John Archibald Wheeler
and the receritification of General Relativity as True Physics

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The lecture that follows is being given by Charles Misner at the University of Maryland as one of the general relativity seminars on Tuesday the third of October 2006.

[Slide 1] This talk is a repeat of one I gave in Erice at the Ettore Majorana School there in the town at the top of a mountain on the West side of Sicily. There have been many summer schools there and this year was the opening of a John Archibald Wheeler International School on Astrophysical Relativity (which I think is the official name of it) and I was asked to give one of the inaugural talks. That's where all this came from. The organizers, Richard Matzner and Ignacio Ciufolini also asked me afterwards if I would prepare a text of the talk. And, although they had taped it, the tapes weren't practical as far as I could see --- I never got any sign that I could get hold of them. So I decided that I would give it again and tape it, which is what all this apparatus is for. It's experimental physics in the sense that when this talk is over I will take the file that's on this little gadget, put it on my computer, and let some program transcribe it for me. We'll see how well that works. The program advertises that its 99.5% accurate which would mean only about five corrections per page but I doubt it's going to come anywhere near that; we'll see. It might be some help in preparing the text.

[Slide 2] An alternative to the announced title "John Wheeler and General Relativity" is "John Wheeler and the Recertification of General Relativity as True Physics". I think the most important contribution he made was to pull general relativity out of the discard pile and get large numbers of good people actively working on it. That's what I'm going to concentrate on. [Slide 3] But note that John Wheeler has gone through several different phases, each lasting about 20 years. In the first phase, where I didn't know him except the very end, he is concentrated on a view that the universe can be understood in terms of particles. In particular he did a lot of work in nuclear physics along that line. But he also a defined the S-matrix early in the 30s and did various other work that he was known for. The period I'm going to concentrate on is the second one where I worked with him closely in parts of it. That was when he decided it wasn't going to be all particles --- it was going to be all fields --- and he started out with gravity and electromagnetism. This work dominated 20 some years up until he retired. Upon retiring as a professor at Princeton he moved to take a professorship in Texas at Austin and there his interests changed. This change had begun at Princeton; in Texas his concentration was on information. His motto became "It from Bit" and many of his best students at that time were in this area of the interpretation of quantum mechanics and in trying to understand the nature of information relevant to physics.

I'll now give you a bit of the biography outlined here [slide 4]. John was born in 1911 which means he celebrated his 95th birthday this summer. He got a Ph.D. from Johns Hopkins in 1933. It was amusing for me to read recently an article by Spencer Weart in "Physics Today" summarizing 75 years of the American Institute of Physics: he said imagine someone who was a graduate student 75 years ago, what he would have gone through. Imagine what that now hundred-year-old physicist would have seen in the past 75 years. I remarked to Weart that it doesn't have to be a hypothetical graduate student. John Wheeler was a graduate student at age 19 in 1931 when the AIP was founded. In his autobiography John describes his interactions with postdocs and others, what kind of physics he was seeing at those times, and continues on through. So he was actually participating in physics for those entire 75 years. Just a few weeks before he became 22 he got his Ph.D. He was still 21 when he got his Ph.D. at Johns Hopkins.

He had a postdoc with Breit. Then his second postdoc was at the Bohr Institute (it wasn't called the Bohr Institute then) with Niels Bohr, and that was a very important development. He continued to interact with and admire Bohr all his life. Immediately on returning from that he got married and went to North Carolina as an Assistant Professor and then a couple years later, three years later I guess, he turned down tenure at North Carolina to move to Princeton as an Assistant Professor because he thought it was a more lively atmosphere for physics.

