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Feynman's War: Modelling Weapons, Modelling Nature

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What do I mean by understanding? Nothing deep or accurate—just to be able to see some of the qualitative consequences of the equations by some method other than solving them in detail.

Feynman to Welton, 10 February 1947.

1. Introduction: Modular Culture

In all of modern physics there are few elements of theory so pervasive as Feynman diagrams—Julian Schwinger once sardonically remarked that these simple spacetime figures brought quantum field theory to the masses. In reaching ‘the masses’ this graphic synthesis of quantum mechanics and relativity played a formative role in the development of quantum electrodynamics, meson theory, S-matrix theory, condensed matter physics, the standard model of particle interactions, and more recently, string theory. Part representation, part abstract symbolism, these line diagrams constitute one of the most intriguing and precise forms of modelling afforded by the physical sciences. With stakes that high, it is not surprising that historians, foremost among them Silvan Schweber, have both followed the growth of quantum electrodynamics after the war in detail and precisely tracked antecedents of the Feynman diagrams from cloud chamber photographs to schematic electronic diagrams; from E. C. G. Stueckelberg’s spacetime diagram drawn in the 1930s showing a positron as an electron moving backwards in time to John Wheeler’s emphasis to his student Feynman that all physics could be understood through scattering.¹ Once the graphs come

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¹ The definitive work on Feynman’s pre- and postwar physics in Schweber (1994); see for example, his discussion of Stueckelberg (1942).

into existence, a subsequent history can be traced that includes their effect on pedagogy through the provision of an image-based knowledge. Here I find especially helpful the work of David Kaiser.²

My aim here is different: I, too, am concerned with the historical location of Feynman diagrams. But I will focus on a specific pattern of theoretical modeling that emerged in wartime Los Alamos, one I will refer to as the *culture of modularity*. This war-tempered stance towards theory stressed the development of widely applicable elements of theory that could be combined the way modularised instruments could be engaged one way and then rearranged in another. By exploring how Feynman contributed to and drew from this wartime approach to theory, we will be able to cast his immediate postwar work in a different light. It is *not* my view that the war work ‘caused’ Feynman to create his version of quantum electrodynamics: quantum electrodynamics theory played essentially no role at all in the building of the atomic bomb. Instead, I aim to capture something that is both more interesting and more subtle. I will argue that the war work, which lasted at full intensity for two and a half years (plus an additional eighteen months during which Feynman wrote up his results) helped shape Feynman’s very approach to theory. Grappling with an extraordinary array of problems at Los Alamos, Feynman refined a distinctively modular, rule-oriented style of theorising that he carried into the postwar era.

It should be said, and kept in mind, that, unlike Schwinger, Feynman never pointed to the role of war work as pivotal for his postwar work. Schwinger could not have been clearer: ‘[T]hose years of distraction’ at the MIT Rad Lab, Schwinger wrote, were more than mere distraction. ‘The waveguide investigations showed the utility of organizing a theory to isolate those inner structural aspects that are not probed under the given experimental circumstances [...]. And it is this viewpoint that [led me] to the quantum electrodynamics concept of self-consistent subtraction or renormalization’ (Galison, 1997, Ch. 9; Schwinger, 1980, p. 16). Lacking such a manifest link, I aim here to explore the character of Feynman’s war work in order to capture a mode of theoretical practice that developed between his already-established style and the evolving culture of theory at the Los Alamos-based Manhattan Project. For while there are many ways to locate the genealogical roots of Feynman’s work—in the nineteenth-century mathematical physics of a Heaviside or a Green, in early quantum mechanics, in scattering theory, or in the roots of prewar visualisation—I would argue that the stature of Feynman’s approach changed in the war, in his eyes and in others’. By the end of the war, Feynman and his group could calculate things about the bomb core that no one else could touch. And this confidence gave a new epistemic purchase to his postwar modular approach to diagrammatics.

² Kaiser (s.d.) has a series of important studies now in press or in preparation. These include ‘Stick-Figure Realism’, submitted to *Representations* and ‘Do Feynman Diagrams Endorse a Particle Ontology?’.

The identification of originary sources of ideas is surely part of what we want from a history of theory. In understanding the creation of Monte Carlo simulations, for example, it is significant to know that statistical sampling was a tactic used by some prewar statisticians or that random processes have been employed since antiquity—as in the famous dart-throwing method—to generate an approximation to π . But in the years just after World War II, the combination of nuclear weapon design with stochastic techniques, pseudo-random number generation, and the new electronic computers completely altered the face of calculation: Monte Carlos moved from an isolated technique to a central method for the resolution of nuclear weapon design, and then more generally to problems of reactor building, particle interactions, radiation transfer, aerodynamics, hydrodynamics and even pure mathematics (Galison, 1997, Ch. 8). Similarly, the roots of self-regulating systems can be tracked back to hydraulic systems in antiquity—but when electrical feedback came to govern debate over World War II air defense, self-guided torpedoes and proximity fuses, a cohort of experts emerged in numbers strong enough to make the burgeoning field of postwar cybernetics a going concern. In each such instance, and there are numerous others, it is essential to engage both in a 'filamentary' conceptual history as *lineal descent* and to reckon with a more dispersed sense of historical development, one that takes into account the staggering scale of techniques deployed repetitively and extensively at the heart of scientific warfare.

And so I will maintain that the spreading *roots* of the modular culture of theory go back to the whole of early twentieth century physics, to Dirac's bra and ket notation, to Feynman's own work on the absorber theory and least action formulation of quantum mechanics—and before, no doubt, to Green himself. But I will also insist that the *efflorescence* of Feynman's modular and visualisable approach to theory took place in Los Alamos, in the high-pressure laboratory-factory world of the first atomic bombs.

When Feynman was preparing to introduce his first published 'Feynman diagram' in the spring of 1949, he told his readers: 'We shall be discussing the solutions of equations rather than the time differential equations from which they come' (Feynman, 1949, p. 771). For Feynman, the diagrammatic, solution-oriented presentation would make the simultaneous satisfaction of relativity and quantum mechanical constraints more evident—and, most importantly, the diagrams focussed attention on the underlying *physical processes* of particle scattering. But Feynman's papers did not look like Schwinger's in many ways—not just because of the presence or absence of diagrams. Feynman's papers of 1948–1949 were all built around solutions, around simple expressions that 'moved particles' from point to point. For example, he let $K(3, 4; 1, 2) = K_{0a}(3, 1) K_{0b}(4, 2)$ stand for the amplitude that a particle labelled 'a' will go from (x_1, t_1) to (x_3, t_3) , and a particle 'b' from (x_2, t_2) to (x_4, t_4) (see Fig. 1). And when he came to the diagrams that have an intermediate particle being exchanged, he continued with a distinctly physical, particulate picture of how to imagine the subvisible world.

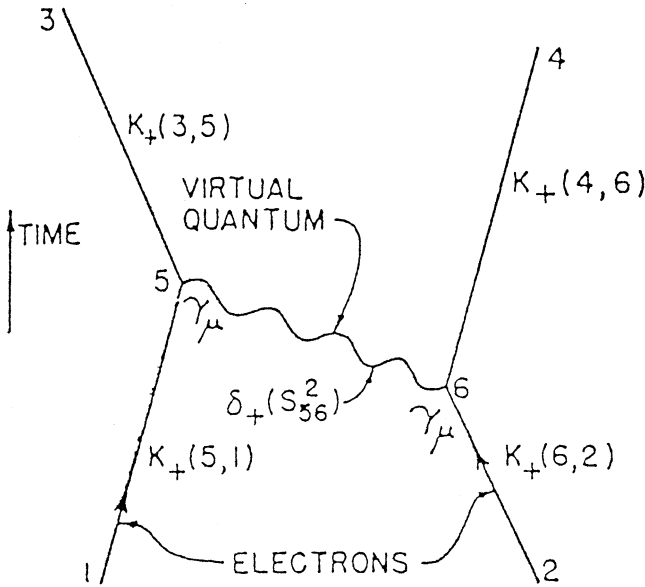


Fig. 1. First Published Feynman Diagram. Source: Feynman (1949, p. 772).

$K(5, 1)$ took particle a from (1) to (5), where it absorbed a quantum, then headed off from (5) to (3). Particle b travelled from (2) to (6), emitted a quantum, and then proceeded to (4). As Feynman made clear, the whole point was to introduce these elementary pieces—the propagators—in such a way that their generalisation to more complicated processes would be immediately evident. It was a rule-governed game, with interchangeable pieces that snapped into place: inserts for absorption, for emission, for external lines, for internal lines. When the elements were set, it only remained to integrate over the various allowed states, yielding a probability amplitude for the process in question. It is this approximate, visualisable, quantitative and modular approach that I want to help locate. But to do so, as if with Feynman's electrons, we need to travel back in time.

In the 1930s, Feynman had precociously rocketed himself through MIT's undergraduate curriculum, beginning his exploration of solid state physics, quantum mechanics, general relativity and many other aspects of contemporary physics. Under the tutelage of John Wheeler, at Princeton, Feynman learned to take radically new, wider, almost philosophical perspectives on physical problems. Wheeler told Feynman, for example, that all the electrons in the world might actually be one single electron travelling back and forth through time (Schweber, 1994, pp. 372–397). Together, they explored the twin solutions to Maxwell's equations, one a delayed potential (the 'usual' solution that travels at light speed) and the other an advanced potential (the 'unphysical' solution

normally dismissed for violating causality). But instead of cutting the advanced potential out of physics by *ad hoc* excision, they introduced an absorber, rendering the theory acceptable with *all* its solutions intact. Feynman's final work with Wheeler was on his Ph.D. thesis, in which the young doctoral candidate introduced the idea of the path integral formalism: the probability amplitude of a particle in a state $|q, t\rangle$ going to a different state $|q', t'\rangle$ is given by the sum of the paths from one point to the other:

$$\langle q', t' | q, t \rangle = \frac{1}{A'} \sum \exp \left[\frac{i}{\hbar} S(\Gamma) \right], \quad (1)$$

where Γ is a path from (q, t) to (q', t') , $S(\Gamma) = \int_{\Gamma} L(q, \dot{q}, t) dt$, for paths Γ and the sum is over paths. The factor $1/A'$ is a calculable coefficient. It was a new way of looking at physics, radical in its way, but (at least at its moment of inception) without experimental consequences.

After the war, as we know, various pieces of the puzzle fell into place: a now-visualisable version of the path-integral formulation, a reformulated positron theory, and a quantum electrodynamics with its elementary processes visualisable through the diagrams. As Silvan Schweber has pointed out, when Feynman wrote up his *Reviews of Modern Physics* article on the path integral in the summer of 1947, 'there was now an unmistakable visual aspect to the formalism. Although no pictures appear in the paper, the text explicitly enjoins the reader to conceive of the amplitude for a particle to go from (x, t) to (x', t') as receiving contributions from all the trajectories' that can be drawn between them. It was only *after* the war that Feynman reported, 'I could see the path' (Feynman, 1942; Schweber, 1994, p. 409). Along with these paths came the new formulation of quantum electrodynamics in which diagrams represented on one side a process, such as an electron radiating a photon, and on the other side algebraic elements that went into a well-defined integral. It made calculation of any given process a modular matter: write down the appropriate diagrams, invoke the rules, and then calculate the integrals.

What happened to produce this visual approach in its modular brilliance? I aim here to explore Feynman's passage from the prewar years to the postwar era, reglossing, to a certain extent, the elements that we know were in place by 1942, and taking seriously Feynman's less well-known wartime physics. I will have nothing to say about Feynman's often-told stories of his virtuoso safe cracking and drumming in the woods, and instead will focus on recently declassified nuclear weapons work and what it can tell us about the striking theoretical culture of Los Alamos. In this sense I am less interested in Feynman's biography *per se* than in his function within the development of mid-twentieth century theory.

The order of the argument is this: I break Feynman's war work into a series of 'projects', each of which put him in contact with people and forms of work far from anything he had seen in his doctoral work with Wheeler. With a textured understanding of Feynman's war work, it will then be possible to characterise

that part of Feynman's approach to physics that remained consistent through the war and, at the same time, to throw into relief striking changes over that same time. Combining the synchronic and diachronic components of Feynman's theoretical style, we will be able to view his postwar calculational rules and diagrammatics in a new way.

The projects are these: first, soon after arrival, Feynman was enmeshed in the study of instruments and experimental devices, giving him direct contact, for the first time in his career, with what he called the 'realities' of set-ups such as counter experiments designed to measure the number of fission neutrons produced in a sample of U-235. These realities demanded an almost tangible picture of the passage of neutrons through the apparatus; they also immersed him in the grubby details of losses, distortions, cross sections, the properties of materials. Second, Feynman became directly involved in the 'Water Boiler', a small nuclear reactor designed to experiment on fundamental properties of the chain reaction. Here Feynman used fluctuations in the number of coincident neutron counts to allow the measurement of how close a particular assembly of active material was to criticality. Third, Oppenheimer sent Feynman to Oak Ridge as a safety supervisor—his job was to offer advice to the plant supervisors so they could avoid accidentally driving its waste or products into a deadly critical mass. Facing military brass and a wide range of architects, chemical, and mechanical engineers, Feynman had to provide clearly-applicable rules and procedures that would allow his interlocutors to understand where the flow of neutrons presented a direct threat. Fourth, Feynman developed an integral theorem that fixed one critical distribution of neutrons and active material (for a bomb or reactor) in terms of another. And in the fifth project, theory division leader Hans Bethe assigned Feynman the numerical calculation of implosion. Here again Feynman structured the task into repetitive, rule-governed, modularised operations in an efficient order, tracking the state of the plutonium core as it imploded towards criticality. This brought the abstract differential equations to the bottom line: how hot, how fast, how much yield?

Dwarfing all these projects was Bethe's delegation of the 'hydride bomb' to Feynman. This weapon, imagined to function around a uranium hydride core, was supposed to have run on far less precious U-235 than the 'ordinary' metal bomb. Hydrogen atoms would slow the neutrons, and slow neutrons cause more fission than fast ones. But understanding the diffusion of neutrons in the hydride presented one of the thorniest theoretical problems of the war: how would the chain reaction work where one could not assume (as all other theory groups had) that neutrons all had the same energy?

