
# John Wheeler's work on particles, nuclei, and weapons

Kenneth W. Ford

**For a time, Wheeler regarded nuclear physics as the best way to "do battle with nature." But then he became attracted to the simplicity of the muon, which is immune to the strong nuclear force. He himself, however, could not escape the ramifications of that force in a world at war.**

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**John Wheeler** died at his home near Princeton, New Jersey, on 13 April 2008 at the age of 96. Ten years earlier he had published an autobiography with the appropriate subtitle "A Life in Physics."[1] He had a happy marriage that lasted 72 years, and he was proud of his three children and their accomplishments. Yet his life and his physics were indeed indistinguishable. From the time he coauthored his first paper with William Meggers[2] when he was 19 until he made his final entries in a bound notebook[3] when he was 95, he never stopped thinking about physics—or, as he would have expressed it, "doing battle with nature."

Wheeler liked to say that his lifetime was divided into three periods. First came the "everything is particles" period, extending from the early 1930s to the early 1950s, in which he focused on electrons, positrons, photons, muons, and atomic nuclei, with time out for weapons work. Then came his "everything is fields" period, from 1952 into the late 1970s, when gravitation was at the center of his interest—with national defense still getting some attention. That period merged into the "everything is information" period of his later years, when he explored the fundamentals of quantum measurement and introduced ideas such as the mutability of physical law and what he characterized as "it from bit."

In this article, I describe some of John Wheeler's major achievements in the early part of his career. The companion article by Charles Misner, Kip Thorne, and Wojciech Zurek on page 40 then picks up the story, recounting some of the principal achievements of his later years.

I must remark at the outset that Wheeler's physics was also inseparable from his teaching and mentoring. He took seriously his own oft-stated dictum: "If you would learn, teach!" Well over a hundred physicists owe their start in the field, their love of physics, and perhaps a good deal of their scientific achievements to his one-on-one guidance early in their careers. I was one of them, as were Misner, Thorne, and Zurek. And in addition to those individuals, there were thousands more who, over the years, were inspired in his classes.

In what follows, I allude here and there to important collaborative work that Wheeler did with his students or young colleagues. But I leave it to Terry Christensen, a historian of science, to discuss Wheeler's mentorship in more detail in the

**Figure 1. John Wheeler in Copenhagen** at age 23. (Photo courtesy of the AIP Emilio Segrè Visual Archives.)

other companion piece in this issue (page 55).

## "Everything is particles"

When Wheeler received his PhD from the Johns Hopkins University in 1933, he was not yet 22. In his dissertation, guided by Karl Herzfeld, he used the still relatively new quantum mechanics to study the scattering and absorption of light by the helium atom. In the process, he gained a lifelong affection for dispersion relations.

With a National Research Council (NRC) fellowship in hand, he considered, but rejected, doing a postdoctoral year with the 30-year-old J. Robert Oppenheimer. There was something about Oppenheimer that bothered Wheeler—something not cured by their frequent interactions in the decades that followed. As Wheeler put it much later in his autobiography, "He did not convey humility or a sense of wonder or of puzzlement."[1]

Wheeler opted instead to work with the 33-year-old Gregory Breit at New York University. "Like me," Wheeler later wrote of Breit, "he seemed to be always puzzling and was not afraid to let his puzzlement show."[1] With Breit, Wheeler learned what was then called "pair theory" (the quantum theory of electrons, positrons, and photons), and together they calculated the cross section for the scattering of light by light, a process unknown in classical physics.[4] Not until 1997, with high-powered lasers available, was the process observed.[5] How fortunate for today's ubiquitous wireless communication that photons largely ignore each other.

With a renewed NRC fellowship, Wheeler decided to spend his second postdoctoral year with Niels Bohr in Copenhagen. That seems to have been a year of mostly learning and consolidation for him. Wheeler, shown in figure 1, and a fellow postdoc, Milton Plesset, did coauthor three papers during that year, on different aspects on the interaction of gamma rays with heavy atoms. One of those papers got shelved, forever as it turned out, because Bohr found it merely "interesting." Another was put aside because it called for more calculations than the authors were able to complete. The third one, on pair production by inelastic scattering of gamma rays, did get published.[6]

Bohr's effective veto did not in any way diminish the enormous admiration and affection that Wheeler developed for Bohr. Wheeler would continue to hold him in the greatest esteem. There is even some evidence that, years later, Wheeler's muted reaction to the introduction by his student Hugh Everett of what came to be called the "many worlds" interpretation of quantum mechanics was shaped by his concern over what Bohr might think of it. In Wheeler's later years at Princeton University, he displayed likenesses of just two people on the wall of his office, Bohr and Johannes Kepler. Earlier, at the University of Texas, Charles Darwin and Abraham Lincoln had places of honor in his office.