With the discovery of fission he and Bohr put together a nuclear liquid drop model that allowed one to estimate some of the important parameters of fission. Then things became much more classified and he worked throughout the Second World War on the development of the A-bomb. He was principally responsible for the operation at Hanford which was to produce plutonium in a nuclear reactor. After the war he came back and encouraged the beginnings of some cosmic ray research at Princeton and tried to get back into nuclear physics with things like a collective model which he was late in publishing. But then the cold war distracted him again as there was a political call to produce the H-bomb. He, after the war and late during late the war, was very much motivated by the fact that he had a brother he admired very much who was in fact fighting in Europe, or maybe not in Europe, and he always felt that if only the A-bomb had come 4 months earlier his brother might still be alive. He was very much concerned about that. I think he also felt that he didn't pay enough attention to politics when he was in Copenhagen at the Bohr Institute, and that if one had thought more about what was going on in and not a just let Hitler have his own way for many years, then also the world would've been a better place. Some of the troubles of World War II could have been avoided. So he was always hawkish on strong defense. I think in particular some of his colleagues at Princeton and others didn't think he played an admirable role in the Oppenheimer hearings. Oppenheimer was unenthusiastic about the H-bomb, initially on good scientific grounds because all the proposals were impossible and never have worked, but the later when the way to do it was found, Strauss, the Chairman of the AEC, really didn't like him (and Teller didn't like Oppenheimer --- you can read about that in the book about Oppenheimer that has recently come out) and so their way of getting Oppenheimer out of the councils was not just to say well we want an advisory committee that will pursue defense and since you're not enthusiastic about it we'll replace you. That would have been difficult since he was the hero of the World War II A-bomb and so forth, so instead they attacked his loyalty and claimed he was not a reliable person to have secrets --- he might be favorable to the communists or something. He certainly had a Communist background which was well-known to all authorities when they appointed him director of Los Alamos. Wheeler really didn't really make any strong statements in his support and said so little that he was considered to be supporting this approach to just get rid of Oppenheimer on any grounds. That caused number of hard feelings. Wheeler did spend part time, well a lot of time, on the H-bomb, much of it eventually done in the neighborhood of Princeton just outside the University on Route 1. That came to an end in '52 I arrived in Princeton ‘53 when John's H-bomb work was just finishing up and he was free to start doing something else, which turned out to be gravity.

The main part of the talk will be about those years where he was focusing on gravity and related things. From '53, until at age 65 John retired from Princeton for the purpose of going to Texas and spent 10 years there until he had some health problems (he had a bypass). He then retired again and moved to Meadow Lakes in Hightstown just outside Princeton where he has been living ever since. Just in the past year, instead of he and his wife running their own apartment and taking pretty much care themselves, they've required some of the assisted living and such things that are available there. This summer was the first time he did not get to his house in Maine since he bought in 1955. As you can calculate, he's 95 years old. His wife is actually 98. They are a continuing to live there, but John's hearing is so poor that it's difficult to have a conversation with him and has been for several years. But he does hire a secretary to come around a few hours every week to work with him. Ken Ford says that John did in fact write a chapter in a book which was published this year, so he's not totally out of everything. But he's under very difficult circumstances right now, I would say.

[Slide 5] Here's a picture of him. Three years ago he received the first Einstein prize. (Peter

Bergman received the prize at the same time as John Wheeler and Bryce DeWitt received it just before he died last year. That enterprise needs contributors. If there are any highly financially comfortable people in the room, they could consider donating to that prize through the Physical Society.)

Wheeler was always a teacher. He talks about teaching in order to learn. In fact he said after the H-bomb work was finished and he settled down to full-time physics that the first thing he did was ask to be assigned to teach the relativity course. He says in his autobiography that he wanted to learn relativity and the best way learned a thing is to teach it. So that's what he did. He taught that course for a few years but the first year, I guess, was the learning experience. When he taught other things --- such as the freshman honors physics, probably (I haven't heard of him teaching the regular introductory premed courses but he did teach freshman physics on many occasions) he would always start out with inspiration. He would start out making sure the students felt that there was something they were going to be really happy to learn going on here. He would typically start by describing the current research he was doing or that his students were doing. Then he would gradually move, after a lecture or a lecture and a half on the current forefront physics that he was enthusiastic about, into some of the background needed for that. He would eventually get around to vectors, acceleration, and things of that sort.
With his research students (there is this third point up there) he thinks questions are really at least as important, or maybe more important, than the answer. If you pick the really right questions then there is some point to a lot of hard work, because you can probably find an answer. It wasn't the right question if it can't be answered in some way. So he was always interested in what are the important questions to be focused on at any given time. When he thought he had the right question he would put a lot of, and continuing, effort into it. But he always worked with students. He has a rather small fraction of his papers that are single author papers. There are perhaps a number of single author papers where the author was his student, but where John was actually one of the major contributors. So he often would encourage his students to publish whereas many other people would have put their name on the same paper because there was a lot of interaction going on. In many other cases he was involved in work with students and all the names appeared which I think is a standard practice. But all his papers had authors listed alphabetically, which almost always put his name at the end. He finally met a student in Texas who outranked him for last place ---Zurek. He really believes that students not only do the physics but they ask so many questions that you have to think hard. They come at things from different angles, etc. etc. so that he really appreciated the opportunity to work with students and felt that he could get a lot more physics done by letting them interfere and collaborate.