Taken together an understanding of these wartime modelling methods will make sense of Feynman's scientific trajectory from his remarkable, yet still abstract studies with Wheeler on the absorber theory and Lagrangian formulation of quantum mechanics, to the experimentally-directed, visualisable, modular rendition of theory, with which he commenced his postwar research. Feynman's vision of theory, as instantiated in his (secret) version of neutron diffusion and then (very public) rendition of quantum electrodynamics, helped

found a form of theory of enormous power in the years after World War II. This was an orientation of theory that stressed physical pictures and plausible rules of calculation over mathematical rigour, over interpretive philosophy and over formal aesthetics.

The fundamental problem facing theorists on the bomb project was this: in a limited time, they had to produce accurate, quantitative predictions of the efficiency and critical mass of the chain reaction in a wide variety of geometries. There was no time to devise detailed models for each configuration of fissile material and neutron-reflecting tampers, just as on the radar project physicists could not start calculating *ab initio* for each new arrangement of waveguides and junctions. At MIT, the radar physicists had to provide effective circuits for the various waveguides so the radio engineers could manipulate them. Similarly, for the Los Alamos physicists facing engineers, architects, and experimentalists, much rode on the theorists' ability to modularise aspects of their work so it could be passed to non-theorists.³ They had to figure out ways of characterising the 'neutronics' using certain building blocks—whether those building blocks were standardised effective amplifiers or new theoretical techniques to model neutron diffusion.

Feynman learned from and contributed to this culture of modularity. Whether he was grappling with the human efficiency of crunching numbers using Marchant calculators, or inventing easily taught rules for tracking neutrons in tampers, Feynman developed highly movable *theoretical modules*. These simple, often visualisable mechanisms took complex human, physical and calculational configurations and sorted them into simpler parts that could be recombined in a myriad of ways to calculate rapid, approximate, yet reliable answers. It was a kind of theory particularly appropriate to the constantly rearranged devices they were to represent.

Of course, some aspects of Feynman's work continued into the war from the prewar period. In particular, both the absorber and the Lagrangian formulation of quantum mechanics shifted attention towards the solutions, away from the fundamental equations of motion. And during the war, he continued on that track, though most other theorists thought differently about neutron diffusion. Most of the Los Alamos theorists (those in Serber's group, for example) launched their studies from the equation of motion for the neutrons—the Boltzmann transport equation—and then simplified it so that it could be solved in particular, idealised circumstances. By contrast, Feynman sidestepped the Boltzmann equation altogether, choosing instead to deal with its elementary solutions. He then built up complex solutions out of the simple elements.

³ One might conjecture that the production of theoretical or calculational rules would be particularly associated with interdisciplinary or inter-speciality coordination. For rules are most useful precisely when the recipients of the rule do not share the background knowledge of the donor group. This process of regularisation when speaking to outside groups bears some similarity to the dynamics of the formation of interlanguages. See Galison (1997, Ch. 1, 9).

Formally, Feynman focussed on the Green's functions or 'kernels'⁴ $K(x \rightarrow x')$, which gave the likelihood of a neutron at x being found a certain time later at point x' . Summarising Feynman's approach to theory, one might present the contrast between the equations of motion and the space of solutions through the following scheme:

Feynman avoids fundamental equations: And instead focusses on the solutions:

1. Maxwell's Equations	Advanced and retarded potentials
2. Schrödinger Equation	Path integral formal solution
3. Detailed electronics	Effective circuit formulation
4. Boltzmann Equation	Kernel Method
5. Hamiltonian formulation of QED	Green's Function Approach

My concern in this paper is to explore the historical circumstances that enabled Feynman's modular, pictorial, and proudly unmathematical approach to theoretical physics to emerge with such force. There are two components of this flourishing: Feynman's own scientific 'style of reasoning' and the specific 'theoretical culture' of wartime Los Alamos. (By style of reasoning, following Hacking and Davidson, I have in mind the characteristic choice of certain poles from a series of polar choices among concepts.⁵) By the end of the war, Feynman, as I will argue here, tended to emphasise concepts as they were used to express the solutions of specific problems in visualisable, not formal, terms; to use plausible not rigorous approximations; to invoke particulate not field-theoretic elements; and to build up from the simple to the more complex, rather than deduce downwards from general equations. Feynman aimed to begin his analyses through a physical, almost tangible picture of basic *processes* in the subvisible world; he did not tend to propel his analysis with the assumption of broad physical principles (such as manifest gauge invariance, spacetime covariance, or the principle of equivalence). These binary choices—and others related to them—are what I have in mind by Feynman's style of reasoning as it stood, tempered by the wartime work, in the late 1940s.⁶

One aim here is to characterise the formation of Feynman's theoretical style with as much precision as possible. But a second goal is to understand how that shift in style took place in the hothouse world of wartime Los Alamos.

⁴ A Green's function is the solution to a differential equation. For present purposes, the kernel and the Green's function are the same, though there are certainly kernels that are not (differential) Green's functions.

⁵ On the notion of scientific style, see Hacking (1982) and Davidson (1992); also Hacking (1996) and Davidson (1996).

⁶ Other styles even in closely-related areas of physics may look quite different—one style may raise and solve certain problems that another obscures; and yet elsewhere their relative success may invert. The highly mathematical field theory of the Russian school of Bogoliubov and his colleagues led to results in quantum field theory that would have been less manifest in the more phenomenological, experiment-oriented field theory. Conversely, theorising close to experiment was essential to the American development of weak interaction theory in the 1950s.

For in his war-pressured encounter with machines, experimentalists, chemical engineers, military personnel, architects and the human 'computers', Feynman found himself immersed in a world quite different from that of an undergraduate at MIT or a Ph.D. student at Princeton. Bomb work in the war assigned particular value to certain kinds of work. Builders pressed Feynman to know if a storage area of uranium in solution would catastrophically go critical, computers urged him to improve algorithms to increase the number of cycles they could process in the least time, designers had to know which configurations would yield the greatest explosion, generals demanded of everyone in the laboratory that the bomb be ready in time to make a difference in *this*, not some future war. In such an environment, not all theory was equally prized.

Those aspects of theorising most highly valued by the lab community are what I have in mind by referring to the *modular theoretical culture* of wartime Los Alamos, an encompassing set of values and practices quite different, it should be said, from that of other important theory-generating sites. Niels Bohr's 1927 Institute for Theoretical Physics in Copenhagen was a place where the analysis of concepts, language, and epistemology represented a valued part of theorising, whether it was debating the meaning of quantum jumps, the nature of complementarity, or the implications of the correspondence principle. At James Clerk Maxwell's 1870 Cavendish Laboratory, other aspects of theory were particularly stressed—for example, the link to mechanical analogies. And Edward Witten's late twentieth-century circle of string theorists within the Institute for Advanced Study held its own characteristic culture, at least provisionally replacing attentiveness to experimenters by a determined alliance with algebraic geometers.⁷ Each of these approaches to theory emerged at specific historical moments, and each was sufficiently distinctive to have its detractors, even among physicists who were well versed in the relevant technical areas. Such criticisms are useful for historical understanding: they highlight just what is new. Here, in the final section of the paper, the great Dirac serves as that critic: his uneasiness about Feynman-type theorising circled directly around the values expressed by the new modular-effective physics.

Oversimplifying, one might say this: a particular theoretical culture picks out certain styles of reasoning above others, and links concepts differently to wider fields of action and meaning, assigning some salience and consigning others to obscurity. One might imagine an orchestra playing in a specific concert hall; some notes ring out in resonance while others are damped down into the inaudible.

⁷ For a more detailed discussion of the notion of material and theoretical cultures, see Galison (1997). On Bohr's Copenhagen Institute, see e.g. Pais (1991, e.g. Chapter 14) and Cassidy (1992). On the Cavendish, its local theoretical culture and the role of theoretical technologies, see the particularly valuable double article by Warwick, 'Cunningham and Campbell' (1992, 1993); on Thomson and the contrast between his work and that of the Cavendish, see Smith and Wise's remarkable *Energy and Empire* (1989); on the ways in which string theory shifted the principal interlocutor from the experimentalist to the mathematician, see Galison (1995, s.d.).

2. Understanding Solutions

2.1. Confronting experimenters

In the spring of 1942, Feynman was finishing his thesis, all too aware that, as it stood, his reformulation of quantum mechanics was yet to make contact with laboratory consequences of quantum electrodynamics. ‘The final test of any physical theory’, he insisted, ‘lies, of course, in experiment. No comparison to experiment has been made in this paper’ (Feynman, 1942). But if his graduate work had not brought him into contact with the bench, other events would. While still working on his thesis at Princeton, Feynman was recruited by Robert Wilson to the atomic bomb project. Before the end of the day, Feynman was at a desk working on an isotope separation scheme. And before leaving for Albuquerque on 28 March, 1943 (Gleick, 1992, p. 160), Feynman found himself on a mission to Fermi’s Chicago Metallurgical Laboratory to find out what people there knew: ‘I was sent to Chicago with the instructions to go to each group, tell them I was going to work with them, have them tell me about a problem to the extent that I knew enough detail so that I could actually sit down and start to work on the problem, and as soon as I got that far to go to another guy and ask for a problem’ (Feynman, 1975, pp. 5–6). The plan worked, and Feynman was instantly engaged with the details of various experiments.

When Feynman arrived in Los Alamos, his wanderings between various groups brought him face to face with the experimenters and their specific set-ups. For example, in a meeting with two experimentalists, Thoma Snyder and J. McKibben, Feynman assisted them in formulating the protocol for their experiment on the fission neutrons produced as a neutron beam bombarded a sample of U-235. At the heart of the apparatus was a ‘long boron counter’, noted for its efficiency in counting fast neutrons independent of the neutrons’ energy—see Fig. 2 (Snyder, 1943). The proton beam entered from below, hitting a lithium target which produced 100 kV neutrons. These neutrons were slowed in a 25 cm-radius spherical volume of paraffin (CH₂)ⁿ, and emerged through a 4 cm by 4 cm hole as a collimated beam. The paraffin slowed the neutrons (making them more likely to cause fission in the U-235), and acted as a shield against neutrons emerging in directions other than those making a beeline for the target. A centimeter-thick veil of B₄C around the lithium target added further shielding against slow neutrons in stray directions. And a thin layer of neutron-absorbing cadmium protected against thermal (room temperature) neutrons (*ibid.*). When the collimated neutrons hit a sample of ‘25’ (U-235), some of the nuclei fissioned, and the fissioning neutrons took off in all directions. The boron counter, itself shielded by paraffin, B₄C and some cadmium, aimed to capture a representative sample of these fission neutrons. Knowing the beam strength of neutrons, Feynman and his experimental colleagues could then determine the number of fission neutrons per incident neutron.

Across the laboratory, in experiment after experiment, the output pulses of counters like the one used in the Snyder experiment had to be fed into amplifiers,

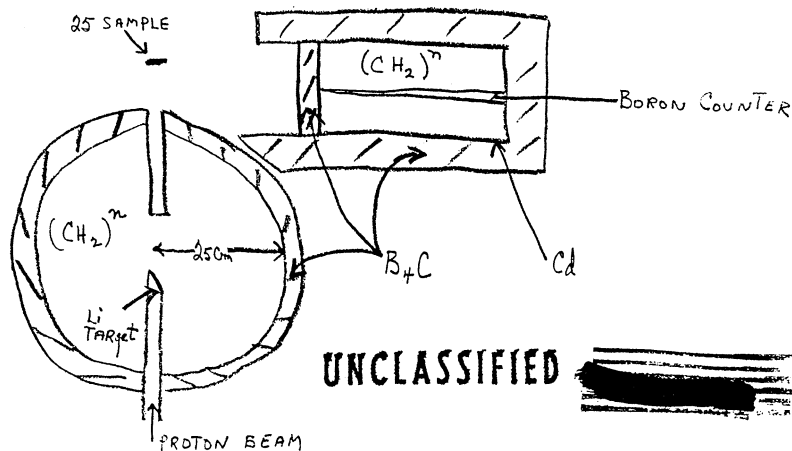


Fig. 2. Counting Neutrons. Source: Snyder (1943).

scalars and coincidence circuitry, each one of which risked introducing distortion. To bring some order into the chaos of such equipment being designed on an *ad hoc* basis, Feynman advanced a theoretical 'reference amplifier', an idealised device that distorted the signal at either the high or low end of its responsive range. His goal was to characterise this distortion by establishing a benchmark relationship between phase and amplification for each frequency, to be used as a black-box standard against which real amplifiers could be assessed.⁸ His strategy was to ignore the morass of inductors, capacitors, and triodes that populated the real amplifiers and focus on the *solution*: the ratio of input signal to output signal.

More specifically, Feynman reasoned this way. Suppose one sent a short-lived pulse (approximating a Dirac delta function) into an amplifier, and it came out as a response, $O(t)$. An arbitrary input signal $f(t)$ could be described as a superposition of these $\delta(t)$. Therefore the response to $f(t)$ would be a combination of the response $O(t)$ to each of the constituent $\delta(t)$.⁹ It would therefore be quite sufficient to establish a measure of the distortion in phase and amplification that

⁸ In a postwar letter Wheeler wrote to Feynman about a presentation Feynman had given just before the war, Wheeler remarked: 'I'm writing you now because I remember you gave a report at Journal Club one Monday evening in 1941 on the relation between phase change and amplitude gain for a linear amplifier. The little black box had two input leads and two output leads. The magician was able to deduce all he needed from the requirement that energy shouldn't come out of the box on the right hand side before it had been put in on the left', Wheeler to Feynman, 10 November 1949; Feynman to Wheeler, 8 December 1949, Richard P. Feynman Papers, Caltech Archives, 3.10 (hereafter RPPFP).

⁹ Specifically, if $f(t) = \int f(t')\delta(t - t')dt'$, then the response $R(t)$ to input $f(t)$ would be the weighted response to the various $O(t)$: $R(t) = \int f(t')O(t - t')dt'$.

