies the differential equations

---

[1] *Feldquantisierung*, lecture notes, Zürich, 1950-1951.

[2] J. H. Van Vleck. *Proc. Nat. Acad. Sci.* **14**, 178 (1928); C. Morette. *Phys. Rev.* **81**, 848 (1951).

$$
\begin{aligned}
&(i\hbar\frac{\partial}{\partial t''} - H'')\langle q'',t''|q',t'\rangle_c \\
&\qquad\qquad = \frac{\hbar^2}{2} g''^{1/4} D^{-1/2} g''^{ij} (g''^{-1/4} D^{1/2})_{.ij} \langle q'',t''|q',t'\rangle_c \\
&(-i\hbar\frac{\partial}{\partial t'} - H')\langle q'',t''|q',t'\rangle_c \\
&\qquad\qquad = \frac{\hbar^2}{2} g'^{1/4} D^{-1/2} g'^{ij} (g''^{-1/4} D^{1/2})_{.ij} \langle q'',t''|q',t'\rangle_c
\end{aligned}
$$

where for brevity we write $g''^{ij} \equiv g^{ij}(q'')$, $g'^{ij} \equiv g^{ij}(q')$, etc.; where the dot followed by indices denotes covariant differentiation with respect to either the q''^i or q'^i (as indicated by the context) and where the operator H is defined by

$$
H\psi \equiv -\frac{\hbar^2}{2} g^{-1/2} \frac{\partial}{\partial q^i} (g^{1/2} g^{ij} \frac{\partial \psi}{\partial q^j}) \equiv -\frac{\hbar^2}{2} g^{ij} \psi_{.ij}.
$$

DE WITT pointed out that if it were not for a certain peculiar phenomenon which occurs when the space of the q's is curved, the operator H could immediately be regarded as the Hamiltonian operator for the quantized system. In order to discuss this phenomenon some further development is necessary:

If one recalls that the classical action defines a canonical transformation by the equations

$$
p_i'' = \frac{\partial S}{\partial q''^i}\,, \qquad p_i' = -\frac{\partial S}{\partial q'^i}
$$

then one easily sees that the Van Vleck determinant is just the Jacobian involved in transforming from a specification of the classical path by means of the variables q''^i, q'^i, to a specification in terms of initial variables q'^i, p_i'. From the Hamilton-Jacobi equations one sees furthermore that the action may be expressed in the form

$$
S = \frac{t''-t'}{2} g''^{ij} p_i'' p_j'' = \frac{t''-t'}{2} g'^{ij} p_i' p_j'.
$$

Therefore, noting that as $t'' \to t'$ the p_i' become infinite except when $q''^i = q'^i$ (for all i), one may write, for an arbitrary function f,

$$\begin{aligned}
&\lim_{t''\to t'}\int f(q'')\langle q'',t''|q',t'\rangle_c\, g^{1/2}(q'')dq''^1...dq''^n \\
&= (2\pi i\hbar)^{-n/2}\lim_{t''\to t'} D^{-1/2}(q',t''|q',t')f(q')\int e^{\frac{i}{\hbar}\frac{t''-t'}{2}g'^{ij}p'_ip'_j}dp'_1...dp'_n \\
&= \lim_{t''\to t'}(t''-t')^{-n/2}D^{-1/2}(q',t''|q',t')g^{1/2}(q')f(q').
\end{aligned}$$

In order to evaluate this last expression one must evaluate the Van Vleck determinant. This is easily done by expanding the action about the point substituting it in the Hamilton-Jacobi equation. One finds, after a straightforward computation,

$$\begin{aligned}
&S(q'',t''|q',t') \\
&= \frac{1}{t''-t'}\{\frac{1}{2}g'_{ij}(q''^i-q'^i)(q''^j-q'^j) \\
&\quad + \frac{1}{12}(g'_{ij,k}+g'_{jk,i}+g'_{ki,j})(q''^i-q'^i)(q''^j-q'^j)(q''^k-q'^k) \\
&\quad + \frac{1}{72}[g'_{ij,kl}+g'_{ik,lj}+g'_{il,jk}+g'_{kl,ij}+g'_{lj,ik}+g'_{jk,il} \\
&\quad - g'^{mn}([ij,m]'[kl,n]'+[ik,m]'[lj,n]'+[il,m]'[jk,n]')] \\
&\quad \times (q''^i-q'^i)(q''^j-q'^j)(q''^k-q'^k)(q''^l-q'^l)+O(q''-q')^5\}
\end{aligned}$$

$$\begin{aligned}
D(q'',t''|q',t') &= \left|-\frac{\partial^2 S}{\partial q''^j\partial q'^i}\right| \\
&= \frac{g''^{1/2}g'^{1/2}}{(t''-t')^n}[1+\frac{1}{6}R'_{ij}(q''^i-q'^i)(q''^j-q'^j)+O(q''-q')^3]
\end{aligned}$$

where

$$\begin{aligned}
[ij,k] &= \frac{1}{2}(g_{ik,j}+g_{jk,i}-g_{ij,k}) \\
R_{ij} &= -g^{kl}R_{ikjl} \\
R_{ikjl} &= \frac{1}{2}(g_{ij,kl}-g_{il,kj})-g_{kj,il}+g_{kl,ij}) \\
&\quad + g^{mn}([ij,m][kl,n]-[kj,m][il,n]),
\end{aligned}$$

commas followed by indices denoting differentiation with respect to the q's. Here a convention has been chosen so that the scalar $R = g^{ij}R_{ij}$ is positive for a space of positive curvature.

Using the final expression for the Van Vleck determinant one infers

$$\lim_{t''\to t'}\langle q'',t''|q',t'\rangle_c = \delta(q'',q')$$

and

$$\begin{aligned}
&\lim_{t''\to t'}\frac{\hbar^2}{2}g''^{\,1/4}D^{-1/2}g''^{\,ij}(g''^{\,-1/4}D^{1/2})_{.ij}\langle q'',t''|q',t'\rangle_c \\
&= \frac{\hbar^2}{12}R'\delta(q'',q')
\end{aligned}$$

where $\delta(q'',q')$ is the invariant delta-function in q-space. Referring to the differential equations satisfied by the classical kernel $\langle q'',t''|q',t'\rangle_c$, one sees that it equals the true quantum transformation function $\langle q'',t''|q',t'\rangle_+$ of a system possessing the Hamiltonian operator

$$H_+ \equiv H + \frac{\hbar^2}{12}R,$$

up to the first order in $(t''-t')$. That is,

$$\langle q'',t''|q',t'\rangle_c = \langle q'',t''|q',t'\rangle_+ + O(t''-t')^2$$

where

$$(i\hbar\frac{\partial}{\partial t''}-H''_+) = \langle q'',t''|q',t'\rangle_+ = 0$$

satisfying the boundary condition

$$\langle q'', t | q', t \rangle_+ = \delta(q'', q') \qquad \text{for all } t.$$

The "first order contact" between $\langle q'', t'' | q', t' \rangle_c$ and $\langle q'', t'' | q', t' \rangle_+$ is already sufficient to determine the behavior of wave packets for the quantized system. If the system has a Hamiltonian operator H_+ then its wave packets will more approximately along the classical paths of a classical system which has simply $H = \frac{1}{2} g^{ij} p_i p_j$ as Hamiltonian function. *Conversely* if the quantized system has Hamiltonian operator H then the motions of its wave packets will be along the classical paths for a "classical" system which has the Hamiltonian function $H_- \equiv \frac{1}{2} g^{ij} p_i p_j - \frac{\hbar^2}{12} R$. Evidently there is an ambiguity here in the choice of Hamiltonian operator when the space is curved, and there is nothing in the classical theory to resolve it for us.

The Feynman formulation of quantum mechanics follows immediately from the order contact between $\langle q'', t'' | q', t' \rangle_c$ and $\langle q'', t'' | q', t' \rangle_+$. Breaking the transformation function up into infinitely many pieces by means of the composition law

$$\langle q'', t'' | q', t' \rangle_+ = \int \langle q'', t'' | q''', t''' \rangle_+ \, d^n q''' \langle q''', t''' | q', t' \rangle_+$$

where $d^n q \equiv g^{1/2} dq^1 \ldots dq^n$, one may write

$$\begin{aligned} \langle q'', t'' | q', t' \rangle_+ = \lim_{\substack{N \to \infty \\ \Delta t \to 0}} \int d^n q^{(1)} \cdots \int d^n q^{(N)} \langle q'', t'' | q^{(N)}, t^{(N)} \rangle_c \\ \cdots \langle q^{(2)}, t^{(2)} | q^{(1)}, t^{(1)} \rangle_c \langle q^{(1)}, t^{(1)} | q', t' \rangle_c \end{aligned}$$

where $\Delta t = \max(t'' - t^{(N)}, \ldots t^{(2)} - t^{(1)}, t^{(1)} - t'), t'' > t^{(N)} > \cdots > t^{(2)} > t^{(1)} > t'$. It is evident, from the expression for $\langle q'', t'' | q', t' \rangle_c$ and the form of the expansion for the action S, that the (in the limit) infinitely multiple integral receives significant contributions from the integrand only when the differences $q''^{\,i} - q^{(N)i}, \ldots q^{(2)i} - q^{(1)i}, q^{(1)i} - q'^{\,i}$ are of the order of $(\hbar \Delta t)^{1/2}$

or smaller. Therefore, if the symbol $\doteq$ is used to denote equivalence as far as use in the infinitely multiple integral is concerned, one may write

$$
\begin{aligned}
\langle q'',t''|q',t'\rangle_c &\doteq (2\pi i\hbar)^{-\frac{n}{2}} \frac{1+\frac{1}{12}R'_{ij}(q''^{\,i}-q'^{\,i})(q''^{\,j}-q'^{\,j})}{(t''-t')^{n/2}} e^{\frac{i}{\hbar}S(q'',t''|q',t')} \\
&\doteq (2\pi i\hbar)^{-\frac{n}{2}} \frac{1+\frac{1}{12}R'^{\,ij}p'_i p'_j (t''-t')^2}{(t''-t')^{n/2}} e^{\frac{i}{\hbar}S(q'',t''|q',t')} \\
&\doteq (2\pi i\hbar)^{-\frac{n}{2}} \frac{1+\frac{i\hbar}{12}R'(t''-t')^2}{(t''-t')^{n/2}} e^{\frac{i}{\hbar}S(q'',t''|q',t')}
\end{aligned}
$$

since

$$
\begin{aligned}
&\lim_{t''\to t'} [2\pi i\hbar(t''-t')]^{-n/2}(t''-t')\int f(q'')p'_i p'_j e^{\frac{i}{\hbar}S} d^n q'' \\
&= \lim_{t''\to t'} \left(\frac{t''-t'}{2\pi i\hbar}\right)^{n/2} (t''-t')g^{-1/2}(q')f(q')\int p'_i p'_j e^{\frac{i}{\hbar}\frac{t''-t'}{2}g'^{\,ij}p'_i p'_j} dp'_1 \ldots dp'_n \\
&= i\hbar g_{ij}(q')f(q')\,,
\end{aligned}
$$

and therefore

$$
\langle q'',t''|q',t'\rangle_c \doteq [2\pi i\hbar(t''-t')]^{-n/2} e^{\frac{i}{\hbar}S_-(q'',t''|q',t')}
$$

where S_- is the action function for the system with classical Hamiltonian function

$$
\begin{aligned}
&H_- \equiv \frac{1}{2}g^{ij}p_i p_j - \frac{\hbar^2}{12}R, \ \text{satisfying} \\
&\qquad S_-(q'',t''|q',t') = S(q'',t''|q',t') + \frac{\hbar^2}{12}R'(t''-t') + O[(q''-q')(t''-t')].
\end{aligned}
$$

The Feynman formulation now becomes

$$
\begin{aligned}
&\langle q'',t''|q',t'\rangle_+ \\
&= \lim_{\substack{N\to\infty \\ \Delta t\to 0}} \int\cdots\int [2\pi i\hbar(t''-t^{(N)})]^{-n/2} d^n q^{(N)} \cdots [2\pi i\hbar(t^{(2)}-t^{(1)})]^{n/2} d^n q^{(1)} \\
&\quad [2\pi i\hbar(t^{(1)}-t')]^{n/2} \exp\frac{i}{\hbar}[S_-(q'',t''|q^{(N)},t^{(N)}) + \cdots \\
&\quad + S_-(q^{(2)},t^{(2)}|q^{(1)},t^{(1)}) + S_-(q^{(1)},t^{(1)}|q',t')]
\end{aligned}
$$

and the dependence of the "sum-over-paths" on the metric g_{ij} is seen to occur through the invariant volume elements $d^n q^{(1)} \ldots d^n q^{(N)}$. This last expression is sometimes written in the symbolic form

$$\langle q'', t'' | q', t' \rangle_+ = \int_{q',t'}^{q'',t''} \delta[q] \exp\left(\frac{1}{\hbar} \int_{t'}^{t''} L_- \, dt \right)$$

where the symbol $\delta[q]$ indicates that a "functional integration" is to be performed. In linear examples (i.e., linear equations of motion) the functional integration can often be easily performed without even referring to its rigorous definition as an infinitely multiple integral. This is not likely to be so simple in the present nonlinear case because of the occurrence of a variable metric in the invariant volume elements $d^n q^{(1)} \ldots d^n q^{(N)}$.

It will be noted that the last result is even more curious than the previous result obtained with the Pauli method using $\langle q'', t'' | q', t' \rangle_c$. In order to obtain the transformation function for a quantized system having Hamiltonian operator H_+ one must use, in the Feynman summation, the action corresponding to a classical system having Hamiltonian H_- or Lagrangian $L_- = \frac{1}{2} g_{ij} \dot{q}^i \dot{q}^j + \frac{\hbar^2}{12} R$. When DE WITT first discovered this phenomenon he thought that he had made an error in sign and that the occurrences of $\frac{\hbar^2}{12} R$ actually cancelled each other when one passed from the Pauli form on to the Feynman representation. J. L. Anderson later convinced him, however, of the reality of the phenomenon, by carrying out a computation patterned directly after Feynman's original paper.

Instead of stating the result in a symmetric manner one may also state it in the following forms: If the true classical action $\int L \, dt$ $(\hbar \to 0)$ is used in the Feynman summation then one generates the transformation function $\langle q'', t'' | q', t' \rangle_{++}$ satisfying

$$(i\hbar \frac{\partial}{\partial t''} - H''_{++}) \langle q'', t'' | q', t' \rangle_{++} = 0$$

where

$$H''_{++} = H + \frac{\hbar}{6} R.$$

On the other hand, in order to generate the transformation function $\langle q'', t'' | q', t' \rangle$ satisfying

$$(i\hbar \frac{\partial}{\partial t''} - H'') \langle q'', t'' | q', t' \rangle = 0$$

one must use in the Feynman summation the action for a “classical” system possessing the Lagrangian function

$$L_{--} = \frac{1}{2} g_{ij}\dot{q}^i\dot{q}^j + \frac{\hbar}{6}R$$

and Hamiltonian function

$$H_{--} = \frac{1}{2} g^{ij} p_i p_j - \frac{\hbar}{6}R.$$

FEYNMAN remarked that quantization of a system like $L = \frac{1}{2} g_{ij}\dot{q}^i\dot{q}^j$ is necessarily ambiguous when the space is curved. One must first imbed the space in higher dimensional space which is *flat*, since only in spaces which are flat (at least to a very high degree of approximation) do we have experiments to guide us to the appropriate form of the quantum theory. The original space must then be conceived of as the limiting form of a thin shell, the “particle” which moves in the original space being constrained to the shell by a very steep potential. But then the Schrödinger equation for the particle will depend on the precise manner in which the thickness of the shell tends to zero, and this is arbitrary.

DE WITT agreed, but pointed out that the manner in which the thickness of the shell tends to zero can be described by the addition of a simple “potential-like” term to the Lagrangian function appearing in the Feynman formulation. To illustrate this he considered the case of a particle constrained to move on the surface of an ellipsoid. He first took for the constraining shell the region between two confocal ellipsoids:

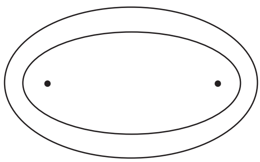

Figure 21.1

In order to eliminate, right at the start, the degree of freedom transverse to the shell - which must have no reality in the end, anyway - one may suppose that the wave function has a simple node on each ellipsoid and none in between (i. e., transverse ground state). Since we know that confocal ellipsoids form a separable system for Schrödinger equation, we may immediately factor out the transverse part of the wave function and at the same time subtract a constant term proportional to $(\Delta x)^{-2}$ from the energy, where Δx denotes the thickness of the shell at some point. The remaining part of the wave function then satisfies the Schrödinger equation with the simple operator H appropriate to an ellipsoid. This corresponds to a Feynman formulation using the classical Lagrangian L_{--}, for which the classical paths are *attracted* to the region of greatest positive curvature, the attraction being described by a potential $-\frac{\hbar^2}{6}R$. In the present case the region of greatest positive curvature is the "nose" of the ellipsoid and here the shell is thinnest. The three-dimensional wave function undergoes a "crowding" at this point. Since the transverse part of the wave function is constant, this crowding must be borne by the "active" two-dimensional part. The tendency of the amplitude to increase in the nose region is describable in classical terms as an attraction. A wave packet would actually display the effect of this attraction.

The Feynman formulation which starts with the classical Lagrangian L, on the other hand, corresponds to the use of a shell of uniform thickness:

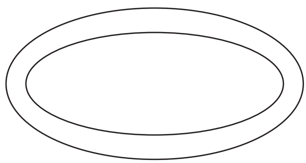

Figure 21.2

All points of the limiting ellipsoid are here weighted equally. DE WITT conjectured that the use of any other shells of varying thickness would quite generally be describable in terms of additions to the classical Lagrangian of potentials proportional to $\hbar^2$.

(Editor's Note: - DE WITT's argument is not entirely rigorous. He, of course, avoided discussion of a spherical shell since the curvature is then constant and has only the effect of uniformly shifting all energy levels. However, there is a difficulty for ellipsoidal systems in that the separation constants are not themselves separable, and hence the transverse part of the wave function is not rigorously factorable for arbitrary behavior of the non-transverse part. It may, however, be approximately factorable when the shell is thin.

Attention should also be called to the fact that the behavior of wave packets is *not* described by the Lagrangian appearing in the Feynman formulation but by the Lagrangian of the Pauli formulation, which is half-way between that of Feynman and the corresponding quantum form. Thus, when Feynman uses L, Pauli uses L_+ to obtain the same quantum theory. Or when Feynman uses L_{--}, Pauli uses L_-. It is the Van Vleck determinant of the Pauli formulation which gives the key to the motion of wave packets, through its conservation law. Remembering that $\dot{q}^i = g^{ij} p_j$, one may write that conservation law in the form

$$\frac{\partial D}{\partial t''} + \frac{\partial}{\partial d''^i}(D\dot{q}'^i) = 0.$$

If a wave packet is replaced by an ensemble of classical particles then D gives a measure of the density of these particles at any time and place.)

WHEELER remarked that it had been suggested that an experimental determination of the Lagrangian to be used in the Feynman formulation in curved spaces might be achieved through observations on molecules which have "flags" on them - for example ethyl alcohol:

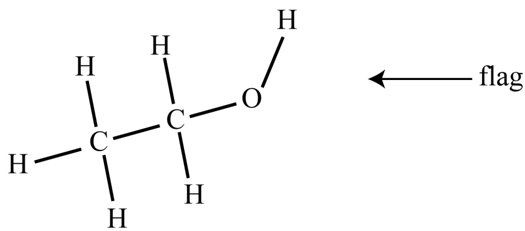

Figure 21.3

If some of the bonds are regarded as rigid (e.g., at room temperature) then the Lagrangian for such a molecule contains a metric corresponding to a space which is not flat. However, WHEELER pointed out that this system, in the last analysis, has its existence in a flat space - that the rigidity of the bonds is an idealization, and that, in fact, the binding forces themselves provide the specification of the "shell" on which this system is constrained to move. Hence such a system could tell us nothing about which Lagrangian to use, for example, in the Feynman quantization of the gravitational field where the ambiguity does arise.

ANDERSON pointed out that at any rate DE WITT's approach leaves no ambiguity in choice of metric. In fact, no other choice is possible if one requires $\lim_{t''\to t'} \langle q'', t'' | q', t' \rangle_c = \delta(q'', q')$. This relation will not generally be satisfied if one chooses a metric for q-space which is unrelated to the g_{ij} appearing in the Lagrangian.

DE WITT then went on to discuss the second important and pressing problem which arises in the quantization of nonlinear systems, namely, the "factor ordering problem": How should one order non-commuting operators to obtain appropriate quantum analogs of various classical equations?

When the matrix (g_{ij}) is non-singular the factor ordering Hamiltonian operator is easily solved. The Hermitian requirement on the momenta p_i leads to the quantum representation requirement

$$p_i = -i\hbar g^{-1/4} \frac{\partial}{\partial q^i} g^{1/4} = -i\hbar [\frac{\partial}{\partial q^i} + \frac{1}{4} (\ln g)_{,i}]$$

in arbitrary curvilinear coordinates. Hence, in order that $H\psi = -\frac{\hbar^2}{2} g^{ij} \psi_{.ij}$, one must write

$$H = \frac{1}{2} g^{-1/4} p_i g^{1/2} g^{ij} p_j g^{-1/4}$$

The Hermitian character of H is obvious from the symmetry of this expression.