When I set about to prepare this lecture for Erice, the focus was on those 20 years at Princeton. It turned out that just as he went from Princeton to Texas, there was a book prepared as a gift to him by Peter Putnam, one of his students. That book attempted to gather, from anyone who had benefited from their association with Wheeler, some written comments. The request was for letters to John Wheeler to put in the book, with a CV and list of students. [slide 7] That means I just had to pull out my copy of this and let that do a lot of the review of what was going on in the 20 years. That's what I'm going to do next. We start with a list of students from the back of the book. [slide 8] They were organized by dates and, of course, some of his early students are known to everybody, such as Richard Feynman. Arthur Wightman was a very prominent field theorist in Princeton for many years and a leader in the field. Of course Aage Bohr was not actually a student of John's. He came with his father to Los Alamos during World War II, and John of course visited Copenhagen many times immediately after the war and saw a lot of Aage Bohr and Aage felt that he had had a close enough association with John and that he wanted to be included in this list of people who were making a tribute to John. Then we go on to the mid-50s. [slide 9] We start out here with the early 50s people who are mostly nuclear physics. John Toll wasn't, John Toll did dispersion relations so he was doing field theory with John. Ken Ford was one of his later, nuclear physics, students and later in life Ken Ford with some support from the Sloan foundation was able to spend a lot of time helping John Wheeler put together an autobiography, which was published a few years ago. Ken still lives in Philadelphia. He goes and sees John several times a month and tries to help him with this correspondence and things like that. Also from the 50s is Jim Griffin, who I think was John's last nuclear physics student. He's been an important member this department for many years; after having spent some time at Los Alamos before he came here. Larry Wilets is also a very well-known nuclear physicist in Washington State. He leads one of the major groups, as important as the one here. I'm not sure about Hill, but he did very important work at that time. But I don't keep track of nuclear physics so I can't tell you all these things now. Then John was going into relativity. His first Ph.D. student was Art Komar, who did some things on the quantum gravity and postdocd with Peter Bergman and was thereafter more associated, in his work, with Bergman than with Wheeler. Later he served various times at the National Science Foundation as the gravitation program officer, usually substituting for a year for somebody who was away on sabbatical. Joe Weber was with John Wheeler and went to the Institute for Advanced Study so he could work with Wheeler as he was getting into general relativity. Wheeler gave him a lot of encouragement on his development of gravitational wave ideas. In 1955 Joe Weber, myself, and Peter Putnam accompanied Wheeler to Leiden in the Netherlands where John was a visiting professor for the spring semester. Tullio Regge came to visit at that time. I'll have some notes from Tullio Regge about that experience later.. There is a number of other people some of who well-known here. Like Dieter. John Klauder who is now down in Florida, I believe, and Jim Hartle was actually an undergraduate working with the Wheeler and with Dieter Brill. There is a very important paper, Brill-Hartle, which came out of Hartle's senior thesis. It was published in the Phys Rev. Peter Putnam this very eccentric and interesting guy was responsible for gathering all the stuff together was a student of John's first as an undergraduate, and then during the time I was there he came back as a graduate student and finished a Ph.D. But his main interest was Eddington's fundamental theory and John got him away from that long enough to do a Ph.D. He worked around Route 128 in Boston and every time he was offered a raise he would say no, give me some stock instead. So he accumulated stock in some of the startup companies of the 1950s in Route 128. Later on he arranged --- but I think also tried to avoid using his mother's money for anything (she was in family of the publishing company) --- but he arranged large gifts, I'm sure a substantial amount from himself. He gave all his stock to the university and said don't sell up until I tell you, and he told them in exactly the right time about 20 years later. They collected their millions and, as he directed, bought fabulous pieces of sculpture by absolutely first-rate people that are scattered around campus at Princeton.