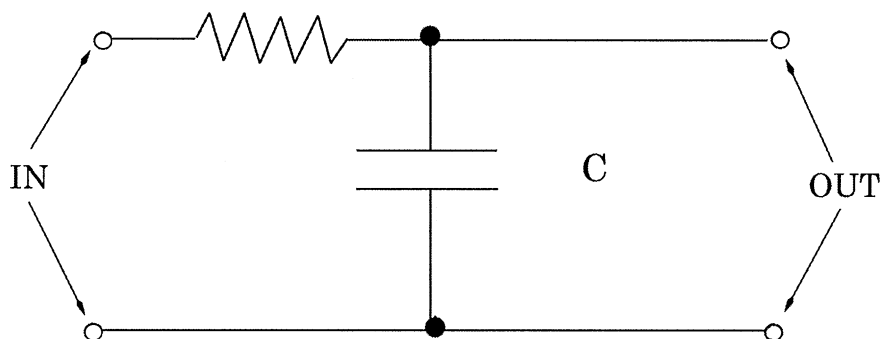


Fig. 3. *Effective Circuit, Amplifier Distortion*. Source: Feynman (1946b). [Reconstruction, original illegible].

the spike received as it passed through the amplifier. To do this, Feynman designed a simple ‘black box’ that would roughly simulate the high and low distortions introduced by amplifiers—as in Fig. 3.

For this particular effective circuit and for several others with high- and low-end distortions, Feynman carefully graphed the frequency versus phase change charts. Though hardly a radical step in electronics, Feynman’s systematic and simple analysis of distortion problems into a handful of clearly understandable reference curves represented quite a change from the contemplation of absorber theory and the more ‘philosophical’ inquiries that had marked his early career.

Thoma Snyder’s experiment and the black-box reference amplifier were but two among the many experimental problems that Feynman addressed, and his intimate involvement with experimental design and analysis was widely appreciated. By mid-1944, he had risen to become a key liaison between the theoretical groups and all the experimental laboratories.¹⁰ The association left its mark on Feynman: as he recalled later of this time of designing counters and electronic equipment: ‘I enjoyed discovering that I could do it. But I was always sort of semipractical. I liked calculating things that ended up with something. I got a kick out of this, because it was real’.¹¹ Facing the ‘real’, Feynman’s work hovered closed to the counters, amplifiers and set-ups that he confronted each day. Unlike his prewar work on advanced potential absorbers and path

¹⁰ Feynman, interview with C. Weiner, American Institute of Physics 4 March–28 June 1966, on 5 March 1966, p. 43, (hereafter AIP): ‘I would go to all the laboratories, see what problems they had, what things they wanted calculated, bring back the need for calculation—or if we had something worked out, explain it to them at the other end. So I would go all over Los Alamos. I knew everything that was going on. I was the only guy besides Oppenheimer who knew what was going on in every division’. Other such mediators included Victor Weisskopf, Robert Serber and Robert Christy. Interview with Roy Glauber, 5 March 1998.

¹¹ Feynman, interview with Hoddeson and Baym, LANL T-79-0004, p. 19.

integrals, Feynman's war experience was tied directly and immediately to experimental results, experiments that demanded numbers, approximations, error estimates, and clear pictures of the flight of neutrons as they moved through paraffin tunnels, uranium targets and electronic counters.

2.2. *Confronting criticality: fluctuations in the Water Boiler*

Of all the machines at Los Alamos, none was as dramatic as the nuclear reactor being planned for the site. Bringing active material to criticality was, after all, the entire mission of the laboratory, and here was an experimental set-up that would achieve it, a slow-motion fission revealing key data about the chain reaction. Donald Kerst from Purdue brought with him a crew that up till that point had largely been concerned with the Superbomb, the first speculative advances towards a thermonuclear weapon. But once at Los Alamos, Kerst proposed the building of a reactor using enriched uranium sulfate in a one-foot steel sphere, surrounded by a BeO reflector and neutron shielding. (See Fig. 4.) Water would slow down the neutrons making the U-235 easier to fission. Bob Christy made a first calculation of how much material would be needed before the sphere would go critical (Hoddeson *et al.*, 1993, pp. 199ff). But how to test that estimate, and in general how would one track the approach to criticality in an experimental system? Feynman: 'We wanted to check whether the theory of how close we would be to critical was right or not. One way to do it was to put enough material together and make it explode. Well, that's dangerous'.¹² And here the Water Boiler could help—Feynman's idea was to use two neutron counters to measure fluctuations in the rate of coincidences. By measuring these departures from the mean, he could determine the spread in the number of neutrons produced when a neutron destroys a nucleus. With Feynman's formula the experimenters could plug in a measure of the fluctuations in coincidence counts and get out a measure of how close their experiment was to critical. More generally, knowing the dispersion in the number of fission neutrons gave everyone a better picture of the statistical evolution of a chain reaction in blocks of experimental material, in the reactor, or in the atomic bomb.

As with the amplifiers, Feynman aimed to explore a complicated and yet essential experimental set-up with a solution-oriented black-box approach that by-passed a deductive approach to neutron diffusion. No detailed calculation of neutronics; instead, a simple, visualisable picture of how the chain reaction would increase fluctuations in the coincidence rate as criticality loomed.

¹² Hoddeson *et al.* (1993, p. 183) discuss the important development by Feynman and Bethe of a formula for the efficiency of a nuclear weapon. This was before any of the detailed hydrodynamic calculations, so they reasoned from very basic physical principles—efficiency should depend on initial neutron multiplication, and they guessed that the decrease of multiplication as the core expanded would be proportional to the relative expansion. From that guess and some of Serber's results for the case with a small excess over criticality, they derived their formula. Quotation is from Hoddeson and Baym, LANL T-79-0004, p. 12.

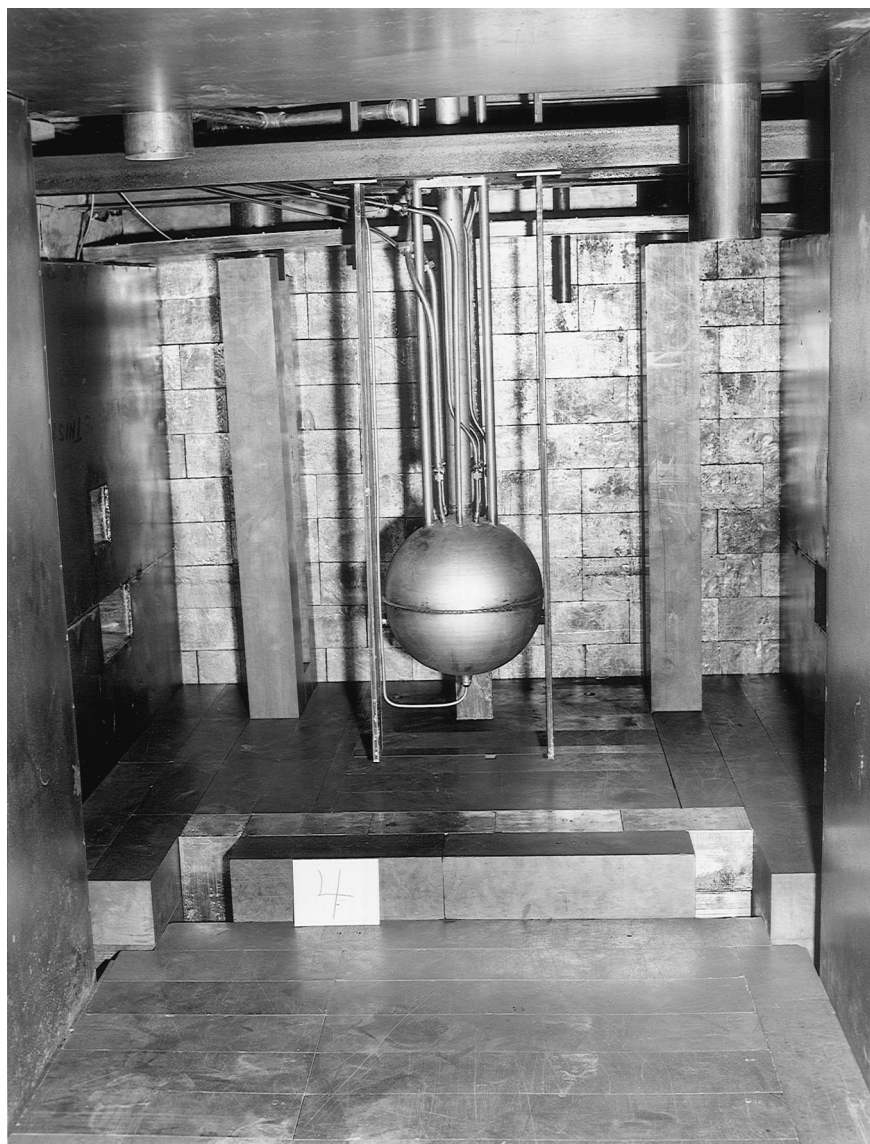


Fig. 4. *The Water Boiler*. Source: *Los Alamos Archives*, 12784.

Feynman: 'I believe that I was very clever in this thing. I was always very happy with the way I solved it—a rather nice neat way of analyzing it'. Looking at the pages on which these calculations were first made, Feynman added: 'The fact that it says El Fidel Motel in Albuquerque on this piece of paper is because I often visited my wife. Every weekend I visited my wife in Albuquerque, and I stayed at the El Fidel Hotel, which was a fire trap. Suddenly one day I realized

it was a fire trap, and I couldn't stay there anymore. It scared me—terrified me'.¹³ Soon the lab began to contemplate the momentary assembly of a critical mass of U-235 uranium hydride, and Feynman dubbed the experiment 'tickling the dragon's tail'¹⁴, and fluctuations became a way of measuring just how close the fire-breathing dragon was to waking. It would be a fire alarm in the nuclear tinderbox.

Assessing the background rate of coincidences, Feynman and his collaborators assumed that the neutrons would arrive at the two counters randomly, that is as modelled by a Poisson distribution (Feynman, 1946a; de Hoffmann, 1944). (Classically, a Poisson distribution modelled such things as the number of independent insurance claims filed in a day.) In a Poisson distribution, the variance is equal to the mean, and so if $\langle c \rangle$ is the average number of counts per unit time, then

$$(\langle c^2 \rangle - \langle c \rangle^2) / \langle c \rangle = \text{measure of fluctuations} = 1. \quad (2)$$

Any excess above this Poisson result would therefore be evidence of some dependence among the neutrons, as would be the case should neutrons be descended from a common ancestor in a chain reaction. The goal, therefore, was to find this quantity, labelled Y , that would measure the supra-Poisson excess:

$$(\langle c^2 \rangle - \langle c \rangle^2) / \langle c \rangle = 1 + Y. \quad (3)$$

Here Y is a function of the number of neutrons per fission, of the dispersion of the number of neutrons per fission, of the nearness to criticality, of the efficiency of counter and of the time window in which the counter is sensitive.

Significantly, Feynman and his collaborators captured the situation in a spacetime diagram drawn with time in the vertical direction and space horizontal (Fig. 5). Such an image must be kept in mind when viewing Feynman's early postwar spacetime 'Feynman diagrams', where again particles are absorbed, emit other particles, and scatter as reckoned by a concatenation of independent algebraic rules. Consider $t_1 < t_2$, with intervals around each time dt_1 and dt_2 . Wanted was the number of coincidences between counts in dt_1 and dt_2 . A coincidence, Feynman noted, can come about in two ways. First, there may be 'accidental' counts resulting from two completely different lineages, so to speak, e.g. count A and count D . Or, the counts may be 'coupled', as in A and C since both had a nearest common fission ancestor at event X . The number of coincidences was therefore the sum of those that are accidental and those that are coupled. Assuming that all neutrons were equally likely to cause fission, the theorists could demonstrate that, with the reactor critical (total number of prompt neutron 'descendants' equal to unity, $K_p = 1$) the *measured* excess

¹³ Feynman, interview with Hoddeson and Baym, LANL T-79-0004, pp. 13–14. It seems that after his first foray into fluctuations, Feynman teamed up with Frederick de Hoffmann and Serber, and also continued the work on his own, culminating in a long, still partially censored, unfinished paper written up on 26 July 1946.

¹⁴ Feynman, October 1944, from Hoddeson *et al.* (1993), pp. 346–347.

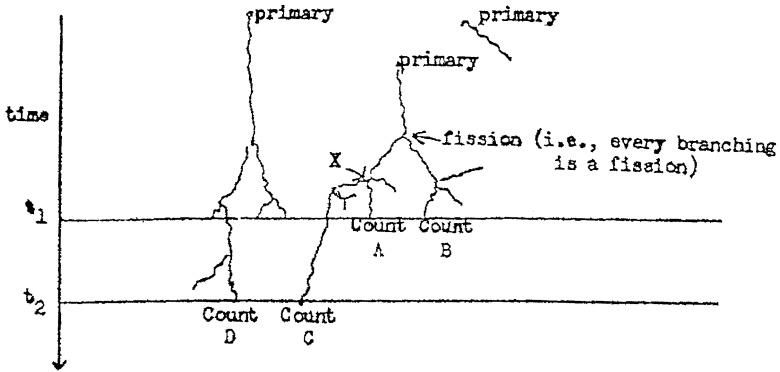


Fig. 5. Spacetime Diagram, Neutron Fluctuations. Source: de Hoffmann (1944).

fluctuation, Y , could be expressed in terms of the *measured* mean number of prompt neutrons ($\langle v_p \rangle$), the *measured* counter efficiency (ϵ), the mean time between fissions for a neutron (τ) and the (sought after) mean squared number of fission neutrons ($\langle v_p^2 \rangle$). From that point forward, whenever the excess fluctuations Y were measured, K_p would be known. This would be useful in subsequent approaches to criticality in the reactor and in other experimental situations.