For covariant theories, on the other hand, (g_{ij}) is singular, constraints are present, and the analysis becomes much more complicated. Moreover, since (g_{ij}) is singular it is not obvious immediately what "measure" to use in a Feynman formulation.

(Editor's Note: - The following is taken from a set of mimeographed notes which DE WITT distributed to the conferees. The actual exposition of it was broken up by many questions, mostly on the necessity of going through a careful layout of identities and constraints. These questions are best answered by the more complete uninterrupted presentation given below. One may add, to the purposes of an unambiguous derivation already mentioned in the previous paragraphs (finding the correct "factor ordering" and the "measure"), the search for the true observables of the theory.)

As a prototype of a covariant theory DE WITT considered a system possessing a Lagrangian function of the form

$$L \equiv \frac{1}{2} g_{ij} \dot{q}^i \dot{q}^j + a_i \dot{q}^i - v$$

for which the equations of motion are

$$g_{ij} \ddot{q}^j + [jk, i] \dot{q}^j \dot{q}^k - f_{ij} \dot{q}^j + v_{,i} \stackrel{w}{=} 0$$

where $f_{ij} = a_{j,i} - a_{i,j}$. These equations of motion are, of course, invariant under point transformations of the q's among themselves as well as under "phase transformations" $a_i \to a_i + P_{,i}$ where P is an arbitrary function of the q's, The "covariance" of the theory is described by an additional transformation group under which the equations of motion remain invariant, whose infinitesimal elements have the general form $q^i \to q^i + \delta q^i$, where

$$\delta q^i \equiv (\rho^i_\alpha + \sigma^i_{\alpha j} \dot{q}^j) \delta \Lambda^\alpha + \tau^i_\alpha \delta \dot{\Lambda}^\alpha .$$

The ρ^i_α , $\sigma^i_{\alpha j}$, τ^i_α are certain definite functions of the q's, while the $\delta\Lambda^\alpha$ are arbitrary infinitesimal functions of the time and of the q's and any of their time derivatives. Such a transformation may be called a *gauge transformation*; in general relativity it is an infinitesimal coordinate transformation, the q_i being the metric field variables.

The Lagrangian function must be altered under a gauge transformation by a total time derivative $\delta \dot{F}$. It is not hard to see that δF must have the general form

$$\delta F \equiv (A_\alpha + B_{\alpha i} \dot{q}^i + \frac{1}{2} C_{\alpha ij} \dot{q}^i \dot{q}^j) \delta \Lambda^\alpha + D_\alpha \delta \dot{\Lambda}^\alpha$$

where the A_α, $B_{\alpha i}$, $C_{\alpha ij}$, D_α are functions of the q's only. DE WITT assumed that $C_{\alpha ij} \equiv 0$, as this simplifies the analysis in certain respects. This assumption is actually incorrect for the gravitational field, but is not expected to alter the main qualitative features of the analysis. (It will be discussed more fully at a later point.) By performing a gauge transformation on the Lagrangian and making a comparison with $\delta \dot{F}$ one finds that the following identities must be satisfied:

$$
\begin{aligned}
&A_\alpha \equiv -v_{,i}\tau^i_\alpha + a_i\rho^i_\alpha \\
&B_{\alpha i} \equiv a_i\sigma^j_{\alpha i} \\
&D_\alpha \equiv a_i\tau^i_\alpha \\
&g_{ij}\tau^j_\alpha \equiv 0 \\
&g_{ij}\sigma^j_{\alpha k} \equiv 0 \\
&[ij,k]\,\tau^k_\alpha \equiv 0 \\
&[ij,l]\,\sigma^l_{\alpha k} + [jk,l]\,\sigma^l_{\alpha i} + [ki,l]\,\sigma^l_{\alpha j} \equiv 0 \\
&f_{ij}\tau^j_\alpha \equiv g_{ij}\rho^j_\alpha \\
&f_{ik}\sigma^k_{\alpha j} + f_{jk}\sigma^k_{\alpha i} \equiv g_{ij,k}\rho^k_\alpha + g_{ik}\rho^k_{\alpha,j} + g_{jk}\rho^k_{\alpha,i} \\
&f_{ij}\rho^j_\alpha \equiv (v_{,j}\tau^j_\alpha)_{,i} - v_{,j}\sigma^j_{\alpha i} \\
&v_{,i}\rho^i_\alpha \equiv 0 \\
&f_{ij}\tau^i_\alpha\tau^j_\beta \equiv 0 \\
&g_{ik}(\tau^k_{\alpha,j}\tau^j_\beta - \tau^k_{\beta,j}\tau^j_\alpha) \equiv 0.
\end{aligned}
$$

DE WITT called these *identities of invariance.*

It is the identity $g_{ij}\tau^j_\alpha \equiv 0$ which shows that (g_{ij}) must be a singular matrix in a “gauge invariant” theory. This has the consequence that the momenta of the Hamiltonian formalism are not all independent. It also has the consequence that the initial conditions on the motion must be subject to constraints in order that the motion actually be able to make the action integral $\int L\,dt$ an extremum. Some, at least, of these constraints are obtained by multiplying the equations of motion by τ^i_α. Using identities of invariance one finds

$$\rho^i_\alpha g_{ij}\dot{q}^j + v_{,i}\tau^i_\alpha \overset{w}{=} 0.$$

These will constitute the complete set of constraints if their time derivatives to all orders vanish solely as a consequence of the equations of motion. If the time derivatives do not automatically vanish they must be made to vanish by adding extra constraints, whose time derivatives, in turn, must be made to vanish, etc.

DE WITT introduced a second set of identities by assuming that the τ^i_α form a complete and independent set of null eigenvectors of g_{ij}. One may then infer that

$$\begin{aligned}\sigma^i_{\alpha j} &\equiv \tau^i_\beta S^\beta_{\alpha j}\\ \tau^i_{\alpha,j}\tau^j_\beta - \tau^i_{\beta,j}\tau^j_\alpha &\equiv \tau^i_\gamma t^\gamma_{\alpha\beta}\\ \rho^i_{\alpha,j}\tau^j_\beta - \tau^i_{\beta,j}\rho^j_\alpha - \rho^i_\gamma S^\gamma_{\alpha j}\tau^j_\beta &\equiv \tau^i_\gamma u^\gamma_{\alpha\beta}\end{aligned}$$

where the $S^\beta_{\alpha j}$, $t^\gamma_{\alpha\beta}$, $u^\gamma_{\alpha\beta}$ are certain coefficients. DE WITT called these *identities of completeness.*

A final set of identities are obtained from the group property of gauge transformations. Requiring that the difference between the results of applying two infinitesimal gauge transformations in different orders be also a gauge transformation (of the second infinitesimal order) one finds

$$\begin{aligned}\delta_2\delta_1 q^i - \delta_1\delta_2 q^i &\equiv (p^i_\gamma + \sigma^i_{\gamma j}\dot q^j)\delta_{1\,2}\Lambda^\gamma + \tau^i_\gamma\delta_{1\,2}\dot\Lambda^\gamma\\ \delta_2\delta_1 F - \delta_1\delta_2 F &\equiv (A_\alpha + B_{\alpha i})\delta_{1\,2}\Lambda^\alpha + D_\alpha\delta_{1\,2}\dot\Lambda^\alpha\end{aligned}$$

where

$$\begin{aligned}\delta_{1\,2}\Lambda^\gamma \equiv{}& \frac{1}{2}r^\gamma_{\alpha\beta}(\delta_1\Lambda^\alpha\delta_2\Lambda^\beta - \delta_2\Lambda^\alpha\delta_1\Lambda^\beta)\\ &+\frac{1}{2}(S^\gamma_{\alpha j}\tau^j_\delta S^\delta_{\beta k} - S^\gamma_{\beta j}\tau^j_\delta S^\delta_{\alpha k})\dot q^k(\delta_1\Lambda^\alpha\delta_2\Lambda^\beta - \delta_2\Lambda^\alpha\delta_1\Lambda^\beta)\\ &+S^\gamma_{\alpha j}\tau^j_\beta(\delta_1\Lambda^\alpha\delta_2\dot\Lambda^\beta - \delta_2\Lambda^\alpha\delta_1\dot\Lambda^\beta)\\ &+\delta_2\delta_1\Lambda^\gamma - \delta_1\delta_2\Lambda^\gamma\end{aligned}$$

only if

$$\rho^i_{\alpha,j}\rho^j_\beta - \rho^i_{\beta,j}\rho^j_\alpha \equiv \rho^i_\gamma r^\gamma_{\alpha\beta}$$

for certain coefficients $r^\gamma_{\alpha\beta}$, and if

$$\begin{aligned}
t^i_{\alpha,j}\tau^j_\beta - t^i_{\beta,j}\tau^j_\alpha - t^i_\gamma S^\gamma_{\alpha j}\tau^j_\beta + \tau^i_\gamma S^\gamma_{\beta j}\tau^j_\alpha &\equiv 0 \\
S^\gamma_{\alpha j,k} - S^\gamma_{\alpha k,j} - S^\gamma_{\delta j}S^\delta_{\alpha k} + S^\gamma_{\delta k}S^\delta_{\alpha j} &\equiv 0 \\
\rho^i_{\alpha,j}\rho^j_\beta - \rho^i_{\beta,j}\rho^j_\alpha - \rho^i_\gamma S^\gamma_{\alpha j}\rho^j_\beta + \rho^i_\gamma S^\gamma_{\beta j}\rho^j_\alpha &\equiv \rho^i_\gamma v^\gamma_{\alpha\beta} \\
\rho^i_{\alpha,j}\tau^j_\beta - \tau^i_{\beta,j}\rho^j_\alpha - \rho^i_\gamma S^\gamma_{\alpha j}\tau^j_\beta + \tau^i_\gamma S^\gamma_{\beta j}\rho^j_\alpha &\equiv \tau^i_\gamma v^\gamma_{\alpha\beta} \\
f_{ij}\rho^i_\alpha\rho^j_\beta + v_{,i}\tau^i_\gamma v^\gamma_{\alpha\beta} &\equiv 0
\end{aligned}$$

where the $v^\gamma_{\alpha\beta}$ are certain coefficients satisfying

$$\begin{aligned}
&v^\gamma_{\alpha\beta,i} + v^\delta_{\alpha\beta}S^\gamma_{\delta i} - v^\gamma_{\delta\beta}S^\delta_{\alpha i} - v^\gamma_{\alpha\delta}S^\delta_{\beta i} \equiv 0 \\
&r^\gamma_{\alpha\beta} \equiv S^\gamma_{\alpha i}\rho^i_\beta - S^\gamma_{\beta i}\rho^i_\alpha + v^\gamma_{\alpha\beta}.
\end{aligned}$$

DE WITT called these *identities of integrability.* It will be observed that they lead to the identifications

$$\begin{aligned}
t^\gamma_{\alpha\beta} &\equiv S^\gamma_{\alpha i}\tau^i_\beta - S^\gamma_{\beta i}\tau^i_\alpha \\
u^\gamma_{\alpha\beta} &\equiv r^\gamma_{\alpha\beta} - S^\gamma_{\alpha j}\rho^j_\beta.
\end{aligned}$$

DE WITT then went on to remark that the above three groups of identities comprise all the identities that are available, or needed, for the study of the dynamical behavior of the system under consideration, and he called attention to the fact that they are obtained entirely within the framework of the classical theory. In developing the Hamiltonian formulation for the system DE WITT followed the method of Dirac[3] which distinguishes between weak equations ($\overset{w}{=}$) and strong equations ($\overset{s}{=}$). Equations of motion and constraints (reduced to their lowest order) are weak equations. Equations of identity ($\equiv$) are strong equations. Also, the product of two weakly vanishing functions is strongly equal to zero.

The canonical momenta for the system are

$$p_i \overset{w}{=} \frac{\partial L}{\partial \dot{q}^i} \equiv g_{ij}\dot{q}^j + a_i.$$

[3] *Can. J. Math.* **2**, 129 (1950).

Multiplication by τ^i_α gives immediately the set of constraints to which the momenta are subject:

$$\Phi_\alpha \stackrel{w}{=} 0$$
$$\Phi_\alpha \equiv (p_i - a_i)\tau^i_\alpha.$$

Completing the set of vectors τ^i_α through the introduction of a set of independent vectors τ^i_μ, and introducing also "inverses" τ_i^α , τ_i^μ satisfying $\tau^i_\alpha \tau_j^\alpha + \tau^i_\mu \tau_j^\mu = \delta^i_j$, one may write the "energy function" in the form

$$\begin{aligned} E &\equiv p_i \dot{q}^i - L \\ &\stackrel{s}{=} p_i \dot{q}^i - \frac{1}{2} g_{ij} \dot{q}^i \dot{q}^j - a_i \dot{q}^i + v + \frac{1}{2} g^{ij} (p_i - a_i - g_{ik} \dot{q}^k)(p_j - a_j - g_{jl} \dot{q}^l) \\ &\equiv H + \Phi_\alpha \tau_i^\alpha \dot{q}^i \end{aligned}$$

where

$$H \equiv \frac{1}{2} g^{ij} (p_i - a_i)(p_j - a_j) + v$$
$$g^{ij} \equiv \tau^i_\mu \tau^j_\nu g^{\mu\nu} \quad , \quad g^{\mu\sigma} g_{\sigma\nu} \equiv \delta^\mu_\nu \quad , \quad g_{\mu\nu} \equiv g_{ij} \tau^i_\mu \tau^j_\nu.$$

The final expression for the energy function provides an illustration of Dirac's theorem to the effect that the energy is always strongly equal to a function H of the q's and p's, which may be called the Hamiltonian function, plus a linear combination of the Φ's with coefficients which depend on the velocities $\dot{q}^i$. In a theory without constraints the energy is identical with the Hamiltonian function. When constraints are present the two are only weakly equal.

If the coefficients multiplying the Φ's are completely undetermined by the equations of motion, and hence completely arbitrary, the Φ's are said to be "of the first class." This point always requires special investigation. Using the equations of motion together with the constraints one may easily verify that the time rate of change of any function F of the q's and p's is given by

$$\dot{F} \stackrel{w}{=} (F, H) + (F, \Phi_\alpha) \tau_i^\alpha \dot{q}^i$$

where ( ) denotes the Poisson bracket. In particular, the time rate of change of a Φ is given by

$$\dot{\Phi}_\alpha \stackrel{w}{=} (\Phi_\alpha, H) + (\Phi_\alpha, \Phi_\beta)\tau_i^\beta \dot{q}^i.$$

Since the Φ's vanish their time derivatives must also vanish. This will happen automatically if the Poisson brackets (Φ_α, H) and $(\Phi_\alpha, \Phi_\beta)$ vanish, at least weakly. If these Poisson brackets do not all vanish then the $\tau_i^\beta \dot{q}^i$ cannot all be completely arbitrary and the $\dot{q}^i$ will be subject to additional constraints.

Using the relation $g^{ik} \equiv \delta_j^i - \tau_\alpha^i \tau_j^\alpha$ together with identities of invariance, one finds, after a straightforward computation,

$$(\Phi_\alpha, H) \equiv \Phi_\beta[(\tau_{j,k}^\beta - \tau_{k,j}^\beta)\tau_\alpha^k g^{ij}(p_i - a_i) + \tau_i^\beta \rho_\alpha^i] - \chi_\alpha \stackrel{w}{=} 0$$

where

$$\chi_\alpha \equiv (p_i - a_i)\rho_\alpha^i + v_{,i}\tau_\alpha^i \stackrel{w}{=} 0.$$

The latter equations are simply the velocity constraints reexpressed in terms of the momenta. The Φ-equations, existing only in the canonical formalism, are sometimes known as "primary constraints." The χ-equations, which follow from them, are then called "secondary constraints." The χ's as well as the Φ's must have vanishing time derivatives. This leads one to investigate also the Poisson brackets $(\chi_\alpha, H), (\chi_\alpha, \Phi_\beta)$.

Using only the identities of invariance and completeness one finds

$$\begin{aligned}
(\Phi_\alpha, \Phi_\beta) &\equiv \Phi_\gamma t_{\alpha\beta}^\gamma \stackrel{w}{=} 0 \\
(\chi_\alpha, H) &\equiv \Phi_\beta(\tau_{i,k}^\beta \rho_\alpha^k + \tau_k^\beta \rho_{\alpha,i}^k - \tau_k^\beta \rho_\gamma^k S_{\alpha i}^\gamma) g^{ij}(p_j - a_j) \\
&\qquad + \chi_\beta S_{\alpha i}^\beta g^{ij}(p_j - a_j) \stackrel{w}{=} 0 \\
(\chi_\alpha, \Phi_\beta) &\equiv \Phi_\gamma u_{\alpha\beta}^\gamma + \chi_\gamma S_{\alpha j}^\gamma \tau_\beta^j \stackrel{w}{=} 0.
\end{aligned}$$

The vanishing (in the weak sense) of all these Poisson brackets means that there exist no further constraints and the Φ's are all of the first class. There still remains, however, one further Poisson bracket which it is necessary to examine, namely $(\chi_\alpha, \chi_\beta)$. Dirac has shown (*op. cit.*) that if this Poisson bracket does not vanish (at least weakly) then some of the canonical variables may be eliminated from the theory through the introduction of a new type of bracket, a modification of the ordinary

Poisson bracket, which DE WITT has called the "Dirac bracket." It is the Dirac bracket which corresponds to the commutator (or anticommutator) in the quantum form of the theory.

In the evaluation of $(\chi_\alpha, \chi_\beta)$ identities of integrability are needed for the first time. One finds

$$(\chi_\alpha, \chi_\beta) \equiv \chi_\gamma r^\gamma_{\alpha\beta} \stackrel{w}{=} 0.$$

Evidently Dirac brackets are here the same as ordinary Poisson brackets. Under these circumstances the χ's are said to be "of the first class." It is characteristic of any theory in which the constraints arise as a result of a gauge invariance principle that all of the Φ's and χ's are of the first class.

DE WITT called attention to the fact that up to this point not all of the identities of integrability have been used. The classical theory, which is completed at this point, can in fact get along *without* the unused identities. In the quantum theory, however, the unused identities turn out to be crucial.

DE WITT first considered the Φ and χ equations, which, in the quantum theory, are to be regarded as *supplementary conditions* on the state vector $\underline{\psi}$ of the system:

$$\Phi_\alpha \underline{\psi} = 0 \ , \quad \chi_\alpha \underline{\psi} = 0.$$

Since the classical expressions for the Φ_α and χ_α involve quantities which, in the quantum theory, do not commute, the factor ordering problem here makes its appearance. According to DE WITT the quantum analogs of Φ_α and χ_α should be taken as

$$\Phi_\alpha = \frac{1}{2}\{p_i - a_i, \tau^i_\alpha\} + \frac{1}{2} i\hbar S^\beta_{\alpha i} \tau^i_\beta$$
$$\chi_\alpha = \frac{1}{2}\{p_i - a_i, \rho^i_\alpha\} + v_{,i}\tau^i_\alpha + \frac{1}{2} i\hbar S^\beta_{\alpha i} \rho^i_\beta$$

where $\{\,\}$ denotes the anticommutator bracket. With the aid of the previously unused identities of integrability one may then show that

$$
\begin{aligned}
[\Phi_\alpha, \Phi_\beta] &= i\hbar\, t^{\gamma}_{\alpha\beta}\, \Phi_\gamma \\
[\chi_\alpha, \Phi_\beta] &= i\hbar\, u^{\gamma}_{\alpha\beta} \Phi_\gamma + i\hbar\, S^{\gamma}_{\alpha i} \tau^{i}_{\beta}\, \chi_\gamma \\
[\chi_\alpha, \chi_\beta] &= i\hbar\, r^{\gamma}_{\alpha\beta}\, \chi_\gamma
\end{aligned}
$$

where $[\ ] = i\hbar(\ )$ denotes the commutator bracket and where the ordering of factors on the right is now important. Since the p's and χ's all stand to the extreme right the corollaries

$$[\Phi_\alpha, \Phi_\beta]\underline{\psi} = 0\,, \quad [\chi_\alpha, \Phi_\beta]\underline{\psi} = 0\,, \quad [\chi_\alpha, \chi_\beta]\underline{\psi} = 0$$

of the supplementary conditions are automatically satisfied. DE WITT pointed out that the appropriate quantum analog of a classical quantity may have one or more terms in it (such as the $\frac{1}{2} i\hbar S^{\beta}_{\alpha i} \tau^{i}_{\beta}$ above) which are proportional to $\hbar$ or powers of $\hbar$, and which do not appear in the corresponding classical expression (since $h \to 0$).