Immediately after returning from Copenhagen in June 1935, Wheeler married Janette Hegner, to whom he had become engaged a year earlier. They moved to Chapel Hill, where Wheeler was a faculty member at the University of North Carolina for the next three years. Their first two children, Letitia and James, were born in North Carolina in 1936 and 1938.

Wheeler's interest at the time focused on nuclear forces and nuclear structure, simply because he considered those subjects to be central to the basic physics of the time. Perhaps his most notable paper at North Carolina was on what he called resonating group structure in nuclei,[7] a precursor of later work uniting single-particle and collective aspects of nuclear structure. It was in this paper that he introduced the *S*-matrix (*S* for "scattering"). Interestingly, Wheeler's first doctoral student was a woman, Katharine Way, who went on to a significant career in nuclear physics.

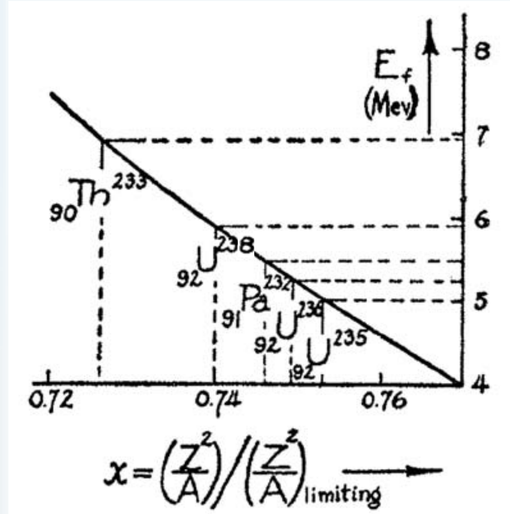


**Figure 2. From the 1939 paper** by John Wheeler and Niels Bohr on the theory of nuclear fission,[9] this graph implies a momentous conclusion that the authors do not point out. The vertical scale marks the estimated threshold energy $E_f$ that a captured neutron must provide to induce fission. The horizontal-axis variable *x* is the square of the nuclear charge *Z* divided by the mass number *A* (including the captured neutron), normalized to the stability limit of that quotient. For the then-hypothetical nucleus $^{239}94$, now known as plutonium, *x* is 0.770, appreciably greater than that of uranium-235. So its fission threshold energy implied by the theoretical curve in the figure would be less than that of $^{235}U$, the rare isotope to which the authors ascribed the recently observed fission.

During that period, Wheeler collaborated with Edward Teller, then at George Washington University, on a paper on nuclear rotational states, probably the first published work on that subject.[8] Nuclear rotation would later become an important part of the "collective" model of the nucleus. Wheeler had met Teller in Copenhagen (was anybody *not* in Copenhagen?) and they developed a lifelong friendship. As it turned out, they saw eye to eye on matters of national defense and the interface of politics and science. They wrote no more joint papers on pure physics, but they did collaborate on various studies related to defense research.

## Princeton and fission

In 1938 Wheeler moved to Princeton, which was to be his home for about 50 of the next 70 years. He did leave Princeton for periods of time: for the Manhattan Project during World War II, for work on the H-bomb in the early 1950s, for visiting professorships at various universities in the US, Europe, and Japan, and in 1976 for a 10-year appointment as a professor of physics at the University of Texas in Austin. He called all of his work in pure physics after North Carolina and before Texas, wherever he might actually have done it, his "Princeton physics."

