Path Integral Formulation
Sum over Histories Formulation
Lagrangian Formulation
Amplitude Formulation

Feynman (1941; age 23)

The probability to go from a to b is the square of an amplitude

$$P(b,a) = | \, Amp(b,a) \, |^2$$

The amplitude is the weighted sum over all possible ways to go to b from a

$$Amp(b,a) = \text{constant} \sum_{\text{all paths}} \exp(\mathrm{i}S/\hbar)$$

S is the classical action

Feynman Path Integrals

Non-Relativistic

Two formulations of classical mechanics

Hamiltonian formulation
H = T + V
=> Schrodinger equation formulation of QM

Lagrangian formulation
L = T - V
=> Path integral formulation of QM

Ten good things about the path integral formulation

One bad thing about the path integral formulation

Operators in a Hilbert Space
versus
No Operators
No Hilbert Space

Connection to Euler-Lagrange

- If you compare the Lagrange equations with the Euler-Lagrange equation we developed in the previous chapter, you see that they are identical.
- Since the Euler-Lagrange equation is the solution to the problem of stationarity of a path integral, we see that

$$S = \int_{t_1}^{t_2} \mathrm{L}\, dt$$

  is stationary for the path followed by the particle.
- This integral has a special name in Physics—it is called the **action integral**, and when it is a minimum it is called the principle of least action, although that is a misnomer in the sense that this could be a maximum, or even an inflection point.
- The principle is also called Hamilton's Principle:

> *The actual path that a particle follows between two points 1 and 2 in a given time interval, t_1 to t_2, is such that the action integral*
>
> $$S = \int_{t_1}^{t_2} \mathrm{L}\, dt$$
>
> *is stationary when taken along the actual path.*

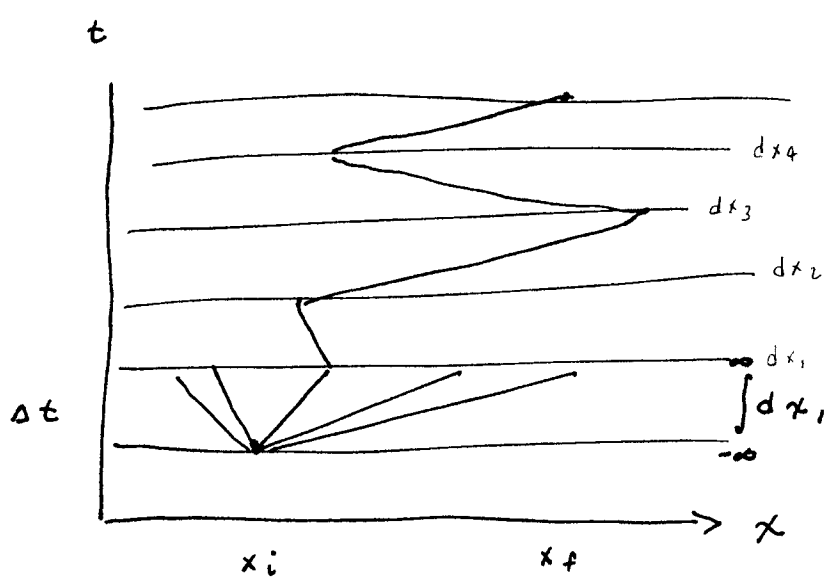
HOW TO DO THE PATH INTEGRAL?

$$A \sum_{\substack{\text{all} \\ \text{paths}}} e^{i\, S[x(t)]/\hbar}$$

$$A \int_{x_i}^{x_f} e^{i\, S[x(t)]/\hbar} \, D[x(t)]$$

FUNCTIONAL INTEGRAL

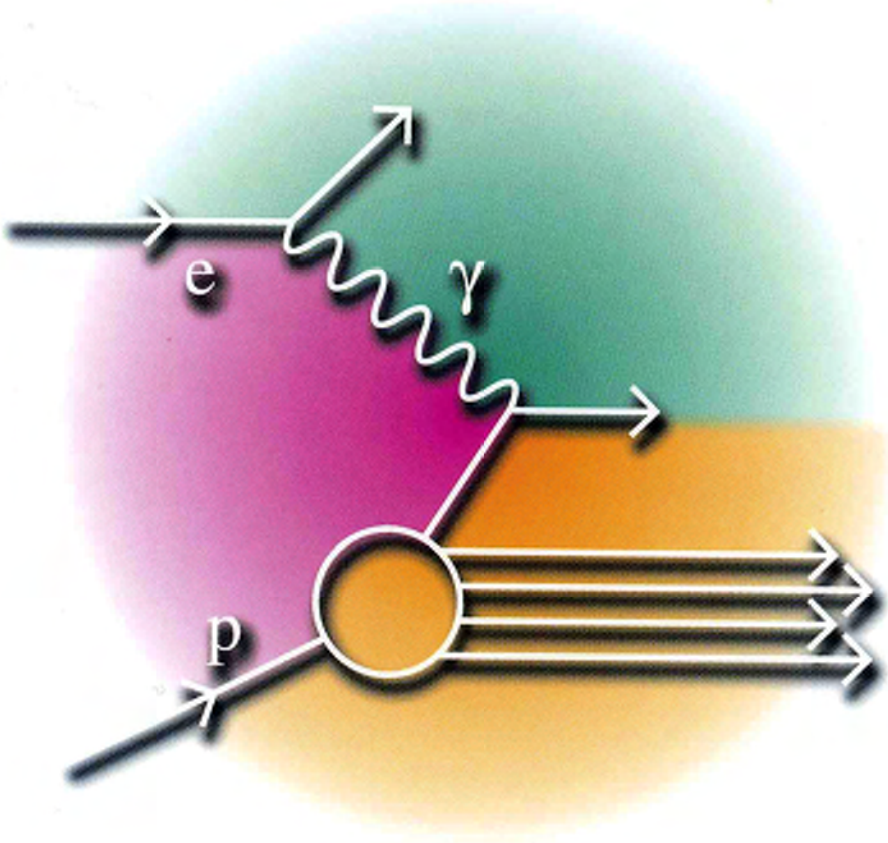
$S[x(t)]$ IS A FUNCTIONAL



$$\lim_{\Delta t \to 0}$$

Quantum Field Theory

A MODERN INTRODUCTION



Michio Kaku

Michio Kaku lists seven advantages of the path integral formulation of quantum mechanics:

1. The path integral formalism yields a simple, covariant quantization of complicated systems with constraints, such as gauge theories. While calculations with the canonical approach are often prohibitively tedious, the path integral method yields the results rather simply, vastly reducing the amount of work.

2. The path integral formalism allows one to go easily back and forth between the other formalisms, such as the canonical or the various covariant approaches. In the path integral approach, these various formulations are nothing but different choices of gauge.

3. The path integral formalism is based intuitively on the fundamental principles of quantum mechanics. Quantization prescriptions, which may seem rather arbitrary in the operator formalism, have a simple physical interpretation in the path integral formalism.

4. The path integral formalism can be used to calculate nonperturbative as well as perturbative results.

5. The path integral formalism is based on c-number fields, rather than q-number operators. Hence, the formalism is much easier to manipulate.

6. At present, there are a few complex systems with constraints that can only be quantized in the path integral formalism.

7. Renormalization theory is much easier to express in terms of path integrals.

M. Kaku QFT: A modern introduction (1993)

(8) Can derive the Schrodinger equation from the path integral

(9) Can apply the path integral to the entire universe

(10) Path integral provides a deeper and more intuitive view of QM

I went to a beer party in the Nassau Tavern in Princeton. There was a gentleman, newly arrived from Europe (Herbert Jehle) who came and sat next to me. Europeans are much more serious than we are in America because they think a good place to discuss intellectual matters is a beer party. So he sat by me and asked, "What are you doing" and so on, and I said, "I'm drinking beer." Then I realized that he wanted to know what work I was doing and I told him I was struggling with this problem, and I simply turned to him and said "Listen, do you know any way of doing quantum mechanics starting with action--where the action integral comes into the quantum mechanics?" "No," he said, "but Dirac has a paper in which the Lagrangian, at least, comes into quantum mechanics. I will show it to you tomorrow."

Next day we went to the Princeton Library (they have little rooms on the side to discuss things) and he showed me this paper. Dirac's short paper in the *Physikalische Zeitschrift der Sowjetunion* claimed that a mathematical tool which governs the time development of a quantal system was "analogous" to the classical Lagrangian.

Professor Jehle showed me this; I read it; he explained it to me, and I said, "What does he mean, they are analogous; what does that mean, *analogous?* What is the use of that?" He said, "You Americans! You always want to find a use for everything!" I said that I thought that Dirac must mean that they were equal. "No," he explained, "he doesn't mean they are equal." "Well," I said, "let's see what happens if we make them equal."

So, I simply put them equal, taking the simplest example . . . but soon found that I had to put a constant of proportionality *A* in, suitably adjusted. When I substituted . . . and just calculated things out by Taylor-series expansion, out came the Schrödinger equation. So I turned to Professor Jehle, not really understanding, and said, "Well you see Professor Dirac meant that they were proportional." Professor Jehle's eyes were bugging out -- he had taken out a little notebook and was rapidly copying it down from the blackboard and said, "No, no, this is an important discovery."

Feynman's thesis advisor, John Archibald Wheeler (age 30), was equally impressed. He believed that the amplitude formulation of quantum mechanics--although mathematically equivalent to the matrix and wave formulations--was so much more natural than the previous formulations that it had a chance of convincing quantum mechanics's most determined critic. Wheeler writes:

action principle in classical mechanics. I was learning from these discussions with Feynman that the integrated action of classical theory, in a sense more precise than ever before appreciated, is—apart from a universal factor, $\hbar = 1.054 \times 10^{-27}$ g cm^2/sec—only another name for the phase of the probability amplitude associated with the classical history.

Visiting Einstein one day, I could not resist telling him about Feynman's new way to express quantum theory. "Feynman has found a beautiful picture to understand the probability amplitude for a dynamical system to go from one specified configuration at one time to another specified configuration at a later time. He treats on a footing of absolute equality every conceivable history that leads from the initial state to the final one, no matter how crazy the motion in between. The contributions of these histories differ not at all in amplitude, only in phase. And the phase is nothing but the classical action integral, apart from the Dirac factor, $\hbar$. This prescription reproduces all of standard quantum theory. How could one ever want a simpler way to see what quantum theory is all about! Doesn't this marvelous discovery make you willing to accept quantum theory, Professor Einstein?" He replied in a serious voice, "I still cannot believe that God plays dice. But maybe," he smiled, "I have earned the right to make my mistakes."

Undeterred I persisted, and still do, in regarding Feynman's PhD thesis as marking a moment when quantum theory for the first time became simpler than classical theory. I began my upcoming graduate course in classical mechanics with Feynman's idea that the microscopic point particle makes its way from A to B, not by a unique history, but by pursuing every conceivable history with democratically equal probability amplitude. Only out of Huygens's principle, only out of the concept of constructive and destructive interference between these contributions—and this only in an approximation—could one understand the existence of the classical history. Feynman sat there and took the course notes, of which I still have a mimeographed copy. On many a puzzling point he helped us both to find new light by discussions in class and out.

Any Career for the Kid from Far Rockaway?

While Richard was working on his thesis, his father, Melville Arthur Feynman, sales manager for a medium-sized uniform company, made a brief call on me in my office one day. How important he had been in

Derive the Schrodinger equation 2

Feynman's derivation of the Schrödinger equation[a)]

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R. P. Feynman's "path integral" approach to quantum field theory emerged from his discovery that the Schrödinger equation could be derived from an expression of P. A. M. Dirac's involving the action. Feynman gave the history of his discovery in his Nobel lecture, but withheld most of the details; when he did publish the discovery, he did not provide the framework. This article provides some historical background and puts both of Feynman's presentations together. © *1996 American Association of Physics Teachers.*

I. INTRODUCTION

A recent book by Schweber[1] describes the early history of quantum electrodynamics, the first example of quantum field theory. If a student picks up Schweber's book, or any text of quantum field theory, she will find the name of Feynman writ very large. Many of the standard techniques of quantum field theory derive from Feynman's "sum over histories" approach to quantum mechanics. Feynman's first triumph with this approach was his derivation of the Schrödinger equation. Feynman, a consummate teacher with a unique gift for simplicity, presented his work to two very different audiences. In his Nobel lecture,[2] he explained how he came to the calculation, but left out most of the mathematics. For the experts he worked through it more than once,[3] but did not provide much of a starting point. This article attempts to put together these two descriptions into a story more accessible than either alone.

II. FEYNMAN'S BACKGROUND: NUMERICS AND LAGRANGIANS

From an early age Feynman taught himself as much mathematics as he could, not only theorems but the practical and numerical parts as well.[4] His calculational skill was legendary, even among those whose careers were devoted to numerical analysis. For example, in Acton[5] we read:

> Art, mostly in the form of intuition, still plays too large a part to permit consistent results when invoked by different persons... In the hands of a Feynman the technique works like a Latin charm; with ordinary mortals the result is a mixed bag.

Consequently, given any formal expression, Feynman was very likely to play around with it, and as far as possible, winkle numbers out of the thing. In particular, he was a master of approximation.[6] It is said that at Los Alamos, Enrico Fermi, John von Neumann and Feynman would check each other's calculations, Fermi using a slide rule, Feynman a Marchant (mechanical) calculator, and von Neumann his head; all three obtaining very nearly the same answer at very nearly the same time.

Feynman seems to have had unusually good high school teachers. One of these, a Mr. Bader, introduced Feynman to the Euler–Lagrange equations, and Lagrange's approach to mechanics. Feynman's account of this is fascinating.[7] This material is taught usually in the second year of an American physics degree.

In 1788 Joseph Louis Lagrange, developing earlier work of Leonhard Euler, published *Méchanique Analytique*, a new approach to Newtonian mechanics based on the calculus of variations. A familiar problem in calculus is to find the point at which a given function takes on its greatest or least value. The corresponding problem in the calculus of variations is, given the integral of an unknown function, find the function that makes the value of the integral a maximum or minimum. The calculus of variations, one of Feynman's favorite and most powerful techniques,[8] arose from a single problem, the *brachistochrone*:[9] Given two points A and B in a vertical plane, find the shape of a ramp which will take a mass sliding under the force of gravity from A to B in the shortest possible time. The problem was originally published in 1696 by Johann Bernoulli, who apparently hoped that his brother, Jakob, would fail to solve it. Though his solution took several months (Newton is said to have solved it the same night he read it!), Jakob alone recognized the essential point: any curve which has a minimum property globally (in the large) must have that same property *locally* (in the small), as Bernoulli's diagram (see Fig. 1) will help make clear:

Suppose the curve ACEDB has the desired minimal property (in this case, the path of least time from A to B). Let C and D be two points on the curve. Then, said Bernoulli, CED must have this same property, i.e., it must be the quickest path from C to D. If it were not, then there would be some other path, CFD say, which would be faster from C to D. But, were that so, the path ACFDB would be faster than the path ACEDB, which is contrary to hypothesis. Thus, the path quickest overall must be quickest in between any intermediary points, and the property which holds globally must also hold locally.

Bernoulli's "local principle" turns an integral equation (the time from A to B) into a differential equation, far more amenable to analysis. As will be seen, this is precisely what Feynman did.