Similar considerations are involved in finding the quantum analog of the function H. It is convenient to return for a moment to the classical theory: Since the Φ's and χ's are all of the first class, the quantities $\tau^{\alpha}_{i} \dot{q}^{i}$ appearing in the dynamical equation (i.e., for $\dot{F}$) are completely arbitrary. One is at liberty to set them equal to arbitrary functions f^α of the q's and p's through the addition of extra supplementary conditions:

$$\tau^{\alpha}_{i} \dot{q}^{i} \overset{w}{=} f^{\alpha}.$$

The energy function E is then strongly equal to

$$\overline{H} \equiv H + f^{\alpha} \Phi_\alpha.$$

Furthermore, the dynamical equation may be replaced by

$$\dot{F} \overset{w}{=} (F, \overline{H}) \overset{w}{=} (F, H) + f^{\alpha} (F, \Phi_\alpha).$$

The conditions $\dot{\Phi} \overset{w}{=} 0$, $\dot{\chi} \overset{w}{=} 0$ are, of course, unaffected.

In the quantum theory the dynamical equation may be taken as

$$i\hbar\dot{F} = [F, \overline{H}]$$

in which the quantum form of H is written as above with the Φ's standing to the right. The time derivatives of the supplementary conditions, namely

$$\dot{\Phi}_\alpha \underline{\psi} = 0 \ , \quad \dot{\chi}_\alpha \underline{\psi} = 0$$

will then be satisfied provided

$$[\Phi_\alpha, H]\underline{\psi} = 0 \ , \quad [\chi_\alpha, H]\underline{\psi} = 0.$$

The quantum form of H will be chosen in such a way that these equations are automatically satisfied by virtue of the supplementary conditions. The supplementary conditions when combined with the dynamical equations for q_i will generally imply

$$(\tau_i^\alpha \dot{q}^i - f^\alpha)_Q \underline{\psi} = 0$$

where $(\ )_Q$ indicates some suitable quantum analog for $\tau_i^\alpha \dot{q}^i - f^\alpha$, and in this way the consistency of the quantum scheme can be checked for arbitrary choices of the f^α.

It is to be noted that one is here working in the Heisenberg picture in which the state vector $\underline{\psi}$ is time independent. The arbitrariness in the choice of the functions f^α shows that there are many different Heisenberg pictures, all equally valid. They all lead, however, to a single unique Schrödinger picture:

$$\psi(t) = e^{-\frac{i}{\hbar}\overline{H}t}\underline{\psi} = e^{-\frac{i}{\hbar}Ht}\underline{\psi}.$$

One may arrive directly at the Schrödinger picture by starting, in the classical theory, from the "homogeneous velocity" formalism (Dirac, *op. cit.*) in which the time is introduced as a canonical variable with a conjugate momentum λ. This leads to a new Φ equation, namely $\Phi \stackrel{w}{=} 0$ where $\Phi \equiv \lambda + H$. In the quantum theory, representation of λ $-i\hbar\frac{\partial}{\partial t}$ puts the corresponding condition on the state vector in the form

$$i\hbar\dot{\psi} = H\psi$$

which is just the Schrödinger equation.

Construction of the quantum form of H is more difficult than the construction of the quantum forms of Φ_α and χ_α for two reasons: (1) H is quadratic rather than linear in the momenta. (2) No metric has yet been defined in q-space. If there were no gauge transformation group for the system, g_{ij} would be nonsingular and would, as DE WITT had already pointed out, provide the natural metric. In the present case DE WITT suggested building up a nonsingular metric $\overline{g}_{ij}$ as a sort of superstructure with g_{ij} as a core, in the following manner:

$$(\overline{g}_{ij}) = \left[\tau_i^\alpha \tau_i^\mu\right] \begin{bmatrix} h_{\alpha\beta} & k_{\alpha\nu} \\ k_{\mu\beta} & g_{\mu\nu} + k_{\mu\gamma}k_\nu^\gamma \end{bmatrix} \begin{bmatrix} \tau_j^\beta \\ \tau_j^\nu \end{bmatrix}$$

matrix multiplication as indicated, where $h_{\alpha\beta}$, $k_{\mu\alpha}$ $(= k_{\alpha\mu})$ are *arbitrary* functions of the q's. However, $h_{\alpha\beta}$ is assumed to have an inverse $h^{\alpha\beta}$ which is used to raise the indices α, β, γ, etc., while $g^{\mu\nu}$ is used to raise the indices μ, ν, σ, etc. $(\overline{g}_{ij})$ will then have an inverse given by

$$(\overline{g}^{ij}) = \left[\tau_\alpha^i \tau_\mu^i\right] \begin{bmatrix} h^{\alpha\beta} + k^{\alpha\sigma}k_\sigma^\beta & -k^{\alpha\nu} \\ -k^{\mu\beta} & g^{\mu\nu} \end{bmatrix} \begin{bmatrix} \tau_\beta^j \\ \tau_\nu^j \end{bmatrix}$$

and a determinant given by

$$\overline{g} = \tau^{-2} hg$$

where $\tau = \left|\tau_\alpha^i \tau_\mu^i\right|$, $h = \left|h_{\alpha\beta}\right|$, $g = \left|g_{\mu\nu}\right|$. The metric $\overline{g}_{ij}$ may be used to make a special choice for the function $\overline{H}$, namely

$$\overline{H} \equiv \frac{1}{2}\overline{g}^{ij}(p_i - a_i)(p_j - a_j) + v$$

corresponding to the choice

$$f^\alpha \equiv -2\tau_\mu^i k^{\mu\alpha}(p_i - a_i) + (h^{\alpha\beta} + k^{\alpha\mu}k_\mu^\beta)\Phi_\beta.$$

The equations of motion generated by this "Hamiltonian" may be derived from a Lagrangian function of the form

$$\overline{L} \equiv \frac{1}{2}\overline{g}_{ij}\dot{q}^i\dot{q}^j + a_i\dot{q}^i - v.$$

The quantum theory generated by this Lagrangian function is not identical with that which has been developed here, but becomes so upon the addition of the equations $(\tau_i^\alpha \dot{q}^i - f^\alpha)_Q \underline{\psi} = 0$ as supplementary conditions. This is a standard procedure in quantum electrodynamics in which these supplementary conditions become the "Lorentz conditions."

A choice of metric having been adopted, one may now express the quantized momenta explicitly in the form

$$p_i = -i\hbar\overline{g}^{-1/4}\frac{\partial}{\partial q^i}\overline{g}^{1/4}$$

and one finds that the supplementary conditions become

$$\tau^i_\alpha\psi_{\cdot i} + [-\frac{i}{\hbar}a_i\tau^i_\alpha + \frac{1}{2}\tau^i_{\alpha\cdot i}] = 0$$
$$\rho^i_\alpha\psi_{\cdot i} + [-\frac{i}{\hbar}(a_i\rho^i_\alpha - v_{\cdot i}\tau^i_\alpha) + \frac{1}{2}\rho^i_{\alpha\cdot i}]\psi = 0$$

a dot followed by an index denoting covariant differentiation with respect to the metric $\overline{g}_{ij}$, with, however, the modification

$$\tau^i_{\alpha\cdot j} \equiv \tau^i_{\alpha,j} + \overline{\left\{ {i \atop kj} \right\}}\tau^k_\alpha - \tau^i_\beta S^\beta_{\alpha j}$$
$$\rho^i_{\alpha\cdot j} \equiv \rho^i_{\alpha,j} + \overline{\left\{ {i \atop kj} \right\}}\rho^k_\alpha - \rho^i_\beta S^\beta_{\alpha j}.$$

$\overline{\left\{ {i \atop kj} \right\}}$ being the Christoffel symbol defined by $\overline{g}_{ij}$. A special significance possessed by the coefficients $S^\beta_{\alpha j}$ is here emphasized. The $S^\beta_{\alpha j}$ provide a kind of *affine connection* in terms of which one may define parallel displacements which are invariant not only under point transformations but also under transformations associated with the indices α, β, etc., of the form

$$\tau'^{\,i}_\alpha = \tau^i_\beta X^\beta_\alpha$$
$$\rho'^{\,i}_\alpha = \rho^i_\beta X^\beta_\alpha$$
$$\sigma'^{\,i}_{\alpha j} = \sigma^i_{\beta j}X^\beta_\alpha + \tau^i_\beta X^\beta_{\alpha,j}$$
$$\delta\Lambda'^{\,\alpha} = X^{-1\alpha}{}_\beta\,\delta\Lambda^\beta$$
$$S'^{\,\beta}_{\alpha i} = X^{-1\beta}{}_\gamma S^\gamma_{\delta i}X^\delta_\alpha + X^{-1\beta}{}_\gamma X^\gamma_{\alpha,i}$$
$$v'^{\,\gamma}_{\alpha\beta} = X^{-1\gamma}{}_\zeta v^\zeta_{\delta\varepsilon}X^\delta_\alpha X^\varepsilon_\beta$$

where the X^β_α are arbitrary functions of the q's, the matrix (X^β_α) possessing, however, an inverse $(X^{-1\beta}{}_\alpha)$. These transformations, which DE WITT called *X-transformations*, leave all the identities of invariance, completeness, and integrability unchanged. The X-transformation procedure may be extended to the indices μ, ν, σ, etc., by the definitions

$$\begin{aligned}
\tau'^{\,i}_{\mu} &= \tau^{i}_{\mu} X^{\alpha}_{\mu} + \tau^{i}_{\nu} X^{\nu}_{\mu} \\
\tau'^{\,\alpha}_{i} &= X^{-1\alpha}_{\ \ \beta}\tau^{\beta}_{i} - X^{-1\alpha}_{\ \ \beta} X^{\beta}_{\mu} X^{-1\mu}_{\ \ \nu}\tau^{\nu}_{i} \\
\tau'^{\,\mu}_{i} &= X^{-1\mu}_{\ \ \nu}\tau^{\nu}_{i}
\end{aligned}$$

where the X^{α}_{μ}, X^{μ}_{ν} are also arbitrary functions of the q's, with (X^{μ}_{ν}) possessing as an inverse $(X^{-1\mu}_{\ \ \nu})$. The whole theory (in particular, the metric $\overline{g}^{ij}$) will then be invariant under X-transformations provided

$$\begin{aligned}
h'_{\alpha\beta} &= h_{\gamma\delta} X^{\gamma}_{\alpha} X^{\delta}_{\beta} \\
k'_{\alpha\mu} &= k_{\beta\nu} X^{\beta}_{\alpha} X^{\nu}_{\mu} + h_{\beta\gamma} X^{\beta}_{\alpha} X^{\gamma}_{\mu}.
\end{aligned}$$

In order to show the invariance of the quantum theory under point, phase, gauge, and X-transformations one does not actually have to use the differential representation of the momenta, convenient though it generally is. For example, the point transformation law for the momenta is

$$p'^{\,i} = \frac{1}{2}\left\{ \frac{\partial q^{j}}{\partial q'^{\,i}}, p_{j} \right\},$$

and, remembering that the indices i, j, etc., on the quantities g_{ij}, $\overline{g}_{ij}$, τ^{i}_{α}, τ^{i}_{μ}, p^{i}_{α}, $S^{i}_{\alpha j}$, etc., are all tensor indices under point transformations, one finds by straightforward computation that $\overline{H}' = \overline{H}$, $\Phi'_{\alpha} = \Phi_{\alpha}$, $\chi'_{\alpha} = \chi_{\alpha}$, provided one writes

$$\overline{H} = \frac{1}{2}\overline{g}^{-1/4}(p_i - a_i)\overline{g}^{1/2}\overline{g}^{ij}(p_j - a_j)\overline{g}^{-1/4} + v + \hbar^2\overline{Z}$$

where $\overline{Z}$ is a scalar function of the q's, as yet undetermined. Similarly, under X-transformations one finds $\overline{H}' = H$ and

$$\Phi'_{\alpha} = X^{\beta}_{\alpha}\Phi_{\beta}\,, \quad \chi'_{\alpha} = X^{\beta}_{\alpha}\chi_{\beta}.$$

Since the Φ's and χ's stand to the right the supplementary conditions remainunchanged: $\Phi'_{\alpha}\psi = 0$, $\chi'_{\alpha}\psi = 0$. Invariance of the quantum theory under gauge transformations is not immediately apparent at this point, but becomes so subsequently. Phase transformations, on the other hand, are trivial:

$$a_i' = a_i + P_{,i}\,, \quad p_i' = p_i + P_{,i}\,,$$

or, if one is using the fixed differential representation for the momenta, one places the burden of keeping the theory phase-invariant on the state vector (or wave function) by the law

$$\psi' = e^{\frac{i}{\hbar}P}\psi.$$

Having introduced X-transformations, DE WITT then pointed out that the affinity $S^{\beta}_{\alpha ij}$ satisfies the very important condition of being *integrable*. This means that one can carry out an X-transformation which will make the $S^{\beta}_{\alpha ij}$ *vanish everywhere*. The required transformation is given by the solutions of the simultaneous equations

$$X^{\beta}_{\alpha,i} = -S^{\beta}_{\gamma i}X^{\gamma}_{\alpha}.$$

The solubility of these equations is guaranteed by the identity of integrability $S^{\beta}_{\alpha i,j} - S^{\beta}_{\alpha j,i} - S^{\beta}_{\gamma i}S^{\gamma}_{\alpha j} + S^{\beta}_{\gamma j}S^{\gamma}_{\alpha i} \equiv 0$. After this transformation has been carried out one has

$$\tau^{i}_{\alpha,j}\tau^{j}_{\beta} - \tau^{i}_{\beta,j}\tau^{j}_{\alpha} \equiv 0,$$

which implies that there exists a point transformation $q^i \to q^{\alpha}, q^{\mu}$ such that

$$\tau^{i}_{\alpha} = \frac{\partial q^i}{\partial q^{\alpha}}.$$

Making this point transformation, together with an additional X-transformation

$$X^{\alpha}_{\beta} = \delta^{\alpha}_{\beta}\,, \quad X^{\alpha}_{\mu} = -k^{\alpha}_{\mu}\,, \quad X^{\mu}_{\nu} = \delta^{\mu}_{\nu}\,,$$

and letting the τ^{i}_{μ} be given by

$$\tau^{i}_{\mu} = \frac{\partial q^i}{\partial q^{\mu}},$$

one arrives at a great simplification in the formalism. Using a simple replacement of indices $i \to \alpha, \mu$; $j \to \beta, \nu$, etc., to denote quantities in the new "coordinate system," one has

$$\begin{aligned}
&\tau^{\beta}_{\alpha} = \delta^{\beta}_{\alpha}\ ,\ \ \tau^{\mu}_{\alpha} = 0\ ,\ \ \tau^{\alpha}_{\mu} = 0\ ,\ \ \tau^{\mu}_{\nu} = \delta^{\mu}_{\nu}\\
&\overline{g}_{\alpha\beta} = h_{\alpha\beta}\ ,\ \ \overline{g}_{\sigma\mu} = k_{\sigma\mu} = 0\ ,\ \ \overline{g}_{\mu\nu} = g_{\mu\nu}\\
&\overline{g} = hg\ \ .
\end{aligned}$$

Moreover, the identities of invariance and integrability take the forms

$$\begin{aligned}
&g_{\alpha\beta} = 0\ ,\ \ g_{\sigma\mu} = g_{\mu\alpha} = 0\\
&g_{\mu\nu,\alpha} = 0\\
&f_{\alpha\beta} = 0\ ,\ \ f_{\mu\alpha} = g_{\mu\nu}\rho^{\nu}_{\alpha}\\
&g_{\mu\nu,\sigma}\rho^{\sigma}_{\alpha} + g_{\mu\sigma}\rho^{\sigma}_{\alpha,\nu} + g_{\nu\sigma}\rho^{\sigma}_{\alpha,\mu} = 0\\
&f_{\alpha\mu}\rho^{\mu}_{\beta} = v_{,\alpha\beta}\ ,\ \ \ f_{\mu\beta}\rho^{\beta}_{\alpha} + f_{\mu\nu}\rho^{\nu}_{\alpha} = v_{,\alpha\mu}\\
&v_{,\beta}\rho^{\beta}_{\alpha} + v_{,\mu}\rho^{\mu}_{\alpha} = 0\\
&\rho^{\gamma}_{\alpha,\beta} = v^{\gamma}_{\alpha\beta}\ ,\ \ \ \rho^{\mu}_{\alpha,\beta} = 0\\
&\rho^{\gamma}_{\alpha,\mu}\rho^{\mu}_{\beta} - \rho^{\gamma}_{\beta,\mu}\rho^{\mu}_{\alpha} = \rho^{\gamma}_{\delta}v^{\gamma}_{\alpha\beta} - v^{\gamma}_{\alpha\delta}\rho^{\delta}_{\beta} + v^{\gamma}_{\beta\delta}\rho^{\delta}_{\alpha}\\
&\rho^{\mu}_{\alpha,\nu}\rho^{\nu}_{\beta} - \rho^{\mu}_{\beta,\nu}\rho^{\nu}_{\alpha} = \rho^{\mu}_{\gamma}v^{\gamma}_{\alpha\beta}\\
&f_{\mu\gamma}(\rho^{\mu}_{\alpha}\rho^{\gamma}_{\beta} - \rho^{\mu}_{\beta}\rho^{\gamma}_{\alpha}) + f_{\mu\nu}\rho^{\mu}_{\alpha}\rho^{\nu}_{\beta} + v_{,\gamma}v^{\gamma}_{\alpha\beta} = 0\\
&v^{\gamma}_{\alpha\beta,\delta} = 0\ ,\ \ \ v^{\gamma}_{\alpha\beta,\mu} = 0.
\end{aligned}$$

The identity $f_{\alpha\beta} = 0$ implies that a phase transformation ($P_{,\alpha} = -a_{\alpha}$) can be carried out which makes a_{α} vanish. Assuming that it has already been performed and noting that

$$\begin{aligned}
\tau^{i}_{\alpha\cdot i} &= \tau^{\beta}_{\alpha\cdot\beta} + \tau^{\mu}_{\alpha\cdot\mu} = \frac{1}{2}(\ln\overline{g})_{,\beta}\tau^{\beta}_{\alpha} = \frac{1}{2}(\ln h)_{,\alpha}\\
\rho^{i}_{\alpha\cdot i} &= \rho^{\beta}_{\alpha,\beta} + \rho^{\mu}_{\alpha,\mu} + \frac{1}{2}(\ln\overline{g})_{,\mu}\rho^{\mu}_{\alpha} + \frac{1}{2}(\ln\overline{g})_{,\beta}\rho^{\beta}_{\alpha}\\
&= v^{\beta}_{\alpha\beta} + \frac{1}{2}(\ln h)_{,\mu}\rho^{\mu}_{\alpha} + \frac{1}{2}(\ln h)_{,\beta}\rho^{\beta}_{\alpha} + \frac{1}{2}g^{\nu\sigma}(g_{\nu\sigma,\mu}\rho^{\mu}_{\alpha} + g_{\nu\mu}\rho^{\mu}_{\alpha,\sigma} + g_{\sigma\mu}\rho^{\mu}_{\alpha,\nu})\\
&= v^{\beta}_{\alpha\beta} + \frac{1}{2}(\ln h)_{,\mu}\rho^{\mu}_{\alpha} + \frac{1}{2}(\ln h)_{,\beta}\rho^{\beta}_{\alpha}
\end{aligned}$$

one may now write the supplementary condition in the forms

$$\psi_{,\alpha} + \frac{1}{4}(\ln h)_{,\alpha}\psi = 0$$
$$\rho_\alpha^\mu \psi_{,\mu} + \left[-\frac{i}{\hbar}(a_\mu \rho_\alpha^\mu - v_{,\alpha}) + \frac{1}{2} v^\beta_{\alpha\beta} + \frac{1}{4}(\ln h)_{,\mu}\rho_\alpha^\mu \right] \psi = 0.$$

The first supplementary condition has the important result that the arbitrary quantities $h_{\alpha\beta}$ can now be actually eliminated entirely from the quantum theory, just as they are not needed in the classical theory. That is, having carefully built up a "superstructure" around the metric g_{ij} (or $g_{\mu\nu}$), one then proceeds to throw it away. To do this one simply introduces a new wave function

$$\psi_1 = h^{1/4}\psi$$

satisfying

$$\psi_{1,\alpha} = 0$$
$$\rho_\alpha^\mu \psi_{1,\mu} + \left[-\frac{i}{\hbar}(a_\mu \rho_\alpha^\mu - v_{,\alpha}) + \frac{1}{2} v^\beta_{\alpha\beta} \right] \psi_1 = 0.$$

This definition removes from the wave function that part of the metric density which refers exclusively to the q^α, and corresponds to the use of $g^{1/2}\prod_\mu dq^\mu$ as invariant volume element. The condition $\psi_{1,\alpha} = 0$ means that the new wave function does not depend on the variables q^α. *The q^α are non-observable or "non-physical" variables of the system.* This was, in fact, already true in the classical theory, since the q^α can be made to undergo arbitrary changes by means of gauge transformations.