By chance, Wheeler was the first person in America to hear about nuclear fission (see Wheeler's recollection, reprinted on page 35). Bohr said nothing about it when Wheeler met him at the dock in New York on 16 January 1939, even though he had apparently thought of little else during the Atlantic crossing. Bohr wanted to keep the news under wraps until Otto Frisch and Lise Meitner had time to prepare and submit a paper giving their interpretation of the December 1938 results of Otto Hahn and Fritz Strassmann in Berlin. But Leon Rosenfeld, who had traveled with Bohr, didn't know about this "embargo," so he excitedly described fission to Wheeler as the two of them rode the train to Princeton that afternoon. Bohr was not on the train with them. He and his son Erik stayed in New York to spend some time with Enrico Fermi before going on to Princeton. By that evening, physicists in Princeton knew. By the next day, those in New York knew. Within two weeks, the world knew.

When Bohr reached Princeton, he asked Wheeler to join him in working out the theory of nuclear fission. Surely, that had not been the originally intended purpose of his visit. The collaboration extended over about five months, half of that time in person while Bohr was still in Princeton, and half by letter after Bohr returned to Denmark. From it came the now-famous paper "The Mechanism of Nuclear Fission,"[9] published on 1 September 1939, the very day that Germany's invasion of Poland launched World War II.

It is an interesting oddity of history that the Bohr–Wheeler paper made no explicit mention of element 94, now called plutonium, even though it was evident from a graph and numbers in the paper that the isotope of that element with mass number 239 should, like uranium-235, be fissionable by slow neutrons (see figure 2). Wheeler has told me that he and Bohr were aware that $^{239}94$ would probably be readily fissionable, but they chose not to mention it because they hadn't conceived of the possibility of making the transuranic isotope in quantity.

It remained for Louis Turner at Princeton in the spring of 1940 to point out that not only was this yet-to-be-created isotope likely to be readily fissionable but, more significantly, it could be manufactured in (yet to be built) reactors—in kilogram quantities suitable for nuclear weapons. As it would turn out, Wheeler's main task in the Manhattan Project was to contribute to the design and operation of the plutonium-producing reactors in Hanford, Washington.

Between his work on the theory of fission in 1939 and his joining the Manhattan Project early in 1942, Wheeler's most notable contributions came in collaborative work with his graduate student Richard Feynman, who completed his PhD in 1942. As Wheeler has recounted in his autobiography, in 1940 or 1941 he conceived of the idea that a positron can be viewed as an electron moving backward in time. He was so excited by the idea, he wrote, that the very evening when it came to him at home, he called up Feynman at the Graduate College, where Feynman lived. "Dick," he said, "I know why all electrons and all positrons have the same mass and the same charge. They are the same particle!"[1] In his 1965 Nobel Prize lecture, Feynman credited Wheeler with ideas that were key to his own achievements in quantum electrodynamics.

Wheeler and Feynman also pursued work in classical electrodynamics, including the working out of a field-free theory of action at a distance—in which, indeed, "everything is particles." That work was interrupted by Japan's 7 December 1941 attack on Pearl Harbor. But Wheeler and Feynman were able to make progress on it in their occasional meetings in Los Alamos during the war. The result was two important papers, one in 1945 on radiation reaction and one in 1949 on action at a distance.[10]

Also during the war years, Wheeler responded to a competition of the New York Academy of Sciences and submitted a paper entitled "Polyelectrons,"[11] which won the prize and was published in 1946. In it, he worked out the theory of positronium and also calculated the properties of a positronium ion (two electrons and a positron), an entity that was finally detected in 1981 by Allen Mills, who, some 20 years earlier, had completed his junior paper and senior thesis at Princeton under Wheeler's guidance. The polyelectron paper was, Wheeler confessed, much more modest than the grand vision he had harbored for some years that perhaps the universe in its totality is made only of electrons, positrons, and photons.