Lagrange's approach concerns the integral of the difference of kinetic and potential energies. Denote the kinetic energy by K and the potential energy by U. The kinetic energy K depends on the object's velocity $v=dx/dt$, while the potential energy depends usually only on the object's position x. Then, form the Lagrangian function L as the difference $K-U$. Now, said Lagrange, insist that the actual physical path followed by an object is just that unique trajectory which minimizes the integral S;

$$S=\int_{t_1}^{t_2} L\,dt=\int_{t_1}^{t_2}(K-U)\,dt. \tag{2.1}$$

In the past, this integral was called "Hamilton's Principal Function" after the 19th century Irish mathematician Will-

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Derbes

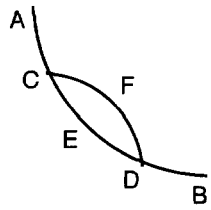


Fig. 1. Bernoulli's diagram.

iam Rowan Hamilton. Today, following Feynman, everyone calls it "the action." The methods of the calculus of variations applied to the action S lead to the Euler–Lagrange equation

$$\frac{\partial L}{\partial x}-\frac{d}{dt}\left(\frac{\partial L}{\partial(dx/dt)}\right)=0. \tag{2.2}$$

In the simplest cases, $K=\frac{1}{2}mv^2$ where m is a (constant) mass and U is a function of x only, so the Euler–Lagrange equation produces

$$-\frac{\partial U}{\partial x}=\frac{d}{dt}(mv), \tag{2.3}$$

which is just Newton's second law. That is, the path that minimizes the action S is the *same* path for which Newton's second law holds true. Since the second law works well to describe the paths of objects, it seems to be the case that somehow nature chooses, out of an infinite set of paths, the *one* that minimizes S.

III. FROM CLASSICAL TO QUANTUM MECHANICS

The fundamental equation for quantum mechanics, analogous to Newton's second law for classical mechanics, is Schrödinger's equation:

$$-\frac{\hbar^2}{2m}\frac{\partial^2\psi}{\partial x^2}+U\psi=i\hbar\frac{\partial\psi}{\partial t}. \tag{3.1}$$

Schrödinger obtained this equation by analogy with the connection between geometric optics, when light is treated as if it consisted of small particles, and wave optics, in which the wave nature of light determines its behavior. In geometric optics, the path of light may be found also with a variational principle, Fermat's principle: Light travels from one given point to another in the shortest time. Mathematically, this means that the phase ϕ of the light wave must vary as little as possible from one point to another. The wave Z itself may be described by a function of the form[10]

$$Z=ae^{i\phi}, \tag{3.2}$$

where Z is the wave function, a its amplitude and ϕ its phase. If we ignore the wave nature of light, treating the light as particles, we effectively take the wave length to be very small. For classical mechanics, we minimize the action S, just as in Fermat's principle we minimize ϕ. So a reasonable thing to do is to *guess* with Schrödinger that the light wave phase ϕ corresponds to the action S, and consequently a wave description of mechanics *might* take the approximate form[11]

$$\psi=ae^{iS/\hbar}. \tag{3.3}$$

The division by Planck's constant is to make the argument of the exponential dimensionless; both Planck's constant and S have the same dimensions. Schrödinger knew that the action S obeyed the Hamilton–Jacobi equation;

$$\frac{\partial S}{\partial t}+\frac{1}{2m}\left(\frac{\partial S}{\partial x}\right)^2+U=0. \tag{3.4}$$

(In many cases, this is just the conservation of energy in disguise; the first term equals $-E$, and the second term is the square of the momentum mv divided by $2m$, or the kinetic energy.) Using the guess for ψ above, it follows:

$$\frac{\partial S}{\partial x}=-\frac{i\hbar}{\psi}\frac{\partial\psi}{\partial x} \tag{3.5}$$

and making due allowance for the complex nature of ψ, we can recast the Hamilton–Jacobi equation as

$$-E+\frac{1}{2m}\left(\frac{\partial S}{\partial x}\right)^*\left(\frac{\partial S}{\partial x}\right)+U=0, \tag{3.6}$$

where the * indicates complex conjugation. That is,

$$-E+\frac{1}{2m}\left(\frac{i\hbar}{\psi^*}\frac{\partial\psi^*}{\partial x}\right)\left(-\frac{i\hbar}{\psi}\frac{\partial\psi}{\partial x}\right)+U=0 \tag{3.7}$$

or rearranging

$$(U-E)\psi^*\psi+\frac{\hbar^2}{2m}\left(\frac{\partial\psi^*}{\partial x}\right)\left(\frac{\partial\psi}{\partial x}\right)=0 \tag{3.8}$$

We can call the expression on the left hand side M and, following Schrödinger, use it as a Lagrangian with generalized coordinates ψ, ψ^*, $\partial\psi/\partial x$, and $\partial\psi^*/\partial x$. That is, we minimize the integral T;

$$T=\int M\,dx=\int (U-E)\psi^*\psi+\frac{\hbar^2}{2m}\left(\frac{\partial\psi^*}{\partial x}\right)\left(\frac{\partial\psi}{\partial x}\right)dx. \tag{3.9}$$

The Euler–Lagrange equation (for ψ^*) then gives

$$\frac{\partial M}{\partial\psi^*}-\frac{\partial}{\partial x}\left(\frac{\partial M}{\partial(\partial\psi^*/\partial x)}\right)=0 \tag{3.10}$$

or

$$(U-E)\psi-\frac{\hbar^2}{2m}\frac{\partial^2\psi}{\partial x^2}=0, \tag{3.11}$$

which, after rearrangement, is just the (time-independent) Schrödinger equation.

IV. FEYNMAN'S DERIVATION

Feynman's Nobel lecture describes his earliest attempts while a graduate student of John A. Wheeler's at Princeton to make sense of the quantum theory of photons and electrons. He soon became convinced that the Lagrangian was the key to the problem. While attending a party at the Nassau Tavern, Feynman asked a visiting European professor, Herbert Jehle, if he knew of any work incorporating the Lagrangian into quantum mechanics. Jehle did, and the next day showed Feynman an article by P. A. M. Dirac.[12]

To describe Dirac's work, it may be helpful to recall another analogy from optics, Huygens' principle, which treats light not as particles but as waves. Given one wave front, Huygens took that front as the progenitor of smaller waves, which collectively made up the next wave. In this manner the

wave disturbance propagated forwards. Mathematically, Huygens' principle takes the form of an integral equation:

$$\psi(x,t_2)=\int G(x,y)\psi(y,t_1)dy \quad \text{where } t_2>t_1. \tag{4.1}$$

The function $G(x,y)$ is called a "kernel" or a "propagator." Dirac's article made the suggestion that this kernel was "analogous" to an expression we have already seen, namely $\exp(iS/\hbar)$. It would be criminal not to let Feynman tell in his own words what came next:[13]

> Professor Jehle showed me this, I read it, he explained it to me, and I said, "what does he mean, they are analogous; what does that mean, *analogous*? What is the use of that?" He said, "you Americans! You always want to find a use for everything!" I said that I thought that Dirac must mean that they were equal. "No", he explained, "he doesn't mean they are equal." "Well," I said, "let's see what happens if we make them equal."

As Bernoulli before him, Feynman knew that if it was true for any two times, it had to be true for times separated by a small interval Δt, which he denoted ϵ. This would allow him to use all his approximation techniques. Then

$$S=\int L dt \approx L_{\text{average}}\epsilon. \tag{4.2}$$

In the simplest case, the Lagrangian is just the difference of the kinetic energy, $K=\frac{1}{2}mv^2$, and a potential energy $U(x)$. Then

$$K_{\text{average}}=(\tfrac{1}{2})m(\Delta x/\Delta t)^2=(\tfrac{1}{2})m(x-y)^2/\epsilon^2, \tag{4.3a}$$

$$U_{\text{average}}=U(\tfrac{1}{2}(x+y)), \tag{4.3b}$$

$$S\approx \tfrac{1}{2}m(x-y)^2\epsilon-U(\tfrac{1}{2}(x+y))\epsilon, \tag{4.3c}$$

so if Feynman's first guess was correct,

$$G(x,y)=\exp(iS/\hbar)$$
$$\approx\exp\{(\mathrm{i}m(x-y)^2/2\epsilon\hbar)-(U(\tfrac{1}{2}(x+y))\epsilon/\hbar)\}. \tag{4.4}$$

The exponential of the sum is the product of the exponentials, so the kernel becomes

$$G(x,y)\approx\exp(\mathrm{i}m(x-y)^2/2\epsilon\hbar)$$
$$\times\exp(-U(\tfrac{1}{2}(x+y))\epsilon/\hbar). \tag{4.5}$$

The first factor has ϵ in the denominator, and the second in the numerator. Since ϵ is supposed to be small, the second factor may be expanded in the well-known Taylor series for $\exp(x)$. That is, to first order in ϵ,

$$G(x,y)\approx\exp(\mathrm{i}m(x-y)^2/2\epsilon\hbar)(1-U(\tfrac{1}{2}(x+y))\epsilon/\hbar). \tag{4.6}$$

Putting this expression into Dirac's integral equation gives

$$\psi(x,t+\epsilon)\approx\int \exp\left(\frac{\mathrm{i}m(x-y)^2}{2\hbar\epsilon}\right)$$
$$\times\left\{1-\frac{i}{\hbar}\epsilon U\left(\frac{x+y}{2}\right)\right\}\psi(y,t)dy. \tag{4.7}$$

Although the integration extends from $-\infty$ to ∞, Feynman felt that the rapid oscillation of the exponential factor (due to the small sizes of Planck's constant and the time interval) would cause the integrand to be very small, except where $x-y$ was likewise small. So, he decided to rewrite the integral in terms of the difference $x-y=\xi$;

$$\psi(x,t+\epsilon)\approx\int \exp\left(\frac{\mathrm{i}m\xi^2}{2\hbar\epsilon}\right)\left\{1-\frac{i}{\hbar}\epsilon U\left(x-\frac{1}{2}\xi\right)\right\}$$
$$\times\psi(x-\xi,t)d\xi. \tag{4.8}$$

Since the integrand is large only when ξ is small, it made sense to Feynman to expand $\psi(x-\xi)$ in a Taylor series:

$$\psi(x-\xi,t)=\psi(x,t)-\xi\frac{\partial\psi(x,t)}{\partial x}+\frac{1}{2}\xi^2\frac{\partial^2\psi(x,t)}{\partial x^2}-\cdots. \tag{4.9}$$

Evidently, it was at this point that Feynman discovered his first guess could not be exactly right, because to lowest order in ϵ the integral equation becomes

$$\psi(x,t)\approx\int \exp\left(\frac{\mathrm{i}m\xi^2}{2\hbar\epsilon}\right)\psi(x,t)d\xi=\sqrt{\frac{2\pi i\hbar\epsilon}{m}}\,\psi(x,t) \tag{4.10}$$

from the well-known Gaussian integral, $\int\exp(-\alpha x^2)dx=\sqrt{\pi/\alpha}$. Consequently, the kernel G could not be equal to the expression $\exp(iS/\hbar)$; at best it could be only proportional. Feynman then put in a constant of proportionality A into his original guess [cf. (4.4)]:

$$G=A\exp(iS/\hbar) \quad \text{with } A=\sqrt{\frac{m}{2\pi i\hbar\epsilon}}. \tag{4.11}$$

Then, plugging in the Taylor expression, and writing $U(x-\xi/2)\,\epsilon=U(x)\epsilon$ to lowest order, the integral equation is

$$\psi(x,t+\epsilon)\approx A\int \exp\left(\frac{\mathrm{i}m\xi^2}{2\hbar\epsilon}\right)\left\{1-\frac{i}{\hbar}\epsilon U(x)\right\}$$
$$\times\left\{\psi(x,t)-\xi\frac{\partial\psi}{\partial x}+\frac{1}{2}\xi^2\frac{\partial^2\psi}{\partial x^2}\right\}d\xi \tag{4.12}$$

$$\approx\left\{1-\frac{i}{\hbar}\epsilon U(x)\right\}\psi(x,t)$$
$$-A\frac{\partial\psi}{\partial x}\int \exp\left(\frac{\mathrm{i}m\xi^2}{2\hbar\epsilon}\right)\xi d\xi$$
$$+\frac{1}{2}A\frac{\partial^2\psi}{\partial x^2}\int \exp\left(\frac{\mathrm{i}m\xi^2}{2\hbar\epsilon}\right)\xi^2 d\xi. \tag{4.13}$$

(There are other terms, but these are the only ones which are no more than linear in the time interval, ϵ.) The term involving the first derivative of ψ is an odd function of ξ, so it equals zero. Then differentiating the Gauss integral above with respect to α (another technique Feynman was fond of[14]) yields the identity

$$\int x^2\exp(-\alpha x^2)dx=\frac{1}{2\alpha}\sqrt{\frac{\pi}{\alpha}} \tag{4.14}$$

so that the term involving the second derivative of ψ is

$$\frac{1}{2}A\frac{\partial^2\psi}{\partial x^2}\int \exp\left(\frac{\mathrm{i}m\xi^2}{2\hbar\epsilon}\right)\xi^2\,d\xi$$
$$=\frac{1}{2}A\frac{\partial^2\psi}{\partial x^2}\frac{i\hbar\epsilon}{m}\sqrt{\frac{2\pi i\hbar\epsilon}{m}}=\frac{1}{2}\frac{i\hbar\epsilon}{m}\frac{\partial^2\psi}{\partial x^2}. \quad (4.15)$$

Gathering all these terms together, the integral equation becomes

$$\psi(x,t+\epsilon)\approx\psi(x,t)-\frac{i}{\hbar}\epsilon U(x)\psi(x,t)+\frac{1}{2}\frac{i\hbar\epsilon}{m}\frac{\partial^2\psi}{\partial x^2}, \quad (4.16)$$

which, after multiplying by $i\hbar$, can be rearranged into

$$i\hbar\frac{\psi(x,t+\epsilon)-\psi(x,t)}{\epsilon}\approx-\frac{\hbar^2}{2m}\frac{\partial^2\psi}{\partial x^2}+U(x)\psi(x,t). \quad (4.17)$$

But the left-hand side is, to lowest order, just $i\hbar\partial\psi/\partial t$. This is *exactly* the (time-dependent) Schrödinger equation. Continuing with Feynman's Nobel lecture:

> So, I simply put them equal, taking the simplest example where the Lagrangian is $\frac{1}{2}M\dot{x}^2-V(x)$ but soon found I had to put a constant of proportionality A in, suitably adjusted. When I substituted $A\exp(i\epsilon L/\hbar)$ for [the kernel] to get [the equivalent of equation (3.7)], and just calculated things out by Taylor series expansion, out came the Schrödinger equation. So, I turned to Professor Jehle, not really understanding, and said, "well, you see Professor Dirac meant that they were proportional." Professor Jehle's eyes were bugging out—he had taken out a little notebook and was rapidly copying it down from the blackboard, and said, "no, no this is an important discovery. You Americans are always trying to find out how something can be used. That's a good way to discover things!" So, I thought I was finding out what Dirac meant, but, as a matter of fact, I had made the discovery that what Dirac thought was analogous was, in fact, equal. I had then, at least, the connection between the Lagrangian and quantum mechanics, but still with wave functions and infinitesimal times.

Within a day or two, Feynman realized that any arbitrary time Δt—equal to 2ϵ, 3ϵ, ..., $N\epsilon$—could be built up by this method, repeating the integration N-fold times. Such a procedure Feynman named "a sum over histories," and the resulting multiple integral a "path integral." Soon thereafter America entered the Second World War. Upon finishing his doctorate in the spring of 1942, Feynman set out for Los Alamos, where his energies would be entirely devoted to the construction of the atomic bomb. He was not to find the time to extend his methods fully to quantum electrodynamics until after the war's conclusion.

[a]In memory of Diana Van Valen.

[1]Silvan S. Schweber, *QED and the Men Who Made It* (Princeton U.P., Princeton, NJ, 1994).

[2]Richard P. Feynman, Nobel Lecture in Physics 1965, "The Development of the Space-Time View of Quantum Electrodynamics," Science **153**, 699–708 (1966); Reprinted in *Harvard Project Physics Reader*, "The Nucleus," edited by Gerald Holton, F. James Rutherford, and Fletcher G. Watson (Holt, Rinehart & Winston, New York 1970), Vol. 6.