In the new representation $g_{\mu\nu}$ by itself provides a natural metric in the reduced space of the q^μ. To see how this works for the Hamiltonian operator, one can multiply the Schrödinger equation

$$i\hbar\dot{\psi} = \overline{H}\psi$$

by $h^{1/4}$. Using the explicit form for $\overline{H}$, one finds, after a straightforward calculation,

$$i\hbar\dot{\psi}_1 = -\frac{1}{2}\hbar^2 g^{\mu\nu}\psi_{1\cdot\mu\nu} + i\hbar(a^\mu\psi_{1\cdot\mu} + \frac{1}{2}a^\mu_{\cdot\mu}\psi_1) + (\frac{1}{2}a^\mu a_\mu + v + \hbar^2 Z)\psi_1$$

where the dots now denote covariant differentiation with respect to the metric $g_{\mu\nu}$, and where

$$Z = \overline{Z} + \frac{1}{2}\hbar^{-1/4} g^{-1/2} \frac{\partial}{\partial q^{\mu}} (g^{1/2} g^{\mu\nu} \frac{\partial}{\partial q^{\nu}} h^{1/4}) + \frac{1}{2} h^{-1/4} \frac{\partial}{\partial q^{\alpha}} (h^{\alpha\beta} \frac{\partial}{\partial q^{\beta}} h^{1/4}).$$

The quantity Z is invariant under point transformations which are restricted to the variables q^{α} alone.

The new Schrödinger equation may be written in the form

$$i\hbar\dot{\psi}_1 = H\psi_1$$

where

$$H = \frac{1}{2} g^{-1/4} (p_{\mu} - a_{\mu}) g^{1/2} g^{\mu\nu} (p_{\nu} - a_{\nu}) g^{-1/4} + v + \hbar^2 Z,$$

the explicit differential form for the momenta p_{μ} in the new representation being

$$p_{\mu} = i\hbar g^{-1/4} \frac{\partial}{\partial q^{\mu}} g^{1/4}.$$

The existence of the second supplementary condition suggests that there are further non-physical variables in addition to the q^{α}. To show this one must first eliminate the term in $(a_{\mu}\rho^{\mu}_{\alpha} - v_{,\alpha})$ from the supplementary condition. This can be done by carrying out a phase transformation P satisfying

$$P_{,\mu}\rho^{\mu}_{\alpha} = v_{,\alpha} - a_{\mu}\rho^{\mu}_{\alpha}$$

and at the same time satisfying

$$P_{,\alpha} = 0$$

so as not to disturb the condition $a_{\alpha} = 0$. The integrability of these equations is insured by the identities of invariance and integrability, as one may verify by straightforward computation, showing that the expressions for $(P_{,\mu\beta} - P_{,\beta\mu})\rho^{\mu}_{\alpha}$ and $(P_{,\mu\nu} - P_{,\nu\mu})\rho^{\mu}_{\alpha}\rho^{\nu}_{\alpha}$ vanish. One may then write

$$\rho^{\mu}_{\alpha}\psi_{1,\mu} + \frac{1}{2} v^{\beta}_{\alpha\beta}\psi_1 = 0.$$

Next, by differentiating the identity $\rho^\mu_{\alpha,\nu}\rho^\nu_\beta - \rho^\mu_{\beta,\nu}\rho^\nu_\alpha = \rho^\mu_\gamma v^\gamma_{\alpha\beta}$ one can show that

$$v^\delta_{\alpha\varepsilon}v^\varepsilon_{\beta\gamma} + v^\delta_{\beta\varepsilon}v^\varepsilon_{\gamma\alpha} + v^\delta_{\gamma\varepsilon}v^\varepsilon_{\alpha\beta} = 0.$$

The coefficients $v^\gamma_{\alpha\beta}$ are then immediately recognizable as the structure constants of a *Lie group*. The secondary constraints χ_α are, in fact, the infinitesimal generators of the group. In the general case the structure constants will be non-vanishing and the Lie group will be non-Abelian. This immediately leads one to various possibilities depending on the classification of the Lie group in question. As an illustration, suppose that the Lie group is the irreducible rotation group in m dimensions. The indices α must then range over $\frac{1}{2}m(m-1)$ different values. This is *not* however the number of further non-physical variables. A point transformation $q^\mu \to x_a, q^A [a = 1 \ldots m, A = 1 \ldots n - \frac{1}{2}m(m+1)]$ can be made such that the new variables x_a describe the subspace in which the rotations actually take place. The quantity $v^\beta_{\alpha\beta}$ vanishes for the rotation group and the second supplementary condition is found to reduce to $\rho^a_\alpha \psi_{1,a} = 0$ which, when analyzed, is seen to state simply that the wave function is invariant under rotations in the subspace of the x_a. Therefore, in this case only $m-1$ further variables can be eliminated from the theory as non-physical. The wave function can still depend on the rotation-invariant combination $x_a x_a$ as well as on the q^A.

The primary constraints Φ_α also, of course, generate a Lie group. But this group is necessarily Abelian. DE WITT assumed, for simplicity, that the group generated by the χ_α is also Abelian (so that $v^\gamma_{\alpha\beta} = 0$) and furthermore that the vectors $\rho^i_\alpha$are linearly independent of the τ^i_α and of each other. In this case, when $S^\beta_{\alpha i} = 0$, one may carry out a point transformation $q^i \to q^\alpha, q^{\alpha'}, q^A$ such that

$$\tau^i_\alpha = \frac{\partial q^i}{\partial q^\alpha}\,, \quad \rho^i_{\alpha'} = \frac{\partial q^i}{\partial q^{\alpha'}}.$$

(Here, in order to avoid confusion one must use a prime to distinguish indices related to the ρ^i_α from those related to the τ^i_α.) In the new "coordinate system" one has

$$\tau_\alpha^\beta = \delta_\alpha^\beta\,, \quad \tau_\alpha^{\beta'} = 0\,, \qquad \tau_\alpha^A = 0\,,$$
$$\rho_{\alpha'}^\beta = 0\,, \qquad \rho_{\alpha'}^{\beta'} = \delta_{\alpha'}^\beta\,, \quad \rho_{\alpha'}^A = 0\,,$$

and the identities of invariance and integrability become

$$\begin{aligned}
&g_{\alpha'\beta',\gamma} = 0\,, \qquad g_{\alpha'A,\gamma} = 0\,, \quad g_{AB,\gamma} = 0\\
&g_{\alpha'\beta',\gamma'} = 0\,, \quad g_{\alpha'A',\gamma'} = 0\,, \quad g_{AB,\gamma'} = 0\\
&f_{\alpha\beta} = 0\,, \qquad\quad f_{\alpha'\beta'} = 0\\
&g_{\alpha'\beta'} f_{\alpha'\beta} = -v_{,\alpha\beta}\\
&g_{\alpha' A} = f_{A\alpha}\\
&f_{A\alpha'} = v_{,\alpha A}\\
&v_{,\alpha'} = 0.
\end{aligned}$$

If the phase is adjusted so that

$$a_\alpha = 0 \ \text{ and } \ a_{\alpha'} = v_{,\alpha}$$

then one also has

$$\begin{aligned}
&g_{\alpha' A} = -a_{A,\alpha}\\
&a_{A,\alpha'} = a_{\alpha',A} - f_{A\alpha'} = v_{,\alpha A} - v_{,\alpha A} = 0.
\end{aligned}$$

Moreover, the supplementary conditions become

$$\psi_{1,\alpha} = 0\,, \quad \psi_{1,\alpha'} = 0$$

so that the $q^{\alpha'}$, like the q^α, are now "non-physical" variables.
The quantity v is seen to be independent of the $q^{\alpha'}$ and to have a dependence on the q^α which is no more than quadratic (since $v_{,\alpha\beta\gamma} = -g_{\alpha'\beta',\gamma} = 0$). One may choose the origin of the unobservable "coordinates" q^α in such a way that

$$v = \frac{1}{2} w_{\alpha\beta} q^\alpha q^\beta + u$$

where $w_{\alpha\beta}$ and u are independent of the q^α and $q^{\alpha'}$. Furthermore, since $a_{A,\alpha'} = 0$ and $a_{A,\alpha\beta} = g_{\alpha' A,\beta} = 0$, one may write

$$a_A = b_{A\alpha} q^\alpha + b_A$$

where $b_{A\alpha}$ and b_A are independent of the q^α and $q^{\alpha'}$. In order to complete the elimination of the $q^{\alpha'}$ from the theory, a natural metric $\underline{g}_{AB}$ for the reduced space of the q^A must be introduced. DE WITT showed that it must be defined in the following manner:

$$\begin{bmatrix} g_{\alpha'\beta'} & g_{\alpha'\beta} \\ g_{A\beta'} & g_{AB} \end{bmatrix} = \begin{bmatrix} -w_{\alpha\beta} & -b_{B\alpha} \\ -b_{A\beta} & \underline{g}_{AB} - b_{A\gamma} w^{\gamma\delta} b_{B\delta} \end{bmatrix}$$

$$\begin{bmatrix} g^{\alpha'\beta} & g^{\alpha' B} \\ g^{A\beta'} & g^{AB} \end{bmatrix} = \begin{bmatrix} -w^{\alpha\beta} + w^{\alpha\gamma} b_{C\gamma} \underline{g}^{CD} b_{D\delta} w^{\delta\beta} & -w^{\alpha\gamma} b_{C\gamma} \underline{g}^{CB} \\ -\underline{g}^{AC} b_{C\gamma} w^{\gamma\beta} & \underline{g}^{AB} \end{bmatrix}$$

in which use has been made of the relations $g_{\alpha'\beta'} = -v_{,\alpha\beta} = -w_{\alpha\beta}$, $g_{A\beta'} = -a_{A,\beta} = -b_{A\beta}$, and in which the matrix $(w_{\alpha\beta})$ is assumed to have an inverse $(w^{\alpha\beta})$.

With this definition one has $\underline{g}^{AC} \underline{g}_{CB} = \delta^A_B$ and

$$g = w\underline{g}$$

where $w = |w_{\alpha\beta}|$ and $\underline{g} = |\underline{g}_{\alpha\beta}|$. The determinant w forms that part of the metric density g which refers exclusively to the non-physical variables $q^{\alpha'}$. It may be removed from the normalization of the wave function ψ_1 by introducing a new wave function

$$\psi_2 = w^{1/4} \psi_1 .$$

Since $w_{,\alpha} = 0$, $w_{,\alpha'} = 0$, ψ_2 is independent of the q^α and $q^{\alpha'}$. It may becalled the "true physical wave function" of the system referring only to the "physical" variables q^A. Its Schrödinger equation may be found by multiplying the Schrödinger equation $i\hbar\dot{\psi}_1 = H\psi_1$ by $w^{1/4}$ making use of the relations

$$\begin{aligned} a_{\alpha'} &= v_{,\alpha} = w_{\alpha\beta} q^\beta \\ g^{A\alpha'} a_{\alpha'} &= -\underline{g}^{AC}(a_C - b_C) \\ g^{\alpha'\beta'} a_{\alpha'} a_{\beta'} &= -v + u + (a_A - b_A)\underline{g}^{AB}(a_B - b_B). \end{aligned}$$

One finds, after a straightforward computation,

$$i\hbar\dot{\psi}_2 = \underline{H}\psi_2$$

where

$$\begin{aligned}
\underline{H} &= \frac{1}{2}\underline{g}^{-1/4}(p_A - b_A)\underline{g}^{1/2}\underline{g}^{AB}(p_B - b_B)\underline{g}^{-1/4} + u + \hbar^2\underline{Z}\,, \\
\underline{Z} &= Z + \frac{1}{2}w^{-1/4}\underline{g}^{1/2}\frac{\partial}{\partial q^A}(\underline{g}^{1/2}\underline{g}^{AB}\frac{\partial}{\partial q^B}w^{1/4})\,, \\
p_A &= -i\hbar\underline{g}^{-1/4}\frac{\partial}{\partial^A}\underline{g}^{1/4}\,.
\end{aligned}$$

The operator $\underline{H}$ remains invariant under point transformations which are restricted to the variables q^A alone.

The operators q^A, p_A and any quantities constructed out of them are the *true observables* of the theory. The theory is now reduced to its simplest terms. In this form it is quite easy to see that the Schrödinger equation is consistent with the constraints

$$\psi_{2,\alpha} = 0\,, \quad \psi_{2,\alpha'} = 0.$$

One has only to require that $\underline{Z}$ be independent of the q^α and $q^{\alpha'}$. $\underline{H}$ will then be a true observable (the true energy operator, depending only on the q^A and p_A) and the wave function ψ_2 will remain independent of the q^α and $q^{\alpha'}$ at all times.

The gauge invariance of the quantum theory is now also immediately evident. In the "coordinate" system q^α, $q^{\alpha'}$, q^A, gauge transformations are given simply by

$$\delta q^\alpha = \delta\dot{\Lambda}^\alpha\,, \quad \delta q^{\alpha'} = \delta\Lambda^\alpha\,, \quad \delta q^A = 0$$

and hence

$$\delta\underline{H} = 0\,, \quad \delta\psi_2 = 0.$$

There remains only the term $\hbar^2\underline{Z}$ which is unknown. However this term, as Feynman pointed out previously, depends on how the system is constrained to a "shell" which passes over to the space of the q^A in the limit, and this is arbitrary.

It is of interest to examine the Feynman quantization method in the present context. The path summation may be defined by breaking the time interval into small pieces and integrating over the physical variables q^A only, using the measure defined by the metric $\underline{g}_{AB}$. The variables q^α may be chosen as arbitrary functions of the time (corresponding to the original arbitrariness in the $\tau_i^\alpha \dot{q}^i$) and the original Lagrangian function L may be used to compute the action, provided the variables $q^{\alpha'}$ are made to vary in time in such a way as to satisfy the velocity constraints which in present notation, become

$$g_{\alpha'\beta'}\dot{q}^{\beta'} + g_{\alpha' A}\dot{q}^A + v_{,\alpha} = 0$$

or

$$\dot{q}^{\alpha'} = q^\alpha - w^{\alpha\beta} b_{A\beta}\dot{q}^A.$$

For the Lagrangian function then becomes

$$\begin{aligned} L &= \frac{1}{2} g_{AB}\dot{q}^A\dot{q}^B + g_{\alpha' A}\dot{q}^{\alpha'}\dot{q}^A + \frac{1}{2} g_{\alpha'\beta'}\dot{q}^{\alpha'}\dot{q}^{\beta'} + a_A\dot{q}^A + a_{\alpha'}\dot{q}^{\alpha'} - v \\ &= \frac{1}{2}\underline{g}_{AB}\dot{q}^A\dot{q}^B + b_A\dot{q}^A - u \end{aligned}$$

which is precisely the form which gives rise to the Hamiltonian $\underline{H}$. One may therefore express the transformation function in the symbolic form

$$\langle q''^A, t'' | q'^A, t' \rangle = \int_{q'^A, t'}^{q''^A, t''} \delta[q^A] \exp(\frac{i}{\hbar}\int_{t'}^{t''} L\, dt)$$

with the understanding that the functional integration is to be carried out over all paths between the points q'^A, t' and q''^A, t'' which satisfy the velocity constraints with q^α given, each path being assigned a weight according to the measuredefined by $\underline{g}_{AB}$. (Here, potential-like terms proportional to $\hbar^2$, which may be added to L, are ignored.)

It is not actually necessary to restrict the summation to the variables q^A only. For example, one may work with the transformation function

$$\langle q''^{\alpha'}, q''^A, t'' | q'^{\alpha'}, q'^A, t' \rangle = \int_{q'^{\alpha'}, q'^A, t'}^{q''^{\alpha'}, q''^A, t''} \delta[q^{\alpha'}, q^A] \exp(\frac{i}{\hbar}\int_{t'}^{t''} L\, dt)$$

which connects the values of the wave function ψ_1 at two different times. Since ψ_1 is independent of the $q^{\alpha'}$ for physical states, one may write

$$\psi_2(q''^A,t'') = \int \langle q''^A,t''|q'^A,t'\rangle \underline{g}'^{\,1/2} \prod_A dq'^A \psi_2(q'^A,t')$$
$$= w''^{\,1/4}\psi_1(q''^A,t'')$$
$$= w''^{\,1/4}\int \langle q''^A,q''^\alpha,t''|q'^A,a'^{\alpha'},t'\rangle \underline{g}'^{\,1/2}\prod_A dq'^A w'^{\,1/2}\prod_{\alpha'} dq'^{\alpha'}\psi_1(q'^A,t')$$

which implies

$$\langle q''^A,t''|q'^A,t'\rangle = \int \prod_{\alpha'} dq'^{\alpha'} w''^{\,1/4}\langle q''^A,q''^{\alpha'},t''|q'^A,q'^{\alpha'},t'\rangle w'^{\,1/4}.$$

The expression on the right must be independent of the $q''^{\alpha'}$. Hence one may integrate over these variables provided one divides the result by an infinite normalization factor. Moreover, in the path summation expression for $\langle q''^A,q''^{\alpha'},t''|q'^A,q'^{\alpha'},t'\rangle$ the choice of time-like behavior for the variables q^α is immaterial. Hence one may write

$$\langle q''^A,t''|q'^A,t'\rangle = N^{-1}\int_{q'^A,t'}^{q''^A,t''} \delta[q^i]\exp(\frac{i}{\hbar}\int_{t'}^{t''} L\,dt)$$

where N is a suitable infinite normalization factor, the functional integration being now carried out over *all* paths for which $q^A = q'^A$, q''^A at $t=t'$, t'' respectively (all possible end-point values for the variables q^α , $q^{\alpha'}$ being included in the summation) each path being given a weight according to the measure defined by metric density $g^{1/2}$ multiplied by the Jacobian of the transformation $q^i \to q^\alpha$, $q^{\alpha'}$, q^A. In calculation of actual physical quantities one never needs to mention the normalization factor N explicitly, since what is involved is always a ratio of the form

$$\frac{\langle q''^A,t''|F(t)|q'^A,t'\rangle}{\langle q''^A,t''|q'^A,t'\rangle}$$

where F is some true observable (function of the q^A, p_A alone), and the normalization factors in front of the functional integrals cancel.

DE WITT concluded his remarks with some comments about his simplifying assumption, made at the beginning, that the coefficients $C_{\alpha ij}$, occurring in the expression for δF, vanish. In the case of the Lagrangian for the gravitational field the $C_{\alpha ij}$ do not vanish. This has the result that the secondary constraints χ_α are no longer linear in the momenta, but quadratic. In fact, it had been pointed out earlier in the conference that the quadratic term, in the gravitational case, is just the energy density for the gravitational field. This created a certain amount of perplexity, leading some of the participants to think that perhaps all true observables are simply constants of the motion. However, this problem is not yet settled. DE WITT pointed out that the discovery of the non-physical variables associated with the χ_α will be considerably more difficult in the case $C_{\alpha ij} \neq 0$. The resulting quadratic dependence of the χ_α on the momenta means that the non-physical variables can no longer be found simply by a point transformation. A more complicated type of canonical transformation will be required.

DE WITT suggested that the use of parametrized space-like surfaces might help this situation. But he also pointed out that the equations like $\tau^i_\alpha = \partial q^i / \partial q^\alpha$ become variational differential equations in field equations in field theories, and that there is some uncertainty about the ease with which they can be solved in nonlinear contexts. The search for answers to these questions will provide a large program for the future.

FEYNMAN raised the question of the interest of the true observables. Commenting on Belinfante's remark that true observables are very useful to get meaningful answers, FEYNMAN proposed to formulate the problems not in terms of incident gravitons, photons, etc., but in terms of fermions or other sources of these incident and outgoing fields only, in an action-at-a-distance fashion.

LICHNEROWICZ remarked that the classical treatment may have the following geometrical interpretation:

"(1) It may be simpler to consider directly the configuration space-time of the system, i.e., to treat t as a local coordinate. Let V_{n+1} be the configuration space-time.

"(2) The geometrical frame is then the fiber bundle space of $2n+1$ dimensions - say U_{2n+1} - consisting of the tangents of V_{n+1} at all points.

"(3) The gauge transformations have, for infinitesimal generators, vector fields Z_α of U_{2n+1}. The precise hypothesis for the Lie algebra of the Z_α fields is the following: Z can be decomposed into a direct sum $X+Y$ where Y is a tangent to the fibers. This decomposition is such that Y depends on the points of V_{n+1} only, and X depends linearly on the directions. Y_α defines a field of plane surfaces on which the pseudo-metric g_{ij} vanishes.

"(4) By introducing an extra dimension, one can have a Lagrangian corresponding to a Riemannian pseudo-metric (degenerate quadratic form), and it seems that the explicit calculations in the classical part could then be simplified."

ANDERSON and LAURENT reported on the use and extension to the gravitational case of Feynman's method in its field theoretic form.

In order to investigate the question of what variables to employ in the Feynman integral in the gravitational theory, ANDERSON has looked at the corresponding problem in electrodynamics. "There the situation is somewhat simpler because of the linearity of the theory and consequently it is possible to see in a fairly straightforward fashion what one is to do in this case. Adopting the Feynman prescription *in toto* means that one must look for solutions to the field equations which are then in turn substituted into the Lagrangian for the electromagnetic field and integrate over the time interval.