Feynman's reasoning itself is of interest here, as he began to calculate the number of coupled coincidences, that is those with a common fission ancestor. To each pictorial element in the figure, he assigned an independent term, which he then listed:

- 1) fission at time t in a time interval dt , which is: Sdt
- 2) probability that a number of neutrons v will be released: l_v
- 3) number of neutrons at t_1 due to those created at t : $v \exp[-\alpha(t_1 - t)]$
- 4) probability that neutron at t_1 will register a count in dt_1 : $(\epsilon/\tau)dt_1$
- 5) probability that remaining $v - 1$ neutrons will be present at t_2 :
 $(v - 1) \exp[-\alpha(t_2 - t)]$
- 6) probability that a neutron at t_2 will register a count in dt_2 : $(\epsilon/\tau)dt_2$

Assembled and integrated, from the time t of the ancestor fission X equal to minus infinity to time $t = t_1$, Feynman derived the following criticality alarm:

$$Y = \epsilon \frac{\langle v_p^2 \rangle - \langle v_p \rangle^2}{\langle v_p \rangle (1 - K_p)^2}.$$

An increase in the number of fluctuations revealed just how far the experimenters were from criticality *without* a comprehensive theory of the neutronics deep in the heart of the massive chain reactions.

Here was a visualisable process, laid out in a spacetime diagram, and in which Feynman assigned each *elementary process* an independent term. Integrating

them, Feynman could produce a vital empirical result that experimenters could use as they probed the dangerous edge of criticality. It was a strategy not forgotten.

2.3. *Confronting engineers: gunk tank neutronics*

Feynman's deepening understanding of uranium in water was deployed for engineering. When Bob Christy fell ill in April 1944, Bethe and Oppenheimer dispatched Feynman to Oak Ridge to assess the nuclear safety of that burgeoning nuclear factory. How many barrels of solution could be stored in a room, for example, without inadvertently assembling a critical mass and spewing radioactivity over the laboratory? Feynman, by then an expert on the vast reduction of critical mass due to the water, showed the engineers where to dissolve neutron-absorbing cadmium salts in the barrels or where to insert dry cadmium sheets if dissolved salts were chemically dangerous.¹⁵ Facing chemists, generals, architects, and engineers, Feynman's analysis of such 'dirty' systems shunted aside the crystal-clear world of mathematical rigour and analytic solutions.

Feynman's first visit to Oak Ridge culminated in a meeting on 25 April 1944, with Colonel K. D. Nichols, Major W. E. Kelley, Lt L. R. Zumwalt and a variety of others. Feynman had completed a comprehensive inspection of the Oak Ridge facilities and spent the night of 24–25 April furiously compiling the report. After reassuring the managers that any quantity of 'tube alloy' (unenriched U-238 with 0.7% naturally-occurring U-235) could be safely stored, he addressed uranium concentrated to 5% U-235 and 50% U-235. For the 5% solution of U-235 he noted that up to 30 pounds could be stored in water, and any likely amount of 5% solution could be kept safely if four chloride atoms per uranium atom were kept in solution. Enriched to 50% U-235, the situation was much more delicate—only 350 grams could be kept safely in water solution in the absence of cadmium (Feynman, 1944).

Extraordinary as well as ordinary circumstances had to be considered. Feynman cautioned the military and industrial representatives about the possibility of floods, where the invading waters might drop the critical mass. Storage had to be designed so that it was impossible for anyone to stack boxes of material too closely. And hot chloride in steam-heated ovens or water-cooled chambers always presented the danger that the chloride could be converted to oxide by water, separating the tube alloy chemically and mechanically from the chlorine, and launching the whole into the danger zone. Feynman had prescriptions for mixing absorbers such as rare earths, cadmium, boron, mercury and gold into sludge, he urged the covering of boxes by cadmium and counselled his audience to guard against flood and to build fire-proof containers (*ibid.*). (See Fig. 6.)

Then, building by building, plant by plant, Feynman reported his recommendations—through storage rooms, containers, recovery reactors, electromagnetic

¹⁵ Feynman, interview with C. Weiner, AIP, on 5 March 1966, pp. 38–40.

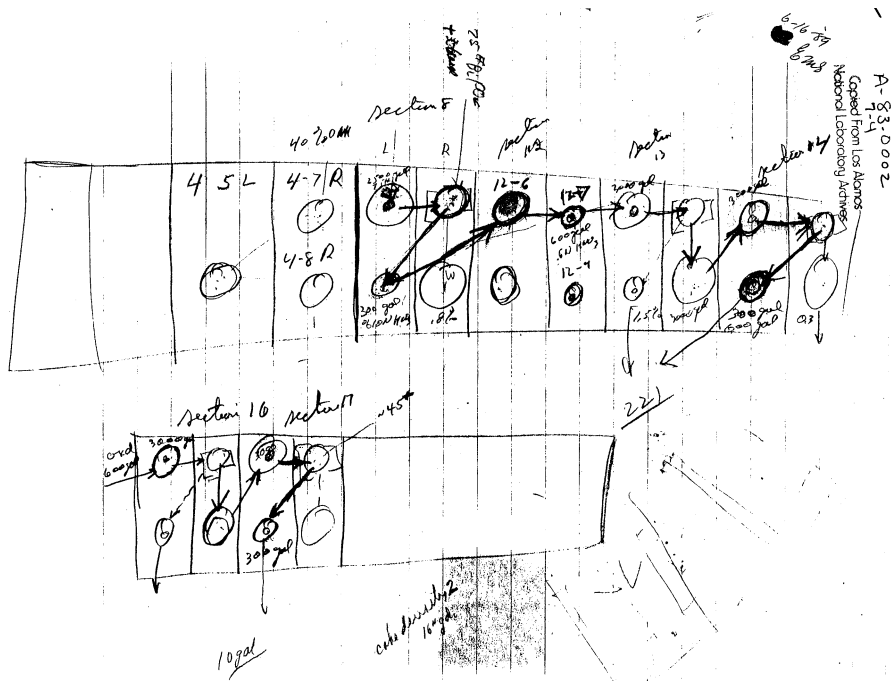


Fig. 6. Feynman, Sketches for Oak Ridge Safety Report, A-83-0002, 7-4. LANL Archives.

'racetrack' isotope separators all the way up to the final product plant where the U-235 was at 90% concentration. Throughout, Feynman had to bring his calculations to numerical conclusion: numbers of grams, amount of cadmium, batch size and separation procedures. And everywhere, as Feynman noted, he had to allow for a 'reasonable safety factor' in the theoretical errors introduced in his calculations (*ibid.*). When the engineers had a question—could they build a counter-activated dump of an absorbing reagent, for example—Feynman became point man.¹⁶

In a sense, as he and the engineers worked out problem after problem, Feynman was teaching the engineers to reason in the style of nuclear physics and scattering theory, just as he himself was becoming increasingly familiar with the language of 'safe' estimates, error margins, and the absolute demand for numbers. After one set of calculations for the 'Gunk Storage Tanks' going up in Oak Ridge Building 9208 during November 1944, Lt Colonel John R. Ruhoff took up Feynman's preliminary reckonings, made his own estimates and suggested that the tanks be coated with cadmium. In response, Feynman

¹⁶ J. L. Patterson, Chemical Production Division Supt to Major W. E. Kelley, Contracting Officer's Representative, 18 September 1944; Kelley to Feynman, 21 September 1944; Feynman to Kelley, 27 September 1944, A-83-0002, 7-3.

re-analyzed the calculations, deliberately overestimating the neutron flux into a tank 'A' with a striking *visualisable* analysis that showed the cadmium to be superfluous. First, Feynman assumed tank A was amidst an infinite array of tanks. He noted that if a fraction f of neutrons leaving tank A entered another tank (rather than the floor, ceilings, or walls), then the number of particles that enter A from all other tanks would be that same fraction f of the number that leave A. This is because there is the same probability that a particle would go from A to B as from B to A, by geometrical symmetry. (Tank A 'saw' as much of the other tanks as the other tanks 'saw' of it.) So one could, Feynman argues, equivalently imagine that A was isolated and surrounded by a material with coefficient of reflection f .

In designing the process for building 9208, a safe mass of active material significantly less than critical was computed assuming an infinite surround of water with a reflection coefficient of 0.80, so if $f < 0.80$, no further safety measures were needed. Since the total solid angle subtended by the other tanks was no more than 0.20 and the floor and ceilings kicked back about 0.40 of the outgoing neutrons, there was a total reflectivity of no more than $f = 0.60$, well below $f = 0.80$. On this basis Feynman assured the lieutenant colonel that no more cadmium would be needed.¹⁷

Over the course of the next months, and amidst an unending stream of demands for different geometries, chemistry and locations, Feynman used a variation principle to produce rules of calculation to identify quantities of 'material' safe against chain reaction. And here, as everywhere in his wartime work, he avoided recourse to fundamental equations of motion or analytic rigour, instead focussing directly on the solutions—on the probability that a neutron at a source point X would slow down, make it to another point Y, and cause a fission. If U is the fraction of fast neutrons that slow down to room temperature ('become thermal') and F is the fraction of thermal neutrons that cause fission, then there is a simple rule: a system is critical if $2.15 UF = 1$. Feynman chose 2.15 because that is the number of fast neutrons produced on average by one neutron—so the equation simply gave the criterion for the chain to continue. Throughout his report, Feynman wrote to the engineers about the determination of U and F as if he and they were watching the individual neutrons as they moved, scattered and fissioned: 'If the neutrons which are slowed down in the slab did not wander from place to place as they sought an atom of [U-235] to produce fission, the calculation of F would be simple' (Feynman, 1945b).

Assuming from the start that the neutrons were all of a single velocity and were just as likely to cause fission no matter where they were, Feynman was, explicitly, not after exact solutions. 'It is to be emphasized', he insisted, 'that the method is only approximate, as accuracy has been sacrificed to speed and simplicity in calculation' (*ibid.*). Suppose, for example, that one were faced with

¹⁷ Feynman to Lt Col. John R. Ruhoff, 22 November 1944, A-83-0002, 7-3.

the problem of determining the limiting safe width for an infinite slab of 8% U-235 in water? Plug in the numbers, consult the attached chart, and find $w = 13.12$ cm. Simple reapplications of the first result gave further rules for including the effects of impurities in the water, rules for finite blocks rather than infinite slabs, and rules for cadmium in the enriched sludge (*ibid.*).

The admixture of approximation methods, neutron diffusion, nuclear cross sections, floods, fires and wooden walls marked Feynman's correspondence with the Oak Ridge engineers. From April of 1944 to September 1945, whatever else Feynman was doing, he was also deeply enmeshed in the barely-existing field of nuclear engineering. Out of this interaction came characteristic rules and modular reasoning: visualisable, approximate, from-the-ground-up calculations applied to neutrons, pans, sheds and sludge. Visionary statements reinterpreting established laws of physics ceded to the exigencies of living in a world he had to reach outside the home culture of theoretical physics as he knew it before the war. Now a billion dollar plant was churning out U-235, and only a calculation stood between thousands of workers and nuclear disaster.

2.4. *Confronting devices: a clever theorem*

Aside from finding the critical mass for the Water Boiler, warning of experimental approach to criticality, and avoiding criticality in the Oak Ridge storage sheds, there remained the central mission of the laboratory: producing an arsenal of plutonium and U-235 bombs. In late 1943, active material was extraordinarily scarce—as Feynman noted—and every effort turned to eking out active material for weapons work.¹⁸ Feynman wondered how to think about alternative distributions of the U-235 that were being proposed one after another. Which one would have the highest yield, the highest efficiency, the least amount of active material? Once again, Feynman found a theorem that linked solutions one to another. More specifically, Feynman derived an integral theorem relating an unknown distribution to a known one by enclosing both in a single relation (Feynman, 1946c, and War Notebook). (See Fig. 7).

His reasoning began with a 'displacement' integral—one that gave the density of neutrons at point (2) by integrating over all points (1) in the volume where collisions produce neutrons that then end up moving without collision to point (2):

$$N(2) = \int Q(1, 2)(1 + f(1))\sigma(1)N(1)d[\text{Volume}(1)]. \quad (5)$$

Here Feynman pictured a unit source of neutrons at (1), that is, one neutron produced per unit time and volume. He defined $(1/v)Q(1, 2)$ to be the density due to the source at point (2). $Q(1, 2)$ was the *kernel*, the solution to the transport equation—it was proportional to the likelihood of a neutron moving,

¹⁸ Feynman, entry under 'April 15 [1944], Oppenheimer. Materials & Schedules', in Feynman's war notebook, Los Alamos National Laboratory Archives. Hereafter, 'War Notebook'.

Theorem If m, m' are the fission producing (in general, zero) neutrons for two different distributions of $(\rho), \rho, \rho'$ in a homogeneous background,

then
$$\int (\rho - \rho') m m' dvol = 0$$

Very general, any slowing down or scattering properties (if m is defined as that $m\rho =$ rate of fission per cent)

For simplicity in special application let fission be produced only by slow neutrons so $m =$ density of slow

Corollary $\int \rho dvol = Mass$ is minimum if $m =$ constant $m \neq 0$ where $\rho \neq 0$.
(ii: if $\rho m =$ const. ρ)

Corollary Suppose $\rho = f(r)$ from 0 to R , and zero outside. $\Delta R, \epsilon$ very small.

$\rho' = (1 + \epsilon)f(r)$ " 0 to $R + \Delta R$ and zero outside.

$\therefore \rho' - \rho = \epsilon f(r)$ " 0 to R
 $= f(R)$ " R to $R + \Delta R$.

$0 = \int (\rho' - \rho) m m' dvol = \pm$ first order set $m = m_0 = \epsilon \int_0^R (m(r))^2 f(r) dr + m(R)^2 f(R) \Delta R = 0$

and $\Delta M = \int (\rho' - \rho) dvol = \epsilon \int_0^R f(r) dvol + \Delta R \cdot f(R) \cdot 4\pi R^2$ $\therefore \epsilon$ is negative if ΔR is +

whence
$$\Delta M = \epsilon \left[\int_0^R \left\{ f - \left[\frac{m(r)}{m(R)} \right]^2 \right\} f(r) dr \right]$$

Corollary of all systems where $\rho =$ const. $f(r)$ out to a certain radius R , and zero thereafter, that one uses the least mass which

makes
$$\int_0^R (m(r))^2 f(r) dr = m(R)^2 \int_0^R f(r) dr$$

Corollary of all systems where $\rho =$ constant inside a certain radius R , and zero thereafter, that one ^{which} uses the least mass $\frac{M}{V}$ has the ^{root} mean square of the neutron density over the sphere, equal to its value at the surface.
$$m(R) = \sqrt{\frac{\int_0^R (m(r))^2 dvol}{\frac{4\pi R^3}{3}}}$$

Fig. 7. Feynman's Theorem. Source: Wartime Notebook, LANL Archives.

without a collision, from (1) to (2). Note that $\sigma(1)$ stood for the total nuclear cross section at (1), given as the product of the neutron-nucleus cross section and number of nuclei per volume; Feynman took $1 + f(1)$ to be the number of neutrons released per collision at point (1). Throughout, Feynman reasoned visually. For example, he invited the reader to see the probability of a neutron travelling from (1) to (2) as a matter of overcoming a series of hurdles that were,

necessarily, identical to the hurdles faced by a neutron travelling in the opposite direction. Using that symmetry of direction and manipulating the equation for $N(2)$, Feynman derived the relation

$$\int (f' - f)\sigma N'Nd[\text{Volume}] = 0, \quad (6)$$

where N' and N were two different neutron density distributions and f' and f were the two corresponding distributions of nuclei.

