John Wheeler and the Recertification of General Relativity as True Physics

Charles W. Misner

Abstract This lightly edited transcript of my 2006 lecture at the Erice "J.A. Wheeler" school introduces Wheeler by describing him from several different viewpoints. There is brief mention of his early work on nuclear physics and its military applications as well as Wheeler's attitude toward defense work. Also, just briefly, his Texas efforts to probe the foundations of quantum mechanics are mentioned. Considerable attention is given to his methods of working, and to his relationship with students and their reactions to him. A further section describes his push to bring general relativity into the main stream of physics and the influence this had on the development of black hole ideas and on the study of gravitational waves.

1 Introduction

The title, "John Wheeler and the Recertification of General Relativity as True Physics" reflects what I think is the most important contribution John Wheeler made: to pull General Relativity out of the discard pile and get large numbers of good people actively working on it. That will be my concentration in this paper.

But note that John Wheeler went through several different phases of interest in Physics, each lasting about 20 years. In the first phase, where I didn't know him except at the very end, he concentrated on a view that the universe can be understood in terms of particles. He defined the S-matrix early in the 1930s and did much other work in atomic and nuclear physics, and contributed very significantly to the Manhattan project, which produced the first nuclear bombs.

The period I'm going to concentrate on is the second one; 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.

C.W. Misner (✉)
Department of Physics, University of Maryland, College Park, Maryland, 20742–4111, USA
e-mail: misner@umd.edu

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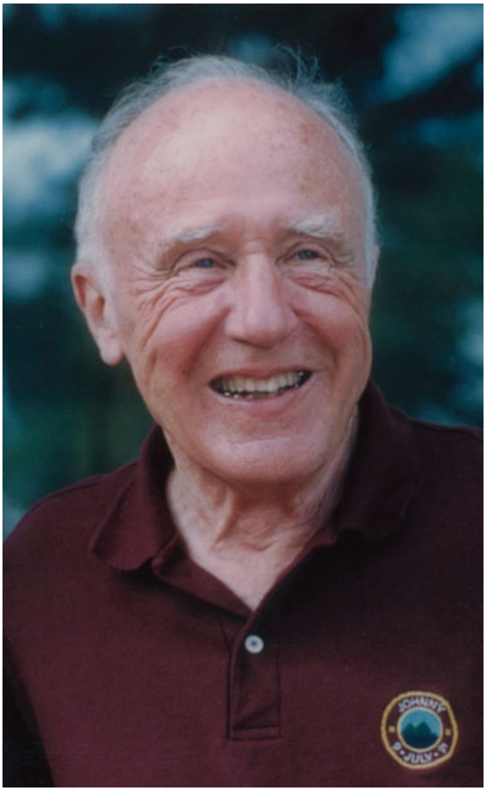


Fig. 1 John Archibald Wheeler

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 again.

This last 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.

This paper was written in 2006, two years before John Wheeler's death (Fig. 1). John Archibald Wheeler died on 13 April 2008 at age 96. Janette Wheeler (John's wife) died on 17 October, 2007. She was 99.

2 John Archibald Wheeler

- Born 9 July 1911
- Ph.D. Johns Hopkins (Herzfeld) 1933
- NYU (Breit) 1933–1934, Bohr Institute 1934–1935

- Married Janette Hegner 10 June 1935
- Asst. Prof. Princeton 1938
- Bohr–Wheeler nuclear fission paper 1939
- A-bomb, Hanford Washington 1942–1945
- H-bomb 1950–1952
- Moved to Austin, Texas, 1976
- Retired from Texas 1986
- Died 13 April 2008

John was born in 1911 which means he celebrated his 95th birthday the summer this lecture was given. 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 50 years of the American Institute of Physics: he said imagine someone who was a graduate student 50 years ago, what he would have gone through. Imagine what that now 100-year-old physicist would have seen in the past 50 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 50 years.