The chief difficulty in this procedure rests in the fact, of course, that one of the equations of motion for the field is free of second time derivatives and therefore represents a restriction on initial and final values which one may impose on the field conditions. However, if one transforms to a set of field variables which include A_{tr}, A_{lo}, and the scalar field φ, then the equations of constraint do not depend on A_{tr}. In fact, the Lagrangian originally looks like

$$L = \frac{1}{2}(\dot{\vec{A}} + \nabla\varphi)^2 - \frac{1}{2}(\nabla \times \vec{A})^2$$

and the equations of constraint appear as

$$\nabla \cdot (\dot{\vec{A}} + \nabla\varphi) = 0$$

so in terms of A_{tr}, A_{lo}, ψ equations of constraint look like

$$\dot{\vec{A}}_{\mathrm{lo}} + \nabla\varphi = 0.$$

Thus solutions satisfying all field equations yield for L

$$L = \frac{1}{2}\dot{\vec{A}}_{\mathrm{tr}}^{2} - \frac{1}{2}(\nabla \times \vec{A})^2.$$

Thus the construction of the Feynman integral is then seen to be in terms of A_{tr} alone, and the measure is given uniquely by the Lagrangian - in this case simply 1.

If one were to extend this line of reasoning to the gravitational case, it is seen that one must look for a set of variables which have the property that they do not appear in the constraint equations and thus are capable of being given arbitrary initial and final values. The Feynman integral would then be over these variables and the measure would derive from the Lagrangian expressed in terms of these variables. As yet this is only a program of approach. However, one can see that it is possible to eliminate immediately as unphysical variables the $g_{4\mu}$ which corresponds in the electromagnetic case to the scalar potentials. The elimination of the longitudinal part of the gravitational field is of course a much more difficult problem and has in no way been solved as yet, although it appears to be the central problem to the quantization."

LAURENT showed how the Feynman action method works gauge-invariantly in quantum electrodynamics and then suggested an extension to gravitation theory whereby the use of "extended gauge-invariance" is forced upon the formalism. (The following is the reproduction of his notes. For a more complete exposition of these and related questions, the reader may consult a recent article of B. E. Laurent.[4]

"In the following I wish to explain why I think that it is worthwhile to try Feynman's action method in covariant theories. This is merely a sketch and does not pretend to be either complete or mathematically rigorous. It has been of great help to me to be able to discuss these problems with Professor O. Klein and Dr. S. Deser.

I shall start with the simplest generally covariant theory known, that is electrodynamics, which is covariant with respect to gauge transformations.

Feynman's well-known action principle[5] has been used by several authors[6] to calculate ratios of time-ordered vacuum matrix elements in non-covariant field theories. These authors have shown that such ratios may be written as follows:

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[4] *Nuovo Cimento* **IV**, 1445 (1956).

[5] R.P. Feynman: *Rev. Mod. Phys.* **20**, 367 (1948).

[6] P. T. Matthews, A. Salam: *Nuov. Cim* **2**, 120 (1955); W. K. Burton, A. H. DeBorde: *Nuov. Cim* **4**, 254 (1956); P. W. Higgs: *Nuov. Cim* **6**, 1263 (1956).

$$\begin{aligned}\langle A(1)\ \dots\ Z(n)\rangle_0 &= \frac{(0,+\infty|TA_0(1)\ \dots\ Z_0(n)|0,-\infty)}{(0,+\infty|0,-\infty)} \\ &= \frac{\int(A(1)\ \dots\ Z(n)e^{iI}\delta A\ \dots\ \delta Z}{\int e^{iI}\delta A\ \dots\ \delta Z}\end{aligned} \tag{21.1}$$

Here I is the action-functional for the fields (or field) and all $A(1)...Z$ stand for the field-functions in the n space-time points $x_1...x_n$. The integrals are functional integrals over the functions in all space-time, performed after a certain small imaginary term has been added to the action (as Burton and DeBorde show, this picks out a vacuum state). An extended class of operators may be constructed as sums of time-ordered products $TA_0(1)...Z_0(n)$. To be general I shall in the following write $F(A,...,Z)$ instead of $A(1)...Z(n)$ in (21.1). $F(\ )$ is here a functional of the fields $A,...,Z$.

In the usual Lorentz-gauge electrodynamics a vacuum propagator is to be written as follows:

$$\langle F(A,\psi,\overline{\psi})\rangle_0 = \frac{\int F(A_\mu,\psi,\overline{\psi})e^{iI_L}\delta A_\mu\delta\psi,\delta\overline{\psi}}{\int e^{iI}\delta A_\mu\delta\psi\delta\overline{\psi}} \tag{21.2}$$

A special device must here be used to obtain the wanted anticommuting properties of ψ, $\overline{\psi}$[7]

$$I_L = \int d^4x(\overset{0}{g}{}^{\mu\nu} A_\mu\ A_\nu + i\,\overline{\psi}\,\overset{0}{\gamma}{}^{\mu}\,\{\partial_\mu - eA_\mu\}\psi) \tag{21.3}$$

where $\overset{0}{g}{}^{\mu\nu}$ is the Lorentz-metric and $\overset{0}{\gamma}{}^{\mu}$ the Dirac-matrices.

(21.2) is exactly the ordinary vacuum propagator of electrodynamics if we restrict $F(\ )$ so that we have nothing but electrons and transversal photons in the initial and final states.[8] $F(\ )$ may for instance have the following form:

$$F(A_\mu) = N(|k|)\int_{t=t_2} A_\mu\tau_2^\mu e^{-ik_{2\alpha}x^\alpha}d^3x\cdot\int_{t=t_1} A_\mu\tau_1^\mu e^{+ik_{1\alpha}x^\alpha}d^3x \tag{21.4}$$

Here $N(|k|)$ is a normalizing factor
$k_4 = |k|$, $\tau^4 = 0$ and $\tau_m^k = 0$ $(m = 1...3)$.

[7]Feynman, *op. cit.*, and Burton and DeBorde, *op. cit.*.
[8]Switching off of the electron charge e is here assumed.

The operator corresponding to (21.4) gives merely transversal photons in the initial and end states of the propagator.

Observe that in a gauge-transformation

$$A_\mu = A'_\mu + \frac{\partial \lambda}{\partial x^\mu}. \tag{21.5}$$

$F(\ )$ in (21.4) is invariant. We shall assume throughout that our F's have this property.

Let us now re-form (21.2)

$$\begin{aligned}\langle F \rangle_0 &= \frac{\int F(A_\mu, \psi, \overline{\psi}) e^{iI_L} \delta A_\mu \delta \psi \delta \overline{\psi}}{\int e^{iI_L} \delta A_\mu \delta \psi \delta \overline{\psi}} \\ &= \frac{\sum\limits_\nu \int\limits_\nu F(A_\mu, \psi, \overline{\psi}) e^{iI_L} \delta A_\mu \delta \psi \delta \overline{\psi}}{\sum\limits_\nu \int\limits_\nu e^{iI_L} \delta A_\mu \delta \psi \delta \overline{\psi}}\end{aligned} \tag{21.6}$$

We have split up the integrals over all possible A_μ-functions in sums of integrals over part-regions, of which the first is around the A_μ-functions satisfying the Lorentz-condition. The other regions are obtained from the Lorentz-region by gauge-transformations. The regions cover completely the region of all A_μ-functions and they do not overlap. The part-regions can be made as narrow as we like.

Remembering that $F(\ )$ is invariant and that the gauge-transformation is linear we see that the following is true:

$$\begin{aligned}&\int_\nu F(A_\mu, \psi, \overline{\psi}) e^{iI} \delta A_\mu \delta \psi \delta \overline{\psi} \\ &\qquad = \det\left(\frac{\nu}{L}\right) \int_L F(A_\mu, \psi, \overline{\psi}) e^{iI} \delta A_\mu \delta \psi \delta \overline{\psi}\end{aligned} \tag{21.7}$$

I is the complete gauge-invariant action

$$I = I_L + \int d^4x \overset{0}{g}{}^{\mu\nu} \overset{0}{g}{}^{\alpha\beta} \partial_\nu A_\mu \partial_\beta A_\alpha \tag{21.8}$$

$\det\left(\frac{\nu}{L}\right)$ is the Jacobian of the transformation from the ν-th region to the Lorentz-region. In this case $\det\left(\frac{\nu}{L}\right)$ is 1 but we shall keep it in in spite of that to be able to use the result later in gravitation theory.

We may now write

$$\langle F\rangle_0 = \frac{\int_L F(A_\mu,\psi,\overline{\psi})e^{iI}\delta A_\mu \delta\psi\delta\overline{\psi}\sum_\nu e^{i(I_L-I)_\nu}\det\begin{pmatrix}\nu\\ \overline{L}\end{pmatrix}}{\int_L e^{iI}\delta A_\mu \delta\psi\delta\overline{\psi}\sum_\nu e^{i(I_L-I)_\nu}\det\begin{pmatrix}\nu\\ \overline{L}\end{pmatrix}} \tag{21.9}$$

$I_L - I = -\int d^4x \overset{0}{g}{}^{\mu\nu} \overset{0}{g}{}^{\alpha\beta} \partial_\nu A_\mu \partial_\beta A_\alpha$ depends on the region only (when the region is narrow). In the Lorentz-region, $I_L - I = 0$. Hence

$$\langle F\rangle_0 = \frac{\int_L F(A_\mu,\psi,\overline{\psi})e^{iI}\delta A_\mu \delta\psi\delta\overline{\psi}}{\int_L e^{iI}\delta A_\mu \delta\psi\delta\overline{\psi}} \tag{21.10}$$

Further we see that

$$\begin{aligned}\langle F\rangle_0 &= \frac{\int_L F(A_\mu,\psi,\overline{\psi})e^{iI}\delta A_\mu \delta\psi\delta\overline{\psi}\sum_\nu \det\begin{pmatrix}\nu\\ \overline{L}\end{pmatrix}}{(\int_L e^{iI}\delta A_\mu \delta\psi\delta\overline{\psi})\cdot\det\begin{pmatrix}\nu\\ \overline{L}\end{pmatrix}} \\ &= \frac{\sum_\nu \int_\nu F(A_\mu,\psi,\overline{\psi})e^{iI}\delta A_\mu \delta\psi\delta\overline{\psi}}{\sum_\nu \int_\nu e^{iI}\delta A_\mu \delta\psi\delta\overline{\psi}}\end{aligned} \tag{21.11}$$

$$\langle F\rangle_0 = \frac{\int F(A_\mu,\psi,\overline{\psi})e^{iI}\delta A_\mu \delta\psi\delta\overline{\psi}}{\int e^{iI}\delta A_\mu \delta\psi\delta\overline{\psi}} \tag{21.12}$$

From this form it is evident that the propagator is invariant.

We see from this that under the supposition made about $F(\ )$, that it must not change its form when A_μ is replaced by $A_\mu + \frac{\partial\lambda}{\partial x^\mu}$, the two demands are fulfilled that quantization shall, as in (21.10), be performed over the "real degrees of freedom" only and that the procedure shall be covariant as in (21.12).

According to the foregoing it is close at hand to guess that a propagator in a theory that is covariant with respect to linear transformation shall be written in the form (21.12), where $F(\ )$ and I must be invariant in the sense mentioned above[9,10] (in the general case we call it extended gauge-invariance). If in gravitation theory, for instance, $F(\ )$ is a functional of the $g_{\mu\nu}$ only it must not change its form in the linear transformation $g_{\mu\nu}(x) = \frac{\partial x'^{\alpha}}{\partial x^{\mu}} \frac{\partial x'^{\beta}}{\partial x^{\nu}} g'_{\alpha\beta}(x'(x))$,where $x' = x'(x)$ is an arbitrary (non-singular) transformation. We observe that a scalar does not necessarily fulfill the requirement, because, in general, it changes its functional form of x in the transformation. If $F(\ )$ is a function as here described and the integration variables transform linearly, we see from the procedure followed in the electromagnetic case that we may always come from the covariant propagator of type (21.12) to the propagator of type (21.10) , where only so much of the field is quantized as there are "degrees of freedom."[11]

A difficulty in the gravitation case is that we do not know over which variables we ought to integrate, and which measure should be chosen in the functional integral.[12]

The choice of integration variables that seem most natural at first sight is perhaps that of the $g_{\mu\nu}$ or $\sqrt{g}g_{\mu\nu}$ ($g = -\det g_{\mu\nu}$), but as Professor J. A. Wheeler told me in a letter, a better choice is probably one which Dr. C. Misner has suggested, that is the coefficients in the transformation from the consideredmetric in a certain point to the Lorentz metric. However, the question cannot be regarded as solved yet.

(MISNER pointed out that this choice of variables gives an invariant measure which preserves the signature.)

Finally I should like to say a few words about how functionals of the type mentioned earlier may be constructed in gravidynamics.[13] Integrals of scalar densities over all space-time is of this type but they do not seem to be of much use in this connection. Professor O. Klein has pointed out to me that the total energy-momentum pseudo-vector may be useful

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[9]This does not mean that we have no use for other types of F in intermediate calculations. In electrodynamics with truncated Lorentz-gauge action we may, for instance, calculate (21.2) with $F(\ ) = A(1)A(2)$ and that is very useful.

[10]I is not necessarily the classical action-functional for the system (also when such a functional exists), but it should go over into the classical function when $\hbar \to 0$.

[11]In fact, a little more than the "degrees of freedom" are quantized in (21.10) because the gauge is not uniquely determined by the Lorentz-condition. This does not matter for the practical use of (21.2) and in principle we *could* of course have restricted the gauge more closely.

[12]If the chosen "volume element" in the functional integral is a certain factor times the product of differentials (the δA_{μ} in (21.12)), the factor ought to be invariant and we may include it in the action (see footnote 10).

[13]In electrodynamics this question is easily answered: Every gauge-invariant functional is of the wanted type. If the gravitational field constant is switched on and off, the matter can to some extent be made equally simple in gravidynamics.

here, and in fact we see that it is of the correct type if we choose it in the following form:

$$\int \sqrt{g}(G^{\mu 4} + t^{\mu 4})d^3x. \tag{21.13}$$

(I do not think that this formula needs any explanation.)

$\{\sqrt{g}(G^{\mu\nu} + t^{\mu\nu})\}_{,\nu} = 0$ is *identically* satisfied which gives (21.13) the desired property for $g_{\mu\nu}$-transformations which are identity-transformations on the boundaries of the integration-volume. Now all the propagators in the foregoing are limits when the space-time region involved becomes infinitely large[14] and nothing hinders us from prescribing, for instance, the Lorentz-metric outside the region and on its boundaries. This means that every interesting transformation is of the type mentioned. In this case the propagator is dependent on the boundary conditions chosen in infinity."

During the discussion which sandwiched and followed LAURENT's exposition, FEYNMAN asked which action was used in the gravitational case. To LAURENT's answer - $\int \sqrt{-g}Rd^4x$ - FEYNMAN raised the difficulties brought in by the presence in R, of second derivatives.

BELINFANTE suggested that Laurent uses, in Feynman's sum over fields, the action from the muddified theory, together with the determinant derived from the muddified theory. However, it was pointed out that Laurent's proof of the equivalence of a "true" and a "muddified" treatment in the electromagnetic case may not hold in the gravitational case, because the determinant depends on the variables of integration.

FEYNMAN asked also what is the determinant in the path integral expressed in terms of the variables proposed by Misner (the variables c_α^β defined by the equation $\overset{0}{g}_{\mu\nu} = c_\mu^\alpha g_{\alpha\beta} c_\nu^\beta$)? More precisely, what is the expression of the determinant which gives a value to the path integral independent of the mesh introduced for its definition? This question remained unanswered but led to the discussion of the definition of Feynman's path integral in a generally covariant theory. Originally, Feynman's path integral was defined in terms of a time slicing as each time interval goes to zero. In the gravitational case, it is preferable not to single out the time, and to redefine Feynman's method accordingly.

WHEELER pointed out the various advantages of the latent variables (such as A_μ in electrodynamics) and of the true (or physical) variables (such as A_{tr}). At present we have three schemes:

[14] Burton and DeBorde, *op. cit.*; Higgs, *op. cit.*.

- "Sum over fields" expressed in terms of latent variables. The infinities cancel out.
- Add a term to the Lagrangian so as to make the problem non-singular.
- Use only physical variables.

The whole art is working back and forth between latent and true variables. It is important to show the equivalence of the various schemes and to show the equivalence of the various "slicings," but it is a task yet to be accomplished.

BERGMANN summarized the session as follows: "It appears that there are two different methods of Feynman quantization - one principally represented by Bryce DeWitt and Jim Anderson, the other by Bertel Laurent and Stan Deser, who is unfortunately not with us this morning. The difference appears to be that one group wants to settle the question of covariant measure in the function space first and then integrate, whereas the other group prefers to integrate first and ask questions afterwards. To me as an innocent bystander it appears that one group knows what they are doing but don't know how to do it, and the other group is able to proceed immediately, but with some question as to the meaning of their results.

One word about why the two approaches appear to lead to mutually consistent results in electrodynamics: in electrodynamics the components of the metric tensor, that is the second derivative of the Lagrangian with respect to the velocities, are constants. It appears natural that in such a situation there should be no disagreement; but I feel very doubtful, without availability of further strong arguments, about anticipating a similar agreement in general relativity, where the same coefficients are complicated functions of the field variables.

I think it is obvious to all of us that the difference between the two approaches again amounts to an estimate of the relative importance of true observables vs. all dynamical variables. Therefore, permit me to give one more argument that indicates the probable importance of the true observables, or dynamical variables as John Wheeler has called them. In general relativity, it is sometimes useful, for reasons of convenience, to introduce parameters and thus to upgrade the ordinary coordinates into dynamical variables. Alternatively, it is sometimes desirable to introduce the so-called vierbeine (quadruples). If you do that without eventually going back to true observables, then you have a proliferation of variables that must eventually be represented by quantum operators, and I think if you try to do this you will go crazy.

I would like to close with a comment on the motivation for Feynman quantization as such. The proponents of Feynman quantization claim as its virtue that once a completely satisfactory classical theory is available, all they have to do is to follow an algorithm, and the task of quantization will be completed. I must admit to having some lingering doubts about the uniqueness of the procedure, in view of the fact that the integrals converge only conditionally, not absolutely; but presumably these doubts can be resolved. A more physical question is this: If you think for a moment about the classical Lagrangian for a free particle, we all know that there is a multiplicity of corresponding quantum theories, not all of which could have been produced by the unique device of Feynman quantization. There remains thus the possibility that we may have eventually to guess at a complete quantum theory rather than carry out the program in two steps, first classical theory then quantization. I do not think that we shall be able to resolve this question in the immediate future; hence I feel that we are completely justified in following both approaches - to work out methods of quantizing a given c-number theory, and examining the intrinsic structure of a full-blown covariant q-number theory."

Session VIII Quantized General Relativity, Concluded

Chairman: V. Bargmann

Chapter 22
The Possibility of Gravitational Quantization

KURSUNOGLU began by asking that those who had presented techniques for quantizing the gravitational field in the morning session make it clear whether or not it has been decided that quantization is possible according to these techniques. He suggested that before one builds a house, he should buy the ground first. WHEELER asked, "Don't we believe that the that the theory should be quantized?" BARGMANN asked if Kursunoglu's question had to do with matters of convergence. He pointed out that it is not easy to decide whether or not one can quantize, because many things may be meant by this question. WHEELER said in a quantum theory of gravitation we would expect new, fascinating physics to come out, because the physical constants that appear in the quantum theory are the gravitation constant G, the velocity of light c, and the Planck's constant $\hbar$, but whether the results of such a theory would agree with experiment is another question. In the classical theory we have no natural mass or length. BONDI suggested that, when one does construct natural units of mass and length from these constants they may turn out to be much smaller than anything present-day physics is interested in. WHEELER replied that in a session of the day before, he had suggested a way in which one could get quantities which have a range of size inside the interesting realm. GOLD said the theory of electrodynamics required quantization because there was a broad range of phenomena that had to be accounted for; for example, the infrared catastrophe had to be somehow explained. With gravitation, one could say that either it must be quantized because otherwise one might get into a contradiction with the logical structure of quantum theory, or else that there exists a broad range of phenomena which the classical theory (without quantization) is unable to explain. But this latter range of phenomena seems to be missing, unless there is gravitational radiation and some kind of "infrared" divergence occurs there. WHEELER said that it might turn out that the elementary particles depend on gravitational fluctuations for their stability. GOLD said that this was only a hope, and WHEELER agreed that a proof was missing. ANDERSON asked if there was anything wrong with the argument that if it were possible to measure all gravitational fields accurately, that one would have enough information to violate the uncertainty principle. WHEELER answered that this was Gold's first point. GOLD said that he wanted still to be convinced that one gets into a contradiction by not quantizing the gravitational field. DE WITT said that that there is a conceptual problem here in that if the matter fields are quantized, one must decide what to use as the source of the gravitational field. If our experiments lead us to believe that the gravitational field is produced by what one may call the expectation value of the stress-energy tensor, there is still a difficulty because the expectation value depends on the measurements we make on the system. After the system has been prepared in a definite state, a measurement can change the expectation value and so the gravitational field suddenly changes because of a measurement performed on the system. Classical gravitation theory works only because the experimental fluctuations are so small on the scale at which gravitational effects become noticeable.

FEYNMAN then proposed the following experiment.