Here's some more from the 60s the next generation there. [slide 10] David Sharp was very active. I knew him very well and worked with him. He went to Los Alamos and did a lot of things. Karel Kuchar. I think we all know Jim York, who has had a long career at Chapel Hill and is now at Cornell with important contributions in recent years to numerical relativity, but the initial value problem was his strongest point. Joe Redish was an undergraduate thesis student of John Wheeler's. Maybe his interest in teaching was initiated there. Larry Shepley, who retired many years ago from Texas, was a student of John's, and also partly of mine as he finished up just a few years after I came down here to Maryland. Of course Kip is probably John's most distinguished student apart from Feynman and I'm sure everybody knows about all the activities and contributions he's made and is still making. Ed Taylor is on the education side. Ed Taylor joined with John to write a couple books about relativity for the high school student or the general public. Anthony Zee, I'm not sure exactly --- maybe he was undergraduate also --- and went on to be a particle theorist. Roger Penrose came for a postdoc for a substantial period of time. Dennis Sciama, whose name is not on this list yet, wasn't a postdoc, he was more senior. Then Robert Geroch, who is in Chicago now, is a professor, and Arthur Fisher who is distinguished mathematician in Santa Cruz. [slide 11] And there are all these: Demetrios Christodoulou: there are a lot of stories from him coming up. He is a very gifted mathematical physicist who has proved things about the existence of singularities; I think contradicting Penrose's earliest formulation of cosmic censorship. Remo Ruffini spent 10 years at Princeton is a postdoc, assistant professor, etc., and worked with a huge batch of people. I think I found that he had 61 different collaborators from Princeton during his ten years there. Leonard Parker --- I don't know his connection to John --- [Bei-Lok comment]. Bob Wald, who of course you know from an excellent textbook, is professor at Chicago. Jacob Bekenstein, who was famous for his introduction of entropy of a black hole. Yavuz Nutku must have just been an occasional visitor. I mean, he was a student of Chadrasekhar and did a postdoc here. He greatly admired Wheeler. Jim Isenberg wrote an undergraduate thesis with Wheeler and feels that his approach to physics was strongly influenced by that. Claudio Teitelboim spent many years at the Institute for Advanced Study as a regular visitor and worked with Wheeler. John Wyler, I'll tell you more about later. Larry Ford. Larry Smarr spent, I think, a month in Princeton and felt much inspired by it. Well, Smarr is primarily associated with computers, but he came into it through numerical relativity. Isaacson, who was director at NSF, learned how to manipulate the bureaucracy from Marcel Bardon. His first move to get gravitational waves going was to get supercomputers going. So Isaacson was very important in getting the first round of supercomputers financed around the country for use in science (and especially physics) including setting up the Illinois Supercomputers Institute which Larry Smarr led. Smarr started out in Texas with Bryce DeWitt who had done computations on the A-bomb way back when, and knew about computing and was interested in numerical relativity. He got Larry Smarr into that and Larry Smarr did some of the earliest successful work on numerical relativity and then went to Illinois where he encouraged that and lots of other things including the web browser, which was invented there while he was the director. Then he continued mostly in that field, but he was an important player in the early stages of numerical relativity. Gary Horowitz of course is a professor at Santa Barbara and continues to play an important role in physics.

[slide 12] So all these people, including Bei-Lok here, who interacted strongly. Bill Unruh who just was celebrating his 65th birthday in the summer, Zerilli played an important role in early stages perturbations of Schwarzschild and therefore the ring down idea of a signature of black holes. It's now very important. Fred Singer was someone who was from John's early stages --- cosmic ray physics. He's a devil's advocate for many years on the unpopular side of numerous questions, currently global warming. He's done all kinds of things after having done things related to the Van Allen belts of trapped particles around the earth; he was interested in things like that made contributions to it.