John was still 21 (just a few weeks before he became 22) 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 Copenhagen he got married and went to North Carolina as an Assistant Professor. Then three years later 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 that process. 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. Late during the war, and after the war, he was very much motivated by the fact that he had a brother he admired very much who died fighting in the war. He always felt that if only the A-bomb had come four 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 and not 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 John's 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. Later when the way to do it was found, Strauss, the Chairman of the Atomic Energy Commission (AEC), really didn't like him (and Teller also didn't like Oppenheimer – you can read about that in the book, [3], about Oppenheimer that has recently come out). 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 Oppenheimer was the hero of the World War II A-bomb. 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 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 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 in '53 just as John's H-bomb work was finishing up and he was free to start doing something else, which turned out to be gravity.

The main part of this memoir will be about those years when John Wheeler was focusing on gravity and related things. I count them as the period from '53, until in 1976 when at age 65 John retired from Princeton. He then moved 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 Janette 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. He does hire a secretary to come around a few hours every week to work with him. His former student, coauthor, and close friend 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. Three years ago John received the first Einstein prize. (Peter Bergmann 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 reading this, they could consider donating to that prize through the American 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 to learn 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 John thinks questions are really at least as important as, 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 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 also 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. 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. 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 (Fig. 2).

We start with a list of students from the back of the book. 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

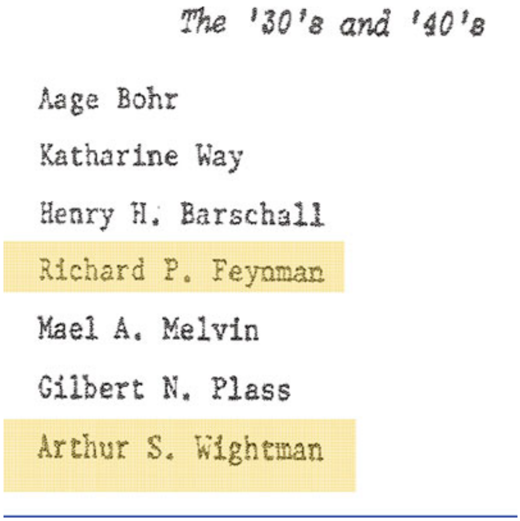


Fig. 2 The 1930s and 1940s

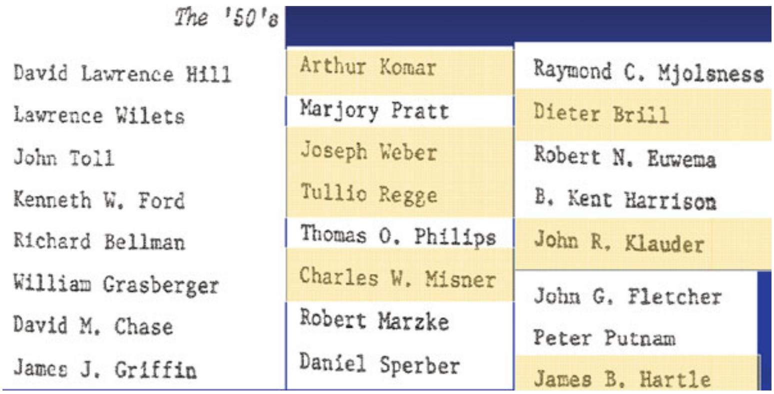


Fig. 3 The 1950s

a lot of Aage Bohr and Aage felt that he had had a close enough association with John that he wanted to be included in this list of people who were making a tribute to John (Fig. 3).

Then we go on to the mid-50s. We start out here with the early 50s people who are mostly in nuclear physics. John Toll was not, but he did dispersion relations and field theory with John. Ken Ford was one of his later, nuclear physics, students. 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 spent some time at Los Alamos before becoming an important member the Physics Department at the University of Maryland for many years. 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 at Maryland. 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.

By then John was going into relativity. His first relativity Ph.D. student was Art Komar, who did some things on quantum gravity and had a postdoc with Peter Bergmann; his work was thereafter more associated with Bergmann than with Wheeler. Later Komar 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 are a number of other people, some of whom are well-known at the University of Maryland, like Dieter Brill. John Klauder is now down in Florida, I believe.