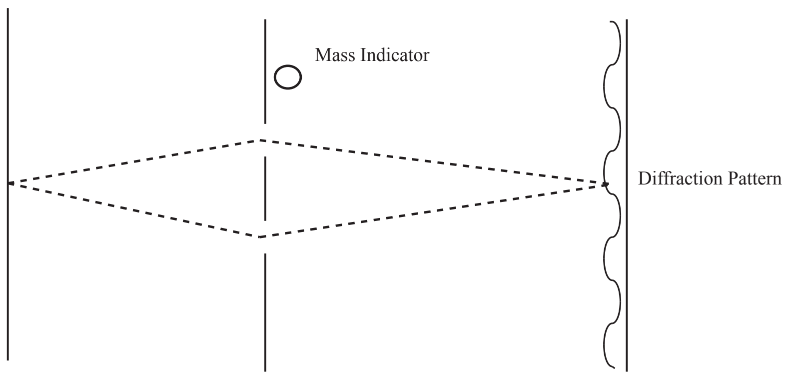

Figure 22.1

If one works in space-time volume of the order of L^3 in space and $\frac{L}{c}$ in time, then the potentials are uncertain by an amount

$$\Delta g = \sqrt{\frac{hG}{c^3L^2}}$$

and $g = \frac{MG}{Lc^2}$ is the potential produced by a mass M in the region L^3. An uncertainty in the potential is then equivalent to an uncertainty in the measurement of M, and one gets $\Delta M \approx 10^{-5}$ grams. This means that if the time of observation is restricted to be less than $\frac{L}{c}$, then the mass can't be determined to better accuracy than this. Of course, if one is allowed an infinite time, M can be determined as accurately as one wants. If, however, this is not possible because the particle must be allowed to pass through the equipment, then unless the mass is at least of the order of 10^{-5}, the apparatus will be unable to discover the difficulty (no contradiction). One can conclude that either gravity must be quantized because a logical difficulty would arise if one did the experiment with a mass of order 10^{-4} grams, or else that quantum mechanics fails with masses as big as 10^{-5} grams.

UTIYAMA then presented a technique for quantizing the gravitational field using Gupta's method. "One of the most attractive points of Einstein's theory of general relativity seems to be that the fundamental tensor $g_{\mu\nu}$ can be interpreted as a potential of the gravity on the one hand, and as a metric tensor of the space-time world on the other hand. In trying to quantize the gravitational field, however, the metric interpretation of $g_{\mu\nu}$ will cause some troubles, especially from the observational viewpoint. Therefore, contrary to Einstein's original intention, the separation of this geometrical concept from the interpretation of $g_{\mu\nu}$ seems convenient for the quantization of the gravitational field. Gupta's [1] approach seems adequate to this line of thought.

We have investigated the cancellation between contributions from the electromagnetic and the gravitational interactions by using Gupta's method of quantization of the weak gravitational field [2].

Under the assumption of the weak field, we can put

$$g_{\mu\nu}(x) = \varepsilon_{\mu\nu} + \varkappa s_{\mu\nu}\ , \ \ h^k_\mu(x) = \delta^k_\mu + \varkappa b^k_\mu(x)$$

$$s_{\mu\nu} = b_{\mu\nu} + b_{\nu\mu}\ , \ \ a_{\mu\nu} = b_{\mu\nu} - b_{\nu\mu}\ ,$$

$$\varepsilon_{\mu\nu} = \begin{bmatrix} 1 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 \\ 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & -1 \end{bmatrix} = \varepsilon^{\mu\nu}$$

where $h^k_\mu (k = 1,2,3,4)$ is the so-called "vierbeine". $s(x)$ or $b(x)$ is, in our approximation, considered as a field quantity in Minkowski-space, and the dual interpretation of $g_{\mu\nu}$ is abandoned. The quantization of s or b is made by following Gupta's method [2].

Let us consider a system of electrons, photons and gravitons interacting with each other. The interaction Lagrangian is

$$L = -\frac{\varkappa}{2}\{T^{(e)}_{\mu\nu} + T^{(p)}_{\mu\nu}\}s^{\mu\nu},$$

where the energy-momentum tensor of the electron-field $T^{(e)}$ includes the interaction between the electron and the photon fields, and $T^{(p)}$ is the energy-momentum tensor of the photon field. In order to avoid the formal difficulties due to the derivative couplings appearing in $T^{(e)}$ and $T^{(p)}$, Schwinger's dynamical principle [3, 4] is suitable to derive the S-matrix in the interaction representation. The S-matrix is written in the form

$$S = \sum_{n=0}^{\infty} \frac{i^n}{n!} \int \cdots \int_{-\infty}^{+\infty} T^*(L(x_1), L(x_2), \ldots L(x_n)) d^4x_1 d^4x_2 \ldots d^4x_n.$$

Following Dyson's analysis of the divergences appearing in any element of S-matrix, we see that there occur so many types of primitive divergences owing to the derivative couplings that this system is unrenormalizable.

In our present treatment the most essential feature of the gravitational field, namely, the non-linearity is not taken into account. Accordingly it is not clear how far our conclusion is reliable."

References

[1] S. N. Gupta: *Phys. Rev.* **96**, 1683 (1954).

[2] S. N. Gupta: *Proc. Phys. Soc. (London)* **65**, 161 (1952).

[3] J. Schwinger: *Phys. Rev.* **82**, 914 (1951).

[4] T. Imamura, S. Sunakawa, R. Utiyama: *Prog. Theor. Phys.* **11**, 291 (1954), especially *cf.* p. 300, Eq. $(3.5)_{\mathrm{L}}$.

Chapter 23
The Necessity of Gravitational Quantization

DE WITT called attention to the fact that there is an ambiguity in the choice of field variables in terms of which one may make an expansion about the Minkowski metric $\eta_{\mu\nu}$. For example, one might use either $\psi_{\mu\nu}$ or $\varphi_{\mu\nu}$ where $g_{\mu\nu} = \eta_{\mu\nu} + \psi_{\mu\nu}$, $\mathfrak{g}_{\mu\nu} = \eta_{\mu\nu} + \varphi_{\mu\nu}$. To be consistent in a self-energy calculation one should expand out to the second order, and the difference in choice leads to a difference in the trace of the non-gravitational (or matter) stress tensor. There are some fields, notably the electromagnetic field, for which this trace vanishes. In this case, you get the same result in the second order, no matter what you expand in terms of. It is a curious thing in the electromagnetic case (although, as Utiyama has pointed out, you do have the derivative coupling and therefore the divergence is of the second or third order), that if you include second order terms in the interaction, the old-fashioned self-energy will be exactly compensated. MISNER asked if the computation has also been done for the neutrino field. DE WITT replied that although it should be done, it has not been done for two reasons: (1) The mathematics of the spinor problem up to the second order is considerably more complicated and has not yet been fully worked out. (2) Interest has in the meantime shifted to the problem of tackling the complete nonlinear gravitational field.

WHEELER pointed out that the linear starting point is incompatible with any topology other than a Euclidean one; that if one has curved space or “wormholes,” you just can’t start expanding this way. BELINFANTE asked if anyone had found it possible to have hole theory in a curved space; i.e. can one make a covariant distinction between positive and negative energy states?

ROSENFELD said that Dirac, just after he had formulated hole theory and was faced with the objection that there is an infinite energy associated with these states, had attempted unsuccessfully to overcome this difficulty by introducing a closed universe.

MISNER said that one could get a quite good qualitative idea of what happens in curved space (the metric being externally impressed) by using the Feynman prescription. Since the action is still quadratic in the interesting field variables, there is no difficulty. In a spherical space there exist states of excitation which do not scatter each other; but as soon as the space has "bumps" in it photons in one state get scattered into other states. Hole theory is possible if the metric is static; however, a time dependent metric causes electrons to go to positive energy states.

BERGMANN pointed out that, on this account, one does not have to exclude hole theory, because if the electrons get excited, there is occasional pair production.

SALECKER introduced a thought experiment, involving a stream of particles falling on a diffraction grating. On account of the de Broglie relation for the waves associated with the stream, $\lambda = \frac{h}{mv}$, one expects that particles of different mass will scatter differently if they fall from a given height. According to general relativity, one expects the same behavior for different masses with the same initial state of motion. Therefore, we arrive at a contradiction with the principle of equivalence.

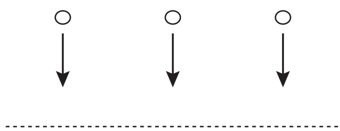

Figure 23.1

FEYNMAN asked if the grating is here allowed to exert forces on the particles which are non-gravitational. DE WITT said that one needs rather a grating (made, for example, out of planets) which acts only through its gravitational field on the stream. FEYNMAN then said that he did not believe that the principle of equivalence denies the possibility of distinguishing between two different masses. Of course, the principle of equivalence would prevent one from distinguishing between masses by means of this particular experiment if only classical laws were operative. However, the introduction of Planck's constant into the scheme of things introduces new possibilities, which are not necessarily in contradiction with the principle of equivalence. As far as this particular experiment is concerned, all that the principle of equivalence would say would be that if one performs the experiment in an elevator, he will obtain the same result as in a corresponding gravitational field. FEYNMAN also emphasized that the quantities G and c by themselves do not lead to a unit of mass, whereas such a unit exists if $\hbar$ is included.

WHEELER pointed out that the principle of equivalence only denies the possibility of distinguishing between the gravitational and inertial masses of a single body, but definitely does *not* prevent one from distinguishing the masses of two different bodies, even when only gravitational forces are involved. For example, we know the relative sizes of the masses of the sun and the various planets solely from observation of their gravitational interactions. BERGMANN added that the principle of equivalence makes a statement about local conditions only. Therefore you can do one of two things: either (1) use a small diffraction grating that is not gravitational, or (2) use a diffraction grating made of planets. In this case, the conditions are certainly not local.

FEYNMAN characterized the point which Salecker had raised as an interesting point and a *true* point, but not necessarily a paradoxical one. If the falling particles are not allowed to react back on the grating, then according to the classical theory they will all follow the same paths. Whereas, in the quantum theory they will give rise to different diffraction patterns depending on their masses.

SALECKER then raised again the question why the gravitational field needs to be quantized at all. In his opinion, charged quantized particles already serve as sources for a Coulomb field which is not quantized. (Editor's Note: Salecker did not make completely clear what he meant by this. If he meant that some forces could be represented by actions-at-a-distance, then, although he was misunderstood, he was right. For the corresponding field can then be eliminated from the theory and hence remain unquantized. He may have meant to imply that one should try to build up a completely action-at-a-distance theory of gravitation, modified by the relativistic necessities of using both advanced and retarded interactions and imbedded in an "absorber theory of radiation" to preserve causality. In this case, gravitation *per se* could remain unquantized. However, these questions were not discussed until later in the session.)

BELINFANTE insisted that the Coulomb field *is* quantized through the ψ-field. He then repeated DeWitt's argument that it is not logical to allow an "expectation value" to serve as the source of the gravitational field. There are two quantities which are involved in the description of any quantized physical system. One of them gives information about the general dynamical behavior of the system, and is represented by a certain operator (or operators). The other gives information about our knowledge of the system; it is the state vector. Only by combining the two can one make predictions. One should remember, however, that the state vector can undergo a sudden change if one makes an experiment on the system. The laws of nature therefore unfold continuously only as long as the observer does not bring extra knowledge of his own into the picture. This dual aspect applies to the stress tensor as well as to everything else. The stress tensor is an operator which satisfies certain differential equations, and therefore changes continuously. It has, however, an expectation value which can execute wild jumps depending on our knowledge of the number and behavior of mass particles in a certain vicinity - if this expectation value were used as the source of the gravitational field then the gravitational field itself - at least the static part of it - would execute similar wild jumps. One can avoid this subjective behavior on the part of the gravitational field only by letting it too become a continuously changing operator, that is, by quantizing it. These conclusions apply at least to the static part of the gravitational field, and it is hard to see how the situation can be much different for the transverse part of the field, which describes gravitational radiation.

FEYNMAN then made a series of comments of which the following is a somewhat condensed but approximately verbatim transcript:

"I'd like to repeat just exactly what Belinfante said with an example - because it seems clear to me that we're in trouble if we believe in quantum mechanics but *don't* quantize gravitational theory. Suppose we have an object with spin which goes through a Stern-Gerlach experiment. Say it has spin 1/2, so it comes to one of two counters.

Connect the counters by means of rods, etc., to an indicator which is either up when the object arrives at counter 1, or down when the object arrives at counter 2. Suppose the indicator is a little ball, 1 cm in diameter."

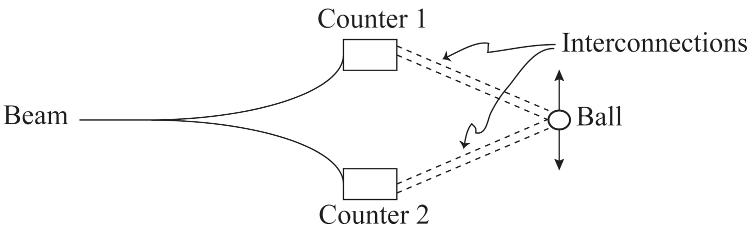

Figure 23.2

"Now, how do we analyze this experiment according to quantum mechanics? We have an amplitude that the ball is up, and an amplitude that the ball is down. That is, we have an amplitude (from a wave function) that the spin of an electron in the first part of the equipment is either up or down. And if we imagine that the ball can be analyzed through the interconnections up to this dimension (≈ 1 cm) by the quantum mechanics, then before we make an observation we still have to give an amplitude that the ball is up and an amplitude that the ball is down. Now, since the ball is big enough to produce a *real* gravitational field (we know there's a field there, since Coulomb measured it with a 1 cm ball) we could use that gravitational field to move another ball, and amplify that, and use the connections to the second ball as the measuring equipment. We would then have to analyze through the channel provided by the gravitational field itself via the quantum mechanical amplitudes."

"Therefore, there must be an amplitude for the gravitational field, *provided* that the amplification necessary to reach a mass which can produce a gravitational field big enough to serve as a link in the chain does not destroy the possibility of keeping quantum mechanics all the way. There is a *bare* possibility (which I shouldn't mention!) that quantum mechanics fails and becomes classical again when the amplification gets far enough, because of some minimum amplification which you can get across such a chain. But aside from that possibility, if you believe in quantum mechanics up to any level then you have to believe in gravitational quantization in order to describe this experiment."

"You will note that I use gravity as part of the link in a system on which I have not yet made an observation. The only way to avoid quantization of gravity is to suppose that if the amplification gets big enough then interference effects can *in principle* no longer play a role beyond a certain point in the chain, and you are not allowed to use quantum mechanics on such a large scale. But I would say that this is the only ‘out’ if you don’t want to quantize gravity.”

BONDI: “What is the difference between this and people playing dice, so that the ball goes one way or the other according to whether they throw a six or not?”

FEYNMAN: “A very *great* difference. Because I don’t really have to measure whether the particle is here or there. I can do something else: I can put an inverse Stern-Gerlach experiment on and bring the beams back together again. And if I do it with great precision, then I arrive at a situation which is not derivable simply from the information that there is a 50 percent probability of being here and a 50 percent probability of being there. In other word , the situation at this stage is *not* 50-50 that the die is up or down, but there is an *amplitude* that it is up and an *amplitude* that it is down - a *complex* amplitude - and as long as it is still possible to put those amplitudes together for interference you have to keep quantum mechanics in the picture.”

“It may turn out, since we’ve never done an experiment at this level, that it’s not possible - that by the time you amplify the thing to a level where the gravitational field can have an influence, it’s already so big that you can’t reverse it - that there is something the matter with our quantum mechanics when we have too much *action* in the system, or too much mass - or something. But that is the only way I can see which would keep you from the necessity of quantizing the gravitational field. It’s a way that I don’t want to propose. But if you’re arguing legally as to how the situation stands...”

WITTEN: “What prevents this from becoming a practical experiment?”

FEYNMAN: “Well, it’s a question of what goes on at the level where the ball flips one way or the other. In the amplifying apparatus there’s already an uncertainty - loss of electrons in the amplifier, noise, etc. - so that by this stage the information is completely determined. Then it’s a *die* argument.”

“You might argue this way: Somewhere in your apparatus this idea of amplitude has been lost. You don’t need it any more, so you *drop* it. The wave packet would be reduced (or something). Even though you don’t know *where* it’s reduced, it’s reduced. And then you can’t do an experiment which distinguishes *interfering* alternatives from just plain odds (like with dice).”

"There's certainly nothing to prevent this experiment from being carried out at the level at which I make the thing go 'clink-clank,' because we do it every day: We sit there and we wait for a count in the chamber - and then we publish, in the *Physical Review*, the information that we've obtained *one pi meson* - And then it's printed (bang!) on the printing presses - stacked and sent down to some back room - and *it moves the gravitational field*!"

"There's no question that if you have allowed that much amplification you have reduced the wave packet. On the other hand it may be that we can think of an experiment - it may be worthwhile, as a matter of fact, to try to design an experiment where you can invert such an enormous amplification."

BERGMANN: "In other words, if it is established that nobody reads the *Physical Review*, then there is a definite 50 percent uncertainty..."

FEYNMAN: "Well, some of the copies get lost. And if some of the copies get lost, we have to deal with probabilities again."

ROSENFELD: "I do not see that you can conclude from your argument that you must quantize the gravitational field. Because in this example at any rate, the quantum distinction here has been produced by other forces than gravitational forces."

FEYNMAN: "Well, suppose I could get the whole thing to work so that there would be some kind of interference pattern. In order to describe it I would want to talk about the interaction between one ball and the other. I could talk about this as a direct interaction like ψ^2/r_{ij}. (This is related to the discussion of whether electrostatics is quantized or not.) However, if you permit me to describe gravity as a field then I must in the analysis introduce the idea that the field has *this* value with a certain amplitude, or *that* value with a certain *amplitude*. This is a typical quantum representation of a field. It can't be represented by a classical quantity. You can't say what the field *is*. You can only say that it has a certain amplitude to be this and a certain amplitude to be that, and the amplitudes may even interfere again... *possibly*. That is, if interference is still possible at such a level."

ROSENFELD: "But what interferes has nothing to do with gravitation."

FEYNMAN: "That's true ... when you finish the whole experiment and analyze the results. *But*, if we analyze the experiment in time by the propagation of an amplitude - saying there is a certain amplitude to be here, and then a certain amplitude that the waves propagate through there, and so on - when we come across *this* link - if you'll permit me to represent it by a gravitational field - I must, at this stage in time, be able to say that the situation is represented now *not* by a particle here, *not* by a result over there, but by a certain amplitude for the field to be this way and a certain amplitude to be that way. And if I have an amplitude for a field, that's what I would define as a quantized field."

BONDI: "There *is* a little difficulty here (getting onto one of my old hobby horses again!) if I rightly understand this, which I'm not sure that I do: The *linkage* must not contain any irreversible elements. Now, if my gravitational link radiates, l've had it!"

FEYNMAN: "Yes, you've had it! Right. So, as you do the experiment you look for such a possibility by noting a decrease of energy of the system. You only take those cases in which the link *doesn't* radiate. The same problem is involved in an electrostatic link, and is not a relevant difficulty."

BONDI: "Oh yes, because in the electrostatic case I can put a conducting sphere around it ..."

FEYNMAN: "It doesn't make any difference if it radiates. If every once in a while the particle which is involved is deflected *irreversibly* in some way, you just remove those cases from your experiment. The occurrence could be observed by some method outside."

BERGMANN: "Presumably the cross section for gravitational radiation is extremely..."

FEYNMAN: "And *furthermore*, we can estimate what the odds are that it will not happen."

BONDI: "I'm just trying to be difficult."

GOLD: "But that need not mean that there is some profound thing wrong with your quantum theory. It can mean merely that when you go into the details of how to make an op..."

FEYNMAN: "There would be a *new principle*! It would be *fundamental*! The principle would be: - *roughly*: *Any piece of equipment able to amplify by such and such a factor* (10^{-5} grams or whatever it is) *necessarily must be of such a nature that it is irreversible.* It might be true! But at least it would be fundamental because it would be a new principle. There are two possibilities. Either this principle - this missing principle - is right, *or* you can amplify to any level and still maintain interference, in which case it's absolutely imperative that the gravitational field be quantized ... *I believe*! *or* there's another possibility which I haven't thought of."

BUCKINGHAM: "The second possibility lands you back in the same difficulty again. If you *could* amplify to any factor, you could reduce to a negligible proportion an additional signal to take an observation on, say, those balls."

FEYNMAN: "*No!*"

BUCKINGHAM: "Because you only need one light quantum."

FEYNMAN: "*No!*"

BUCKINGHAM: "If you could amplify up to any factor this becomes negligible."

FEYNMAN: "It depends! ... You see (pointing to a blank space on the blackboard) this statement that I have written here is not written very precisely as a matter of fact if you look at it you probably can't even see the words. I haven't thought out how to say it properly. It isn't simply a matter of amplifying to any factor. It's too crude - I'm trying to feel my way. We *know* that in any piece of apparatus that has ever been built it would be a phenomenally difficult thing to arrange the experiment so as to be reversible. But is it *impossible*? There's nothing in quantum mechanics which says that you can't get interference with a mass of 10^{-5} gram - or *one* gram."