So that's a list of the people, a very huge number actually, of people who have been very done very significant things --- that were wanting to say things to express their appreciation of John at that time. So we now go through some details of some of them.

[slide 13] Fred Singer has this interesting story. His thesis committee included Oppenheimer and Niels Bohr. "I started to explain my work. Oppie interjected, took over, and held the floor. I sat down while he continued, Bohr fell asleep, and [Wheeler] looked vaguely unhappy. I passed the exam." [slide 14] Here's a note from Regge. John had met Regge at a Rochester conference when he was a grad student in the United States and due to go back to Italy at the end. John was impressed by him and invited him to come to Leiden in Holland and found some money to get him there. There they worked on the perturbations of Schwarzschild. So John's style, which fits quotes from other people later, is that you should preferably know the answer before you start calculating. So John wrote a paper on the perturbations of Schwarzschild; gave it to Regge, and said here's the outline of what we expect to find, please fill in the equations. Regge says that wasn't a bad way to approach the problem. He said he learned a lot from it and was up to the task. At that time the nature of the horizon was not understood well and so I don't think they had the boundary conditions exactly right. They laid out all the technology that was needed and pointed out that this was an important problem. [The singularity means the horizon yes.]

[slide 15] This is a story from someone than you probably don't know (Mjolsness). He was working as a graduate student with John. He was given John's notebook to borrow for a few days and he thought that was a wonderful experience because he could look in there and see how John goes about organizing his work. John did have this habit for, I guess, all of his life of having bound notebooks. I looked at one at a bookstore the other day; it costs $90 to get one of these books -- bound books of blank pages -- but John had very nice bound books. They were always there. When he had a group of students in the office he would sit down and take notes as the discussion went on. He would also make notes to himself about the calculations he was doing, or the work he planned to do. What were the important questions in physics? and so forth. So the student was thought he learned a lot by being able to see into John's way of approaching physics. Those notebooks incidentally have been given to the American Philosophical Society in

Philadelphia and there they are available to researchers.
[Joe Weber, so it's very likely that Joe Weber picked up this way of handling things from his association with John in the 1950s.]
[slide 16] Another saying of John Wheeler's: "an expert is one who's made every mistake in some field" is one version of it. The version I remember of John's on mistakes is "the way to be a very good physicist is to be able to make mistakes faster than anybody else". So he sort of assumed there's a quota and that you've got to have a certain number before you get to some real progress.

[slide 17] Barry Malik, a student who worked with John. Here's that quote "never do a complicated calculation unless you know the answer". Another thing I'd hear so often: if you really understand a phenomena you can build a simple model and explained it to anybody. That is certainly one's John's teaching attitudes: if you really understand something you should be able to explain it to less than your collaborators. He has this phrase "radical conservative". I remember the phrase "dynamic conservativism" [Brill: "daring conservativism"]. This was a view that John used to justify some of his outrageous work like wormholes and geons and whatnot. Quantum foam. But especially on things like wormholes. His attitude and there was: you should not just looked to a theory to explain the things you know, you should look to the equations and see what they predict even if you can't imagine why such a thing should be part of physics. He wanted to push the equations to their limit and see what's in them --- that's the conservativism. We've got these equations, principally Einstein's, why do we just use them in cases where we already understand what to expect? Why don't we push them and see if they've got anything else to tell us that might be a surprise?
As I was having these pages from this privately published book --- there's only a half-dozen copies in existence --- as I was having them copied here in our Xerox room, the woman who was running the Xerox looked at this page, and she said "that's my language!" so one of our staff is Bengali.