Jim Hartle was actually an undergraduate working with 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 Physical Review. Peter Putnam, this very eccentric and interesting guy 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. His main interest was Eddington's fundamental theory though John got him away from that long enough to do a Ph.D. Putnam 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 on Route 128. Later on he arranged large gifts, I'm sure a substantial amount from himself. (I think also tried to avoid using his mother's money for anything – she was in the family of the publishing company.) He gave all his stock to the Princeton, saying "don't sell until I tell you", and he told them at 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 (Fig. 4).

Moving on to the 60s: David Sharp was very active. I knew him very well and worked with him. He had a long and distinguished career at Los Alamos. Karel Kuchaȓ has done much fundamental work on quantum gravity. I think we all know Jim York, who has had a long career at Chapel Hill and is now at Cornell. His work on the initial value problem was his strongest contribution, but he has also done important work in recent years on formulations of Einstein's equations, and applying these ideas to computational relativity. 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 to Maryland from

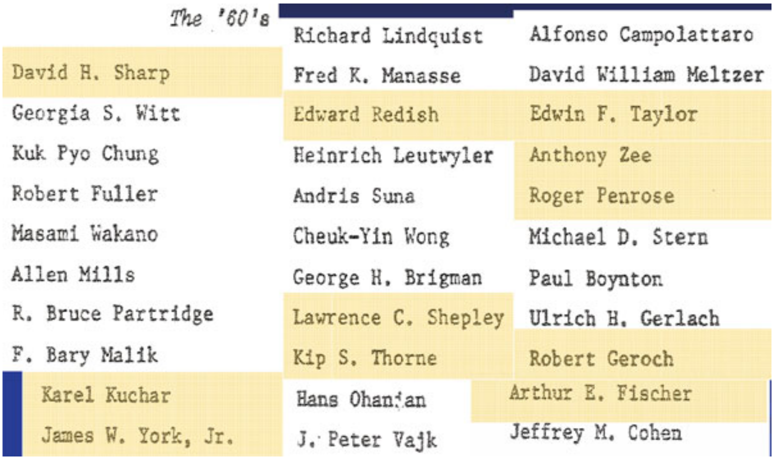


Fig. 4 The 1960s

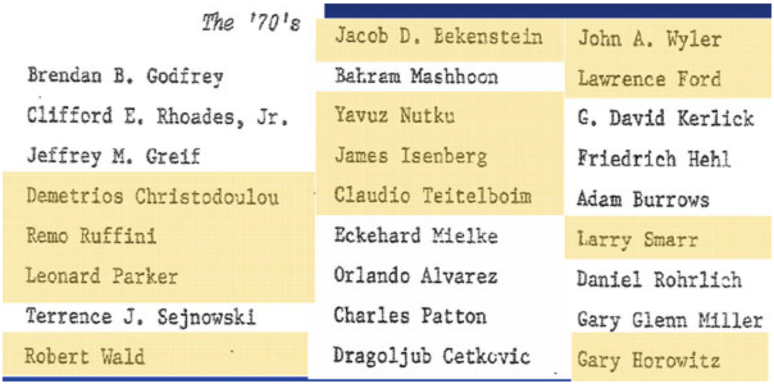
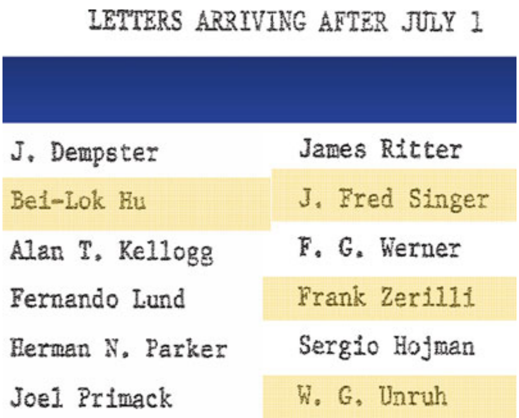


Fig. 5 The 1970s

Princeton. 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. He joined with John to write a couple of 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 a professor in Chicago now, and Arthur Fisher who is distinguished mathematician in Santa Cruz (Fig. 5).