[3]R. P. Feynman, "Space-Time Approach to Non-Relativistic Quantum Mechanics," Rev. Mod. Phys. **20**, 367–387 (1948), reprinted in J. Schwinger, *Quantum Electrodynamics* (Dover, New York 1958); R. P. Feynman and A. R. Hibbs, *Quantum Mechanics and Path Integrals* (McGraw-Hill, New York, 1965), Chap. 4. The bulk of the calculation in Sec. IV is taken directly from this source.

[4]James Gleick, *Genius: The Life and Science of Richard Feynman* (Pantheon, New York 1992), pp. 32–36; Ref. 1, p. 374.

[5]Forman S. Acton, *Numerical Methods That (usually) Work* (Mathematical Association of America reprint, 1990), p. 252. (The original was published by Harper and Row, New York 1972.)

[6]Reference 1, pp. 398–401; Reference 4 pp. 175–178; pp. 182–183. The lecture Gleick refers to here is probably not very different from Lecture 22, Vol. I, "Algebra," in R. P. Feynman, R. B. Leighton and M. Sands, *The Feynman Lectures on Physics* (Addison-Wesley, Reading, MA, 1963); See also R. P. Feynman, *Surely You're Joking, Mr. Feynman* (Norton, New York, 1985), pp. 192–198, "Lucky Numbers."

[7]R. P. Feynman, R. B. Leighton and M. Sands, Ref. 6, Vol. II, Lecture 19, "The Principle of Least Action"; For more about Abraham Bader, see J. Mehra, *The Beat of a Different Drum: The Life and Science of Richard Feynman* (Oxford U.P., Oxford, UK, 1994), pp. 31–33.

[8]Reference 7; see also R. P. Feynman, *The Character of Physical Law* (Modern Library, New York 1994), pp. 46–52 and 97–99. (This is a reprint of the original MIT publication, Cambridge, MA, 1965.)

[9]Constance Reid, *Hilbert* (Springer-Verlag, Berlin, 1969), p. 68. For the original statement of the problem, as well as the solutions of both Bernoulli brothers, see D. J. Struik, *A Source Book in Mathematics, 1200–1800* (Harvard U.P., Cambridge, MA, 1969), pp. 391–99. (This book has been reprinted in paper covers by Princeton U.P., Princeton, NJ, 1993.)

[10]L. D. Landau and E. M. Lifshitz, *Quantum Mechanics* (Addison-Wesley, Reading, MA, 1965), 2nd ed. p. 20. For the connection with the Hamilton-Jacobi equation, see p. 52.

[11]E. Schrödinger, "Quantisierung als Eigenwertproblem," Ann. Phys. **79**, 361–376 (1926). English translations of this paper and others in the same series may be found in *Wave Mechanics*, edited by Gunter Ludwig (Pergamon, Oxford, 1968), and E. Schrödinger, *Collected Papers on Wave Mechanics* (Chelsea, New York, 1978). Schrödinger did not at first make ψ a complex function, using instead the guess $S=K\ln\psi$ and ultimately requiring K to be equal to Planck's constant. Many physicists found Schrödinger's first derivation of his equation less than compelling; see, e.g., Walter Moore, *Schrödinger: Life and Thought* (Oxford U.P., Oxford, UK, 1989), p. 200, or for a contemporary account, G. Birtwistle, *The New Quantum Mechanics* (Cambridge U.P., Cambridge, UK, 1928), p. 137. I am here doing violence to history for the sake of pedagogy.

[12]P. A. M. Dirac, "The Lagrangian in Quantum Mechanics," Phys. Zeits. Sowjetunion **3** (1), 64–72 (1933). Reprinted in Schwinger, Ref. 3.

[13]R. P. Feynman, Nobel Lecture (Ref. 2).

[14]R. P. Feynman, *Surely You're Joking, Mr. Feynman*, pp. 86–87 (Ref. 6).

when we can make time slices, that is, when the operators are time ordered. From the path integral point of view, this is the origin of the time ordering in the matrix elements.

8.2 Derivation of the Schrödinger Equation

In a first quantized formalism, where the action is a function of x^i and not fields, the path integral formalism gives us an added bonus: It gives us a derivation of the Schrödinger equation. Usually, introductory courses in quantum mechanics begin by postulating the Schrödinger wave equation. Certain conventions, such as the quantization of x and p, seem rather arbitrary. Only later emerges the probabilistic interpretation. Here, we reverse this order: we begin with the probabilistic postulates of quantum mechanics and derive the Schrödinger wave equation as a consequence, thus giving a new physical interpretation to that equation.

In the path integral approach, the evolution of a state is given by the transition function $K(b, a)$. From a classical point of view, this can be viewed as the analogue of Huygen's principle, where the evolution of a wave can be determined by assuming that each point along a wave front emits a new wave front. The integration over all these infinitesimal wave fronts then gives us the overall evolution of the wave front. Mathematically, this is given by:

$$\psi(x_j, t_j) = \int_{-\infty}^{\infty} K(x_j, t_j; x_i, t_i)\psi(x_i, t_i)\, dx_i \tag{8.34}$$

Earlier, we derived, assuming only the Lagrangian $(1/2)mv_i^2$, an expression for the nonrelativistic transition function. Now let us calculate how wave fronts move with this transition function. The time evolution, from t to $t + \delta t$, is given by:

$$\psi(x, t + \epsilon) = \int_{-\infty}^{\infty} A^{-1} \exp\left(\frac{im(x - y)^2}{2\epsilon}\right) \psi(y, t)\, dy \tag{8.35}$$

where:

$$A = \left(\frac{2\pi i\epsilon}{m}\right)^{1/2} \tag{8.36}$$

To perform this integration, let dy be replaced by $d\eta$, where $\eta = y - x$:

$$\psi(x, t + \epsilon) = \int_{-\infty}^{\infty} A^{-1} e^{im\eta^2/2\epsilon} \psi(x + \eta, t)\, d\eta \tag{8.37}$$

Now Taylor expand the left-hand side in terms of t, and the right-hand side in terms of η:

$$\begin{aligned}\psi(x,t)+\epsilon\frac{\partial\psi}{\partial t} &= \int_{-\infty}^{\infty} A^{-1}e^{im\eta^2/2\epsilon} \\ &\quad\times\left(\psi(x,t)+\eta\frac{\partial\psi}{\partial x}+\frac{1}{2}\eta^2\frac{\partial^2\psi}{\partial x^2}+\cdots\right)d\eta \qquad (8.38)\end{aligned}$$

The integration over $d\eta$ is easily performed. The integration over the linear term in η vanishes because it is linear, and the integration over the higher terms vanish in the limit $\epsilon \to 0$. This gives us:

$$i\frac{\partial\psi}{\partial t} = -\frac{1}{2m}\frac{\partial^2\psi}{\partial x^2} \qquad (8.39)$$

This is the Schrödinger wave equation, as desired. It is straightforward to insert a potential into the path integral, in which case we derive the Schrödinger wave equation in a potential, which is the traditional starting point for quantum mechanics.

8.3 From First to Second Quantization

So far, we have only investigated the path integral formalism in the first-quantized formalism, reproducing known results. The reader may complain that the path integral formalism is an elaborate, powerful machinery that has only rederived simple results. However, when we make the transition to the second quantized formalism and eventually to gauge theory, we will find that the path integral approach is the preferred formalism for quantum field theory. We saw earlier that the integration over all intermediate points along a path was enforced by inserting the number "1" at each intermediate point:

$$1 = |x_i, t_i\rangle \int dx_i \langle x_i, t_i| \qquad (8.40)$$

The transition to field theory is made by introducing yet another expression for the number "1," this time based on an integration over an infinite number of degrees of freedom. We will use the familiar Gaussian integration:

$$\delta_{ij} = \frac{2}{(\sqrt{\pi})^n}\int \prod_{l=1}^{n} dx_l \; (x_i\, x_j)\exp\left(-\sum_{k=1}^{n}(x_k)^2\right) \qquad (8.41)$$

Chapter 3

The Schrödinger Equation

3.1 Derivation of the Schrödinger Equation

We will consider now the propagation of a wave function $\psi(\vec{r},t)$ by an infinitesimal time step ϵ. It holds then according to (2.5)

$$\psi(\vec{r},t+\epsilon) \;=\; \int_\Omega d^3r_0\, \phi(\vec{r},t+\epsilon|\vec{r}_0,t)\, \psi(\vec{r}_0,t)\;. \tag{3.1}$$

We will expand the l.h.s. and the r.h.s. of this equation in terms of powers of ϵ and we will demonstrate that the terms of order ϵ require that $\psi(\vec{r},t)$ satisfies a partial differential equation, namely the Schrödinger equation. For many situations, but by no means all, the Schrödinger equation provides the simpler avenue towards describing quantum systems than the path ingral formulation of Section 2. Notable exceptions are non-stationary systems involving time-dependent linear and quadratic Lagrangians.
The propagator in (3.1) can be expressed through the discretization scheme (2.20, 2.21). In the limit of very small ϵ it is sufficient to employ a single discretization interval in (2.20) to evaluate the propagator. Generalizing (2.20) to $\mathbb{R}^3$ one obtains then for small ϵ

$$\phi(\vec{r},t+\epsilon|\vec{r}_0,t) \;=\; \left[\frac{m}{2\pi i\hbar\epsilon}\right]^{\frac{3}{2}} \exp\left\{\frac{i}{\hbar}\left[\frac{m}{2}\frac{(\vec{r}-\vec{r}_0)^2}{\epsilon} \;-\; \epsilon\, U(\vec{r},t)\right]\right\}\;. \tag{3.2}$$

From this follows

$$\psi(\vec{r},t+\epsilon) \;=\; \int_\Omega d^3r_0\, \left[\frac{m}{2\pi i\hbar\epsilon}\right]^{\frac{3}{2}} \exp\left\{\frac{i}{\hbar}\left[\frac{m}{2}\frac{(\vec{r}-\vec{r}_0)^2}{\epsilon} \;-\; \epsilon\, U(\vec{r},t)\right]\right\} \psi(\vec{r}_0,t)\;. \tag{3.3}$$

In order to carry out the integration we set $\vec{r}_0 \;=\; \vec{r} + \vec{s}$ and use $\vec{s}$ as the new integration variable. We will denote the components of $\vec{s}$ by $(x_1,x_2,x_3)^T$. This yields

$$\begin{aligned}\psi(\vec{r},t+\epsilon) \;&=\; \textstyle\int_{-\infty}^{+\infty} dx_1 \int_{-\infty}^{+\infty} dx_2 \int_{-\infty}^{+\infty} dx_3\, \left[\frac{m}{2\pi i\hbar\epsilon}\right]^{\frac{3}{2}} \times \\ &\times \underbrace{\exp\left(\frac{i}{\hbar}\,\frac{m}{2}\frac{x_1^2+x_2^2+x_3^2}{\epsilon} \;-\; \epsilon\, U(\vec{r},t)\right)}_{\text{even in } x_1,x_2,,x_3} \psi(\vec{r}+\vec{s},t)\;.\end{aligned} \tag{3.4}$$

It is important to note that the integration is **not** over $\vec{r}$, but over $\vec{s} = (x_1, x_2, x_3)^T$, e.g. $U(\vec{r}, t)$ is a constant with respect to this integration. The integration involves only the wave function $\psi(\vec{r}+\vec{s}, t)$ and the kinetic energy term. Since the latter contributes to (3.4) only for small $x_1^2 + x_2^2 + x_3^2$ values we expand

$$\psi(\vec{r}+\vec{s}, t) \;=\; \psi(\vec{r}, t) \;+\; \sum_{j=1}^{3} x_j \frac{\partial}{\partial x_j}\psi(\vec{r}, t) \;+\; \frac{1}{2}\sum_{j,k=1}^{3} x_j x_k \frac{\partial^2}{\partial x_j \partial x_k}\psi(\vec{r}, t) \;+\; \ldots \tag{3.5}$$

assuming that only the leading terms contribute, a supposition which will be examined below. Since the kinetic energy contribution in (3.4) is even in all three coordinates x_1, x_2, x_3, only terms of the expansion (3.5) which are even separately in all three coordinates yield non-vanishing contributions. It is then sufficient to consider the terms

$$\psi(\vec{r}, t) \quad ; \quad \tfrac{1}{2}\textstyle\sum_{j=1}^{3} x_j^2 \frac{\partial^2}{\partial x_j^2}\psi(\vec{r}, t) \quad ; \quad \tfrac{1}{4}\textstyle\sum_{j,k=1}^{3} x_j^2 x_k^2 \frac{\partial^4}{\partial x_j^2 \partial x_k^2}\psi(\vec{r}, t) \quad ;$$
$$\tfrac{1}{12}\textstyle\sum_{j=1}^{3} x_j^4 \frac{\partial^4}{\partial x_j^4}\psi(\vec{r}, t) \quad ; \ldots \tag{3.6}$$

of the expansion of $\psi(\vec{r}+\vec{s}, t)$.
Obviously, we need then to evaluate integrals of the type

$$I_n(a) \;=\; \int_{-\infty}^{+\infty} dx\, x^{2n} \exp\left(i\, a\, x^2\right) \;, \quad n \;=\; 0, 1, 2 \tag{3.7}$$

According to (2.36) holds

$$I_0(a) \;=\; \sqrt{\frac{i\pi}{a}} \tag{3.8}$$

Inspection of (3.7) shows

$$I_{n+1}(a) \;=\; \frac{1}{i}\frac{\partial}{\partial a} I_n(a). \tag{3.9}$$

Starting from (3.8) one can evaluate recursively all integrals $I_n(a)$. It holds

$$I_1(a) \;=\; \frac{i}{2a}\sqrt{\frac{i\pi}{a}}\;; \quad I_2(a) \;=\; -\frac{3}{4a^2}\sqrt{\frac{i\pi}{a}}\;, \ldots \tag{3.10}$$

It is now important to note that in case of integral (3.4) one identifies

$$\frac{1}{a} \;=\; \frac{2\,\epsilon\,\hbar}{m} \;=\; \mathcal{O}(\epsilon) \tag{3.11}$$

and, consequently, the terms collected in (3.6) make contributions of the order

$$\mathcal{O}(\epsilon^{\frac{3}{2}})\;,\; \mathcal{O}(\epsilon^{\frac{5}{2}})\;,\; \mathcal{O}(\epsilon^{\frac{7}{2}})\;,\; \mathcal{O}(\epsilon^{\frac{7}{2}})\;. \tag{3.12}$$

Here one needs to note that we are actually dealing with a three-fold integral. According to (3.11) holds

$$\left[\frac{m}{2\pi i\hbar\epsilon}\right]^{\frac{3}{2}} \times \left[\frac{i\,\pi}{a}\right]^{\frac{3}{2}} \;=\; 1 \tag{3.13}$$

and one can conclude, using (3.10),

$$\psi(\vec{r}, t+\epsilon) \;=\; \exp\left[-\frac{i\epsilon}{\hbar}\,U(\vec{r},t)\right]\left[\psi(\vec{r},t) \;+\; \frac{1}{4}\,\frac{2i\epsilon\hbar}{m}\,\nabla^2\psi(\vec{r},t) \;+\; \mathcal{O}(\epsilon^2)\right]\,. \tag{3.14}$$

This expansion in terms of powers of ϵ suggests that we also expand

$$\psi(\vec{r}, t+\epsilon) \;=\; \psi(\vec{r},t) \;+\; \epsilon\frac{\partial}{\partial t}\psi(\vec{r},t) \;+\; \mathcal{O}(\epsilon^2) \tag{3.15}$$

and

$$\exp\left[-\frac{i\epsilon}{\hbar}\,U(\vec{r},t)\right] \;=\; 1 \;-\; \frac{i\epsilon}{\hbar}\,U(\vec{r},t) \;+\; \mathcal{O}(\epsilon^2)\,. \tag{3.16}$$

Inserting this into (3.14) results in

$$\begin{aligned}\psi(\vec{r},t) \;+\; \epsilon\tfrac{\partial}{\partial t}\psi(\vec{r},t) \quad &= \psi(\vec{r},t) \;-\; \frac{i\epsilon}{\hbar}\,U(\vec{r},t)\psi(\vec{r},t) \\ &\quad + \frac{i\epsilon}{\hbar}\,\frac{\hbar^2}{2m}\,\nabla^2\psi(\vec{r},t) \;+\; \mathcal{O}(\epsilon^2)\,.\end{aligned} \tag{3.17}$$

Obviously, this equation is trivially satisfied to order $\mathcal{O}(\epsilon^0)$. In order $\mathcal{O}(\epsilon)$ the equation reads

$$i\hbar\frac{\partial}{\partial t}\,\psi(\vec{r},t) \;=\; \left[-\frac{\hbar^2}{2m}\,\nabla^2 \;+\; U(\vec{r},t)\right]\psi(\vec{r},t)\,. \tag{3.18}$$

This is the celebrated *time-dependent Schrödinger equation.* This equation is often written in the form

$$i\hbar\frac{\partial}{\partial t}\,\psi(\vec{r},t) \;=\; \hat{H}\,\psi(\vec{r},t) \tag{3.19}$$

where

$$\hat{H} \;=\; -\frac{\hbar^2}{2m}\,\nabla^2 \;+\; U(\vec{r},t)\,. \tag{3.20}$$

3.2 Boundary Conditions

The time-dependent Schrödinger equation is a partial differential equation, 1st order in time, 2nd order in the spatial variables and linear in the solution $\psi(\vec{r},t)$. The following general remarks can be made about the solution.