Feynman's simple integral relation functioned like a calibration device: new critical assemblies could be immediately tied to known ones, each with its own neutron multiplication factors. Its fundamental simplifying assumption was that the neutrons flew with a single velocity. As always, Feynman then made explicit the basic physical meaning of the expression:

If [both assemblies are] critical it would be expected that in some sense the average f must be the same. The exact sense is given in [this theorem]. It says that there is no difference in the average of f times the total cross section, provided that this average is taken over the whole system with weight equal to the product of the neutron distributions (*ibid.*).

There were some exact results to be had. For example, Feynman examined two different cores, one with neutron multiplication factor f_1 , the other with f_2 , and both assumed to have identical tampers (where $f_1 = f_2$). Then for the tamper, the integral vanished identically since $(f_1 - f_2) = 0$. For the core, the constancy of f_1 and f_2 meant that $(f_1 - f_2)$ could be taken out of the integral and it became possible to write:

$$(f_1 - f_2)\int \sigma N_1 N_2 d[\text{core}] = 0, \quad (7)$$

where N_1 and N_2 were the neutron distributions associated with f_1 and f_2 respectively. This immediately demonstrated that there could be only one constant f for a critical core—otherwise, since N_1 and N_2 are separately everywhere positive, the integral could not vanish.

Feynman went on to use his new formula explicitly to find the minimum mass solutions under various assumptions and to derive the extrapolated end point—the point in the tamper to which neutrons no longer penetrate. He supposed that to the left of $x = 0$ there was a semi-infinite region with no absorption or fission ($f = 0$) but to the right of $x = 0$ there was a semi-infinite tamper ($f = -1$), meaning that every collision resulted in absorption, so no neutrons made it back to the core. Using the new theorem, it became a simple matter for Feynman to solve for N . Building on this simple problem, Feynman tackled one closer to reality—a configuration with a finite tamper. As in each of his projects, he provided an easily movable piece of theory, a module that avoided the necessity of returning to first principles with every new distribution of active material. And, as in each case, the work ended with charts of numbers: in this instance long lists of numbers relating the extrapolated end points to various assumptions about the absorptive nature of the core and tamper.

Though the details of chain reactions did not appear explicitly, the black box had begun to crack open to the movement of neutrons from point (1) to point (2).

Looking through his notebook years later, Feynman mused: 'This time I am doing something on my own. Yeah, yeah, this is some clever stuff I thought of at the time. It probably is clever because there's an interesting theorem here'.¹⁹ Faced with ever more difficult problems assigned to him by Bethe, Feynman had to become more clever yet.

2.5. *Confronting computers: an implosion of numbers*

Numbers were everywhere. Every piece of theory, even the most abstruse, had to end with a bottom line: distributions of active material, extrapolated end points, critical masses, efficiencies. Perhaps the most ambitious numerical exploration was the analysis of the implosion process itself, a task originally assigned by Bethe to Stanley Frankel and Eldred Nelson. According to Feynman, Frankel—ever the methodical analyst—began exploring the tabulator's limits, programming (for example) the machine to compute arctangents that could have been taken from tables. But in the process of such careful exploration—so Feynman believed—he was succumbing to the 'disease' of computer fascination, losing himself in machine subtleties, perhaps the way Feynman himself had when he had become fascinated by the new Marchant calculators.²⁰ Called in to reorganise the calculating team of Special Engineering Detachment troops by Bethe, who wanted answers and soon, Feynman faced a roomful of stove-size punch-card tabulators that clattered at a practically unbearable volume. Essentially, he had to convert what were book-keeping devices to a system that could integrate numerically the partial differential equations for implosion. In the course of doing this, Feynman began a process of modularising labour that had profound effects on later thinking about programming itself.

Though I postpone a full discussion of this phase of Feynman's war work for reasons of space, two features are crucial. First, by this stage of the war Feynman was riveted by the very process of numerical manipulation. At one point, he gave an entertaining lecture to the Math Club. Entitled 'Famous Numbers I Have Known', Feynman's lecture attracted a smattering of younger people and luminaries. Presenting various amusing results he had obtained using the Marchants to examine continued fractions and other mathematical expressions, Feynman, according to Roy Glauber, had John von Neumann in stitches.²¹ On integration itself, Feynman said this as he perused his war notebooks:

I love numerical methods, and everything that I could invent to improve methods of integration I loved. In here [his wartime notebook] is a formula for integrating three times in succession in one step, with estimates of errors and all this stuff.

¹⁹ Feynman, interview with Hoddeson and Baym, LANL T-79-0004, p. 8.

²⁰ Feynman, interview with C. Weiner, AIP, 5 March 1966, pp. 44–45.

²¹ Roy Glauber, interview with author, 5 March 1998.

I guess this is my meat—I love this [...] See, I'm trying other rules, trying to test them out to see how accurate they are. [...] I was testing my numerical methods out on that trial.

Second, and more importantly, Feynman broke up the implosion calculation, and modularised both the human calculators and the problem itself into an algorithmic composition of its component parts. Divided into 'problems' each one of which was a different 'gadget', theory had to keep pace with the ever-shifting disposition of device parts. One report was simply labelled 'Problem 8—Solid Gadget' (Feynman, 1945a). Reflecting on that time, Feynman recalled: '[W]e had three shifts. There were probably about three or four guys on a shift. There were fifteen people in my group. We had a computing factory. It was really something'.²² Parts of the theory had to be sufficiently movable that this computing factory could run as each 'problem' arrived at the assembly line. There was neither the time nor the place to begin each one as it stood alone.

In terms of the Los Alamos mission to deliver a workable nuclear weapon, Feynman's supervision of numerical implosions was probably as important as anything he did. But the problem to which, by far, he devoted most energy was one discussed only in passing in later histories. This other work—the Feynman group analysis of the hydride bomb—was, in my view, that group's most original contribution at Los Alamos. Its importance lay not so much in the contribution it made to the immediate war effort, but in its consolidation of the modular-effective conception of theory that marked Feynman's postwar approach to quantum electrodynamics. It is to this we now turn.

3. Neutron Diffusion: Modularising the Hydride

If wishes were horses, the theorists at Los Alamos would not have had to resort to any form of 'black-box' scheme, they would have taken the 'right' starting point—Boltzmann's equation—and applied it to the specific case of the geometry and physics that were appropriate for the bomb design in question. But Boltzmann's equation was notoriously difficult to solve, and approximations were absolutely necessary. Let us be more precise.

The fundamental quantity for a study of a process of diffusion like that involved with scattering and fissioning neutrons was the angular flux density: $f(x, E, \Omega, t) dx dE d\Omega dt$ multiplied by the velocity v defined as $(2E/m)^{1/2}$. This specified the number of neutrons in a small region of phase space formed by

²² Feynman, interview with Hoddeson and Baym, LANL T-79-0004, p. 54; the block quotation above is *ibid.*, pp. 15–16. Of the department of calculation, Feynman said: 'I would walk through there on the way back from lunch all the time, just walk through that department and just look over their shoulders, I would put my finger down on a number and say, "that's wrong" [...] I would see that they were doing this integral equation, and I would think about it, and I would see I've got a much better way to do that [...] I would come back and say, "You can do it this way and save an awful lot of time", and so on. I liked calculating'.

position space (dx around x), energy (dE around E), and solid angle ($d\Omega$ around the detection Ω), at time t . A simple (“first form”) of Boltzmann’s equation for the neutrons could be glossed as an expression for the time rate of change of neutron density in position–energy space (though of course for a critical reactor core there is no time change in this density, and so the left-hand side would be zero):

$$\begin{aligned} \frac{1}{v} \frac{\partial f(E, \Omega)}{\partial t} & \text{(time rate of change in the phase space density)} = \\ & \Omega \cdot \text{grad}_x f(E, \Omega) \text{ (Stream of neutrons moving without interference)} - \\ & f(E, \Omega) [\sigma_s(E) + \sigma_a(E)] \text{ (Neutrons scattering out)} + \quad (8) \\ & S(E, \Omega) \text{ (Source of neutrons)} + \\ & \int dE' d\Omega' f(E', \Omega') \sigma_s(E' \rightarrow E, \Omega' \rightarrow \Omega) \text{ (Neutrons scattering in)}. \end{aligned}$$

Here $\sigma_s(E)$ and $\sigma_a(E)$ represented cross sections per volume for neutrons to be scattered and absorbed respectively, and the term $\Omega \cdot \text{grad}_x f(E, \Omega)$ provided the change of f along a coordinate in the direction Ω .

One technique popular at Los Alamos was an expansion of the Boltzmann equation in terms of the first several spherical harmonics (a technique familiar from ordinary quantum mechanics), in conjunction with the assumption that the neutrons had one—or at most two—velocities. The resulting much simplified equations could then be solved with varying degree of precision for particular idealised arrangements of the core and the tamper. Using such methods, George Placzek, Bengt Carlson and Carson Mark had begun work on some planar problems at the Canadian branch of the project. Roy Glauber went further with analyses of spherically symmetric systems. These various spherical harmonic papers were considered to be robust enough that, when Serber had a new neutron diffusion method to test, he plotted his result against Glauber’s (Serber, 1945; Carlson, 1946; Glauber, 1944, 1946; Weinberg and Wigner, 1958).

Responding to one of Edward Teller’s intriguing suggestions, Bethe assigned Feynman a problem for which these Boltzmann-based methods manifestly would not work. In mid-war, the paucity of fissionable U-235 and plutonium presented a crisis, and minimising the amount of active material in a bomb was an absolute necessity. Teller had the idea of using uranium hydride (where the light atoms would slow down neutrons) instead of the metallic form of uranium, and the idea appeared to be just the emergency exit they were seeking. But on the hydride project, Feynman could not, as in his various ‘black box’ projects, work around the neutron dynamics. Building on his experience with the Water Boiler where neutrons slowed in the water, Feynman now had to confront, head on, the complexities of the neutrons as they fissioned nuclei, scattered, were absorbed, and left the bomb altogether. Worse, he had to address difficulties that the other theory groups did not: precisely because the hydride weapon was designed to slow the neutrons down, the approximations used elsewhere—that the neutrons were of a single energy and only scattered elastically—was utterly

useless. Just as in the Water Boiler, *all* neutron energies would be present from highest down to lowest. To calculate anything at all on the hydride weapon, at least three, probably more velocity groups would have to appear in the model and the team would have to have a way of treating inelastic scattering in the tamper. Needed was a technique flexible enough to redeploy around the myriad of possible designs of the hydride weapon.

Faced with such complexity, Feynman did not follow the other theorists: he assiduously avoided the Boltzmann equation in its spherical harmonic expansion, and in general he eschewed the strategy of working *down* from equations of motion through approximations to a specific design. Nor was Feynman interested, as were other Los Alamos theorists such as Stanley Frankel and Eldred Nelson, in exact solutions to idealised problems using sophisticated mathematical techniques such as the Wiener–Hopf method, where the integral equation was broken up into three different functionals, each of which existed in different strips on the complex plane. (See Fig. 8.) Nothing in Feynman’s work remotely touched on this sort of project—not in the examination of the analyticity structure of the functions, and not in the general search for exact solutions or such rigorous mathematics (Frankel and Goldberg, 1945a).

Instead, Feynman began from below, starting with the kernel—the elementary solution to the transport equation. Reasoning in part through plausibility arguments, approximations, and guesses, he built up ‘solutions’ using an arsenal of movable parts that could be rearranged to get greater or lesser accuracy, more or fewer velocity groups, a multiplicity of tamper types, and varying finite geometries. At one vital point in his major report on the hydride bomb, Feynman frankly confessed, ‘the mathematical rigor is not all that may be desired’, but that *nonetheless* ‘[i]n the very large number of different practical cases which have been tried [...] we have a method by which we can bracket the correct answer with confidence’ (Feynman and Welton, 1947, p. 24). Typical in some ways of the experimenter’s attitude towards their newly-minted war equipment, Feynman’s view was that if it worked, use it. Mathematics could wait. But to grasp the texture of theory in Feynman’s group, it is necessary to be more specific.