And there are all these in the 1970s: Demetrios Christodoulou; 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, who spent 10 years at Princeton as 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 [Visiting Fellow 1971–1972], but I do know of his work on particle creation and quantum field theory in the early universe. 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 at Maryland. 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. I'll tell you more about John Wyler 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. Smarr 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 (Fig. 6).

So all these people, including Bei-Lok Hu at Maryland, interacted strongly. Bill Unruh just celebrated his 65th birthday in the summer. Zerilli played an important role in early studies of perturbations of the Schwarzschild spacetime and therefore


LETTERS ARRIVING AFTER JULY 1

J. Dempster	James Ritter
Bei-Lok Hu	J. Fred Singer
Alan T. Kellogg	F. G. Werner
Fernando Lund	Frank Zerilli
Herman N. Parker	Sergio Hojman
Joel Primack	W. G. Unruh

Fig. 6 Late contributers to the Putnam book

the ring down idea of a signature of black holes which is now very important as a signature in gravitational wave observations. Fred Singer was from John's early stages – cosmic ray physics. He's been a devil's advocate for many years on the unpopular side of numerous questions, currently global warming. He's done all kinds of things beginning with the Van Allen belts of trapped particles around the earth.

So that's a list of the people, a very huge number actually, – large numbers of whom have done very significant things – that wanted to extend their appreciation of John at that time. So let's go through the details of some of their comments.

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."

Here's a note from Tulio Regge (Fig. 7). 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. 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.

There is a story from someone than you probably don't know (Raymond 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

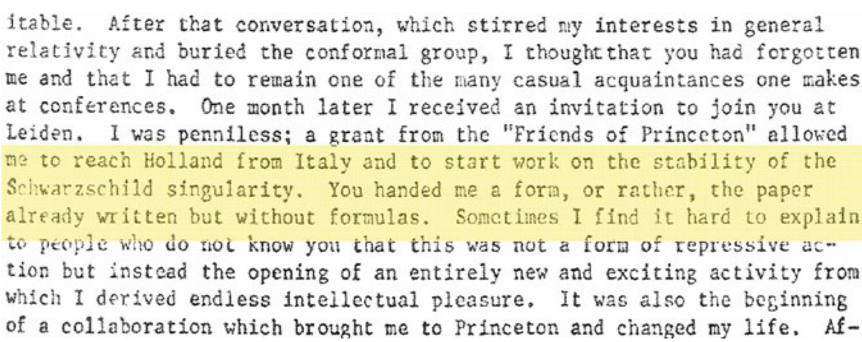

itable. After that conversation, which stirred my interests in general relativity and buried the conformal group, I thought that you had forgotten me and that I had to remain one of the many casual acquaintances one makes at conferences. One month later I received an invitation to join you at Leiden. I was penniless; a grant from the "Friends of Princeton" allowed me to reach Holland from Italy and to start work on the stability of the Schwarzschild singularity. You handed me a form, or rather, the paper already written but without formulas. Sometimes I find it hard to explain to people who do not know you that this was not a form of repressive action but instead the opening of an entirely new and exciting activity from which I derived endless intellectual pleasure. It was also the beginning of a collaboration which brought me to Princeton and changed my life. Af-

Fig. 7 Note from Tulio Regge

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 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. It's very likely that Joe Weber picked up his similar way of handling things from his association with John in the 1950s.

Another saying of John Wheeler's: "an expert is one who's made every mistake in some field" (Fig. 8) is one version; the version I remember 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 of mistakes before you get to some real progress.

Barry Malik, a student who worked with John: that quote again, "never do a complicated calculation unless you know the answer" (Fig. 9). Another thing I'd

him. The one saying of Professor Wheeler that I quote more than any other to my coworkers is 'An expert is one who has made every mistake in some field.' It is amazing how often this is appropriate."