[slide 18] Now Kip Thorne on John's writing. John had a style of writing that many people were able to recognize. Kip could do more than recognize it. and do so, he says one of his students came up with the MTW book and says "This is just too strange a way to write. Why did you let John Wheeler get away with that? Kip said "Wheeler didn't write that section; I wrote it!" Kip studied John's style of writing. That makes it hard to decode MTW. You can probably find if things are written to sort of a dull straightforward way that I had a hand in it, but it's very hard to distinguish Kip from John Wheeler in there. John did the introductions to most of the chapters; made sure there was something like a sentence or two that would be meaningful to anybody that's starts things out. The most obvious peculiarity was a sort of Germanic sentence structure in which you try, as much as you can in English, to put the verb at the end, having started out with a bunch of other qualifications, until the whole point gets made, with emphasis.
[slide 19] So here someone else quoting this point that finding the right answer is never as important as asking the right question.
[slide 20] Jim York wrote a long letter talking about how he got into serious physics by getting a chance to meet John and talk to him. Then John simply used his influence to get Jim invited to the Battelle conference. That got York into the real meat of the business and he never slowed down. He continued to work very hard making contributions to primarily the initial value problem, but eventually, in collaborations with Mme. Choquet-Bruhat, many very rigorous results also in that area.
[slide 21] Demetrios Christodoulou was perhaps even younger than John Wheeler as a graduate student. Someone [Brill: Papatetrou] recommended him to John. John was able to meet him in Paris one year when he was there. So John agreed that this was a very outstanding young man, and talked Princeton into accepting him in spite of the fact that he probably hadn't finished high school or something. He got picked up and learned to do physics at Princeton working with John and very much appreciated the fact that things could be done that way.
[slide 22] Here's Remo Ruffini. As I mentioned, in his letter or in his CV says while at Princeton he collaborated with this long list of people. I went through trying to count from his publications and found about 61 different collaborators. He was there during 10 years from '67 to '76 I think

and during a very active time in the development of the physics of black holes.
[slide 23] Bob Wald says [Wheeler taught him that] one should think in a completely down-to-earth manner and decide by physical intuition what ought to be true first. Then one should obtain a mathematical proof or disproof of one's conjecture. So this is again this idea of John that you really should think through a problem in its broad outlines in an take all the clues you could have from other aspects of physics and other ways of thing about so that you think you know how to work your way through the problem before you get deep into it.
[slide 24] Sergio Hojman. He's just someone pointing out this idea of John's inspiration saying that physics is going on today when you can get your first physics class. You should just meet Newton and find things that were known 400 years ago, but you should realize that there's real physics going on right now. And of course today's [U MD lecture 3 October 2006] Nobel prizes remind us that there are great discoveries being had made right now. John was able to get students in the in undergraduate classes to realize that physics is really going on now in a serious way.
[slide 25] Jim Isenberg, I think he's talking here about his Ph.D. thesis done here at Maryland. But anyway he took it up for John to read and John wrote across the page 'DESLUDGE'. The question is, what does that mean. Jim found it means that, as you would clean out the garage under your car where it had dripped. You want to get rid of all the junk that's messing things up; clean out unnecessary words, etc., etc. Teaching has been an important enterprise, he says, and de-sludged is the only kind he does. John would work with many of his students with lots of effort on the writing.
[slide 26] Here Claudio Teitelboim tells the same thing. John would spend a lot of time correcting English. Claudio said "John, why are you spending so much time on that" and John said "That's what I'm paid for. The central idea should always stand out clearly, sharply, just as in 'Cuba si, Yankee no' ". That was John's address to Teitelboim on making sure that there is a clearly understandable point.
[slide 27] Now on John's writing: there is a paper in the GRG Journal by John A. Wyler which caused a lot of fun when it first came out. It came out as a Princeton preprint. In those days with no web one had standard little reproduced packages of the typewritten papers were sent around to your friends. So this came around to everybody. You really had to read for several minutes into that paper before you realized the author was not John A. Wheeler. Wyler was mimicking John Wheeler's style and the paper only slowly got more outrageous than John could be so you begin to see that it was a spoof. But it was such a good spoof that it later got published. So you can look it up if you want to.
As you see Wyler is writing here. [slide 28] Bill Press wrote the Wyler paper. He had all the tools. This is on the letter in this book from John A. Wyler to Wheeler "dictated Dr. Wyler's absence, proofread and signed by [the secretary] in his presence". So if you have read many things by Wheeler, you would enjoy reading Wyler.
[slide 29] And Larry Smarr again: talking about how John picked him up and gave him a chance to spend a month at Princeton. That was a very active time. He names all these people who are working actively on blackhole related physics. So John, when he found a gifted person, would go really out of his way to make sure they got leg up to go somewhere.