When Feynman summarised the methods developed for the hydride weapon, he began with the simple one-velocity case, letting the diffusion kernel $K(x \rightarrow x')dx'$ be the probability of a neutron originating at x in the core having its first interaction in the core at point x' in the volume dx' (summed over the direct path from x to x' and all the paths taken in the tamper while travelling from x to x'). The theorists defined $\psi(2)$ as the flux density (velocity times density) of neutrons at $x = x_2$ (abbreviated (2)), defined σ as the total cross section (the same for core and tamper), and defined f to be the number of excess neutrons (emitted isotropically at the one velocity) per collision. It then followed that the following integral relation simply stated the fact that the likelihood of a neutron being in a unit volume at (2) was a sum over all the paths a neutron could take to (2) from all other points in the core, $x = x_1$ (abbreviated (1)):

$$\psi(2) = (1 + f) \int d(1)K(1 \rightarrow 2)\psi(1). \quad (9)$$

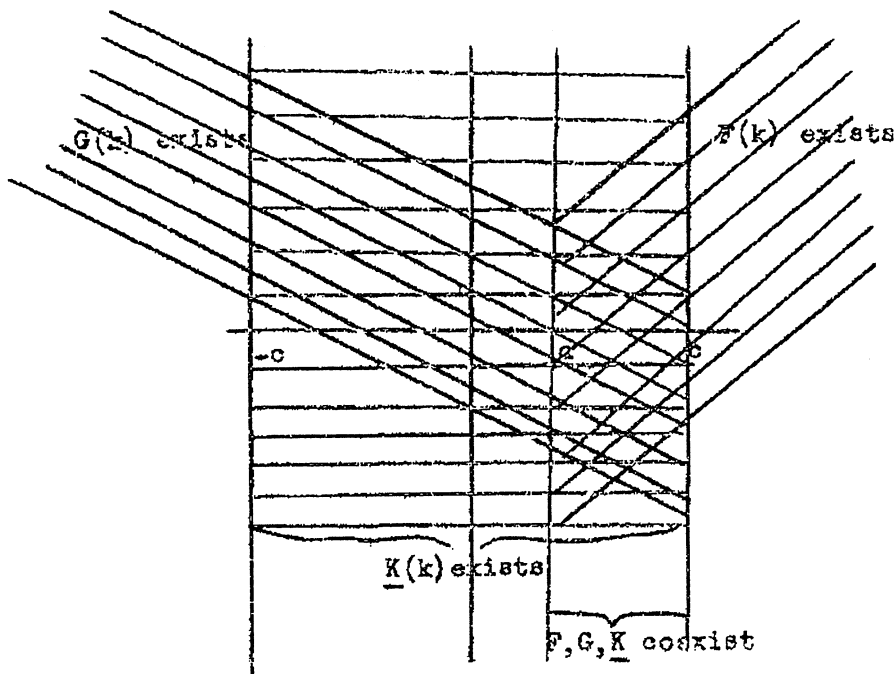


Fig. 8. Wiener-Hopf Method. Source: Frankel and Goldberg (1945a).

This fundamental integral equation served as the launching pad from which Feynman's group developed their many-velocity calculation. Interest in kernels was, of course, hardly unique to Feynman. Perusing reports from the period one can sometimes pull out large oversized sheets with long lists of kernels for a wide spectrum of different uses. What was particular was Feynman's intuitive, from-the-ground-up way of extending them to include approximations to the several-velocity problem.

To attack the problem, Feynman divided up the function ψ into a set of eigenfunctions, $\{\psi_0, \psi_1, \psi_2, \dots\}$ to each of which corresponded eigenvalues $\{1/(1 + f_0), 1/(1 + f_1), \dots\}$. For a spherically-symmetric arrangement of the core, Feynman let $\psi_0 = \sin(kr)/r$ in the core, and for the purely absorptive extension into the tamper, $\psi_0 = \exp(-hr)/r$. Here the wave number k is determined by assuming an infinite core and plugging in the various measured cross sections for fission and scattering. Similarly, the absorptive constant h was fixed by the cross sections for absorption in an infinite tamper; at the core-tamper boundary the two functions were matched. For almost all purposes these first approximations for ψ_0 were all the theorists needed.

Now the fundamental equation said that if neutrons are released at a rate of $(1 + f_0)\sigma\psi_0$ per time per volume, then of these $\psi_0\sigma$ have first collisions per unit time-volume. So one neutron released with the distribution ψ_0 gave rise to $1/(1 + f_0)$

first collisions. This was true for each of the ψ_n , though the higher n were not needed for most purposes. Feynman's team could then ask: for the mode ψ_n , how many neutrons, ν_n , must be released by absorption to maintain criticality in that mode? If ν_n neutrons emerge after an absorption, then the net release per absorption is $(\nu_n - 1)$, and per collision $(\nu_n - 1)(\sigma_a/\sigma)$, where σ_a/σ designated the fraction of all collisions that were absorptive—capture, fission or inelastic scattering. By definition, the number of excess neutrons produced in a collision was f_0 , so

$$f_0 = (\nu_0 - 1)\sigma_a/\sigma. \quad (10)$$

Therefore for each neutron liberated in the distribution ψ_0 , $1/\nu_0$ were absorbed by that same distribution. This relation proved very useful.

Now the theorists turned to the problem of many velocities with a constant total cross section everywhere and no inelastic scattering. This was an exercise of no ultimate use (since they were after the hydride bomb that depended essentially on inelastic scattering) but nonetheless useful to explore as a way of fixing ideas. They let $N(v')$ be the flux density of neutrons with velocity v' . So $N(v')\sigma_a(v')$ was the density of absorptions per unit time for neutrons of velocity v' . Multiplying $N(v')\sigma_a(v')$ by the probability that a neutron absorbed with velocity v' would emerge with velocity v —call this probability factor $S_a(v' \rightarrow v)dv$ —gave the rate of neutrons emerging in the core at velocity v .²³ A factor $1/v(v)$ provided the rate of reabsorption in the core of these emerging v -velocity neutrons. Integrating over all possible v' , Feynman's group therefore had an equation for the rate of absorption in the core of neutrons of velocity v :

$$N(v)\sigma_a(v) = 1/v(v) \int_0^\infty dv' S(v' \rightarrow v) N(v')\sigma_a(v'), \quad (11)$$

which could be rewritten when $A(v)$ definitionally replaced $N(v)\sigma_a(v)$ as the rate of absorption in the core of neutrons of velocity v :

$$A(v) = 1/v(v) \int_0^\infty dv' S(v' \rightarrow v) A(v'). \quad (12)$$

By inserting an explicit expression for S that ensured only elastic scattering, the equation for $A(v)$ then specified a balance that, at criticality, fixed the critical radius in terms of $\nu(v)$. In order to relax the unrealistic assumption that the total cross section was the same everywhere, the Feynman group sandwiched the 'true' solution between two approximations they *could* calculate. In essence the

²³ Here the total cross section σ is the sum of elastic, fissioning, inelastic and capture cross sections: $\sigma = \sigma_e + \sigma_f + \sigma_i + \sigma_c$. If $\varphi(v \rightarrow v')$ is the spectrum of inelastic neutrons scattered at velocity v' from an incoming neutron of v and $x(v)$ is the fission spectrum, then $\sigma(v')S(v' \rightarrow v) = \sigma_c\delta(v - v') + \sigma_f(v')\nu x(v) + \sigma_i(v')\varphi(v' \rightarrow v)$. Note that the true ν —the number of neutrons in fact released by a nucleus when fissioned—appears implicitly inside $S(v' \rightarrow v)$.

lower estimate worked by assuming that the distribution of neutrons of velocity v could be given by an expansion in terms of $\psi_n(x, v)$, where the set of eigenfunctions was specific to each velocity. 'In any reasonable system', Feynman's group wrote, the lowest eigenfunction $\psi_0(x, v)$ will not vary much—that is the group supposed that $\psi_0(x, v) = \sin(kr)/r$ could be used in place of $\psi_0(x, v')$. This they dubbed the *first lower approximation*, and aimed to produce values for the critical radius that were too low. Conversely, the *first upper approximation* functioned by assuming that all the v_n for $n > 0$ were big; this meant that the higher ψ_n were assumed in this approximation to lead to no neutron absorption in the core ($1/v_n = 0$). Since such an assumption *underestimated* the amount of fission, it gave values of the critical core radius that were too large. The next approximation would take the v_n to be large only for $n > 1$, and so on. With a systematic means for finding a series of ever more accurate lower and upper approximations, the true answer (Feynman and his team hoped) could be ever more accurately defined by squeezing it between upper and lower limits.

The key difficulty, as the neutron diffusion team knew, was how to take account of scattering that would change a neutron's velocity. That was, not to put too fine a point on it, the whole purpose of the hydride. One day Feynman told the rest of the crew that he knew how to insert inelastic collisions but that, unfortunately, he was leaving on vacation. While he was gone, two members of Feynman's group, Julius Ashkin and Theodore Welton, struggled hard to figure out how to proceed—without any visible success. When he came back, the others anxiously asked for the answer: 'You said you had it'. 'I did?', Feynman replied. Retreating—the others now practised in leaving him alone at such times—Feynman returned with the following argument.²⁴ Assume, he said, that the core, capable of inducing both inelastic scattering and fission, was surrounded by an infinite tamper itself capable of inelastic scatterings but incompetent to allow fission. Suppose neutrons could come only in two velocities, 1 and 2, and that the inelastic cross section in the tamper absorbed velocity-1 neutrons, and provided a source of the less energetic velocity-2 neutrons, some of which would get back to the core. The goal: assume criticality and solve the balance equation taking into account the fact that these new velocity-2 neutrons will now be flowing back to the core.

Again, Feynman proceeded with his solution-modules, first in the simple case with no inelastic scattering. $S(2 \rightarrow 1)$ was the number of velocity-1 neutrons produced when a velocity-2 neutron was absorbed in the core. $S(1 \rightarrow 1)$, $S(2 \rightarrow 2)$ and $S(1 \rightarrow 2)$ were defined analogously. A_1 and A_2 were the total rates of neutron absorption in the core at velocity 1 and 2, respectively; v_{10} (or v_{20}) gave the values of v needed to make a system critical that had just velocity-1 neutrons (or, respectively, velocity-2 neutrons). With this notation:

$$A_1 = 1/v_{10}[S(1 \rightarrow 1)A_1 + S(2 \rightarrow 1)A_2]. \quad (13)$$

²⁴ Theodore Welton, interview with author, 14 May 1998.

This meant that the rate of neutrons absorbed at velocity 1 in the core was the product of two terms: the factor that gave the likelihood of a neutron of velocity 1 being absorbed in the core ($1/v_{10}$) and the bracketed expression. The bracketed expression provided the rate of a neutron being absorbed at velocity 1 in the core and emitted at velocity 1, added to the rate of a neutron being absorbed in the core at velocity 2 and emitted at velocity 1. Similarly, the rate of neutrons absorbed in the core at velocity 2 due to neutrons scattered in the core was just

$$A_2 = 1/v_{20}[S(1 \rightarrow 2)A_1 + S(2 \rightarrow 2)A_2]. \quad (14)$$

To this expression for A_2 (the rate of neutrons absorbed in the core with velocity 2) must be added the effect of inelastic scattering in the tamper, that is an expression for the rate of neutrons produced in the tamper with velocity 2 and returning to the core where they were absorbed. This additional ‘tamper term’ was the product of two quantities: the first was P_{12} , defined to be the conditional probability that a velocity-2 neutron, freed from the tamper by the inelastic scattering of a velocity-1 neutron, returned to the core where it was absorbed. Naturally, it was necessary to multiply P_{12} by the number of neutrons in the tamper that were inelastically scattered into velocity 2.

Feynman could easily write down the number of neutrons inelastically scattered from velocity 1 into velocity 2 in the tamper. First, he had a factor for the fraction of velocity-1 neutrons absorbed in the tamper—since the fraction of velocity-1 neutrons produced in the core and subsequently reabsorbed in the core is $1/v_{10}$, it followed that the remaining fraction, $(1 - 1/v_{10})$, were absorbed in the tamper. Second, only a certain fraction of the velocity-1 absorptions in the tamper scattered inelastically into velocity 2: $\sigma_{i12}^*/\sigma_{1a}^*$, where σ_{i12}^* was the inelastic cross section in the tamper for a neutron of velocity 1 to emerge with velocity 2, and σ_{1a}^* was the total absorption for a neutron of velocity 1 in the tamper ($\sigma_{i12}^* +$ cross section for true capture). Finally, there was the familiar factor that gave the likelihood of a velocity-1 neutron being produced in the core in the first place: $[S(1 \rightarrow 1)A_1 + S(2 \rightarrow 1)A_2]$. Assembling the various terms of A_2 , yielded, *in toto*:

$$A_2 = 1/v_{20}[S(1 \rightarrow 2)A_1 + S(2 \rightarrow 2)A_2] + P_{12}[\sigma_{i12}^*/\sigma_{1a}^*](1 - 1/v_{10})[S(1 \rightarrow 1)A_1 + S(2 \rightarrow 1)A_2]. \quad (15)$$

It was possible to get an approximate expression for P_{12} —assuming, for example, an exponential decay with radius in the tamper, and the higher normal modes as needed. Then the team applied their approximation methods, assumed a given bomb core radius, and solved for the critical multiplication numbers (or the other way around).

There were many ways to generalise and extend this result, and Feynman and his group pursued several. For example, they generalised to a larger number of velocity groups, defining P_{123} as the probability that a neutron of velocity 1 is

scattered in the tamper at velocity 2, scattered again in the tamper into velocity 3, whereupon it was absorbed back in the core. (They could contemplate P_{1234} and even higher numbers of inelastic scatterings, but the complexity became dreadful and the practical use marginal.) They explored systems departing from spherical symmetry, investigated weapons with holes or inhomogeneities, and examined subcritical systems with a neutron source.

Through this work, a characteristic approach to theory emerged. Everywhere, the emphasis was on modules, on elementary bits of solution such as P_{12} and P_{123} that could be easily combined to suit particular situations. Everywhere, Feynman fastened on the underlying physical *process*: the elementary, picturable fissions, absorptions, inelastic scatterings and elastic scatterings out of which complex, real bombs were built. No other methods of mathematics or physics could displace this homing in on the basic collisions.