Fig. 8 Wheeler on expertise. From a note by Bob Euwema

A perfect legacy of Bohr and Einstein. A firm believer in the simplicity and the universality of physics ("Never do a complicated calculation unless you know the answer" or "If one really understands a phenomenon, one can build a simple model and explain to anybody"). A radical conservative, ready to recognize new ideas but only after examining the old one carefully. A worthy carrier of the Bohrian tradition of being

A great devotee to science but never failing to recognize the greater truth, so vividly described by the medieval Bengali poet "Chandidas"

সবার উপরে মানুষ সত্য

(Above everything lies the humanity). A great believer and true practitioner of democracy, human rights and decency,

Fig. 9 Know the answer before you begin. Wheeler was a *Radical Conservative*. From a note by Barry Malik hear so often: "if you really understand a phenomenon you can build a simple model and explain it to anybody". That is certainly one of John's teaching attitudes: if you really understand something you should be able to explain it to less than your collaborators.

As Malik noted, John has this phrase "radical conservative". I remember the phrase "dynamic conservativism" [Dieter Brill recalls "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 there was: you should not just look 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 Maryland Physics Department Xerox room, the woman who was running the Xerox looked at this page, Fig. 9 and she said "that's my language!" so one of our staff at Maryland is Bengali.

Now Kip Thorne on John's writing (Fig. 10). John had a style of writing that many people were able to recognize. Kip could do more than recognize it. Kip 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 in 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 to start things out. John's most obvious peculiarity was a sort of Germanic sentence

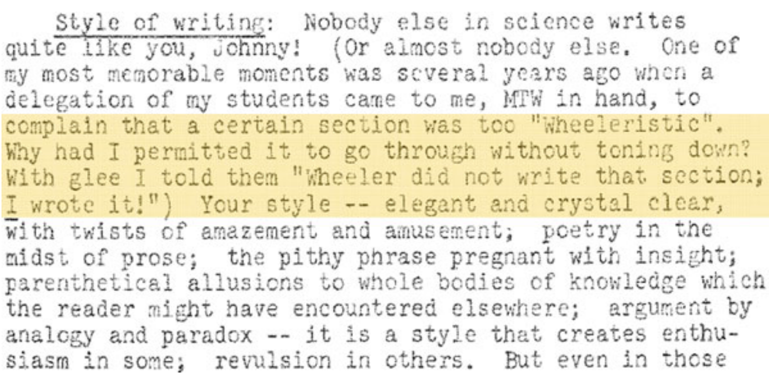

Style of writing: Nobody else in science writes quite like you, Johnny! (Or almost nobody else. One of my most memorable moments was several years ago when a delegation of my students came to me, MTW in hand, to complain that a certain section was too "Wheeleristic". Why had I permitted it to go through without toning down? With glee I told them "Wheeler did not write that section; I wrote it!") Your style -- elegant and crystal clear, with twists of amazement and amusement; poetry in the midst of prose; the pithy phrase pregnant with insight; parenthetical allusions to whole bodies of knowledge which the reader might have encountered elsewhere; argument by analogy and paradox -- it is a style that creates enthusiasm in some; revulsion in others. But even in those

Fig. 10 John Wheeler had a distinctive writing style. From a note by Kip Thorne 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.

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, eventually in collaborations with Mme. Choquet-Bruhat with many very rigorous results in that area.

Demetrios Christodoulou was perhaps even younger than John Wheeler as a graduate student. Papapetrou 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. 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.

Remo Ruffini wrote. As I mentioned, his letter or his CV says that 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 1967 to 1976, I think, during a very active time in the development of the physics of black holes.

Bob Wald says [Wheeler taught him that] "one should always think in a completely down-to-earth manner and decide by physical intuition what ought to be true; then one should obtain a mathematical proof or disproof of one's conjecture." So this again is John's idea that you really should think through a problem in its broad outlines and take all the clues you could have from other aspects of physics and other ways of thinking about it so that you know how to work your way through the problem before you get deep into it.