Due to its linear character any linear combination of solutions of the time-dependent Schrödinger equation is also a solution.

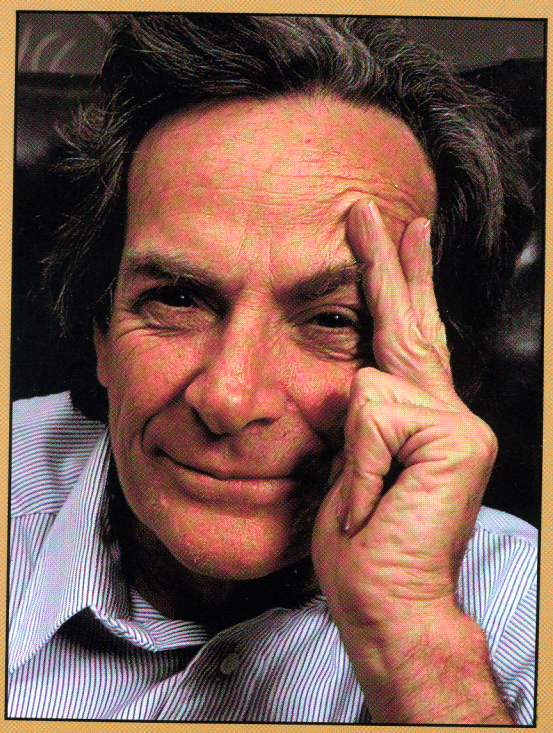
The 1st order time derivative requires that for any solution a single temporal condition needs to be specified, e.g., $\psi(\vec{r},t_1) \;=\; f(\vec{r})$. Usually, one specifies the so-called initial condition, i.e., a solution is thought for $t \;\geq\; t_0$ and the solution is specified at the intial time t_0.

The 2nd order spatial derivatives require that one specifies also properties of the solution on a closed boundary $\partial\Omega$ surrounding the volume Ω in which a solution is to be determined. We will derive briefly the type of boundary conditions encountered. As we will discuss in Chapter 5 below the solutions of the Schrödinger equation are restricted to particular Hilbert spaces $\mathbb{H}$ which are

"Most of the Good Stuff"

Memories Of

RICHARD FEYNMAN



Editors: LAURIE M. BROWN · JOHN S. RIGDEN

how it may be possible to generalize the method so as to describe *not only the dynamics of the universe but also its initial boundary condition* in terms of the classical action S. Furthermore, there are now, as I mentioned above, some indications that the conventional formulation of quantum mechanics may not be justifiable except to the extent that a background space-time emerges with small quantum fluctuations. Hartle in particular has emphasized such a possibility.

One crude way to see the argument is to express the wavefunction of the universe (which we assume to be in a pure state) as a path integral over all the fields in nature (for example, the infinity of local fields represented, roughly speaking, by the superstring), reserving the integral over the metric $g_{\mu\nu}$ for last. The total action S can be represented as the Einstein action S_G for pure gravitation plus the actions S_M for all the other, "matter" fields, including their coupling to gravitation. We have, then, crudely,

$$\text{Amplitude} = \int \mathscr{D} g_{\mu\nu} \exp \frac{iS_G}{\hbar} \times \int \mathscr{D}(\text{everything else}) \exp \frac{iS_M}{\hbar}$$

For the moment, suppose only $g_{\mu\nu}$ configurations corresponding to a simple topology for space-time are allowed.

Before the integration over $g_{\mu\nu}$ is performed, there is a definite space-time, with the possibility of constructing well-defined space-like surfaces in a definite succession described by a time-like variable. There is an equivalent Hilbert-space formalism; we have unitarity (conservation of positive probability); and we can have conventional causality (it corresponds in the Hilbert-space formulation to the requirement of time ordering of operators in the formula for probabilities).

Now, when the integral over $g_{\mu\nu}$ is done, it is no longer clear that any of that machinery remains, since we are integrating over the structure of space-time and once the integral is performed it is hard to point to space-like surfaces or a succession described by a time-like variable. Of course it may be possible to construct a Hilbert-space formulation, with unitarity and causality, in some new way, perhaps employing a new, external time variable of some kind (what Feynman liked to call a fifth wheel), but it is by no means certain that such a program can be carried out.

At this stage, we may admit the possibility of summing over all topologies of space-time (or of the corresponding space-time with a Euclidean metric). If that is the correct thing to do, then we are immediately transported into the realm of baby universes and worm-holes, so beloved of Stephen Hawking and now so fashionable, in which it seems to be demonstrable that the cosmological constant vanishes. In that realm the path integral method appears able to cope, and it remains to be seen to what extent the conventional formulation of quantum mechanics can keep up.

For Richard's sake (and Dirac's too), I would rather like it to turn out that the path integral method is the real foundation of quantum mechanics and thus of physical theory. This is true despite the fact that, having an algebraic turn of mind, I have always personally preferred the operator approach, and despite the added difficulty, in the absence of a Hilbert-space formalism, of interpreting the wavefunction or density matrix of the universe (already a bit difficult to explain in any case, as anyone attending my classes will attest). If notions of transformation theory, unitarity and causality really emerge from the mist only after a fairly clear background metric appears (that metric itself being the result of a quantum mechanical probabilistic process), then we may have a little more explaining to do. Here Dick Feynman's talents and clarity of thought would have been a help.

Turning Things Around

Richard, as is well known, liked to look at each problem, important or unimportant, in a new way—"turning it around," as he would say. He told how his father, who died when he was young, taught him to do that. This approach went along with Richard's extraordinary efforts to be different, especially from his friends and colleagues.

Of course any of us engaged in creative work, and in fact anyone having a creative idea even in everyday life, has to shake up the usual patterns in some way in order to get out of the rut (or the basin of attraction!) of conventional thinking, dispense with certain accepted but wrong notions, and find a new and better way to formulate some problem. But with Dick, "turning things around" and being different became a passion.

The result was that on certain occasions, in scientific work or in ordinary living, when an imaginative new way of looking at things was needed, he could come up with a remarkably useful innovation. But on

action principle in classical mechanics. I was learning from these discussions with Feynman that the integrated action of classical theory, in a sense more precise than ever before appreciated, is—apart from a universal factor, $\hbar = 1.054 \times 10^{-27}$ g cm^2/sec—only another name for the phase of the probability amplitude associated with the classical history.

Visiting Einstein one day, I could not resist telling him about Feynman's new way to express quantum theory. "Feynman has found a beautiful picture to understand the probability amplitude for a dynamical system to go from one specified configuration at one time to another specified configuration at a later time. He treats on a footing of absolute equality every conceivable history that leads from the initial state to the final one, no matter how crazy the motion in between. The contributions of these histories differ not at all in amplitude, only in phase. And the phase is nothing but the classical action integral, apart from the Dirac factor, $\hbar$. This prescription reproduces all of standard quantum theory. How could one ever want a simpler way to see what quantum theory is all about! Doesn't this marvelous discovery make you willing to accept quantum theory, Professor Einstein?" He replied in a serious voice, "I still cannot believe that God plays dice. But maybe," he smiled, "I have earned the right to make my mistakes."

Undeterred I persisted, and still do, in regarding Feynman's PhD thesis as marking a moment when quantum theory for the first time became simpler than classical theory. I began my upcoming graduate course in classical mechanics with Feynman's idea that the microscopic point particle makes its way from A to B, not by a unique history, but by pursuing every conceivable history with democratically equal probability amplitude. Only out of Huygens's principle, only out of the concept of constructive and destructive interference between these contributions—and this only in an approximation—could one understand the existence of the classical history. Feynman sat there and took the course notes, of which I still have a mimeographed copy. On many a puzzling point he helped us both to find new light by discussions in class and out.

Any Career for the Kid from Far Rockaway?

While Richard was working on his thesis, his father, Melville Arthur Feynman, sales manager for a medium-sized uniform company, made a brief call on me in my office one day. How important he had been in

Path integral gives us insight into the extremely nonlocal nature of quantum mechanics.

So, why not teach the path integral method from the very beginning?

Path integral is much more difficult than Schrodinger equation for simple NRQM problems, viz., hydrogen atom and spin.

On the other hand, easier or comparable to the canonical method for relativistic problems.