BUCKINGHAM: "Oh, yes. What I'm saying, though, is that the laws have to be such that the effect of one light quantum is sufficient to determine which side the ball is on, and would be enough to disturb the whole experiment."

FEYNMAN: "Certainly! That's always true. That's just as true no matter what the mass is."

ANDERSON: "Suppose a neutral elementary particle really has a gravitational field associated with it which you could actually use in the causal link. The thing that bothers you is that you may be getting something that is too small to produce a gravitational field."

FEYNMAN: "It's a question of design. I made an assumption in this analysis that if I make the mass too small the fields are so weak I can't get the experiment to operate. That might be wrong too. It may be that if you analyze it close enough, you'll see that I can make it go through a gravitational link without all that amplification - in which case there's no question. At the moment all I can say is that we'd better quantize the gravitational field, or else find a new principle."

SALECKER: "If you assume that gravitation arises as a sort of statistical phenomenon over a large number of elementary particles, then you also cannot perform this experiment."

FEYNMAN: "Yes, it depends what the origin is. One should think about designing an experiment which uses a gravitational link and at the same time shows quantum interference - what dimensions are involved, etc. Or if you suppose that every experiment of this kind is *impossible* to do, you must try to *state* what the general principle is, by trying a few examples. But you have to state it *right*, and that will take some thinking."

DE WITT then remarked that there is still another type of experiment which might some day help to decide the question of whether or not the gravitational field is quantized - namely, producing (or finding in cosmic rays) particles having energies of 10^{19} Bev and observing their interactions. At the level of such structures as "wormholes" these are the energies in which one is interested.

ROSENFELD then gave an amusing historical survey in which he presented some of the ideas which Faraday and Maxwell had on gravitation. Faraday attempted to measure the ability of a moving gravitational field to induce electric current by moving a coil of wire or a heavy mass up and down. Although he detected no effect, he was convinced that such an effect must nevertheless exist. It struck Faraday that an important difference between the gravitational and electric fields was that, apparently, gravitational energy was not absorbed by matter. It also puzzled Maxwell that the gravitational lines of force in the vicinity of two interacting masses exhibited a behavior which did not seem to permit the introduction of stresses in the ether to explain the attraction of the masses; the lines of force have the masses for their sources, but do not end on them: they exert a push, and not a pull, on the masses. It seems that they have to end on some far-away mass distribution, which they can so to speak take as support to push the masses nearer to each other and this gives rise to their apparent "attraction."

ROSENFELD then raised the question of the existence of gravitational waves. Formally, one can get a spherical wave which depends on the third time derivative of the quadrupole distribution of masses. This solution has been obtained, in the linear program, by expanding around the Minkowski metric. However, Rosenfeld does not know if this wave has a definite physical meaning because the energy transported in this way is not described by a tensor but by an expression which has meaning only with reference to the space chosen for background. This uncertainty about the existence of waves has long prevented people like Gupta, for instance, from quantizing them. “It seems to me that the question of the existence and absorption of waves is crucial for the question whether there is any meaning in quantizing gravitation. In electrodynamics the whole idea of quantization comes from the radiation field, and the only thing we know for sure how to quantize is the pure radiation field.

“Now the arguments that Feynman has just raised about the necessity of quantizing gravitation, if one has to avoid contradiction with the uncertainty principle, did not convince me completely - though I must confess I cannot refute them. In his famous device of the two holes, the idea is that the motion of the mass will show through which hole the electron has passed. I do not know if it is as easy as that, because if you put the condition that the exchange of momentum between the electron and this mass does not disturb the interference pattern, it means that the deviation of the electron due to the momentum it has received must lead to a deviation which is less than the distance between the fringes. This distance between the fringes is $\delta x = \frac{\lambda l}{2a}$. Now if you give an extra momentum δp to the electron,this would produce a displacement $\delta' x = \frac{\delta p}{p} l$ and this must be smaller than $\frac{\lambda l}{2a}$. This means that $\delta p < \frac{\lambda p}{2a} = \frac{\hbar}{2a}$. But this means that the uncertainty in positionof the mass must be larger than $2a$ and we don't know whether the mass is at this hole or that hole.”

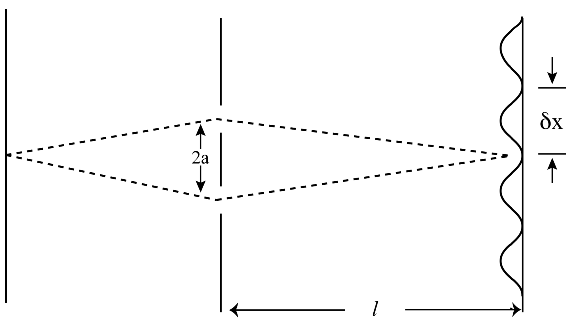

Figure 23.3

FEYNMAN: "Uh, yes ... I know. I might have to use gravitons to scatter the particle, and then make some kind of assumption that I can measure the gravity wave, no matter how weak it is. Now if I consider only gravitostatics, I still have a problem. I still have a quantum theory of gravity. Although it is said that there is no quantum theory of electrostatics, there *is* really, I think. The writing of $\frac{e^2}{r_{ij}}$ in the Schrödinger equation removes electro-statics from a field theory and makes it into the quantization of a rather simple field. I still think, in a certain sense, that when you represent it as a field, and not as a solution which is the result of a field, that we still have to quantize it in order to get it to work."

GOLD: "It could be that inaccuracies are always introduced because no experiment can be, finally, gravitational only. It is possible that the existing quantum theory will already always make sure that nothing can be measured without sufficient accuracy."

FEYNMAN: "If I write down a term $G\frac{mm'}{r_{ij}}$ in the equation for the universe and if I have only particles in the system with no question of field variable, is this a quantum theory of gravitation or is it a classical theory? That's a question of definition. In other words, when this term is included has the gravitation field been quantized?"

ROSENFELD: "The classical theory of the gravitation field would give you this term."

FEYNMAN: "No, it would not, because it cannot produce a Schrödinger equation; it gives you a force between two points, and then you interpret that energy as a Hamiltonian for the particles."

ROSENFELD: "That is only if you impose upon me the obligation to start from a Hamiltonian."

FEYNMAN: "Yes - So if we consider Newtonian gravitation alone we would not have to argue whether to quantize or not (whatever you call this process of including a term in the Hamiltonian); *but* if we consider the dynamic aspect and if we do not consider this process as quantization, then my experiment does not prove the necessity for quantization."

BONDI: "Does this mean, then, if there were no gravitational waves, you would not feel that this experiment would prove the need for quantization?"

FEYNMAN: "It depends on your definition, as I tried to explain, of quantization. If I can use my other experiment - which is just exactly the same, but is a little clearer in a certain respect, because the mass moves back and forth - the question is: How do we analyze the situation? Now listen, we can analyze as we go along and cut the thing in the middle if we want to, and say that this produces a field and the field acts on the other one. That's one way of representing it. If we do it that way, then we have to have an amplitude for the field being here and an amplitude for the field being there. The gravitational field has to be quantized. Incidentally, this uses only gravito-statics. But there's another way of representing the same thing, and that is there is an action at a distance between the two particles: then we do not have to analyze it in the intermediate range as a function of time. I am sorry I did stop at these subtleties when I made up this example."

BONDI: "That is very much clearer now. It does seem to me that this vexed question of the existence of gravitation waves does become more important for this reason."

FEYNMAN: "Yes. There's a delay in this equipment. If there's real delay in this equipment, the information is stored in the field and can't be extracted by looking at the particles of the equipment; and if a quantum theory of the gravitational field is not necessary, there is another possible theory: the action at a distance theory of gravity. That is another way out. It may not be necessary to have a gravitational field at all."

ROSENFELD: "Well, the last point about which I worry very much is this: It is difficult for me to imagine a quantized metric unless, of course, this quantization of the metric is related to the deep-seated limitations of the definitions of space and time in very small domains corresponding to internal structures of particles. That is one prospect we may consider. The whole trouble, of course, which raises all these doubts, is that we have too few experiments to decide things one way or the other."

BELINFANTE then raised the question of the validity of Gupta's successive approximations. These approximations (though possibly valid in a different scheme) cannot be made using Papapetrou's formula in the way proposed by Gupta, for the following reason: This method mixes the various orders of approximation and thus leads to nonsense if fixed up (by adding adjustable stresses after each step) in such a way as to avoid the other contradictions to which the method leads when used uncorrected - the reason being that intermediate approximations lead to sources for the next approximation which do not satisfy conservation laws and thus contradict the differential equations of Papapetrou. (The trouble may be due to Papapetrou's use of a gravitational energy density depending on second derivatives of the field.)

The question of the absorption and production of gravitational waves was raised again. FEYNMAN discussed a device which would absorb gravitational energy, provided one assumes the existence of gravitational radiation (but as he pointed out, "My instincts are that if you can feel it, you can make it."). For this purpose one can use a result already presented at an earlier session that the displacement η of a particle in the path of a gravitational wave satisfies the differential equation

$$\frac{d^2\eta}{dt^2} = R^a_{0ba}\eta.$$

A particle situated initially near a long light rod, oriented parallel to the propagation direction, could be made to scrape against the rod by the transverse-transverse wave amplitudes.

FEYNMAN then presented the result of a calculation of the energy radiated by a double star system in a circular orbit:

$$\frac{\text{Energy radiated in one revolution}}{\text{Kinetic energy content}} = \frac{16\pi}{15}\frac{\sqrt{mM}}{m+M}\left(\frac{u}{c}\right)^5,$$

where the masses of the stars are m and M, and u is the magnitude of their relative velocity. The effect is extremely small, the "lifetime" of the earth going around the sun being of the order 10^{26} years.

WHEELER said that, from the point of view of expanding the field about the Minkowski metric, the behavior of the Schwarzschild solution might be described by saying that the field away from the singularity itself acts partly as a source of gravitation. In this connection, one might conceive of a "gravitational" geon - an object in which the gravitational waves traveling around in circles provide the energy to hold the system together.

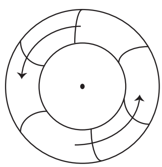

Figure 23.4

Closing Session

Chairman: B. S. DeWitt

Chapter 24
Divergences in Quantized General Relativity

S. Deser

The possible physical effects of general relativity on the elementary problem have usually been considered as negligible in view of the fact that energies at which the former might have a bearing are much too high. It is added that the effects of the new particles and the energies at which current theory loses its validity occur very much below this range and therefore a correct future theory will have solved the present difficulties in a much lower domain. In any case, field theory will have been altered so radically that arguments from the present one will lose their validity. Further, it is sometimes felt that general relativity is a purely macroscopic theory which loses its meaning in microscopic domains, where the concept of metric is not very transparent.

To these "classic" objections there exist several levels of replies. On a general level, it may be pointed out that the effect of a theory is not always first felt through its gross direct dynamical contributions (as, for example, spin). Since general relativity is needed, at least in a formal way, to provide a correct definition of the energy-momentum tensor, it underlies any theories which deal with the energies of fields and particles. The principle of general covariance on which the general theory is based is not in any way restricted *a priori* to macroscopic considerations and it is thus necessary to explore its consequences for any theory. The fact that the metric may not be so simply measurable in microscopic domains (say "within" an elementary particle) is no more an argument against the relevance of relativity than the definition of position measurement in a hydrogen atom is an argument against the use of coordinates in quantum mechanics. Finally, that a future correct theory will exclude the relevance of relativity is not an argument but a wish.

To these arguments of general principle, some very considerable quantitative ones can be added. The whole scheme of local field theory is plagued with divergences occurring because there is no upper limit to the energies involved in it. No satisfactory cutoff method is known which preserves the basic requirements of physical meaningfulness and there is no strong doubt that any can be found within a Lorentz-covariant framework. At this point general relativity may be invoked. Classical considerations show that for any kind of "matter" coupled to the metric field in the Einstein way, there are limitations to the energy densities and masses which can be concentrated or built up in a given region. Similarly, lower bounds exist on the sizes of wave-packets built up from linear wave equations.

$$M < \frac{c^2 d}{4\gamma} \quad , \quad d = \text{dimensions of region}$$

For a high frequency wave packet one has roughly

$$M = \frac{h}{cd} \quad , \quad d = \text{dimensions of wave packet}$$

Therefore, the dimensions of the wave packet must be greater than the fundamental length ($l_0 = 10^{-32}$ cm). In all these investigations it appears that space-time loses its physically meaningful character beyond such limits. It is true that these limits always involve l_0 and are thus quite small, but they do occur at finite energies and their effects may well be felt sooner.

A still more significant point is the following. It is known that under very wide assumptions any theory of coupled fields leads, near the light cone, to singularities in the propagators of the "clothed" particles and to the existence of at least some infinite renormalization constants, independent of perturbation theory. All the known Lorentz-invariant field couplings are of this type, in particular electrodynamics and the renormalizable meson theories. It is just here that the coupling of the gravitational field differs so profoundly from the usual couplings between matter fields. First of all a basic condition for the proof of these theorems in flat space-time is that there exists an energy-momentum vector P_μ, and that the usual commutation relations hold for any operator.

$$iO(x)_{,\mu} \approx [O(x), P_\mu]$$

This is not the case in the general theory, and it is a very basic point there. In fact, a P_μ can, as is well known, only be defined so as to be independent of any inner coordinates and therefore it cannot tell one the change of a quantity that is located somewhere in that region in question. It is connected with the non-linearity of the system and the lack of translational properties of parts of the system by themselves. One cannot simply fix the position of an arbitrary component of the total system, for this would clash with the energy density bounds mentioned above. Furthermore, matter and gravitational energies are not invariantly separable.

A second related departure from usual couplings is the form of the interaction terms. Contrary to normal field theory, there simply is no "free-particle" part of the matter Lagrangian. Here, the coupling enters in the kinetic energy, in a multiplicative fashion, and one cannot really disentangle a non-interacting particle here. This is rightly so, since the particle's mass and energy are defined precisely by the coupling. The equal time matter commutation relationships also would depend upon the positions in space-time involved. This fundamentally different mode of coupling will occur most critically at high energies and forbid any approximation from "free-particles." In particular, each matter field by itself will no longer be represented by a linear wave equation in this domain. Free motion varies with the geometry of the space involved, which is in turn conditioned by the matter present which is doing the moving. Later, it will be seen that the arguments obtained in this investigation for convergence and non-singularity of propagators are based in good part on just this type of coupling, it may well be that a more adequate future theory of particles will incorporate some of these aspects of the non-linear gravitational coupling. Certainly, if it is a field theory, it must escape the pessimistic conclusions for the current theories by some such channel. If so, it may be all the more instructive to see how an improvement seems to emerge in this case.

Before one proceeds to further treat the extended theory, he must decide whether it is necessary to quantize the gravitational field. This will not be discussed here; and the Feynman method will be used, in which the question of which components of the gravitational field are to be quantized seems to be solved by itself. In any case, one need write down whatever physical quantities are of interest as functional integrals, which in principle go over only independent classical configurations of the system and yield the quantum result.

This method will be used to determine the behavior of the matter fields as modified by gravitational interactions. It may be expected, roughly, that the contributions of the "scalar" and "transverse" parts of the gravitational field, though not really separable will limit the density of matter in a given region for the former, while for the latter will "smear out the light cone." This phrase refers to the expectation that the summation over all possible Riemann spaces (corresponding to the various transverse modes) will average away the uniqueness of the Minkowski light cone, and with it the singularities of the matter propagators of such a cone.

The effects of gravitation on propagators of various matter fields will now be considered. Rigorously one should treat, for example, the Feynman function for an electron interacting with the electromagnetic and metric fields. However as is known, in ordinary electrodynamics in flat space the behavior of this propagator cannot be less singular on the light cone than the free one, S_F .

$$S_F' \geq S_F \approx \frac{1}{\gamma p + m}$$

S_F is already singular enough to give the infinite renormalization constants of electrodynamics. Therefore, if one can show that S_F'', the propagator of the electron modified by gravitation alone, behaves better than S_F, this is a step in the right direction and is an indication that the three-field problem will stay good. This is due to the fact that the three-field interaction energy, which has been omitted, presumably would not make things worse. If one gets promising results here then the whole problem should be investigated. Alternately, the use of electrons and photons "clothed" by gravitation as the "non-interacting" elements of quantum electrodynamics seems to be a very good approximation to the total problem. If one calls the S'' functions the matter field functions corrected only for the gravitational field, and if S' reduces to S'' this is all right. It may also be expected that many-particle propagators will behave, if anything, better than the one-particle ones since there will be a bias against too close approach. Gravitational field propagators themselves should also behave non-singularly, i.e., the Schwarzschild type of singularities should vanish (the matter now being "regularly" spread) while the "transverse" propagation should also be free of singularity, just as the photon Green's function is expected to be. Also, it seems that the question of renormalization of gravitational coupling should never appear since the mass, m, plays the double role of "charge" and mass, and the two renormalizations, if any, must be equal. Now, since all "charges" are positive, we expect no vacuum polarization, $Z_3 = 1$, and δm should also be zero in view of the absolute definition of mass and energy levels in the gravitational theory, and of the fact that the mass is defined automatically from the gravitational coupling.

From the above arguments it would appear that the simplest case of a scalar field coupled to the metric is a sufficient example to consider. In the usual field theory the vacuum expectation value

$$S' = \frac{\langle 0|\psi(x)\psi(x')|_T\, 0\rangle}{\langle 0|0\rangle} = \frac{\int \psi(x)\psi(x')e^{iS(\psi,\Phi)}\delta\Phi\delta(\psi,\overline{\psi})}{\int e^{iS}\delta\Phi\delta(\psi,\overline{\psi})} \;,$$

where S is the total action and the integration is over the meson, electron, and positron fields. The action,

$$S = \int \overline{\psi}(\gamma p + m)\psi + \int \Phi[p^2 - \mu^2]\Phi + \int \overline{\psi}\psi\Phi \ .$$

Now, one can eliminate the electron field and the vacuum expectation value becomes equal to

$$\frac{\int e^{i(S_{\text{meson}}+S_0)} G(x,x';\Phi)\delta\Phi}{N}$$

where S_{meson} is due to the pure meson field and S_0 is due to closed loops. The Green's function $G(x,x';\Phi)$ satisfies $(p^2 + m + \Phi)G = \delta(x,x')$, if we replace the spinors by p^2 to simplify the calculation. Now one knows how G behaves near the light cone.

$$G \approx \frac{1}{(x-x')^2} F(x,x';\Phi),$$

where F is a well behaved function (aside from possible "harmless" logarithmic terms). Thus one finds, near the light cone, that S' varies essentially as $\frac{1}{(x-x')^2}$. This, propagation function, S', is singular enough to give one divergences.

Now consider the parallel problem when a matter field is interacting with the gravitational field. The Lagrangian is

$$L = L_{\text{grav}} + \frac{1}{2}\sqrt{-g}\{\Phi\Box^2\Phi - \mu^2\Phi^2\} \ ,$$

where the D'Alembertian depends upon the metric. The mass term will, as usual, not be relevant to the singularity questions. One wants to compute

$$\Delta''(x,x') = \frac{\langle\, 0 \mid \Phi(x)\ \Phi(x') \mid_T 0\, \rangle}{\langle\, 0 \mid 0\, \rangle}$$

where x and x' are to be defined with respect to a flat background frame which is used to make a classical measurement. The functional integration used to define Δ'' is

$$\Delta''(x,x') = \frac{\int \Phi(x)\Phi(x')\ e^{iS}\ \delta\Phi\delta(g\ldots)}{\int e^{iS}\ \delta\Phi\delta(g\ldots)}$$

where $\delta(g...)$ means a summation over all Riemann spaces which preserve the signature of $(+++-)$. Now, as before, the matter variables are eliminated giving

$$\Delta''(x,x') = \frac{\int \Delta g(x,x')\, e^{i(S_{\text{Einstein}}+\Delta S)}\, \delta(g\ldots)}{N}$$

where

$$[\Box^2 g - \mu^2]\Delta g(x,x') = \delta(x,x')$$

and ΔS comes from integration over matter variables. One is interested only in the solution for two points which are on each other's light cones. Again, the general theory of differential equations tells one that near the light cone

$$\Delta g(x,x') \approx \frac{1}{s^2(x,x',g)} F(x,x';g)\,,$$

where $s^2(x,x',g)$ is the square of the interval between the two points and F is a regular function. Here the difference between these propagators and the usual ones of field theory is obvious since the $s^2(x,x',g)$ depends upon the g variables. Now,

$$\Delta''(x,x') \approx \int \frac{e^{i(S_{\text{Einstein}}+\Delta S)}}{s^2(x,x',g)} F\delta(g\ldots)\,.$$

At this point the more or less rigorous mathematical treatment stops and things that are said cannot be proved mathematically.

There will be "very few" spaces where null geodesics will connect x and x' in comparison to the number of integrations possible. It is possible to make a vague analogy to the usual theory of poles in a finite number of dimensions and in such cases one expects the singularities to be smoothed out - with any "measure" which makes sense. For large distances, on the other hand, the exponential may be expected to vary wildly and thus cancel the effects of the various spaces summed over. It is felt that one should not make the linear approximation in the action since this will reinstate the privileged character of the Minkowski metric and the whole point of non-linear gravitation is that the propagator depends upon the space one is in.