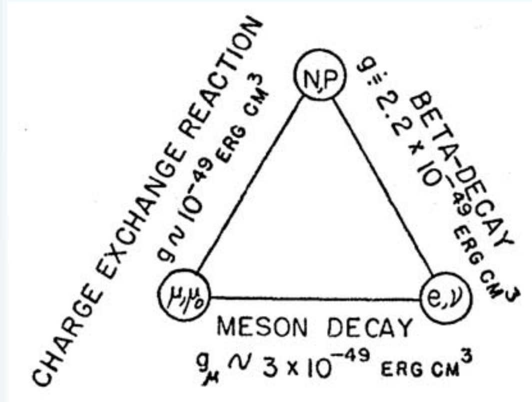


**Figure 3. The "Tiomno triangle,"** from a 1949 paper by John Wheeler and Jayme Tiomno, illustrates their suggestion that a universal weak-interaction of coupling constant $g$ governs not only nuclear beta decay (the triangle's right leg) but also muon decay (bottom leg) and nuclear muon capture (left leg). The symbol $\mu_0$ refers to the muon neutrino, which the authors assumed to be distinct from $v$, the putative beta-decay neutrino. Giampietro Puppi's contemporaneous work generalized the weak-interactions universality to include pion decay.[15]

## The postwar years

For a while after the war, Wheeler continued to work in nuclear physics, the area that had most attracted him in the 1930s. In the fall of 1952, he and David Hill had written a long paper on fission and the collective model.[12] And at the January 1953 meeting of the American Physical Society, Wheeler gave a talk on the subject. But, increasingly, he came to feel that nuclear physics was not the arena that would lead to fundamental insights about nature.

In particular, Wheeler stayed away from pion physics, or what he called "the pion industry." He was much more excited by the identification in 1947 of a "heavy electron" (the muon) in cosmic radiation. Like the electron but unlike the pion, the muon has no strong nuclear interactions. That gave it, for Wheeler, the kind of simplicity he found attractive. He also realized that the negative muon, by virtue of the fact that it is 200 times heavier than the electron, could be a very useful probe of the nucleus, and he initiated the study of muonic atoms in 1953.[13]

Four years earlier Wheeler and his graduate student Jayme Tiomno had worked out the theory of the universal Fermi weak interaction[14] and offered a diagram, which Wheeler liked to call the Tiomno triangle, to illustrate that universality (see figure 3). But a different version of the diagram, based on similar work by Giampietro Puppi in Italy, became more widely known as the Puppi triangle. The history here is a little murky. Puppi's first publication on a

**Figure 4. The Project Matterhorn** B team and support staff at Princeton University in 1952. The B team, headed by John Wheeler, devoted its efforts to thermonuclear weapons. Most of its scientists were in their twenties. Left to right, front row: Margaret Fellows, Peggy Murray, Dorothea Reiffel, Audrey Ojala, Christine Shack, Roberta Casey. Second row: Walter Aron (with climbing rope apropos of the project's name), William Glendenin, Solomon Bochner, John Toll, Wheeler, Kenneth Ford. Third and fourth rows: David Layzer, Lawrence Wilets, David Carter, Edward Frieman, Jay Berger, John McIntosh, Ralph Pennington, unidentified, Robert Goerss. (Photo by Howard Schrader, courtesy of Lawrence Wilets.)

universal Fermi interaction predated the Tiomno– Wheeler paper by about a month, but neither it nor Puppi's later exposition of his ideas on the subject contained such a diagram.[15]

Even in his first years as a researcher in the 1930s, Wheeler could see that cosmic rays offered a source of particles far more energetic than could be imagined coming from any accelerator then envisioned. Right after the war, he promoted and secured a cosmic-ray laboratory at Princeton. "I was in a hurry," he wrote, "and I wanted to see particle research conducted on my doorstep."[1]

There is one more story to tell from Wheeler's "everything is particles" period. In the summer of 1949, armed with a Guggenheim fellowship, he and his family, with his graduate student John Toll in tow, traveled to Europe. Wheeler meant to divide his time between some very far-out ideas (a purely electromagnetic world and a world without spacetime) and the more mundane physics of nuclear structure. He chose to settle in Paris, from where he could travel now and then to Copenhagen. As he wrote later, "I did not want to get back fully into the conversational culture of [Bohr's] institute."[1]