Quantum Field Theory
IN A NUTSHELL

A. Zee

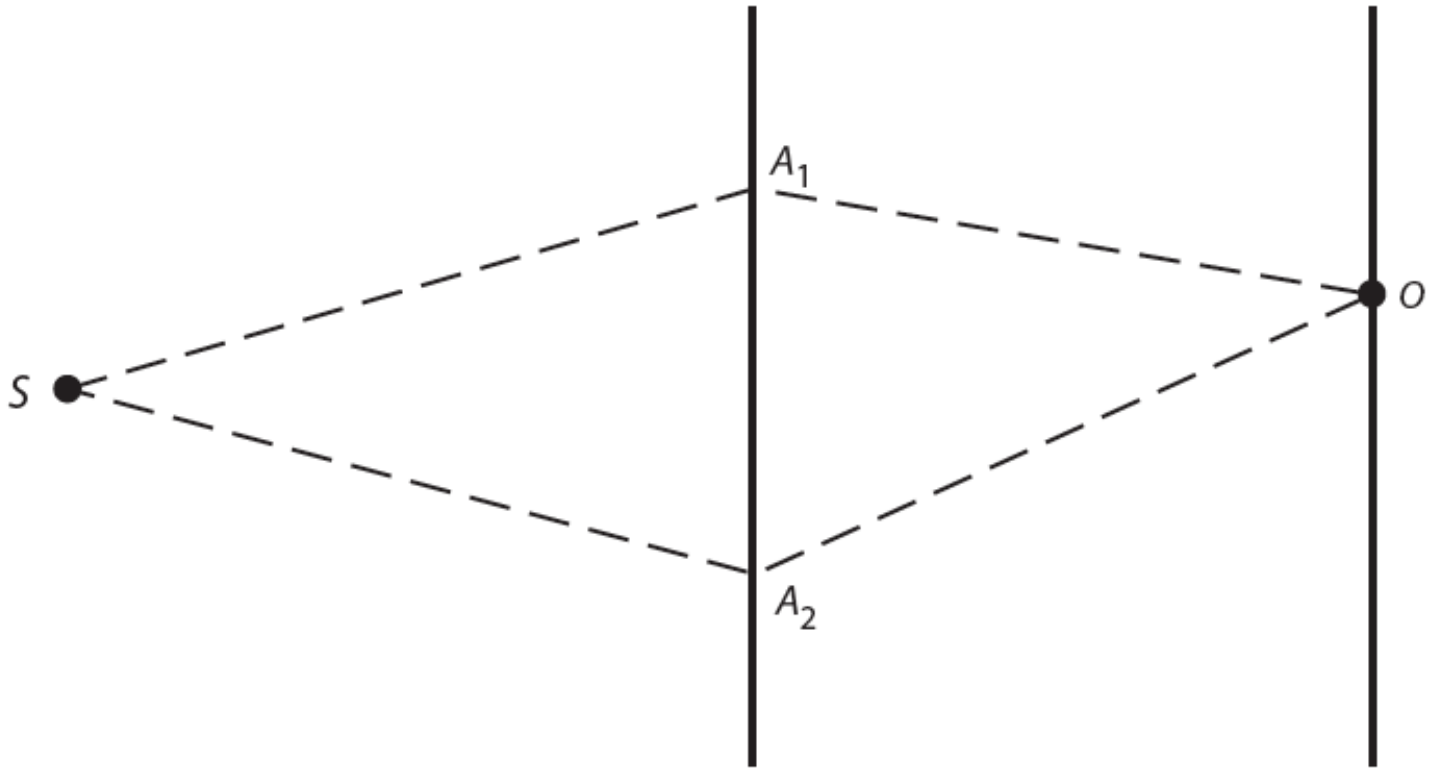


Figure I.2.1

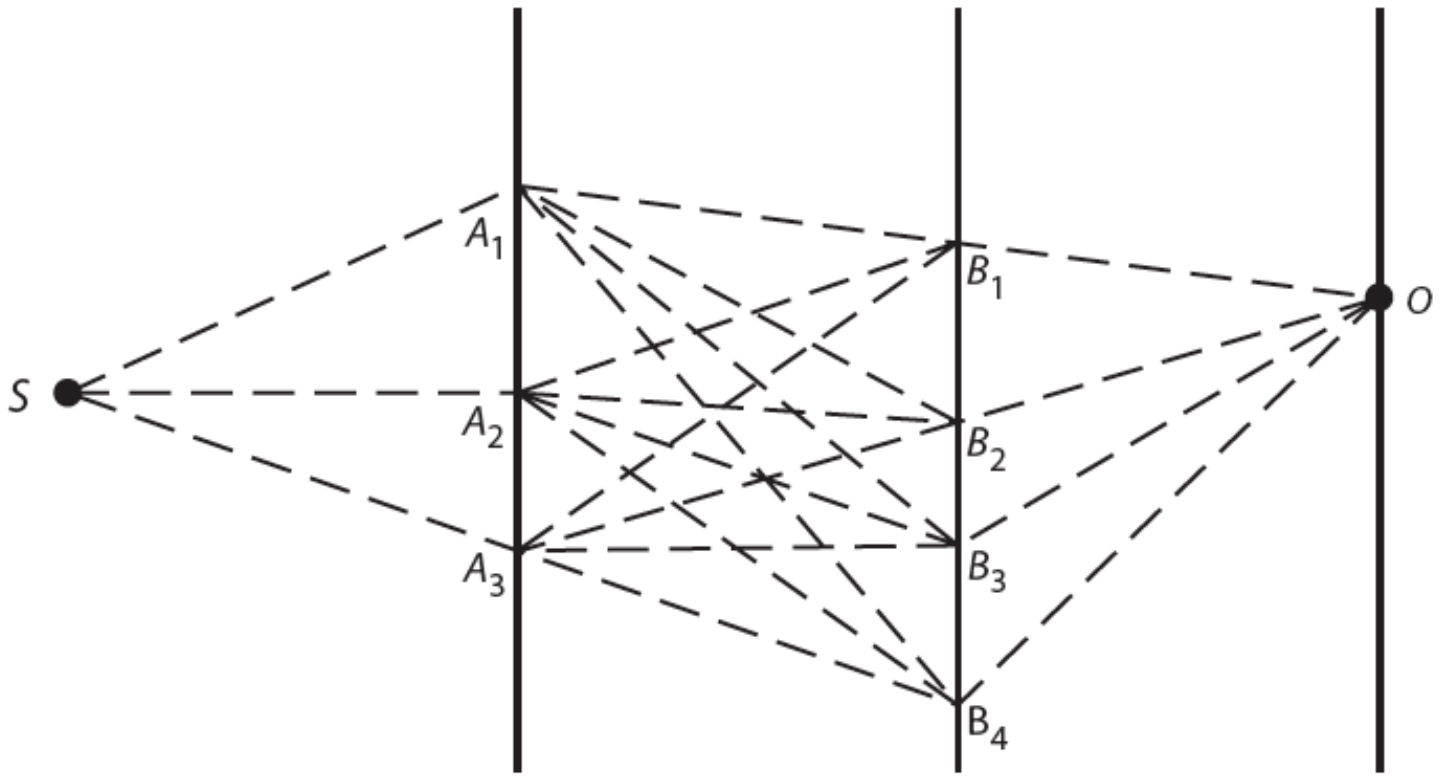


Figure I.2.2

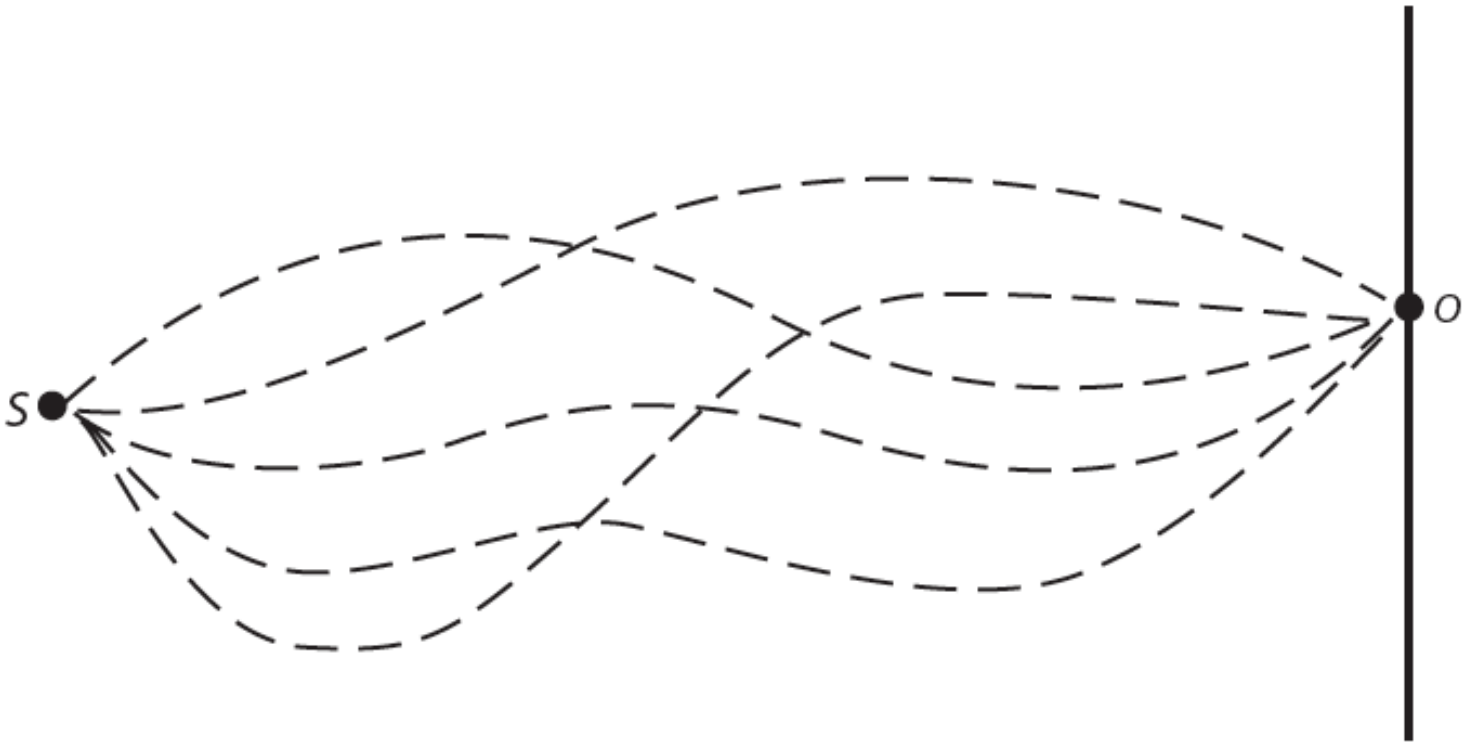


Figure I.2.3

Chapter I.2

Path Integral Formulation of Quantum Physics

The professor's nightmare: a wise guy in the class

As I noted in the preface, I know perfectly well that you are eager to dive into quantum field theory, but first we have to review the path integral formalism of quantum mechanics. This formalism is not universally taught in introductory courses on quantum mechanics, but even if you have been exposed to it, this chapter will serve as a useful review. The reason I start with the path integral formalism is that it offers a particularly convenient way of going from quantum mechanics to quantum field theory. I will first give a heuristic discussion, to be followed by a more formal mathematical treatment.

Perhaps the best way to introduce the path integral formalism is by telling a story, certainly apocryphal as many physics stories are. Long ago, in a quantum mechanics class, the professor droned on and on about the double-slit experiment, giving the standard treatment. A particle emitted from a source S (Fig. I.2.1) at time $t = 0$ passes through one or the other of two holes, A_1 and A_2, drilled in a screen and is detected at time $t = T$ by a detector located at O. The amplitude for detection is given by a fundamental postulate of quantum mechanics, the superposition principle, as the sum of the amplitude for the particle to propagate from the source S through the hole A_1 and then onward to the point O and the amplitude for the particle to propagate from the source S through the hole A_2 and then onward to the point O.

Suddenly, a very bright student, let us call him Feynman, asked, "Professor, what if we drill a third hole in the screen?" The professor replied, "Clearly, the amplitude for the particle to be detected at the point O is now given by the sum of three amplitudes, the amplitude for the particle to propagate from the source S through the hole A_1 and then onward to the point O, the amplitude for the particle to propagate from the source S through the hole A_2 and then onward to the point O, and the amplitude for the particle to propagate from the source S through the hole A_3 and then onward to the point O."

The professor was just about ready to continue when Feynman interjected again, "What if I drill a fourth and a fifth hole in the screen?" Now the professor is visibly

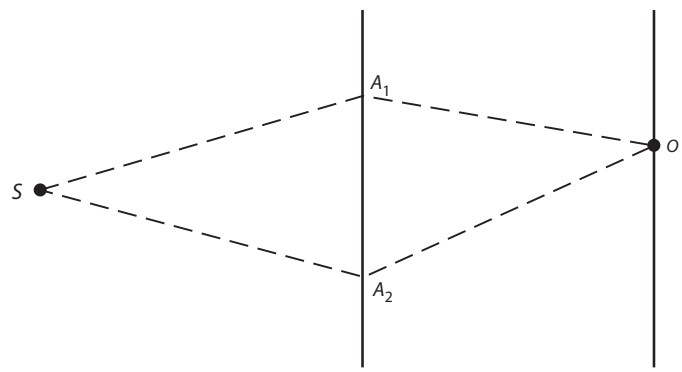


Figure I.2.1

losing his patience: "All right, wise guy, I think it is obvious to the whole class that we just sum over all the holes."

To make what the professor said precise, denote the amplitude for the particle to propagate from the source S through the hole A_i and then onward to the point O as $\mathcal{A}(S \to A_i \to O)$. Then the amplitude for the particle to be detected at the point O is

$$\mathcal{A}(\text{detected at } O) = \sum_i \mathcal{A}(S \to A_i \to O) \tag{1}$$

But Feynman persisted, "What if we now add another screen (Fig. I.2.2) with some holes drilled in it?" The professor was really losing his patience: "Look, can't you see that you just take the amplitude to go from the source S to the hole A_i in the first screen, then to the hole B_j in the second screen, then to the detector at O, and then sum over all i and j?"

Feynman continued to pester, "What if I put in a third screen, a fourth screen, eh? What if I put in a screen and drill an infinite number of holes in it so that the

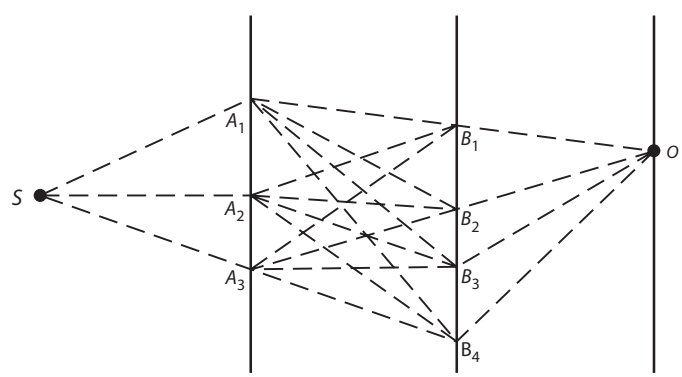


Figure I.2.2

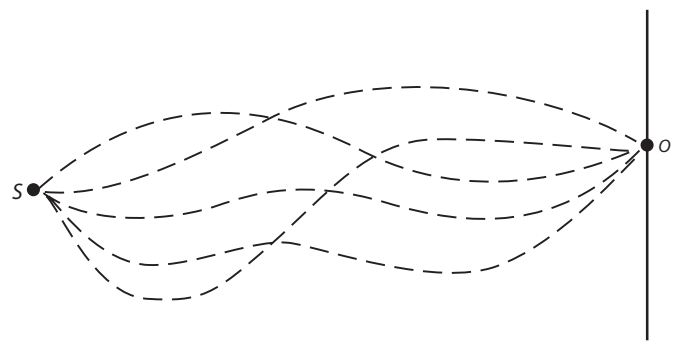


Figure I.2.3

screen is no longer there?" The professor sighed, "Let's move on; there is a lot of material to cover in this course."

But dear reader, surely you see what that wise guy Feynman was driving at. I especially enjoy his observation that if you put in a screen and drill an infinite number of holes in it, then that screen is not really there. Very Zen! What Feynman showed is that even if there were just empty space between the source and the detector, the amplitude for the particle to propagate from the source to the detector is the sum of the amplitudes for the particle to go through each one of the holes in each one of the (nonexistent) screens. In other words, we have to sum over the amplitude for the particle to propagate from the source to the detector following all possible paths between the source and the detector (Fig. I.2.3).

$$\mathcal{A}(\text{particle to go from } S \text{ to } O \text{ in time } T) =$$

$$\sum_{(\text{paths})} \mathcal{A}\left(\text{particle to go from } S \text{ to } O \text{ in time } T \text{ following a particular path}\right) \quad (2)$$

Now the mathematically rigorous will surely get anxious over how $\sum_{(\text{paths})}$ is to be defined. Feynman followed Newton and Leibniz: Take a path (Fig. I.2.4), approximate it by straight line segments, and let the segments go to zero. You can see that this is just like filling up a space with screens spaced infinitesimally close to each other, with an infinite number of holes drilled in each screen.

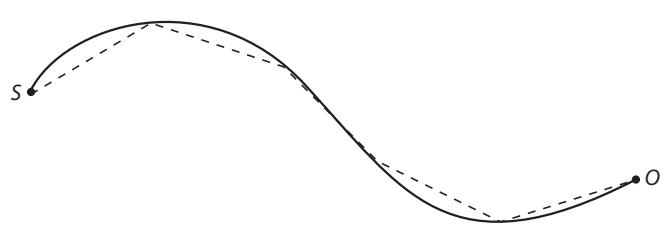


Figure I.2.4

Fine, but how to construct the amplitude $\mathcal{A}$(particle to go from S to O in time T following a particular path)? Well, we can use the unitarity of quantum mechanics: If we know the amplitude for each infinitesimal segment, then we just multiply them together to get the amplitude of the whole path.

In quantum mechanics, the amplitude to propagate from a point q_I to a point q_F in time T is governed by the unitary operator e^{-iHT}, where H is the Hamiltonian. More precisely, denoting by $|q\rangle$ the state in which the particle is at q, the amplitude in question is just $\langle q_F| e^{-iHT} |q_I\rangle$. Here we are using the Dirac bra and ket notation. Of course, philosophically, you can argue that to say the amplitude is $\langle q_F| e^{-iHT} |q_I\rangle$ amounts to a postulate and a definition of H. It is then up to experimentalists to discover that H is hermitean, has the form of the classical Hamiltonian, et cetera.

Indeed, the whole path integral formalism could be written down mathematically starting with the quantity $\langle q_F| e^{-iHT} |q_I\rangle$, without any of Feynman's jive about screens with an infinite number of holes. Many physicists would prefer a mathematical treatment without the talk. As a matter of fact, the path integral formalism was invented by Dirac precisely in this way, long before Feynman.

A necessary word about notation even though it interrupts the narrative flow: We denote the coordinates transverse to the axis connecting the source to the detector by q, rather than x, for a reason which will emerge in a later chapter. For notational simplicity, we will think of q as 1-dimensional and suppress the coordinate along the axis connecting the source to the detector.

Dirac's formulation

Let us divide the time T into N segments each lasting $\delta t = T/N$. Then we write

$$\langle q_F| e^{-iHT} |q_I\rangle \;=\; \langle q_F| e^{-iH\delta t} e^{-iH\delta t} \cdots e^{-iH\delta t} |q_I\rangle$$

Now use the fact that $|q\rangle$ forms a complete set of states so that $\int dq\, |q\rangle\langle q| = 1$. Insert 1 between all these factors of $e^{-iH\delta t}$ and write

$$\begin{aligned}
&\langle q_F| e^{-iHT} |q_I\rangle \\
&= (\prod_{j=1}^{N-1} \int dq_j) \langle q_F| e^{-iH\delta t} |q_{N-1}\rangle \langle q_{N-1}| e^{-iH\delta t} |q_{N-2}\rangle \cdots \\
&\cdots \langle q_2| e^{-iH\delta t} |q_1\rangle \langle q_1| e^{-iH\delta t} |q_I\rangle
\end{aligned} \tag{3}$$

Focus on an individual factor $\langle q_{j+1}| e^{-iH\delta t} |q_j\rangle$. Let us take the baby step of first evaluating it just for the free-particle case in which $H = \hat{p}^2/2m$. The hat on $\hat{p}$ reminds us that it is an operator. Denote by $|p\rangle$ the eigenstate of $\hat{p}$, namely $\hat{p}\,|p\rangle = p\,|p\rangle$. Do you remember from your course in quantum mechanics that $\langle q|p\rangle = e^{ipq}$? Sure you do. This just says that the momentum eigenstate is a plane wave in the coordinate representation. (The normalization is such that $\int (dp/2\pi)\, |p\rangle\langle p| = 1$.) So again inserting a complete set of states, we write

which states that there is a natural limit to the subtlety of any experiment or the refinement of any measurement.

The Structure of the Amplitude. The amplitude for an event is the sum of the amplitudes for the various alternative ways that the event can occur. This permits the amplitude to be analyzed in many different ways depending on the different classes into which the alternatives can be divided. The most detailed analysis results from considering that a particle going from A to B, for example, in a given time interval, can be considered to have done this by going in a certain motion (position vs. time) or path in space and time. We shall therefore associate an amplitude with each possible motion. The total amplitude will be the sum of a contribution from each of the paths.

This idea can be made more clear by a further consideration of our experiment with the two holes. Suppose we put a couple of extra screens between the source and the hole. Call these screens D and E. In each of them we drill a few holes which we number D_1, D_2, . . . and E_1, E_2, . . . (Fig. 1-9). For simplicity, we shall assume the electrons are constrained to move in the xy plane. Then there are several alternative paths which an electron may take in going from the source to the hole in screen B. It could go from source to D_2, and then E_3, and then the hole 1; or it could go from the source to D_3, then E_1, and finally to the hole 1, etc. Each of these paths has its own amplitude. The complete amplitude is the sum of all of them.