Sergio Hojman (Fig. 11) points out John's idea of inspiration in saying that physics is going on today even as you attend your first physics class. In that class you meet Newton and learn things that were known 400 years ago, but you should realize that there's real physics going on right now. And of course there are great discoveries being made right now. John was able to get students in the undergraduate classes to realize that physics is really going on now in a serious way.

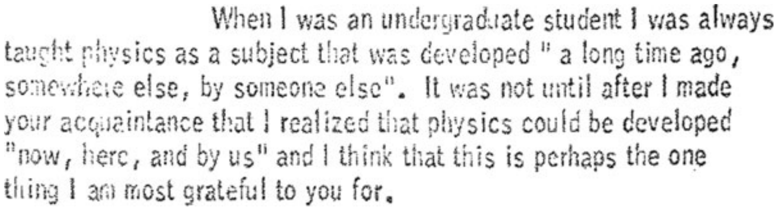

When I was an undergraduate student I was always taught physics as a subject that was developed " a long time ago, somewhere else, by someone else". It was not until after I made your acquaintance that I realized that physics could be developed "now, here, and by us" and I think that this is perhaps the one thing I am most grateful to you for.

Fig. 11 Physics is being developed here and now. From a note by Sergio Hojman the beach where grandson "Wheeler" was playing. (We were at the High Island summer home.) Suddenly John wrote across the top of the page, in large block letters:

"DESLUDGE"

At the time, I had no idea what to make of this strange collection of letters.

Teaching has always been a most important enterprise to John Wheeler [How many full professors want to teach the basic college physics courses, and want to write introductory physics texts?]. And "desludged" teaching is the only kind he does. So it is with the members of the clan. Indeed, one of the clearest

Fig. 12 DESLUDGE! From a note by Jim Isenberg

Dear John:

What clearer evidence of my own scientific debt to you could there be, than that contained in my paper "Rasputin, Science and the Transfiguration of Destiny" (Gen. Rel. Grav. 5, 175 (1974))? I have before me your kind letter of September 1, 1974, expressing appreciation of my small effort. No greater influence, no more important inspiration, no more constant source of ideas, than your own, has any other teacher had on my writings. My existence, indeed, is no less a figment of your conception than of any other. How reasonable to suppose that this influence resulted from a long period of collaboration and association. And yet, nothing could be farther from the truth. There stands out in mind with startling

Fig. 13 John A. Wyler emulated John A. Wheeler's writing style

Jim Isenberg (Fig. 12) wrote about his Ph.D. thesis done at Maryland: he took it up to Maine 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. 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.

Claudio Teitelboim reports 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 advice to Teitelboim on making sure that there is a clearly understandable point.

Now on John's writing: there is a paper in the GRG Journal by John A. Wyler (Fig. 13) 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 which 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. You can look it up if you want to.

Bill Press wrote the Wyler paper. He had all the quirks of John's writing down pat. Figure 13 is a letter in the book from John A. Wyler to Wheeler. It is signed: "dictated in Dr. Wyler's absence, proofread and signed by [the secretary] in his presence". This was the style of John Wheeler's known to hundreds of his correspondents. So if you have read many things by Wheeler, you would enjoy reading Wyler.

Again from the book, Larry Smarr talks about how John picked him up and gave him a chance to spend a month at Princeton, during a very active time. Smarr 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 a leg up to go somewhere.

3 John A. Wheeler and the Renaissance of General Relativity

I will now emphasize less John Wheeler as teacher to talk specifically about his impact on relativity: the recertification of GR as true physics. 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.

In these early years relativity had a very bad reputation among many physicists. Some Caltech physics professor (reported in Kip's book, [10], but not named by

Kip), as Kip was leaving Caltech as an undergraduate for Princeton as a graduate student, told Kip that "General Relativity has little relevance to physics. You should look elsewhere for interesting physics". Kip was being warned away from relativity. In Russia Novikov was getting interested in relativity and Novikov's wife (also a physicist) was given advice by senior physicists that she should get Ivor out of it or he will never have a career; she was told "relativity is a backwater, Ivor should get out of it". (This is in Kip Thorne's book.)