In the spring of that year he had submitted a draft manuscript to Bohr, suggesting that it be coauthored by Bohr, Wheeler, and Hill. Bohr agreed, and in a letter said, "I should like to think a few days whether I might suggest some smaller alterations or additions."[1] Three years later Bohr, still not wholly satisfied with the paper, suggested that it be published by Wheeler and Hill alone, as it finally was.[12] In the fall of 1949, early in that hiatus, Wheeler came up with an explanation for the large deformations observed in some nuclei. He conveyed the idea to Bohr with the hope that it would be incorporated into their joint paper. Bohr sat on the idea and a year later Wheeler received a preprint from James Rainwater setting forth the same idea.[16] For that work, Rainwater shared the 1975 Nobel Prize in Physics. Was Wheeler at least a little irked with Bohr? Not one whit, as far as I have been able to determine. The most that Wheeler could bring himself to say was, "I learned a lesson. When one discovers something significant, it is best to publish it promptly and not wait to incorporate it into some grander scheme. Waiting to assemble all the pieces might be all right for a philosopher, but it is not wise for a physicist."[1]

## Weapons work

Most physicists who were beyond their teen years in 1941, even if only by a little, signed up for war work. In January 1942, a month after Pearl Harbor, Wheeler joined Arthur Compton's "Metallurgical Laboratory" at the University of Chicago, at first leaving his wife and two children in Baltimore, where, later that year, his third child, Alison, was born. In characteristic fashion, Wheeler looked beyond the first reactor (or "pile"), on which Fermi and some of his colleagues were working. He began thinking about the large plutonium-production reactors that would follow.

After being assigned by Compton to be the lab's principal liaison scientist to DuPont, the company charged with designing and building the new reactors, Wheeler went in 1943 to the company's headquarters in Wilmington, Delaware. He had already recognized the possibility that some as-yet-unknown fission fragments might have anomalously large neutron-absorption cross sections and thus might poison the chain reaction. So he supported the overdesigning of the reactors—something that DuPont's conservative engineers were inclined to do anyway.

Wheeler's caution paid off. On 27 September 1944, the first Hanford reactor, within hours of being powered up to its initial plateau of 9 megawatts, began to fizzle and die. After being turned off and "rested" overnight, the reactor was powered up again, only to exhibit the same distressing pattern of gradual demise. Wheeler, analyzing the ups and downs of the reactor's behavior over its first two days, concluded that the culprit was an isotope with a half-life somewhat less than 11 hours that was itself the daughter of another radioactive isotope. It took him only a few minutes standing before a chart of the nuclides on the wall outside his Hanford office to conclude that the offending isotope was xenon-135, with a half-life of 9.2 hours, the daughter of iodine-135. That proved to be correct, and the problem was solved by adding more uranium rods in the vacant tubes that had been provided in the reactor for such a contingency. But the subsequent operation of the reactor required great care in manipulating the control rods, because the reactor had, in effect, two modes of operation: a poison-free mode when starting up at low power and a poison-rich mode when operating at high power.

Four years after the war ended abruptly with the atomic bombing of Hiroshima and Nagasaki, the detonation of a test nuclear explosion by the Soviet Union in the late summer of 1949 brought Wheeler back into weapons work. Heeding a call from Teller, he interrupted his Guggenheim year in Paris to join Teller and others in Los Alamos to work toward a hydrogen bomb. A year later, after a critical design idea advanced by Teller and Stanislaw Ulam made success look likely, Wheeler gained approval to set up a satellite operation at Princeton, which came into being in the late spring of 1951.

Called Project Matterhorn, the Princeton operation had two parts. Part A, as I recall the name, was headed by Lyman Spitzer and devoted to controlled thermonuclear power. Some people have told me it was called Part S, for Spitzer's Stellarator. In any case, that part lives on today as the Princeton Plasma Physics Laboratory. Part B, headed by Wheeler, was devoted to the bomb. Most of Wheeler's small staff were graduate students and fresh postdocs (see figure 4). Convincing senior scientists to join the project, even in the academic setting of Princeton, proved to be as difficult as getting them to return to Los Alamos.

Nevertheless, Wheeler and his young staff made important contributions to the design of the first thermonuclear test device and, in a computing tour de force, successfully predicted its yield to within 30%. Wheeler's personal contribution to that effort was to reduce what was known or guessed about reaction rates and the properties of matter in extremis to a set of coupled differential equations of such simplicity that they could be handled numerically on a then-available computer—the National Bureau of Standards SEAC machine—whose total memory capacity was less than 3 kilobytes.

Matterhorn B closed up shop in 1953, by which time Wheeler was well launched on his new career in relativity and gravitation, which is the subject of the article by Misner, Thorne, and Zurek.

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