Next, suppose we continue to drill holes in the screens D and E until there is nothing left of the screens. The path of an electron must now be specified by the height x_D at which the electron passes

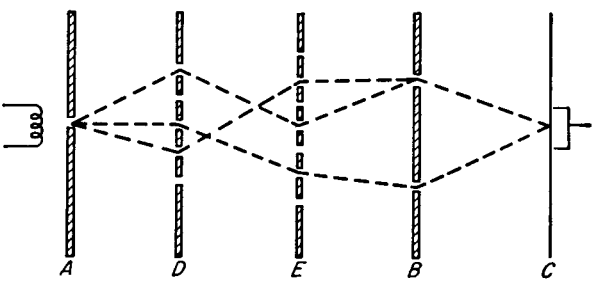


Fig. 1-9 When several holes are drilled in the screens D and E placed between the source in screen A and the final position in screen C, several alternative routes are available for each electron. For each of these routes there is an amplitude. The result of any experiment in which all of the holes are open requires the addition of all of these amplitudes, one for each possible path.

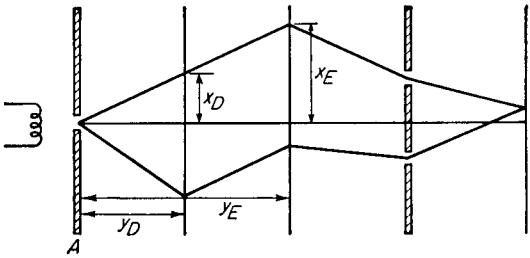


Fig. 1-10 More and more holes are cut in the screens at y_D and y_E. Eventually, the screens are completely riddled with holes, and the electron has a continuous range of positions, up and down along each screen, at which it can pass through the position of the screen. In this case the sum of alternatives becomes a double integral over the continuous parameters x_D and x_E describing the alternative heights at which the electron passes the position of the screens at y_D and y_E.

the position y_D at the nonexistent screen D, together with the height x_E, at the position y_E, as in Fig. 1-10. To each pair of heights there corresponds an amplitude. The principle of superposition still applies, and we must take the sum (or by now, the integral) of these amplitudes over all possible values of x_D and x_E.

Clearly, the next thing to do is to place more and more screens between the source and the hole 1 and in each screen drill so many holes that there is nothing left. Throughout this process we continue to refine the definition of the path of the electron, until finally we arrive at the sensible idea that a path is merely height as a particular function of distance, or $x(y)$. We also continue to apply the principle of superposition, until we arrive at the integral over all paths of the amplitude for each path.

Now we can make a still finer specification of the motion. Not only can we think of the particular path $x(y)$ in space, but we can specify the *time* at which it passes each point in space. That is, a path will (in our two-dimensional case) be given if the two functions $x(t)$, $y(t)$ are given. Thus we have the idea of an amplitude to take a certain path $x(t)$, $y(t)$. The total amplitude to arrive is the sum or integral of this amplitude over all possible paths. The problem of defining this concept of a sum or integral over all paths in a mathematically more precise way will be taken up in Chap. 2.

Chapter 2 also contains the formula for the amplitude for any given path. Once this is given, the laws of nonrelativistic quantum mechanics are completely stated, and all that remains is a demonstration of the application of these laws in a number of interesting special cases.

Path Integrals in Quantum Mechanics, Statistics, Polymer Physics, and Financial Markets

by Hagen Kleinert

Preface

These are lecture notes of a course on path integrals I gave at the Freie Universität Berlin during the winter 1989/1990. My interest in this subject dates back to 1972 when the late R.P. Feynman drew my attention to the unsolved path integral of the hydrogen atom. I was spending my sabbatical year at Caltech and Feynman confessed to me his embarrassment that he could not solve the path integral of this most fundamental quantum system. This made him quit teaching the entire subject in his course on quantum mechanics as he had initially done.[1] In a discussion he said to me: "Kleinert, you figured out all that group theory stuff of the hydrogen atom, why don't you solve the path integral!" He was referring to my 1967 Ph.D. thesis[2] where I had demonstrated that all dynamical questions of the hydrogen atom could be answered using only operations within the dynamical group $O(4,2)$. Indeed, in that work the four-dimensional oscillator played a crucial role and the missing steps to the solution of the path integral were later found to be very few. After returning back home to Berlin I forgot all about the problem since I was busy using path integrals in another context, developing a direct field theoretic passage from quark theories to a collective field theory of hadrons.[3] Later I was applying this theory to condensed matter (superconductors, superfluid ^{3}He) and nuclear physics, where I introduced path integral techniques to set up a field theory of collective phenomena.[4]

[1] Quoting from the preface of the textbook R.P. Feynman and A.R. Hibbs, *Quantum Mechanics and Path Integrals*, McGraw Hill, New York 1965: "By the same time, Dr. Feynman's approach to teaching the subject of quantum mechanics evolved somewhat away from the initial path integral approach."

[2] H. Kleinert, Fortschr. Phys. *6*, 1, (1968), and *Group Dynamics of the Hydrogen Atom*, Lectures presented at the 1967 Boulder Summer School, published in *Lectures in Theoretical Physics*, Gordon and Breach, N. Y., 1968, Vol X B, ed. by A.O. Barut and W.E. Brittin.

[3] See my 1976 Erice lectures, *Hadronization of Quark Theories*, published in *Understanding the Fundamental Constituents of Matter*, Plenum press, New York, 1978, ed. by A. Zichichi.

[4] H. Kleinert, Phys. Lett. B *69*, 9 (1977); Fortschr. Phys. *26*, 565 (1978); *30*, 187, 351 (1982).

The hydrogen problem came up again in 1978 when I had to teach a course on quantum mechanics. At that time it had become customary to give in such a course at least a brief introduction into path integrals and to explain the concept of quantum fluctuations. At the same time, I.H. Duru joined my group as a postdoc from Turkey on a Humboldt fellowship. Since he was familiar with the quantum mechanics of the hydrogen atom I suggested to him the collaboration on the path integral. He quickly acquired the basic techniques and very soon we found the most important ingredient of the solution:[5] The transformation of time in the path integral to a new path dependent pseudotime, combined with a transformation of the coordinates to "square-root coordinates", to be explained in Chapters 13 and 14. Unfortunately, we were able to perform these transformations only in a very formal way which led to the correct result, as we now know, due to good fortune. Our procedure was soon criticized[6] because of the sloppy treatment of the time slicing. A proper treatment could, in principle, have rendered unwanted corrections which we had simply ignored. Some authors went through a detailed time-slicing procedure,[7] but the correct result emerged only by transforming the measure of path integration inconsistently. In fact, when I calculated the corrections according to the standard rules I found them to be zero only in $D = 2$ dimensions.[8] The same treatment in $D = 3$ dimensions gave non-zero corrections which spoiled the beautiful result and left me puzzled. Only very recently I happened to locate the place where the $D = 3$ treatment failed: It was the transformation of the time-sliced measure in the path integral from the original cartesian to the auxiliary "square-root coordinates" in which the system becomes harmonic and integrable. In contrast to $D = 2$, the $D = 3$ transformation is non-holonomic and introduces not only curvature but also torsion. This suggested that the transformations of the time-sliced measure had a hitherto unknown dependence on torsion. Thus it was essential to find first the correct path integral for a particle moving in a space with curvature and torsion. This was a non-trivial task since already in a space with curvature only, the literature was ambiguous giving several prescriptions to choose from which differed by multiples of the curvature scalar added to the energy.[9] The ambiguities are path integral analogs of the so-called operator ordering problem in quantum mechanics. When trying to apply any of the existing prescriptions to spaces with torsion, I always ran into disaster finding non-covariant answers. So, something had to be wrong with all of them. Guided by the idea that in spaces with constant curvature the path integral should give the same result as the operator quantum mechanics based on the commutation rules of the generators of angular momentum I was eventually able to find a consistent *quantum equivalence principle* for path integrals,[10] thus giving a unique answer also to the operator ordering problem. This finally enabled me to solve the leftover problem of the $D = 3$ Coulomb path integral, the absence of the finite time-slicing corrections. The detailed demonstration will be presented in Chapter 13 of this book. In Chapter 14, I treat a variety of one-dimensional systems which have become soluble by the new techniques.

[5] I.H. Duru and H. Kleinert, Phys. Lett. B *84*, 30 (1979), Fortschr. Physik *30*, 401 (1982).

[6] G.A. Ringwood and J.T. Devreese, J. Math. Phys. *21*, 1390 (1980).

[7] R. Ho and A. Inomata, Phys. Rev. Lett. *48*, 231 (1982), A. Inomata, Phys. Lett. A *87*, 387 (1981).

[8] H. Kleinert, Phys. Lett. B *189*, 187 (1987); contains also a criticism of Ref. 7.

[9] B.S. DeWitt, Rev. Mod. Phys. *29*, 337 (1957), K.S. Cheng, J. Math. Phys. *13*, 1723 (1972), H. Kamo and T. Kawai, Prog. Theor. Phys. *50*, 680, (1973), T. Kawai, Found. Phys. *5*, 143 (1975), H. Dekker, Physica *103A*, 586 (1980), G.M. Gavazzi, Nuovo Cimento *A 101*, 241 (1981), M.S. Marinov, Physics Reports *60*, 1 (1980).

Special emphasis will be given, in Chapter 8, to instability (path collapse) problems of Feynman's time sliced path integral in the presence of singular potentials. A general stabilization procedure is presented in Chapter 12 which has to be applied whenever centrifugal barriers, angular barriers, or Coulomb potentials are present.[11]

Another project which Feynman suggested to me, the improvement of a variational approach to path integrals given in his book *Statistical Mechanics* (Benjamin, Reading, 1972; Section 3.5), found a faster solution. We started work during my sabbatical stay at the University of California at Santa Barbara in 1982 when Feynman came on a visit. After a few meetings and discussions the problem was solved and the preprint drafted. Then, unfortunately, Feynman's illness prevented him from reading the final proof of the paper. He was able to do this only three years later when I came for another sabbatical leave to the University of California at San Diego and the paper could finally be submitted.[12]

Due to the recent interest in lattice theories I have found it useful to present the solutions to the harmonic path integrals all at the level of finite time slices, without going immediately to the continuum limit as done in other texts. This should help to understand some typical lattice effects seen in Monte Carlo simulations of various systems.

The path integral description of polymers is introduced in Chapter 15 where stiffness as well as the famous excluded-volume problem are discussed and parallels are drawn to path integrals of relativistic particle orbits. This chapter may be a good preparation to presently ongoing research in the

[10] H. Kleinert, Mod. Phys. Lett. A *4*, 2329 (1989), Phys. Lett B *236*, 315 (1990).

[11] H. Kleinert, Phys. Lett. B *224*, 313 (1989).

[12] R.P. Feynman and H. Kleinert, Phys. Rev. *A 34*, 5080, (1986).

A Path Integral for Spin*

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(Received 1 August 1968)

A path integral for spinning particles is developed. It is a one-particle theory, equivalent to the usual quantum mechanics. Our method employs a classical model for spin which is quantized by path integration. The model, the spherical top, is a natural one from a group-theoretic point of view and has been used before in a similar context. The curvature and multiple connectedness of the top coordinate space [$SO(3)$] provide some interesting features in the sum over paths. The Green's function which results from this procedure propagates all spins, and recovery of the usual Pauli spinors from this formalism is achieved by projection to a specific spin subspace.

1. INTRODUCTION

RECENTLY, Feynman, who invented the subject, had this to say about path integrals[1]:

> ". . . path integrals suffer most grievously from a serious defect. They do not permit a discussion of spin operators . . . in a simple and lucid way. . . . Nevertheless, spin is a simple and vital part of real quantum mechanical systems. It is a serious limitation that the half-integral spin of the electron does not find a simple and ready representation."

This representation, for the nonrelativistic case, is our present concern. The formulation is in terms of a classical model for spin which is familiar and noncontroversial, and our efforts will be directed at path integration of this model.

To our knowledge, existing path-integral theories for spin[2] concentrate on the statistical aspects of the problem and as such are most naturally expressed as field theories. The spin properties of the fermions or bosons of these theories are somewhat secondary and not especially transparent. It would appear that nonrelativistically spin and statistics are separate questions and that a simple *spin* theory should concentrate on just that, leaving the complications of several particles to other considerations. Our goal is then a one-particle theory with optional second quantization.

The idea behind our approach is simple. In principle, there is no difficulty in using path integrals to get the spin of a polyatomic molecule composed of spinless atoms. By a change of variables it is possible to describe this path integral as being over translational, rotational, and internal coordinates. The second of these gives rise to total spin. To get the simplest spinning object we throw away the extra internal coordinates and append to translational coordinates only rotational variables. This will also give half-integral spin since, as is well known, the "ideal" top, as opposed to a bound state of several particles, possesses all spins ($j=0, \frac{1}{2}, 1, \cdots$).

The word "top" is used here because this is the archetype of a mechanical object described by rotational coordinates. Thus the position of a top is determined by a rotation (e.g., that which brings it from some fiducial position), which is to say that its position is given by an element of the group $SO(3)$.

In fact, the relation between half-integral spins and the rotation group is particularly direct in the context of path-integral theory.[3] Ray representations of $SO(3)$ arise because its fundamental group is not trivial—i.e., there are paths in the group which are not deformable into one another. The connection between homotopy theory and representation theory is made via possibly multivalued functions defined on the group manifold. In path integral theory we work directly with the paths. Distinct homotopy classes of paths enter the sum over paths with arbitrary relative phase factors. The selection of these phase factors gives rise to the various multivalued representations. Between given endpoints in $SO(3)$ there are two classes of paths. Depending on the relative sign with which these are added one obtains the propagator for a top of integral or half-integral spin. Incidentally with this viewpoint the distinction between an ideal top and an n-body bound state is evident. As long as the latter can in principle come apart its total coordinate space is R^{3n}, which is simply connected (and therefore only integral spins are allowed).

Another approach to spin theory can be obtained through the use of a Hamiltonian, and Bacry[4] presents a classical phase space and in fact uses fewer coordinates for his spinning particle than we shall. Nevertheless, our desire is to extend Feynman's theory in its most pristine form: a classical system with Lagrangian and variational principle. Furthermore, it is not clear that path integral computations in phase space are feasible for any but the most trivial coordinate systems.

Recovery of the usual Pauli spinor formalism from the top theory described above is easily accomplished by projection to a fixed angular momentum subspace. Similarly the behavior of this top in the presence of an

* Work supported in part by the National Science Foundation and the Army Research Office, Durham.

[1] R. P. Feynman and A. R. Hibbs, *Quantum Mechanics and Path Integrals* (McGraw-Hill Book Co., New York, 1965), p. 355.

[2] See J. R. Klauder, Ann. Phys. (N. Y.) **11**, 123 (1960), and references quoted therein.

[3] M. Hamermesh, *Group Theory* (Addison-Wesley Publishing Co., Inc., Reading, Mass., 1962), pp. 331, 332.

[4] H. Bacry, Argonne National Laboratory report, 1966 (unpublished); also H. Bacry, Commun. Math. Phys. **5**, 97 (1967).

Preface

This book originated in a course given at the Technion some 10 years ago during my first stay, as a visitor, in Israel. Things were different then. Path integrals were not in the mainstream of anything, and I think those who studied this topic did so more from an aesthetic turn of mind than for practical reasons. Either that, or they still carried forth the ideas of the 1950s when path integration had its great, early successes. My own interest in the subject is accidental—while reading an article in Schwinger's reprint collection on quantum electrodynamics the pages slipped and the book fell open to Feynman's *Reviews of Modern Physics* paper. This I read, and resolved, as a thesis topic, to try to produce a path integral for spin.

Path integration has come a long way in the 1970s. In statistical physics it was the basic framework for the first formulation of the renormalization group transformation. It is used extensively in studying systems with random impurities. In particle physics it is basic to the instanton industry and finds application in studies of gauge field theory (even though some of the methods used had been developed for other problems in the 1960s). In chemical, atomic, and nuclear physics path integrals have been applied to semiclassical approximation schemes for scattering theory. And in rigorous studies of quantum field theory and statistical mechanics the functional integral is used again and again.