[slide 30] So now let's emphasize less John Wheeler as teacher and talk a lot his impact on relativity.
[slide 31] A brief summary of relativity in the early years: There was a period where cosmology got started. There were some important works in the 30s – the Einstein-Infeld-Hoffman ideas. There was also a paper on inhomogeneous cosmologies that's been very useful for people. Unified Field theories were the bane of GR in those days. Einstein worked on them. Einstein was convinced that physics should be primarily geometry, and he was trying to stretch the ideas of geometry. It's sort of amusing that about 10 years later, maybe 15, Steve Weinberg was convinced that geometry was irrelevant to physics. He wrote a textbook on General Relativity from a field-theory/special-relativity point of view as much as possible saying that He implies that the geometry is sort of window dressing; it makes things look prettier, but the important stuff is just field theory. A peculiar thing is that it was Weinberg later, who collaborated in proving that physics really is geometry. Except it's not the geometry of space-time, it's the geometry of the

graph paper on which the properties of space-time are conceptually plotted. That's the idea of a curved connection. If you want plot the values of any physical quantity that's of fundamental importance, like a magnetic field, quarks, gluons, etc. you need to plot on curved graph paper. But Einstein wasn't looking there --- didn't have that broad an idea of geometry --- so he never got on the right track of where geometry really comes predominantly into physics. There were also people who did straight General Relativity, who were just very good physicists, but there were limited numbers of them.

[slide 32] In these early years relativity had a very bad reputation among many physicists. Some Caltech physics professor (not named by Kip) reported in Kip's book who, as Kip was leaving for Princeton, told Kip that "General Relativity has little relevance to physics. You should look elsewhere for interesting physics". Kip was being warned away from relativity as he left Caltech as an undergraduate and went to Princeton as a graduate student. In Russia Novikov was getting interested in relativity and Novikov's wife was given advice by senior physicists that she should get Ivor out of it or he will never have a career. So she was told "relativity is a backwater, Ivor should get out of it". (This is in Kip Thorne's book.) [query] She was a physicist, yes.

[slide 33] Okay so after World War II there were a number of active people of the generation ahead of John's students, and so here's a list of many of the important ones at that time.

[slide 34] Wheeler went into this and his "dynamic conservativism" had these mottos:

- Mass without Mass

  John tried to show the collection of fields, electromagnetic waves, running around in circles and held into circles by gravity, could form an object ('Geon') that had a mass even though there was nothing there but zero mess fields. That was one of the things he was pushing then and later discovered that the whole system is unstable. But he went on to.

  - Brill Waves

    proving with Dieter Brill --- supervising Brill's thesis --- (after I'd been there pushing on initial conditions for some time) that with a rigorous solution of the initial value equations and not just approximations us as for geons, but done in a rigorous way, there are bundles of gravitational waves that have mass. It wasn't an attempt to say there was anything stable about them, just that you with nothing but empty space and curvature you can get mass. The other thing was

- Charge without Charge,

  The idea started with John's wormholes. They got turned into good geometry, I guess primarily by me, after I'd read the French book by Lichnerowicz and also a lot of other differential geometry people were teaching me. But then Finkelstein, and Beckedorff working with me, got completely geometrical views of the horizon, which was then called the Scharzschild singularity, and so one could make initial conditions corresponding to Wheeler's diagrams of wormholes and show that they really were consistent with the Einstein equations. One later found out that they wouldn't last long enough for much to happen if you had normal kinds of matter.