A fraction of particles absorbed in certain ways would be multiplied by transfer functions that related the likelihood of finding the particle in position (1) from a start at position (2), or the probability that a neutron entering a collision with velocity 1 would emerge with velocity 2. Visualisable, non-rigorous, modular diffusion kernels marked Feynman's approach—he was *never* in pursuit of the fundamental (Boltzmannian) equations of motion or in search of mathematical rigour like Frankel, Nelson, and Goldberg. There were no Laplace transforms, no tricky contour integrations, and no sophisticated examinations of regions of analyticity in which to solve Wiener–Hopf integral equations. Arguments by mathematical or formal elegance simply had no place. Feynman wanted numerical, calculable answers and a systematic approximation scheme, he sought clear visualisable representations with kernels that summed neutron paths from point (1) to point (2), and above all he aimed at solutions that could be combined and recombined quickly in new ways to cope with more complex, realistic systems. The hydride reports tumbled out—in January, February and April 1944. But when the theoretical formulae were coupled to new experimental results for the energy dependence of various cross sections, the hydride bomb looked less and less like a usable weapon. Fission would begin, the core would heat up, but then, proceeding slowly because of the hydrogen collisions, the radioactive mass would disperse before it could fully detonate.²⁵

As a result of the poor prognosis for the hydride—and a reduced estimate for the amount of plutonium or uranium needed for an effective metal bomb—the hydride sank into obscurity (Hawkins, Truslow and Smith, 1983, pp. 44 and 66). Feynman's star, by contrast, was ascending rapidly. In May 1944, Oppenheimer put Feynman—along with Luis Alvarez, Robert Bacher, Kenneth Bainbridge, Edwin McMillan and Cyril Smith—on a senior committee charged with examining the fabrication and assembly of active materials for all types of nuclear weapons. These would include assembly methods (gun and implosion), materials (plutonium and U-235), and forms of matter (metal, hydride and

²⁵ Theodore Welton, interview with author, 15 May 1998.

deuteride).²⁶ Ranging from problems about metallurgical fabrication to the detailed neutron calculations, it was this group that began to put the various pieces of the puzzle together.

For this fabrication-assembly committee, in June 1944 Feynman took on further criticality calculations for both plutonium and U-235. He had to find the Water Boiler critical mass for both metals, the maximum thickness of sheets of active material immersed in water, the maximum safe radius of a sphere in water or iron, and the complex estimate of behaviour as a sphere changed its shape to a disk.²⁷ As the Trinity test neared, Feynman began listing in his notebook the many questions that he hoped that first detonation would answer. No longer a young upstart, by August of 1945, Feynman was head of Group T-4 (Diffusion Problems), on an administrative par with Peierls (T-1, Implosion Dynamics) or Weisskopf (T-3, Efficiency Theory) (Hawkins, Truslow and Smith, 1983, p. 172).

Following the destruction of Hiroshima and Nagasaki in August 1945, some scientists—including Oppenheimer, Conant, Weisskopf, Teller and Bethe, went public with their moral and political fears and hopes. The Feynman group kept to itself. Richard Ehrlich took a position with General Electric where he worked on reactor theory and design, Ted Welton headed to MIT, then the University of Pennsylvania, and finally, in 1950, back to Oak Ridge. Fred Reines continued at Los Alamos—he switched to experiment—and eventually moved his skills from weapons instrumentation to the detection of the free neutrino at an Atomic Energy Commission reactor. As the war drew to a close, Feynman himself was showered with offers including ones from Berkeley and Princeton, but in the end accepted Bethe's invitation to come to join the faculty at Cornell in the fall of 1945 (Schweber, 1994, pp. 404–405). For almost eighteen months after the war, amidst his resumption of work on electrodynamics, Feynman continued to produce a steady stream of Los Alamos reports, consolidating and clarifying his war work, picturing neutrons as they scattered and fissioned in waste and weapons. On 21 January 1947, Feynman completed one of the last of such tasks, the arduous writing up of the 138 dense pages needed to explain his reasoning and results on the hydride (Feynman, Ashkin and Ehrlich, 1944a, b; Feynman, Ashkin, Ehrlich and Reines, 1944; Feynman and Welton, 1947).

4. The Diffusing Universe

Later in 1947, when Feynman began writing his paper on the theory of the positron, he again formulated it in terms of a diffusion kernel (the Schrödinger equation, though it includes a factor of the imaginary quantity i , is a form of

²⁶ Oppenheimer to Alvarez, Bacher, Bainbridge, Feynman, McMillan and Smith, 5 May 1944. LANL Archives A83-0002, 7-5.

²⁷ Bainbridge to Members of the Committee on Fabrication and Assembly of Active Materials, 9 June 1944. LANL Archives A83-0002, 7-5.

a diffusion equation). Based on the Hamiltonian, the Schrödinger equation tells how things develop in the future from a knowledge of the present. Feynman was after something different:

The author [RPF] has found that the relations are often very much more simply analyzed if the entire time history be considered as one pattern. The entire phenomena [*sic*] is considered as all laid out in the four dimensions of time and space, and that we come upon the successive events. This is applied to simplify the description of the phenomena of pair production in the present paper.²⁸

The contrast of Feynman's approach to quantum electrodynamics with that of Julian Schwinger's was stark. Schwinger began his famous calculation of the Lamb shift precisely by starting with the equations of motion—with the Hamiltonian—he divided it into several parts, rewrote them in terms of creation and annihilation operators and plowed through a difficult computation involving photon energies and polarisations. But just as Feynman had sidestepped the Boltzmann equation in favour of his intuitively constructed kernels, so too he now was after the electron-positron's trajectory. Where Feynman was constructing a widely applicable, 'modular' theory, Schwinger was in pursuit of a coherent and correct, almost deductive analysis from the equations of motion on down. Where Feynman was above all after a representation that could be visualised, Schwinger pursued a more rigorous and austere formal development.²⁹

Schrödinger's equation could be thought of as a type of diffusion process—as Eugene Wigner had pointed out back in 1932 (Wigner, 1932). And in studying this quantum type of diffusion, Feynman re-employed the displacement equation that served him so well in the Los Alamos world of hydride weapons. Only now the function $\psi(2)$ stood for a quantum mechanical wave function and not for a neutron flux density:

$$\psi(2) = \int d(x)K(1 \rightarrow 2)\psi(1). \quad (16)$$

Here, in the quantum case, $K(1 \rightarrow 2)$ represented the Green's function specifying the elementary solution to the Schrödinger equation. (The Schrödinger equation, $i\partial\psi/\partial t = H\psi$ implied $[i\partial/\partial t - H]K(1 \rightarrow 2) = \delta(2, 1)$, which identified K as the Green's function of the equation.) I have used the style of Feynman's notation introduced later in his paper in which 2 stood for x_2, t_2 , and for clarity used Feynman's wartime symbolism in which $K(1 \rightarrow 2)$ is specified instead of $K(2, 1)$.

Making use of Wheeler's prewar suggestion, Feynman dispensed with the idea of separate electrons and positrons, and reconceived the positrons as electrons going backwards in time. By doing so, he transformed (see Fig. 9) the usual process which involves pair creation and pair annihilation with *two* particles into the spacetime trajectory of a *single* particle: the electron. In welding the

²⁸ Feynman, typescript T5, 'Theory of Positrons', RFPF, 6.13.

²⁹ On the postwar contrast between Feynman and Schwinger, see Schweber (1994, pp. 467–473).

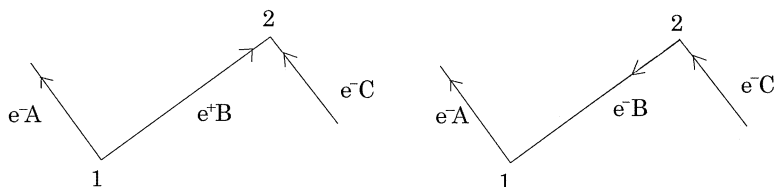


Fig. 9. 'Classical' and 'Feynman' Electron-Positron Trajectories.

electron and positron into one object, Feynman invoked his famous metaphor of the bombardier, calling to mind the war from which the country had just emerged:

A bombardier watching a single road through the bomb-sight of a low flying plane suddenly sees three roads, the confusion only resolving itself when two of them move together and disappear and he realizes he has only passed over a long reverse switchback of a single road. The reversed section represents the positron in analogy, which is first created along with an electron and then moves about and annihilates another electron.³⁰

More subtly, the spacetime track of the electron-positron now becomes a trajectory that can be treated much the way the track of a neutron had been visualised inside the bomb. (See Fig. 10.)

Characteristically, Feynman began with the simplest of all possible cases. Just as he developed his account of neutron scattering from the tamper, step by step, so, too, he started his quantum electrodynamics from an electron scattering once, then twice in a fixed potential. To his friends, Bert and Mulaika Corben, Feynman counselled the same strategy on 19 November 1947:

Take some specific problem or problems, e.g., a single particle in an external field, if that means anything—or two interacting particles. Try to work the thing, if necessary, in one dimension [...] I have always found that it is when I try to do simple problems, that I find the main problems. This way you will find out just what the quantities mean or can mean. Formulary [*sic*], the equation looks very pretty and quite suggestive. It is always impossible for me to judge whether they will yield anything in the end. I hope they do because I would make nature look pretty. I cannot understand mathematics very well as I have told you, and must have physical examples, but that is just the way my mind works.

Feynman had just been to the Washington Theoretical Physics Conference, a meeting, he reported to the Carbens, 'concerned with gravitation and the curvature of the universe and other problems for which there are very powerful mathematical equations—lots of speculation but very little evidence'. Feynman clearly left the war with a profound respect for the measurable and quantifiable.

³⁰ Feynman, typescript T5, 'Theory of Positrons', RFPF, 6.13.

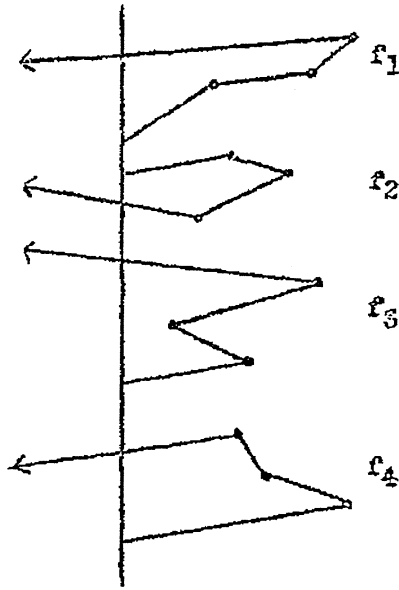


Fig. 10. Visualising Albedo. Closely related to the endpoint problem (how far do the neutrons travel into the tamper?) is the 'albedo problem' in which a specified angular distribution of neutrons hits a surface and one wants to know the fraction reflected back. For the particularly simple case of a uniform angular distribution of incident neutrons, Frankel and Nelson could solve the problem exactly by a very simple visualisable method in which one treated the number of collisions in the tamper successively: one collision, two collisions etc. and then summing. This figure illustrates the different ways the neutrons could scatter three times: backward-backward-backward, forward-backward-backward, and so on. Source: Frankel and Nelson (1944).

The only salvageable bits of the meeting, he reckoned, were Schwinger's comments in which he explained that the electromagnetic self-energy could be explained on the same basis as the Lamb shift and the discrepancy in the hyperfine structure of hydrogen noted by Rabi. Here were numbers and measurements to which theory could be tied.³¹

To see how Feynman began his reasoning about quantum electrodynamics, I want to focus attention on the simplest, first problem he undertook: an electron scattering from a finite potential. Starting with Schrödinger's equation for a Hamiltonian (H) and a wave function (ψ), Feynman writes $i\partial\psi/\partial t = H\psi$. And in this equation, as Feynman noted, the exponential operator moved the wave function from a time to a later time:

$$\exp(-iH\Delta t): \psi(t) \rightarrow \psi(t + \Delta t). \quad (17)$$

³¹ Feynman to the Corbens, 19 November 1947, RFPF, 1.23.

If H has eigenvalues E_n corresponding to eigenfunctions ϕ_n then he could expand the ψ function in terms of these eigenfunctions: $\psi(x, t) = \sum_n C_n \phi_n$. It followed that

$$\psi(x_2, t_2) = \sum_n \exp[-iE_n(t_2 - t_1)] C_n \phi_n(x_2). \quad (18)$$

Notice that this expansion, in combination with the orthonormality of the eigenfunctions, immediately implied $C_n = \int \phi_n^*(x_1) \psi(x_1, t_1) d^3x_1$.

Now, substituting the expression for C_n into the equation for $\psi(2)$ gave:

$$\psi(x_2, t_2) = \sum_n \exp[-iE_n(t_2 - t_1)] \int \phi_n^*(x_1) \psi(x_1, t_1) \phi_n(x_2) d^3x_1. \quad (19)$$

Rearranging terms Feynman then could write:

$$\psi(2) = \int \left\{ \sum_n \exp[-iE_n(t_2 - t_1)] \phi_n^*(x_1) \phi_n(x_2) \right\} \psi(1) d^3x_1. \quad (20)$$

Since $\psi(2) = \int d(x_1) K(1 \rightarrow 2) \psi(1)$, he identified the curly bracket expression with $K(1 \rightarrow 2)$, so

$$K(1 \rightarrow 2) = \sum_n \exp[-iE_n(t_2 - t_1)] \phi_n^*(x_1) \phi_n(x_2). \quad (21)$$

As Feynman put it, $K(1 \rightarrow 2)$ was the ‘total amplitude’ for the arrival of the electron at (2) from (1), an expression tied to his path integral Lagrangian method by thinking of each path as contributing a factor $\exp(iS)$, where the action S was calculated for each path. The total amplitude was then the sum of these paths. Of course, as Feynman made clear, it was possible to use the Hamiltonian method, and to recover (by way of the usual Born approximation) the series of terms involving increasing numbers of scatterings, but as Feynman put it: For some purposes the specification in terms of K is easier to use and visualize’. Sketching another spacetime diagram, Feynman let $U(x, t)$ be a potential allowed to be non-zero over a finite spacetime region, such that $t_1 < t < t_2$ and

$$K(1 \rightarrow 2) = K^{(0)}(1 \rightarrow 2) + K^{(1)}(1 \rightarrow 2) + K^{(2)}(1 \rightarrow 2) + \dots \quad (22)$$

In lowest approximation, he considered $K^{(0)}(1 \rightarrow 2)$ to be just the zeroth order term in the potential U ; $K^{(0)}(1 \rightarrow 2)$ was the ‘free kernel’, so to speak. $K^{(1)}(1 \rightarrow 2)$ was then the first order correction to the zeroth term, and it accounted for corrections in which the particle scattered once in the potential. And $K^{(2)}(1 \rightarrow 2)$ became a spacetime diagram with two scattering events. Double scattering in the potential is, by a natural extension of this reasoning, just

$$K^{(2)}(1 \rightarrow 2) = (-i)^2 \int d(3)d(4) K^{(0)}(4 \rightarrow 2) U(4) K^{(0)}(3 \rightarrow 4) U(3) K^{(0)}(1 \rightarrow 3), \quad (23)$$

for which Feynman offered a visual representation (double scattering in the potential) in Fig. 11b.