This is a book of techniques and applications. My aim is to say what the path integral is and then by example to show how it can and has been used. The approach is that of a physicist with a weakness for but not an addiction to mathematics. The level is such that anyone with a reasonable first course in quantum mechanics should not find difficulty although some of the applications presuppose specialized knowledge; even then, on topics of special interest to me I have supplied background material unrelated to path integrals.

The implications of path integrals for a general understanding of quantum mechanics have been beautifully expounded in Feynman's origi-

nal *Reviews of Modern Physics* paper and in his book on path integrals with Hibbs. For this reason I have touched only lightly on these matters. The Feynman-Hibbs book also includes many applications of path integration, some of which have been given brief treatment here. The emphasis in that volume is on applications developed by Feynman himself, and while they form a considerable body of knowledge there is still enough left over for the present book.

The first part of the book develops the techniques of path integration. Our basic derivation of the path integral presents it as a mathematically justified consequence of the usual quantum mechanics formalism (via the Trotter product formula). Of course we also talk of summing the quantity $\exp(iS/\hbar)$ over all paths, despite the lack of rigorous justification for such terminology. In fact some of our work makes extensive use of this view. Nevertheless, while I have been willing to work without the full blessings of theorems at every step, I have tried to avoid some of the pitfalls that path integrals offer to the unwary. In particular there is a good deal of discussion of the relation $(\Delta\text{distance})^2 \sim (\Delta\text{time})$, a central property of paths entering the Feynman sum over histories. Some of the usual quantum formalism is recovered from the path integral but no great emphasis is placed on this goal. The explicitly solvable path integrals—the harmonic oscillator and variations thereof—are written out, and it is thus shown that the awesome task of summing over paths can in fact occasionally be done. At this early stage we also introduce the Wiener integral, formal first cousin of the path integral and legitimate integral over paths. Here we are able to indulge in an occasional rigorous proof and present a calculation of a first passage time, illustrating the profound connection provided by the Wiener integral between probability and potential theory.

The choice of applications that appear in this book requires a special apology. For a topic to be treated here, I had to first know about it, next understand it (or think I did), then find it amusing, exciting, fundamental, or possessing some similar quality, and finally have the time to present it. There are undoubtedly works that satisfy the third of my criteria but miss out on some other count. Section 32, being a brief treatment of some omissions, reflects the fact that the book had to be finished some time although many beautiful applications would not appear.

As to the applications that do appear.... A lot of space is devoted to the semiclassical approximation. Although the mathematical justification for the stationary phase approximation to the functional integral is not strong, this is an important application, at least in terms of consumer interest. Also, one of the features of Feynman's formulation of quantum mechanics that first impressed me was that the correspondence limit ($\hbar \to 0$) was a wave of the hand away (via the stationary phase approximation). Of course converting the hand waving arguments to mathematics is still an uncompleted job, but that does not detract from the beauty of the ideas. I must also confess that I am drawn to the semiclassical approximation not so much by consumer interest but rather by the way in which so many different strands of nineteenth and twentieth century mathematics are brought together. Between Sections 11 and 18 the following topics—all relevant to the matter at hand—are taken up: (1) variational principles of classical mechanics and *minimum* (rather than merely extremum) properties of paths—the Jacobi equation; (2) the Morse index theorem; (3) asymptotic analysis, order relations, and so on; (4) Sturm-Liouville theory; (5) Thom's catastrophe theory; (6) *uniform* asymptotic analysis.

Starting from semiclassical results it is not difficult to derive both approximations for scattering theory (Section 19) and a path integral theory of optics (Section 20). The optics calls for some unnatural definitions but I think the reward is worth the temporary inelegance: semiclassical results for path integrals lead at once to geometrical (and even physical) optics with a possibility of getting Keller's "geometrical diffraction" theory too (that possibility is suggested but not carried out in this book).

Probably the most famous early application of path integration is to the polaron and we treat that here too. What makes the polaron special from the standpoint of selling path integrals is that it is one of the few places where the path integral not only helps you discover an answer, but also remains the best way to calculate the answer even after you know it. I like the polaron because it is a tractable field theory; the benefits obtained from using the path integral are entirely analogous to those gotten in quantum electrodynamics, but for the latter all steps are more difficult because of the infinities, the vector character of the field, and gauge problems. Results of the path integral treatment of Q.E.D. are mentioned briefly in Section 32.

Three sections are devoted to the problem of formulating a path integral for spin. Not surprisingly I place the most emphasis on the approach I myself have worked on. To be honest, if I had to solve the problem of a hydrogen atom in a magnetic field I would not use this formalism. Nevertheless, the method shows there is *some* way to treat spin by path integrals. It would also appear that some of the connections to homotopy theory developed in the course of working out a path integral for spin are turning out to be important in gauge theories. Unfortunately, path integral treatments of gauge theories get only the briefest mention in this book; this is one of the gaps I especially regret.

The section on relativistic propagators is both central to the book and an incidental side topic. It is central, because if you wish to think of path

9

Path-Integral Methods

The Quantum Theory of Fields Volume 1

by Steven Weinberg

In Chapters 7 and 8 we applied the canonical quantization operator formalism to derive the Feynman rules for a variety of theories. In many cases, such as the scalar field with derivative coupling or the vector field with zero or non-zero mass, the procedure though straightforward was rather awkward. The interaction Hamiltonian turned out to contain a covariant term, equal to the negative of the interaction term in the Lagrangian, plus a non-covariant term, which served to cancel non-covariant terms in the propagator. In the case of electrodynamics this non-covariant term (the Coulomb energy) turned out to be not even spatially local, though it is local in time. Yet the final results are quite simple: the Feynman rules are just those we should obtain with covariant propagators, and using the negative of the interaction term in the Lagrangian to calculate vertex contributions. The awkwardness in obtaining these simple results, which was bad enough for the theories considered in Chapters 7 and 8, becomes unbearable for more complicated theories, like the non-Abelian gauge theories to be discussed in Volume II, and also general relativity. One would very much prefer a method of calculation that goes directly from the Lagrangian to the Feynman rules in their final, Lorentz-covariant form.

Fortunately, such a method does exist. It is provided by the path-integral approach to quantum mechanics. This was first presented in the context of non-relativistic quantum mechanics in Feynman's Princeton Ph. D. thesis,[1] as a means of working directly with a Lagrangian rather than a Hamiltonian. In this respect, it was inspired by earlier work of Dirac.[2] The path-integral approach played a part (along with inspired guesswork) in Feynman's later derivation of his diagrammatic rules.[3] However, although Feynman diagrams became widely used in the 1950s, most physicists (including myself) tended to derive them using the operator methods of Schwinger and Tomonaga, which were shown by Dyson in 1949 to lead to the same diagrammatic rules that had been obtained by Feynman by his own methods.

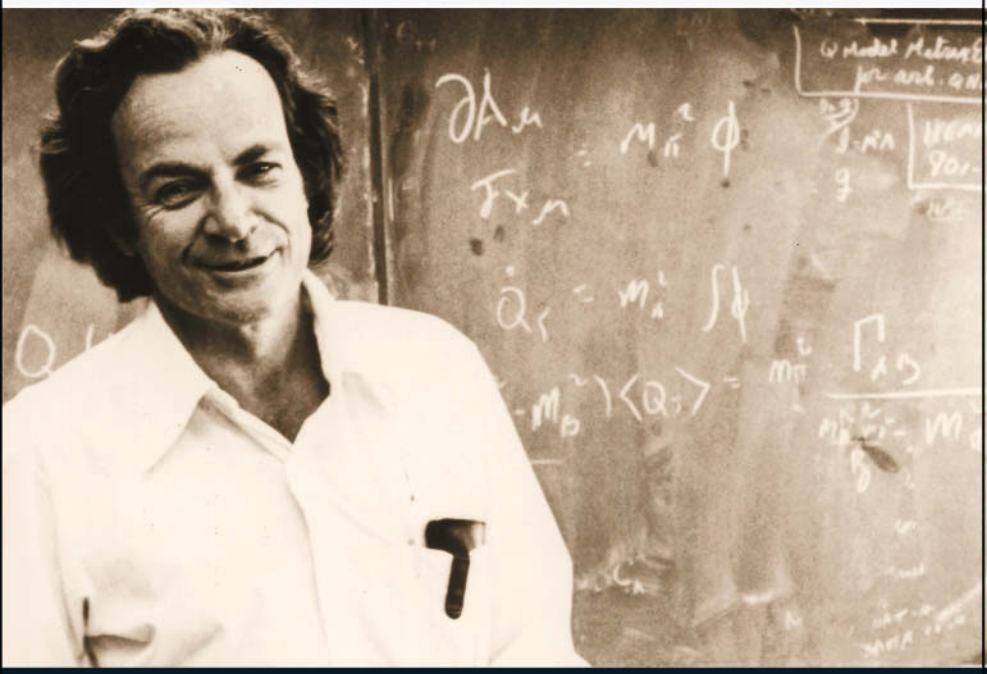
The path-integral approach was revived in the late 1960s, when Faddeev

and Popov[4] and De Witt[5] showed how to apply it to non-Abelian gauge theories and general relativity. For most theorists, the turning point came in 1971, when 't Hooft[6] used path-integral methods to derive the Feynman rules for spontaneously broken gauge theories (discussed in Volume II), including in particular the theory of weak and electromagnetic interactions, in a gauge that made the high energy behavior of these theories transparent. Soon after, as also discussed in Volume II, it was discovered that the path-integral method allows us to take account of contributions to the S-matrix that have an essential singularity at zero coupling constant and therefore cannot be discovered in any finite order of perturbation theory. Since then, the path-integral methods described here have become an indispensable part of the equipment of all physicists who make use of quantum field theory.

At this point the reader may be wondering why if the path-integral method is so convenient we bothered in Chapter 7 to introduce the canonical formalism. Indeed, Feynman seems at first to have thought of his path-integral approach as a substitute for the ordinary canonical formulation of quantum mechanics. There are two reasons for starting with the canonical formalism. The first is a point of principle: although the path-integral formalism provides us with manifestly Lorentz-invariant diagrammatic rules, it does not make clear why the S-matrix calculated in this way is unitary. As far as I know, the only way to show that the path-integral formalism yields a unitary S-matrix is to use it to reconstruct the canonical formalism, in which unitarity is obvious. There is a kind of conservation of trouble here; we can use the canonical approach, in which unitarity is obvious and Lorentz invariance obscure, or the path-integral approach, which is manifestly Lorentz-invariant but far from manifestly unitary. Since the path-integral approach is here derived from the canonical approach, we know that the two approaches yield the same S-matrix, so that the S-matrix must indeed be both Lorentz-invariant and unitary.

The second reason for introducing the canonical formalism first is more practical: there are important theories in which the simplest version of the Feynman path-integral method, in which propagators and interaction vertices are taken directly from the Lagrangian, is simply wrong. One example is the non-linear σ-model, with Lagrangian density $\mathscr{L} = -\frac{1}{2}g_{k\ell}(\phi)\partial_\mu\phi^k\partial^\mu\phi^\ell$. In such theories, using the naive Feynman rules derived directly from the Lagrangian density would yield an S-matrix that is not only wrong but even non-unitary, and that also depends on the way in which we define the scalar field.[7] In this chapter we shall derive the path-integral formalism from the canonical formalism, and in this way we will see what additional sorts of vertices are needed to supplement the simplest version of the Feynman path-integral method.

Feynman's Thesis



A New Approach to Quantum Theory

Laurie M. Brown *(Editor)*

“The young Feynman revealed here was full of invention, verve, and ambition. His new approach to quantum mechanics, after simmering for decades beneath the surface of theoretical physics, burst into new prominence in the 1970s. Now its influence is pervasive, and still expanding. Feynman's original presentation is not only uniquely clear, but also contains insights and perspectives that are not widely known, and might well provide ammunition for another explosion or two.”

Frank Wilczek
2004 Physics Nobel Laureate

“Historians and physicists alike will enjoy this easy-to-read little book ... The thesis itself is a masterpiece of clear exposition ... it is written in Feynman's uniquely chatty style, and reminiscent of the famous Feynman lectures. It is a delight to read and is likely to offer an insight, even to non-physicists, into both physics and the workings of Feynman's mind. I would not hesitate to recommend the book to anyone—working physicists, historians,philosophers and even ‘curious fellows’ who would like to 'peak over the shoulder' of one of the 20th century's great physicists at work.”

CERN Courier

“The path integral approach is now something that every graduate student in theoretical physics is supposed to know ... the thesis provides a very good background for the way these ideas came about. The two companion articles, although available in print, also gives a complete picture of the development of this line of thinking. The helpful introductory remarks by the editor also puts things in the proper historical perspective. This book would be very helpful to anyone interested in the development of modern ideas in physics.”

Classical and Quantum Gravity

“R Feynman was an excellent writer and it is a joy to read his dissertation ... The reprints in this booklet are historical cornerstones in the development of modern theoretical physics, very interesting and still very well readable.”

Zentralblatt MATH

REVIEWS OF

MODERN PHYSICS

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Space-Time Approach to Non-Relativistic Quantum Mechanics

R. P. FEYNMAN

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Non-relativistic quantum mechanics is formulated here in a different way. It is, however, mathematically equivalent to the familiar formulation. In quantum mechanics the probability of an event which can happen in several different ways is the absolute square of a sum of complex contributions, one from each alternative way. The probability that a particle will be found to have a path $x(t)$ lying somewhere within a region of space time is the square of a sum of contributions, one from each path in the region. The contribution from a single path is postulated to be an exponential whose (imaginary) phase is the classical action (in units of $\hbar$) for the path in question. The total contribution from all paths reaching x, t from the past is the wave function $\psi(x, t)$. This is shown to satisfy Schroedinger's equation. The relation to matrix and operator algebra is discussed. Applications are indicated, in particular to eliminate the coordinates of the field oscillators from the equations of quantum electrodynamics.

1. INTRODUCTION

IT is a curious historical fact that modern quantum mechanics began with two quite different mathematical formulations: the differential equation of Schroedinger, and the matrix algebra of Heisenberg. The two, apparently dissimilar approaches, were proved to be mathematically equivalent. These two points of view were destined to complement one another and to be ultimately synthesized in Dirac's transformation theory.

This paper will describe what is essentially a third formulation of non-relativistic quantum theory. This formulation was suggested by some of Dirac's[1,2] remarks concerning the relation of classical action[3] to quantum mechanics. A probability amplitude is associated with an entire motion of a particle as a function of time, rather than simply with a position of the particle at a particular time.

The formulation is mathematically equivalent to the more usual formulations. There are, therefore, no fundamentally new results. However, there is a pleasure in recognizing old things from a new point of view. Also, there are problems for which the new point of view offers a distinct advantage. For example, if two systems A and B interact, the coordinates of one of the systems, say B, may be eliminated from the equations describing the motion of A. The inter-

[1] P. A. M. Dirac, *The Principles of Quantum Mechanics* (The Clarendon Press, Oxford, 1935), second edition, Section 33; also, Physik. Zeits. Sowjetunion **3**, 64 (1933).

[2] P. A. M. Dirac, Rev. Mod. Phys. **17**, 195 (1945).

[3] Throughout this paper the term "action" will be used for the time integral of the Lagrangian along a path. When this path is the one actually taken by a particle, moving classically, the integral should more properly be called Hamilton's first principle function.