[slide 35] And so those were the dynamic conservativism kinds of ideas. [Jacobson: Did Wheeler really regard extended Schwarzschild as mass without mass?] You mean just a vacuum black hole? I think he eventually did, but at the time this was going on, he was trying to fight black holes. He was against black holes (as yet unnamed). That was his concentration on "what is the ultimate fate of a star?" and he expected that there would be something to avoid all the singularities. It took a long time for him to be convinced. Finkelstein certainly published the first good interpretation of the Schwarzschild singularity, the real physical interpretation. The Beckedorff stuff is published in an obscure place. It was published in the proceedings of a conference on Time that Tommy Gold organized at Cornell. I Forget exactly when, but not prior to Finkelstein paper. In fact, Finkelstein's paper pushed Wheeler into the writing the Kruskal paper. So the Kruskal picture, which goes beyond Finkelstein only in unphysical ways in my view, but anyway is mathematically satisfying. So the Beckedorff work came after Kruskal but importantly pointed out how you would match a collapsing star in the Kruskal diagram to the external solution. It showed a physical production of a black hole in a way that you could understand better than Oppenheimer and Snyder. They do everything; at least everything exterior to the horizon they had correct. They knew the physics but then Oppenheimer just was never in that again.

Okay, so here are John's main themes "dynamic conservativism", or as Dieter remembers, "daring conservatism". Look in the physics; get the physics out of the equations; don't just expect them to put the next decimal point on things you already understand.

[slide 36]

John encouraged Weber's work on wave detectors. He worked on the Regge-Wheeler perturbations of Schwarzschild. He influenced Sciama and Penrose. I think Penrose gives Sciama credit for pulling him into relativity from algebraic geometry. Sciama of course was a strong presence in the astrophysical side, a much better astrophysicist than Wheeler I would think. Sciama was a tremendous influence on Penrose who was also this encourage a lot by Wheeler and made visits in early days to Princeton. New experiments were going on; the Dicke experimental group in fact was right there in Princeton.

[slide 37]

The early 60s. Wheeler originally started thinking that the singularity ought to be avoided and then there came all this other work that really made it understandable because it was more geometrical than Oppenheimer-Snyder. Brill-Hartle work of showing gravitational waves by making a gravitational Geon, and the beginnings of numerical relativity in which Hahn and Lindquist made an attempt to actually integrate the Einstein equations numerically in a very simple case. [slide 38]

Then as next decade came on, the Princeton group got much bigger with important work being done by many people. And there are a new research groups building up various places. Sciama got many people working with him. I came down here and began to have other students and postdocs. Schild went to Texas. Thorne, some years later than me, went to Caltech and began building up a group. Infeld. Chandra came into relativity and attracted people there. Hawking. Ted Newman. Ted Newman is somewhat overlooked, I think, because on the Newman-Penrose stuff, he did the first work which got cleaned up by Penrose. But the idea of going to null frames was Newman, and the fact that it would simplify the search for solutions of Einstein's equations was his. The discovery of Kerr solution would have been impossible without the work of Ted Newman. Then the real astrophysics began coming in, and a lot of other developments came during the last years of that decade I was trying to refer to.

[slide 39]

Here that are places you can look for some of this stuff. John Wheeler's autobiography with the assistance of Ken Ford, which was published eight years ago. Kip Thorne wrote a book 10 or 11 years ago, which has a lot of history of these periods in it. There is this book which Peter Putnam organized --- that is, he financed it and pushed people into it, directed John Wheeler's secretary to send out letters recruiting contributions, and a really elaborate binding of this was given to Wheeler at the GR8 meeting. It was an absolutely beautiful thing; probably cost even in 1977 a thousand or two for the binding. Half a dozen other copies are around. I have one; the American Institute of Physics across the road has one; Princeton and Texas have copies. So they exist; especially at the Center for Physics across the road one can find it. Then there is this Bartusiak book where I found some interesting history, and there is a book in the press (I've seen a draft) Kennefick, who is a Caltech physicist who has been doing a lot on the history of physics. And so, you can look forward to that as other sources for the history of this area. So with that. I thank you for your attention. I'm glad to have a chance to be here.

[There are an additional 18 minutes of recording of the discussion which followed on the *.wma audio file. Due to the directionality/noise-cancelling of the microphone, most questioners' voices are inaudible.]