A corroborative argument from quite a different direction, which was mentioned earlier, is the P_μ commutation relation. In a flat space-time, and effectively (by suitable modification) in a non-flat but externally given geometry this commutation rule holds. P_μ determines the translation of the system as a whole and, because of superposition, of any part of it. In a space-time which interacts with matter, however, this relation no longer holds. P_μ does have as a canonical conjugate the center of mass, X_μ, of the system with respect to the flat external frame. However, it cannot be used to locate closely any component of the system. The existence of a P_μ in Minkowski space-time is essential to the pessimistic theorems mentioned before. It is obvious that if P does not have all the properties required there, the proof that any component field, if sufficiently focused on, acts as if it is uncoupled, loses support. It may be argued that the propagators, Δ'', mentioned above, also lose some of their meaning by the same argument. This is true only to the extent that physical quantities always involve space-time integrals of such functions. The fact that the Δ'' are now non-singular is merely due to the fact that one can never force a field to behave as if it is too concentrated - the induced geometry would force a repulsion.

If it is granted that this action introduces an effective cutoff, at about l_0, then such high energy approximations as Landau's could be examined again. As Landau has noted, using l_0 as a cutoff, theory predicts roughly the experimental charge regardless of the value of the bare charge and similarly for the mass. Of course, our results, being generally covariant, cannot be directly translated into flat spacetime language without apparent paradoxes.

One thing that should be done is to look into the vacuum expectation value of the total energy-momentum of the system since this should be zero in general relativity due to the absolute definition of energies; similarly, $\langle T_{\mu\nu} \rangle$ should be nowhere singular.

In the discussion which followed DESER's presentation, LICHNEROWICZ and FOURES commented on the need for an exact definition of the measure. ANDERSON suggested that the integration difficulties might be circumvented by first solving the classical problem of motion of two matter fields. Then, one can eliminate the non-physical variables from the action. WHEELER first brought up the point that the fluctuation of the metric at very small distances, which brings about change in the topology of the space, may make a flat space-time unusable, even as a first approximation. In reference to the work he previously discussed, the use of a matter propagator as a starting point is foreign to that point of view. Also, the whole nature of quantum mechanics may be different in general relativity since in every other part of quantum theory, the space in which physics is going on is thought of as being divorced from the physics.

General relativity, however, includes the space as an integral part of the physics and it is impossible to get outside of space to observe the physics. Another important thought is that the concept of eigenstates of the total energy is meaningless for a closed universe. However, there exists the proposal that there is one "universal wave function." This function has already been discussed by Everett, and it might be easier to look for this "universal wave function" than to look for all the propagators. FEYNMAN said that the concept of a "universal wave function" has serious conceptual difficulties. This is so since this function must contain amplitudes for all possible worlds depending on all quantum-mechanical possibilities in the past and thus one is forced to believe in the equal reality of an infinity of possible worlds. The following argument was proposed to challenge the conclusions of Deser. If one started to compute the mass correction to, say, an electron, one has two propagators multiplying one another each of which goes as $1/s^2$ and are singular for any value of the g's. Therefore, do the spatial integrations first for a fixed value of the g's and the propagator is singular, giving $\delta m = \infty$. Then the superposition of various values of the g's is still infinite. DESER replied that this is partly due to an unallowed interchange of limits.

A resolution was passed at this conference to the effect that there is to be another gravitational conference in Europe during the summer of 1958.

ERNST reported on a paper on Corinaldesi[1] Here, higher order terms are obtained through a quantum treatment of the linearized theory.

[1] *Proc. Phys. Soc.* **69**, 189 (1956).

Chapter 25
Critical Comments
R. P. Feynman

The real problems in gravitational theory are:

1. To understand classical relativistic gravity without the complications of other things.
2. The general theory of cosmology because we may be able to work out the shape of the world.
3. To study the theory to see if anything new is in it which is not contained at first sight.
4. To learn from the philosophy of gravity something to use in other fields such as we have made use of the invariance principles and the ideas of Einstein.
5. Curiosity

There exists, however, one serious difficulty, and that is the lack of experiments. Furthermore, we are not going to get any experiments, so we have to take a viewpoint of how to deal with problems where no experiments are available. There are two choices. The first choice is that of mathematical rigor. People who work in gravitational theory believe that the equations are more difficult than in any other field, and from my viewpoint this is false. If you then ask me to solve the equations I must say I can't solve them in the other fields either. However, one can do an enormous amount by various approximations which are non-rigorous and unproved mathematically, perhaps for the first few years. Historically, the rigorous analysis of whether what one says is true or not comes many years later after the discovery of what is true. And, the discovery of what is true is helped by experiments. The attempt at mathematical rigorous solutions without guiding experiments is exactly the reason the subject is difficult, not the equations. The second choice of action is to "play games" by intuition and drive on. Take the case of gravitational radiation. Most people think that it is likely that this radiation is emitted. So, suppose it is and calculate various things such as scattering by stars, etc., and continue until you reach an inconsistency. Then, go back and find out what is the difficulty. Make up your mind which way it is and calculate without rigor in an exploratory way. You have nothing to lose: there are no experiments. I think the best viewpoint is to pretend that there are experiments and calculate. In this field since we are not pushed by experiments we must be pulled by imagination.

The questions raised in the last three days have to do with the relation of gravity to the rest of physics. We have gravity - electrodynamics - quantum theory - nuclear physics - strange particles. The problem of physics is to put them all together. The original problem after the discovery of gravity was to put gravity and electrodynamics together since that was essentially all that was known. Therefore, we had the unified field theories. After quantum theory one tries to quantize gravity. As far as the two methods of quantization are concerned, I believe that if one works the other will work. The crucial problem is to be able to tell when we have succeeded in quantizing the gravitational field. We should take the same attitude as in other branches of physics and compute the results of some experiments. We don't have the experiments and thus we do not know which results to calculate. However, if someone brought an experimental fact could we check it? Certainly, we can take a linearized approximation and the answer will be right or wrong. The reason for stressing experiments is because of quantum electrodynamics which was worked out in 1928 but was full of difficulties such as the infinite energy levels of the hydrogen atom. When Lamb discovered the spacing between two levels was finite, Bethe then got to work and calculated the difference and introduced the ideas of renormalization. We knew that the number was finite, but someone measured it and thus forced the computation. The real challenge is not to find an elegant formalism, but to solve a series of problems whose results could be checked. This is a different point of view. Don't be so rigorous or you will not succeed.

Quantum mechanics and gravity do have something in common. The energy in quantum mechanics is best given by describing how the wave function changes if one solves the coordinate system a little bit, and gravity is connected with just such transformations of coordinates. Thus, the group-theoretic definition of energy and momentum in quantum theory is not very far away from the geometric connection between energy and what happens when you move the coordinate system.

The connection of gravity with the other parts of physics (nuclear and strange particles) was not mentioned here. This is interesting and strange because from the point of view of a non-specialist there is just as much physics in these other fields. From the experimental side we have much more detail there but have no beautiful theory.

Instead of trying to explain the rest of physics in terms of gravity I propose to reverse the problem by changing history. Suppose Einstein never existed, and his theory was not available, but the experimenters began to discover the existence of the force. Furthermore, suppose one knows all the other laws known now including special relativity. Then people will say we have something new, a force like a Coulomb force. Where did it come from? There will be two schools of thought. First, some people will say this force is due to a new field and second some people will say that it is due to some effect of an old field which we do not recognize. I have tried to do this forgetting about Einstein.

First I will do the case of the new field. The force is proportional to $\frac{1}{r^2}$ and thus it must be mass zero field. Also, I assume that later one gets experiments about the precession of orbits so that the rate of motion does not appear to be proportional to the mass. Someone would try scalar fields, vector fields, ..., and so on. Sooner or later one would get to a spin two field and would say perhaps it is analogous to electrodynamics. Then he would write down

$$\int\left(\frac{\partial A_\mu}{\partial x_\nu}-\frac{\partial A^\nu}{\partial x^\mu}\right)d^4x+e\int \dot{z}_\mu A_\mu ds+\frac{m}{2}\int \dot{z}_\mu^2 ds+\frac{1}{2}\int \dot{z}_\mu\dot{z}_\nu h_{\mu\nu}ds \\ +\int\left(\text{second power of first derivatives of } h'\text{s}\right)$$

or in place of the second term

$$\int A_\mu j_\mu d^4x \ , \ j_\mu = e\int \dot{z}_\mu \delta^4(x-z)dS,$$

and in place of the fourth term

$$\int T_{\mu\nu}h_{\mu\nu}d^4x \ , \ T_{\mu\nu}=\int \dot{z}_\mu\dot{z}_\nu\delta^4(x-z)dS.$$

In all this $h_{\mu\nu}$ is the new field under investigation.

From this one gets some second order equations roughly of the form

$$\frac{\partial^2}{\partial x \partial x} h = T.$$

Also the equation of motion of the particles is

$$g_{\mu\nu}\ddot{z}^{\lambda} = [\mu\nu, \lambda]\dot{z}_{\mu}\dot{z}_{\nu}$$

where $g_{\mu\nu}$ is just a shorthand for $\varepsilon_{\mu\nu} + h_{\mu\nu}$. The field equations of electrodynamics have a property that the current conservation is an identity from the field equations.

Now one asks what is $T_{\mu\nu}$ equal to such that we get automatically $T_{\mu\nu,\nu} = 0$? Now, when one puts in the second power of the first derivatives of h in the action he gets something definite. Incidentally, one gets the linearized form of the gravity equations. If now these equations are solved to see if any progress has been made, one deduces that light is deflected by the sun. One at this point might say that we know too much, that a field theorist would never have thought of this important conservation theorem. This is not true. Pauli deduced this equation without looking for gravity, but by asking himself what must the field equations be for fields of arbitrary spin. Soon someone would realize that something is wrong, for if particles move according to the equation of motion, which they must do from the given action, then the $T_{\mu\nu}$ doesn't satisfy the correct equations. A suggestion, then, would be to add the field energy into the stress energy tensor and say that this also is a source of gravity. Then, $\int th$ would become $\int h(T + t(h))$. However, this would not work because a variation of the h's gives not only what one wants, but some new terms. Finally one asks if there exists an expression of third order in the h's which can be used in the action of the form $(h)(h,)(h,)$ and will give any relief from the difficulty. It is possible to prove from the definition of T and the equation of motion that the true T must satisfy some equation of the form

$$g_{\mu\lambda} T_{\lambda\mu,\nu} = [\mu\nu, \lambda] T_{\mu\lambda}.$$

Then one can go to the next higher order of approximation and this approximation will explain the perihelion of Mercury. Although one is on the right track this process is just an expansion. It is possible, however, to solve the problem mathematically by finding an expression which is invariant under

$$g'_{\mu\nu} = g_{\mu\nu} + g_{\mu\lambda}\frac{\partial A_{\lambda}}{\partial x^{\nu}} + g_{\nu\lambda}\frac{\partial A_{\lambda}}{\partial x^{\mu}} + A_{\lambda}\frac{\partial g_{\mu\nu}}{\partial x^{\lambda}}.$$

To get the solution of this problem one asks a mathematician. However, it could have been solved by noticing that it is a geometric transformation in a Riemannian space. Finally, someone might suggest that geometry determines the metric. This would be a marvellous suggestion but it would be made at the end of the work and not at the beginning.

What does one gain by looking at the problem in this manner? Obviously, one loses the beauty of geometry but this is not primary. What is primary is that one had a new field and tried his very best to get a spin-two field as consistent as possible.

I think quantization would proceed in the same direction as the original solution of the problem. One would consider this just another field to be quantized. From the other viewpoint the geometry is important, but from this viewpoint gravity is just another field. I am sure that an enormous amount of formulae would be collected without having the generally covariant quantum theory. I advertise that this new point of view may, in fact, succeed in the end. Certainly people would not think that the rest of physics could be deduced from gravity. They may well be wrong, but it also may be true that gravity is just one more of a long list of difficult things that some day have to be put together.

Now let us go to the second possibility, old fields. The physicists might try to explain the new force on the basis of the incomplete cancellation of electric charges of the order (number of particles)$^{1/2}$ or some such scheme. However, there are a number of interesting possibilities which are not completely impossible. One important fact is that this field has an infinitesimal coupling constant and already one knows about one weakly coupled field, the neutrino. Also, the neutrino has zero rest mass which is needed for $1/r$ dependence. The neutrino equation is modified, for convenience, to read

$$\Box^2 \psi = \gamma_\mu j_\mu,$$

and the neutrino propagators are assumed to be $\frac{1}{p^2}$. A first trial might be the single exchange of a neutrino between the two interacting bodies. In this case, however, a $1/r$ law does not result because the initial and final states are orthogonal. Another possibility is a two neutrino exchange, but this gives a potential which falls off faster than $1/r$. Next, one could try one neutrino exchange between the two bodies with each body exchanging one neutrino with the rest of the universe. The rest of the universe is assumed to be at some fixed distance, R. This does give rise to a potential which varies as $\frac{1}{rR^3}$. However, the amount of matter varies as R^2 so that upon integration over the universe one gets a logarithmic divergence. The most serious difficulty with this approach is, however, the large effect the sun would have on the earth-moon system. This last trial was so much better than the others one can go a step further and try a four neutrino process with the additional neutrino being exchanged in the rest of the universe. This will also give a $1/r$ potential and a higher order divergence and one must worry about the density of matter, etc. Higher order terms are possible but they are much more difficult to handle. Therefore, the possibility exists that the material from the outside is making the source of gravitation here through the exchange of well known particles, the neutrinos. This is obviously no serious theory and is not to be believed. For one thing the use of the correct propagators would change the r dependence. If one did put in things correctly and got a finite amount for a result, then this is very curious and has not been noticed before. That is one of the advantages of looking at something from a different direction. Anyway, this is what the people who believe in old theories would do.

This different point of view has been advertised in the hope that a few people will start looking at gravitation from a different direction. I think really that it is more likely and more interesting to go from the geometric side, but if a few investigations change directions we may get somewhere.

ROSENFELD comments that the possible connection between neutrinos and gravitation was discussed a long time ago. Also, one says that gravitational energy is carried away by gravitons while energy is conserved in beta decay by the neutrino. These might be alternate ways of saying the same thing.

Chapter 26
Summary of Conference
P. G. Bergmann

Our agenda consisted essentially of two main problems with a connecting link between them. The two main problems are classical theory and quantum theory and the connecting link is the theory of measurement which stands with its feet in both camps.

A call for more experiments at the present time is not likely to produce more experiments, but more experiments would be a wonderful thing. The only two types of experiments that have been suggested at this conference fall in two classes. First, do all old experiments over again with more accuracy and increased care. And as Dicke has pointed out, if an experiment reported an accuracy great enough to check on the energy of interaction of weak interactions it might give a result different, in principle, from those done up to now. The other type of experiments which are going on, and ought to be going on, are those intended to shed light on cosmological problems at all levels. Actually, there exists a third type of experiment which is apparently not feasible, and is not going to be feasible for a long time. This type considers the detection of gravitational waves.

The classical theory must be concerned with a number of questions. First of all we have the mathematical questions. I think that Lichnerowicz and his group have cleaned up the question of the propagation of infinitesimal disturbances so that we now know what the light cone is, not only in the gravitational theory, but also in a non-symmetric theory. As far as the true Cauchy problem is concerned, how can we develop an algorithm which gives us self-consistent conditions on an initial hypersurface from which we can continue? This problem is not solved and is essentially another wording of the search for true observables. The third type of problem which one might classify as mathematical are the global theories which have been investigated by the French group.

Now to the problems that are more concerned with the special question, where things having physical or model significance are tried out. The most important of these questions which must be settled is, are there gravitational waves? At the present there is no general agreement. The other things to be mentioned are interesting but are of less crucial significance. People have studied model universes and stability questions and these are of major importance for cosmology. The geons, which are not to represent particles, may be of considerable use in studying model questions, i.e., how will such a thing behave? One would prefer, however, not to use the dynamical or statistical averagings. Among these classical problems are the unified field theories, and my own inclination is to suspend for a few years the search for bigger and better ones. Even if one believes in exhausting the classical problems first and also believes in unification, there is some question as to whether the unified theories are the correct starting point. At the end of the list of classical problems there is the real problem of the feasibility of separating the strictly classical questions from the quantum questions.

The work of Pirani which gives a simple classical observation of the components of the Riemann-Christoffel tensor should be accepted and made part of our equipment. It enables us to set up a conceptual experiment to measure a specified component of this tensor. This result should be a basic component in the design of new experiments. As far as quantum measurements are concerned, we should separate them into quantum theoretic measurements with and without gadgets. The man who does an experiment in gadgets believes that he can bring clocks and measuring rods into his physical space time without affecting it too much. I remind you of Salecker's experiment. It is dangerous, however, to use gadgets in an experiment without careful thought.

The first point in the discussion of quantum theory is the necessity for quantization. We have had two types of arguments for quantization here. The first is for the essential unity which quantization will bring. This is a very vague and general argument. And the second is related to measurement by gravity and has been discussed by both Feynman and Rosenfeld. As far as technology is concerned, we have talked very little about the non-Feynman methods of quantization. I agree with others that one need not wait until all the mathematical difficulties are cleared up in connection with the Feynman method. If this can be carried through it must also be possible to give a mathematically acceptable form to it. Very little has transpired on the problem of elementary particles. We have been given several word pictures and we must build a framework to hang the pictures on.

Chapter 27
An Expanded Version of the Remarks by R.P. Feynman on the Reality of Gravitational Waves[1]

I think it is easy to see that if gravitational waves can be created they can carry energy and can do work. Suppose we have a transverse-transverse wave generated by impinging on two masses close together. Let one mass A carry a stick which runs past touching the other B. I think I can show that the second in accelerating up and down will rub the stick, and therefore by friction make heat. I use coordinates physically natural to A, that is so at A there is flat space and no field (what are they called, "natural coordinates"?). Then Pirani at an earlier section gave an equation for the notion of a nearby particle, vector distance η from origin A, it went like, to 1^{st} order in η

$$\ddot{\eta}^a + R^a_{0b0}\eta^b = 0 \quad (a, b = 1, 2, 3)$$

R is the curvature tensor calculated at A. Now we can figure R directly, it is not reasonable by coordinate transformation for it is the real curvature. It does not vanish for the transverse-transverse gravity wave but oscillates as the wave goes by. So, η on the RHS is sensibly constant, so the equation says the particle vibrates up and down a little (with amplitude proportional to how far it is from A on the average, and to the wave amplitude.) Hence it rubs the stick, and generates heat.

I heard the objection that maybe the gravity field makes the stick expand and contract too in such a way that there is no relative motion of particle and stick. But this cannot be. Since the amplitude of B's motion is proportional to the distance from A, to compensate it the stick would have to stretch and shorten by certain ratios of its own length. Yet at the center it does no such thing, for it is in natural metric - and that means that the lengths determined by size of atoms etc. are correct and unchanging at the origin. In fact that is the definition of our coordinate system. Gravity does produce strains in the rod, but these are zero at the

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[1] See Feynman's remarks, p.252.

center for g and its gradients are zero there. I think: any changes in rod lengths would go at least as η^3 and not as η so surely the masses would rub the rod.

Incidentally masses put on opposite side of A go in opposite directions. If I use 4 weights in a cross, the motions at a given phase are as in the figure:

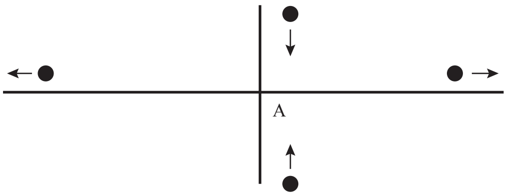

Figure 27.1

Thus a quadrupole moment is generated by the wave.

Now the question is whether such a wave can be generated in the first place. First since it is a solution of the equations (approx.) it can probably be made. Second, when I tried to analyze from the field equations just what happens if we drive 4 masses in a quadrupole motion of masses like the figure above would do - even including the stress-energy tensor of the machinery which drives the weights, it was very hard to see how one could avoid having a quadrupole source and generate waves. Third my instinct is that a device which could draw energy out of a wave acting on it, must if driven in the corresponding motion be able to create waves of the same kind. The reason for this is the following: If a wave impinges on our "absorber" and generates energy - another "absorber" place in the wave behind the first must absorb less because of the presence of the first, (otherwise by using enough absorbers we could draw unlimited energy from the waves). That is, if energy is absorbed the wave must get weaker. How is this accomplished? Ordinarily through interference. To absorb, the absorber parts must move, and *in moving generate a wave* which interferes with the original wave in the so-called forward scattering direction, thus reducing the intensity for a subsequent absorber. In view therefore of the detailed analysis showing that gravity waves can generate heat (and therefore carry energy proportional to R^2 with a coefficient which can be determined from the forward scattering argument). I conclude also that these waves can be generated and are in every respect real.

I hesitated to say all this because I don't know if this was all known as I wasn't here at the session on gravity waves.

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