Step by step, Feynman pressed this visualisable, snap-together approximation scheme ever deeper into the territory of quantum field theory. And with each advance, as in his analysis of the fluctuations (Fig. 5), Feynman took visual elements pictured in spacetime, and assigned them independent algebraic stand-ins. Moving to the Dirac equation, for example, Feynman allowed the electrons

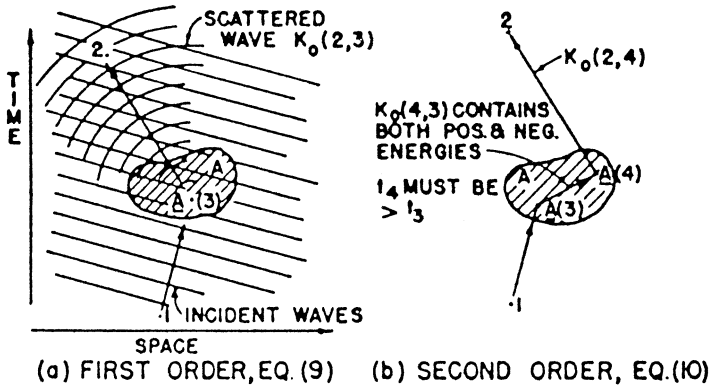


Fig. 11. Positron Scattering From Potential. Source Feynman (1949, p. 751).

to scatter backwards in time, yielding a picture like Fig. 11b only with the intermediate path moving downwards from left to right. He then introduced more complex scatterings, and brought on board rules that corresponded to the emission and absorption of intermediate particles (virtual photons). And while uneasy about their reliability, he even began exploring the extension of these visualisable, modular methods to meson theories. Perhaps most dramatically, by introducing diagrams with loops he was able to eliminate the infinities associated with self-interactions that had hobbled prewar QED. Everywhere, the rule-governed, flexible, picturable theory came as an enormous simplification.

Feynman's desire to disseminate the new rules clearly trumped mathematical-physical niceties. Of one function f_+ that entered into the calculations in a problematic way, Feynman wrote:

One can say therefore, that this attempt to find a consistent modification of quantum electrodynamics is incomplete. For it could turn out that any correct form of f_+ which will guarantee energy conservation may at the same time not be able to make the self-energy integral finite. The desire to make the methods of simplifying the calculation of quantum electrodynamic processes more widely available has prompted this publication before an analysis of the correct form for f_+ is complete (Feynman, 1948, p. 778).

Feynman's new methods found wide acceptance among a generation of younger physicists. It was not simply that Feynman had a way to restore visualisation, it was that the visual elements were identified with such elementary movable rules of formalism that could be used without Hamiltonian field theory and the complexity of the older hole theory. As Feynman had said of his hydride scheme, once learned, the rules of application for the new propagators snapped together easily to yield experimentally-testable results for new systems. Put in a factor of $1/(p - m)$ for an electron's amplitude of propagation, a factor of $1/k^2$ for that of

a photon, and appropriate factors to be summed over for polarisation. Integrate. Perhaps the culmination of this provision of rules was Feynman's analysis of the renormalisation problem—graphs with loops yielding infinities, but where those infinities could be systematically removed by a rule-governed procedure that left finite, experimentally-testable results (Feynman, 1948b). For Feynman, widening the circle of adepts by providing rules of calculation, focussing on observable consequences, and offering visualisable micro-processes were, by the late 1940s, standard fare. For a close reading and suggestions, he could turn to his former Los Alamos theory colleagues Hans Bethe, Theodore Welton, and Julius Ashkin.

But for Paul Dirac—the Dirac Feynman had so admired and with whom he was so frequently compared—Feynman's rule-production culminating in renormalisation proved too much. For over a quarter of a century following Feynman's papers of 1949–1950, Dirac, author of the supremely elegant derivation that led to his eponymous equation, blasted away at the 'complicated and ugly' theory of quantum electrodynamics that in his view 'is just not sensible mathematics'. Only a profound mathematical-aesthetic overturning of QED would render it acceptable. The new physics, while not instrumentally incorrect, was, by Dirac's lights, 'a drastic departure from logic. It changes the whole character of the theory, from logical deductions to a mere setting up of working rules'. Balmer's working rules may have given good results, but as far as Dirac was concerned physical understanding came only with quantum mechanics; Feynman's rules were no better than Balmer's. Or again:

Some physicists may be happy to have a set of working rules leading to results in agreement with observation. They may think that is the goal of physics. But it is not enough. One wants to understand how Nature works. There is strong reason to believe that Nature works according to mathematical laws. All the substantial progress of science supports this view. In elementary particle physics we do not have these mathematical laws, only working rules.³²

Dirac's formation could not have been more different than Feynman's. In Cambridge he had patterned himself on the great mathematical physicists of the 1920s—including Eddington, Jeffreys, Cunningham, Milne and Fowler (Kragh, 1990). This, too, was a specific scientific culture, one that itself differed in key ways from theory as construed in Göttingen, Munich or Berlin. And, appropriately enough, when Dirac made his own contribution to mathematics—his famous delta function—it *had* subsequently been incorporated into a rigorous theory of distributions and convolutions. But out of the war, specifically out of the world of Los Alamos and the MIT Radiation Laboratory, had come an entirely different vision of theory, one that unapologetically sought rules, roughed over the mathematics, and valued procedures that broke complexity

³² I have been guided to these excerpts by H. Kragh's very helpful *Dirac: A Scientific Biography* (Kragh, 1990, pp. 184–185); the extensive quotation is from Dirac (1981, p. 129).

into calculable parts. Even Schwinger's formulation of QED, with its attempt to find solid grounding in a field-theoretical starting point, adopted a black-box approach to renormalisation that did not satisfy Dirac. As Schwinger noted, both he and Tomonaga had learned well the lesson of wartime radar—focus on what could be observed and sidestep the rest. For Dirac, this postwar vision of theory was sufficiently estranged from the prewar world that he could no longer recognise it as a true understanding of nature. An icon, perhaps *the* icon of theoretical physics in the 1930s, wanted something else from an account of nature.

In contrast to Dirac, when Feynman wrote to his friend Theodore Welton, with whom he had travelled the long road through MIT and then Los Alamos, he expected a sympathetic reader. On 16 November 1949, Feynman told Welton he would find the new work easily accessible: 'I assure you you would find [my papers] very simple, at least if you don't try to prove that all the things I say are correct. You know how I work so most of it is just a good guess. All the mathematical proofs were later discoveries that I don't thoroughly understand but the physical ideas I think are very simple. Start with the one about positrons'.³³

5. Conclusion: Theory as Device, Theory as Apparatus

Let me be clear, if repetitive: I am *not* arguing that the culture of theory at Los Alamos was independent of Feynman, a kind of reified exterior apparatus that molded a pliable and unformed Feynman into its image. Nor am I contending that Feynman's Los Alamos theorising was nothing more than a continuation (in method) of his prior work—a simple extension of his visionary studies with Wheeler of the Lagrangian formulation of quantum mechanics, scattering, and absorber theory. I *do* want to emphasise the tools that Feynman brought to New Mexico: an abiding interest in the space of solutions rather than in the equations of motion and a drive to find new perspectives on physical theories. But in the specific culture of theory that prospered in Los Alamos, Feynman's style of reasoning altered in powerful ways. Faced with a constantly changing array of instrument makers, experimenters, calculators, engineers, and bomb designers, Feynman time and again developed techniques to modularise calculations into visualisable, built-up Green's functions that he used to form rules of reckoning to accelerate the production of crucial results. He *never* began with the basic elements of Boltzmannian theory, *never* sought mathematical rigour, and *never* aimed his results far from experiment and measured quantities. Intriguingly, Schwinger, too, adored the Green's function approach to physics, but in his hands the technique looked utterly different. Schwinger aimed to start theory with pure and understandable first principles that anyone could accept; he then strove to maintain every ounce of rigour on the way down to the phenomena in question. As far as Schwinger was concerned, Green's functions,

³³ Feynman to Welton, 16 November 1949, RFPF 3.9.

like every term in his derivations, ought to *emerge* from a principle-based calculation.

When Feynman began the postwar positron paper that launched his quantum electrodynamics, he had nothing like a rigorous grounding for the form of propagators—and had not had one for the ‘propagators’ he used to move neutrons through the core and tamper. As for starting at the high end of theory, it is, I hope, by now no surprise to find that he wrote: ‘The main principle is to deal directly with the solutions to the Hamiltonian differential equations rather than with these equations themselves’ (Feynman, 1949a, p. 749), where those solutions carried elements of graphical representation corresponding to the primitive scattering processes. The move to modularity, to effective, rule-structured and visualisable calculation was one that might have had roots in peace, but it flowered in the exigencies of war.

In the immediate postwar years, Feynman’s picturable kernel-modules that had stood for the sum of diffusing, visualisable paths of particulate neutrons became the kernel-modules that stood for the sum of diffusing, visualisable paths of particulate electron–positron tracks. (See Fig. 12.) Organising interactions in the tamper order-by-order in the number of scatterings became a matter of gathering interactions in a fixed potential, again order-by-order in the number of scatterings. Indeed, after the war, Feynman was so taken with the idea of thinking about the Schrödinger equation as a form of diffusion, that he struggled long and hard to visualise the Dirac equation similarly as a form of the diffusion equation (Schweber, 1994, pp. 406–407).

Much about the generalising process to the Dirac equation and then to a fully interactive quantum electrodynamics followed the template offered by the modular-kernal approach to the hydride problem. Feynman continued to focus on the numerical, measurable results that had to be matched at the end of the day—the Klein–Nishina formula, the Bethe–Heitler formula and the Lamb shift all tied the theory to experimentally observable, quantified results. For Feynman, quantitative success clearly trumped mathematical rigour. Glauber remembers other young Los Alamos theorists vaunting their elaborate contour integration solutions to definite integrals. ‘Sophistry’, Feynman called it as he sought simpler, less formal methods. As Glauber tells it, Feynman maintained precisely the same attitude towards second quantisation (‘more sophistry’) even as this method was supposedly the way to ‘ground’ Feynman’s results.³⁴ In a February 1947 letter to Welton, Feynman confided: ‘Still my stuff sounds mathematical [and] insofar as it is, I still don’t understand it—but I will try soon to reformulate it in terms of seeing how things look to someone riding with the electron.’ ‘What do I mean by understanding?’, Feynman added, ‘[n]othing deep or accurate—just to be able to see some of the qualitative consequences of the equations by some method other than solving them in detail’.³⁵

³⁴ Roy Glauber, interview with author, 5 March 1998.

³⁵ Feynman to Welton, 10 February 1947, RFPF, 3.9.

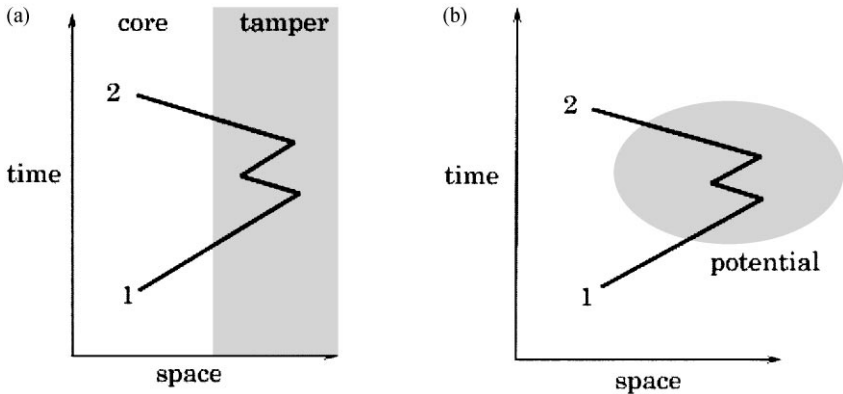


Fig. 12. *Weapon and World.* (a) $\psi(2) = (1 + f) \int d(1)K(1 \rightarrow 2)\psi(1)$. Diffusion kernel: core–tamper. Scatter in tamper. Sum over neutron paths. No neutron–neutron interactions. (b) $\psi(2) = \int d(1)K(1 \rightarrow 2)\psi(1)$. Diffusion kernel: Schrödinger with fixed potential. Scatter in potential. Sum over positron paths. No electron–electron interactions.

What Feynman wanted in the hybrid calculations *and* in his quantum electrodynamics was a description that spoke directly to the underlying, elementary physical processes out of which everything else would be built up: scattering, absorption, fission, radiation. It is in his focus on these modular *processes* that the various elements we have been discussing converge: his orientation towards elementary solutions, visualisation and modular rules. Feynman's rules were never merely instrumental: they were always rules aimed to capture the picturable building blocks of matter in collision.

Taken together, Feynman's wartime and postwar physics contrasted with the very impressive work of other physicists working on similar problems—with the approach of Stanley Frankel and Eldred Nelson during the war, for example, or with that of Julian Schwinger in the years that followed; but far more dramatically with that of Paul Dirac. Put compactly, Feynman's war and postwar work instantiated a theoretical culture that was modular-effective in its compositional, elemental character rather than narrative-deductive in passing from basic equations of motion down through approximations to the case at hand. The modular-effective approach to physics valued the rapid acquisition of quantitative prediction, the flexibility of application, and the simple physical forms of argumentation that constituted a visualisable theory. It devalued all that diverted attention from those goals, most importantly the elaboration of formalism of any sort—bringing down three decades of Dirac's wrath. At the end of his *Theory of Fundamental Processes* (1962), Feynman tersely noted that the (Feynman) 'rules for processes' for a few particles would be sufficient to allow understanding even those processes involving large numbers of particles. 'We have', he concluded, 'described all of physics' (Feynman, 1962, p. 168).

Reduced to an epigraph I would put it this way: neutron diffusion in the core became particle diffusion throughout spacetime, but by way of an extension of

methods within a particular Los Alamos culture of theory, not through any application of the specific wartime. In a subtle, yet profoundly important sense, in Feynman's hands the core of the atomic bomb was transformed into a universe covering all space and time.

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