After World War II there were a number of active people in General Relativity, of the generation ahead of John's students; Post WWII GR leaders:

John Wheeler (Princeton)
Dennis Sciama (Cambridge)
Zel'dovich (Moscow)
Bondi (London)
Lichnerowicz & Choquet–Bruhat (Paris)
Bergmann (Syracuse)
Schild (Austin)
DeWitt (Chapel Hill)

Wheeler went headlong into General Relativity, 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.
– **Brill Waves**
  Proved 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 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.
– **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 relying a lot on the differential geometry people were teaching me. 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 anything to pass through them if you had normal kinds of matter.

And so those were the dynamic conservativism kinds of ideas. In the early 1960s John was fighting to understand black holes (as yet unnamed). His concentration was on “what is the ultimate fate of a star?” He expected that there would be something to avoid all the singularities. It took a long time for him to be convinced otherwise. Finkelstein certainly published the first good interpretation of the “Schwarzschild singularity” (the horizon), the real physical interpretation [4]. The [2] results are published in an obscure place, the proceedings of a 1963 conference on Time that Tommy Gold organized at Cornell [8], after the Finkelstein paper. In fact, Finkelstein’s paper pushed Wheeler into writing [sic] the Kruskal paper [7]. The Kruskal picture, which goes beyond Finkelstein only in unphysical ways in my view, is anyway 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 [9]. Oppenheimer and Snyder do everything; at least everything exterior to the horizon they had correct. They knew the physics but Oppenheimer never considered the subject again. Did Wheeler really regard extended Schwarzschild (just a vacuum black hole) as mass without mass? I think he eventually did.

John’s main theme, “dynamic conservativism” (or as Dieter Brill remembers, “daring conservatism”) drove him to look to the physics; get the physics out of the equations, encourage experimental/observational relativity. [*It also, together with desired quantum treatments for cosmology, led him to stressedly support Everett’s interpretation of quantum mechanics, which has been the subject, fifty years after Everett’s thesis, of two conferences in 2007.*]

John encouraged Weber’s work on wave detectors. He worked on the Regge–Wheeler perturbations of Schwarzschild. He influenced Dennis Sciama and Roger 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 encouraged 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. John saw 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.

Then as next decade came on, the Princeton group got much bigger with important work being done by many people. And there are new research groups building up various places. Sciama got many people working with him. I came to Maryland 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 formulation, he did the first work which got cleaned up by Penrose. The idea of going to null frames was Newman’s, and the idea that doing so would simplify the search for solutions of Einstein’s equations was his. The discovery of the Kerr solution would have been much delayed 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 as I have tried to show.

Here are places you can look for some of this history: John Wheeler's autobiography with the assistance of Ken Ford [11], which was published eight years ago. Kip Thorne wrote a book 10 or 11 years ago [10], which has a lot of history of these periods in it. There is the book which Peter Putnam organized – that is, he financed it and pushed people into it, and directed John Wheeler's secretary to send out letters recruiting contributions. This volume, in a really elaborate binding, 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 (less elegantly bound) copies are around. I have one; the American Institute of Physics; Princeton and Texas have copies, so they exist. Then there is a book by Bartusiak [1] containing some interesting history, and there is a book in the press (I've seen a draft [5, 6]) by 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 and other sources for the history of this era.

I hope this has given a flavor of the really important influence John Wheeler has had on General Relativity.

After his retirement from Princeton in 1975, John Wheeler moved to The University of Texas at Austin. He established an active and dynamic group with interests in fundamentals of quantum mechanics, quantum measurement, and quantum computing. He also pursued ongoing interests: sum over histories, the structure of field theories, Mach's principle, new formulations of the Einstein system, and astrophysics. Seven people received Ph.D.s from Texas under Wheeler's direction or co-direction: William Wootters, Paul Gleichauf, Ignazio Ciufolini, Warner Miller, David Henry King, Arkady Kheyfets, Benjamin Schumacher. Wheeler retired from The University of Texas at Austin in 1985, but remained a frequent visitor well into the 1990s.

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