A New Approach to Quantum Mechanics (thesis)

Preface vii-xii

Abstract

I. Introduction

II. Least action in classical mechanics
1. The Concept of a Functional
2. The Principle of Least Action
3. Conservation of Energy. Constants of the Motion
4. Particles Interacting through an Intermediate Oscillator

III. Least Action in Quantum Mechanics
1. The Lagrangian in Quantum Mechanics
2. The Calculation of Matrix Elements in the Language of a Lagrangian
3. The Equations of Motion in Lagrangian Form
4. Translation to the Ordinary Notation of Quantum Mechanics
5. The Generalization to any Action Function
6. Conservation of Energy. Constants of the Motion
7. The Role of the Wave Function
8. Transition Probabilities
9. Expectation Values for Observables
10. Application to the Forced Harmonic Oscillator
11. Particles Interacting through an Intermediate Oscillator
12. Conclusion

Space-Time Approach to Non-Relativistic Quantum Mechanics (paper)

1. Introduction

2. The superposition of probability amplitudes

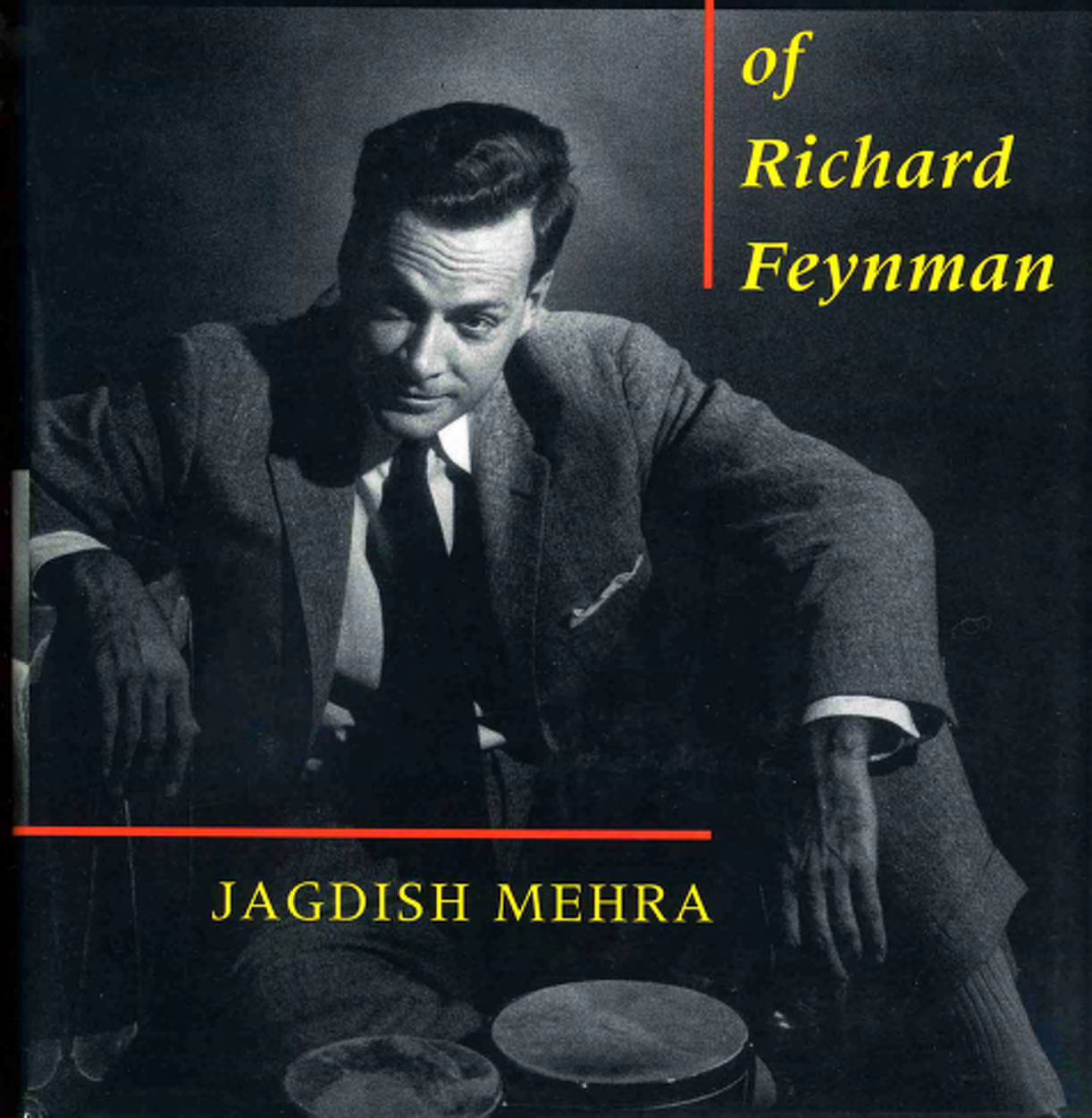
3. The probability amplitude for a space-time path

4. The calculation of the probability amplitude for a path

5. Definition of the wave function
6. The wave equation
7. Discussion of the wave equation
The classical limit
8. Operator algebra
Matrix elements
9. Newton's equations
The commutation relation
10. The Hamiltonian
Momentum
11. Inadequacies of the formulation
12. Possible generalizations
13. Application to eliminate field oscillators
14. Statistical mechanics
Spin and Relativity

THE BEAT OF A DIFFERENT DRUM

The life and science of Richard Feynman



JAGDISH MEHRA

6

The principle of least action in quantum mechanics

6.1 Introduction

Richard Feynman had to take the qualifying examination to become a candidate for the Ph.D. degree. At Princeton, the graduate students were not restricted to pursuing any special courses, but they had to take a very stiff qualifying examination; it was a long written examination for a day or two, followed by an oral examination. Three groups of professors would examine the candidate. The graduate students used to worry about this examination, and most of them dreaded it. They had a maximum of three chances in which to pass it, and some of them had failed it once or twice. To prepare for it was a serious matter.

In order to get ready for the qualifying examination, Feynman went to Cambridge, Massachusetts, where he studied for about six to eight weeks in the library of MIT during the summer of 1940. Nobody among the young people there knew him any longer, and he had the peace of mind to work in the library. He stayed at his old fraternity, where they gave him a room to stay. He was able to study without interruption or distraction. There he fully organized his knowledge of physics, things he knew and others which he did not know. He prepared a notebook, entitled 'Things I don't know about'. In it, he summarized all the subjects as best as he could, and he was rather proud of that notebook.

Feynman tried to reduce every part of physics to its fundamentals: to learn what were the essentials and the body of knowledge that was derived from them. In this way, he was able to see the pattern of each field: electrodynamics, relativity theory, quantum mechanics, statistical mechanics—everything was in his notebook. He would use this notebook many years later to prepare the lecture courses he would give at Cornell University and, later on, at the California Institute of Technology.[1] In this way, everything became organized in his mind, never to be forgotten. He made good use of this basic knowledge when, over two decades later, he gave his famous *Feynman lectures on physics*

for freshmen and sophomore students at Caltech.[2] He was able to organize everything in a logical manner, so that the effort of memorization would be minimal. That required an arrangement by means of which all the logical interconnections were clear and self-contained. He completed a summary of all of fundamental physics in a concentrated and devoted study of about two months at MIT in preparation for his qualifying examination. Then he returned to Princeton, where he took the qualifying examination in the fall of 1940.

In the oral examination, one of the professors asked him about the order of colors in the rainbow: which color was at the top and which at the bottom? Feynman answered he did not know, and proceeded to work it out by drawing the curve for the index of refraction versus the wavelength.[1,3] In the curve, Feynman mixed up the indices of refraction for the blue and the red colors, but corrected himself just in time.[1,3] H. P. Robertson asked Feynman a question about stellar aberration and relativity, which he answered incorrectly.[1,3] When you look at a star through a telescope, it appears to move in a little circle due to aberration. How would the earth look in a telescope from a star? Feynman answered that the earth would also look as if it moved in a little circle. Feynman and Robertson had much argument about it, but Feynman succeeded in persuading him that he was right. When he thought more about it afterwards, he concluded that he had been wrong. The circles in the two cases would be of different sizes, and the two observations were not related. Special relativity would fail because acceleration is involved, and the motion would appear to be in a circle in which the velocity would change.[1,3]

After Feynman graduated from MIT, he wanted to get a summer job at the Bell Telephone Laboratories, and went out a couple of times to visit there. William Shockley, who knew him from the lab at MIT, would show Feynman around each time; he enjoyed these visits very much, but he did not get a job there. In the spring of 1941, Feynman went once again to the Bell Labs in New York to apply for a summer job. Shockley again showed him around and, afterwards, some of the young research scientists took him to a seafood restaurant for lunch. They were all pleased that they were going to have oysters but Feynman, who had grown up in a town near the ocean, could not even look at oysters; he forced himself to eat a couple of oysters, but felt awful and gave up. However, 'this time, which must have been my fourth or fifth time touring the Bell Labs, they accepted me. I was very happy. In those days it was hard to find a job where you could be with other scientists.'[4] But, instead of joining the Bell Labs for summer work, Feynman joined the army for wartime work.

Even before the United States of America declared war after Pearl Harbor in December 1941, there was a lot of propaganda about joining the war effort, and a lot of patriotic feelings about helping the country's business. Many businessmen were going to Plattsburg in upstate New York to join whatever service jobs were available. Feynman also thought that his technical ability to

10

Feynman's path to quantum mechanics

10.1 Introduction

Feynman began his 1948 paper in the *Reviews of Modern Physics*, entitled 'space–time approach to non-relativistic quantum mechanics', by stating: 'It is a curious historical fact that modern quantum mechanics began with two quite different mathematical formulations: the differential equation of Schrödinger, and the matrix algebra of Heisenberg [as well as the q-number formulation of P. A. M. Dirac]. The two, apparently dissimilar approaches, were proved to be mathematically equivalent. These two points of view were destined to complement one another and to be ultimately synthesized in Dirac's transformation theory.

'This paper will describe what is essentially a *third formulation* of non-relativistic quantum theory. This formulation was suggested by some of Dirac's remarks concerning the relation of classical action to quantum mechanics.[1] The probability amplitude is associated with [the] entire motion of a particle at a particular time.'[2]

With these words Richard Feynman introduced one of his now most well known papers. In the spring of 1947 he decided to publish the most important parts of his Ph.D. thesis. Feynman had thought about publishing this work in a regular journal earlier, but World War II intervened, and he was not able to do so: 'The war interrupted the work.'[3]

'During the war, I didn't have time to work on these things very extensively, but wandered about on buses and so forth, with little pieces of paper (in my pockets), and struggled to work on it and discovered indeed that there was something wrong. . ..'[4] He discovered that he was not able to exclude the possibility of a complex value for the energy in his quantum action-at-a-distance theory. And if this was to be the case, one would obtain obviously wrong results for the probabilities of the events in this theory. In particular, the sum of the probabilities of all possible independent events in such a theory would not be equal to one. But this sum must be equal to one, because in every case some of these events should happen.

Principles of Quantum Mechanics

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The main features of Eq. (2.3.6) are the following.

(1) The problem of two mutually interacting particles has been transformed to that of two fictitious particles that do not interact with each other. In other words, the equations of motion for $\mathbf{r}$ do not involve $\mathbf{r}_{CM}$ and vice versa, because $\mathscr{L}(\mathbf{r}, \dot{\mathbf{r}}; \mathbf{r}_{CM}, \dot{\mathbf{r}}_{CM}) = \mathscr{L}(\mathbf{r}, \dot{\mathbf{r}}) + \mathscr{L}(\mathbf{r}_{CM}, \dot{\mathbf{r}}_{CM})$.

(2) The first fictitious particle is the CM, of mass $M = m_1 + m_2$. Since $\mathbf{r}_{CM}$ is a cyclic variable, the momentum $\mathbf{p}_{CM} = M\dot{\mathbf{r}}_{CM}$ (which is just the total momentum) is conserved as expected. Since the motion of the CM is uninteresting one usually ignores it. One clear way to do this is to go to the CM frame in which $\dot{\mathbf{r}}_{CM} = 0$, so that the CM is completely eliminated in the Lagrangian.

(3) The second fictitious particle has mass $\mu = m_1 m_2/(m_1 + m_2)$ (called the *reduced mass*), momentum $\mathbf{p} = \mu\dot{\mathbf{r}}$ and moves under a potential $V(\mathbf{r})$. One has just to solve this one-body problem. If one chooses, one may easily return to the coordinates $\mathbf{r}_1$ and $\mathbf{r}_2$ at the end, using Eqs. (2.3.1) and (2.3.2).

*Exercise 2.3.1.** Derive Eq. (2.3.6) from (2.3.5) by changing variables.

2.4. How Smart Is a Particle?

The Lagrangian formalism seems to ascribe to a particle a tremendous amount of foresight: a particle at (x_i, t_i) destined for (x_f, t_f) manages to calculate ahead of time the action for every possible path linking these points, and takes the one with the least action. But this, of course, is an illusion. The particle need not know its entire trajectory ahead of time, it needs only to obey the Euler–Lagrange equations at each instant in time to minimize the action. This in turn means just following Newton's law, which is to say, the particle has to sample the potential in its immediate vicinity and accelerate in the direction of greatest change.

Our esteem for the particle will sink further when we learn quantum mechanics. We will discover that far from following any kind of strategy, the particle, in a sense, goes from (x_i, t_i) to (x_f, t_f) along all possible paths, giving equal weight to each! How it is that despite this, classical particles do seem to follow $x_{cl}(t)$ is an interesting question that will be answered when we come to the path integral formalism of quantum mechanics.

2.5. The Hamiltonian Formalism

In the Lagrangian formalism, the independent variables are the coordinates q_i and velocities $\dot{q}_i$. The momenta are derived quantities defined by

$$p_i = \frac{\partial \mathscr{L}}{\partial \dot{q}_i} \tag{2.5.1}$$

8

The Path Integral Formulation of Quantum Theory

We consider here an alternate formulation of quantum mechanics invented by Feynman in the forties.‡ In contrast to the Schrödinger formulation, which stems from Hamiltonian mechanics, the Feynman formulation is tied to the Lagrangian formulation of mechanics. Although we are committed to the former approach, we discuss in this chapter Feynman's alternative, not only because of its aesthetic value, but also because it can, in a class of problems, give the full propagator with tremendous ease and also give valuable insight into the relation between classical and quantum mechanics.

8.1. The Path Integral Recipe

We have already seen that the quantum problem is fully solved once the propagator is known. Thus far our practice has been to first find the eigenvalues and eigenfunctions of H, and then express the propagator $U(t)$ in terms of these. In the path integral approach one computes $U(t)$ directly. For a single particle in one dimension, the procedure is the following.

To find $U(x, t; x', t')$:

(1) Draw all paths in the x–t plane connecting (x', t') and (x, t) (see Fig. 8.1).
(2) Find the action $S[x(t)]$ for each path $x(t)$.
(3) $$U(x, t; x', t') = A \sum_{\text{all paths}} e^{iS[x(t)]/\hbar} \tag{8.1.1}$$

where A is an overall normalization factor.

‡ The nineteen forties that is, and in his twenties. An interesting account of how he was influenced by Dirac's work in the same direction may be found in his Nobel lectures. See, *Nobel Lectures—Physics*, Vol. III, Elsevier Publication, New York (1972).

21

Path Integrals: Part II

In this chapter we return to path integrals for a more detailed and advanced treatment. The tools described here are so widely used in so many branches of physics, that it makes sense to include them in a book such as this. This chapter will be different from the earlier ones in that it will try to introduce you to a variety of new topics without giving all the derivations in same detail as before. It also has a list of references to help you pursue any topic that attracts you. The list is not exhaustive and consists mostly of pedagogical reviews or books. From the references these references contain, you can pursue any given topic in greater depth. All this will facilitate the transition from course work to research.

In Chapter 8 the path integral formula for the propagator was simply postulated and shown to lead to the same results as the operator methods either by direct evaluation of the propagator (in the free particle case) or by showing once and for all that the Schrödinger equation followed from the path integral prescription for computing the time evolution.

We begin this chapter by doing the reverse: we start with the operator Hamiltonian $H = P^2/2m + V$ and derive the propagator for it as a path integral. We shall see that there are many types of path integrals one can derive. We will discuss

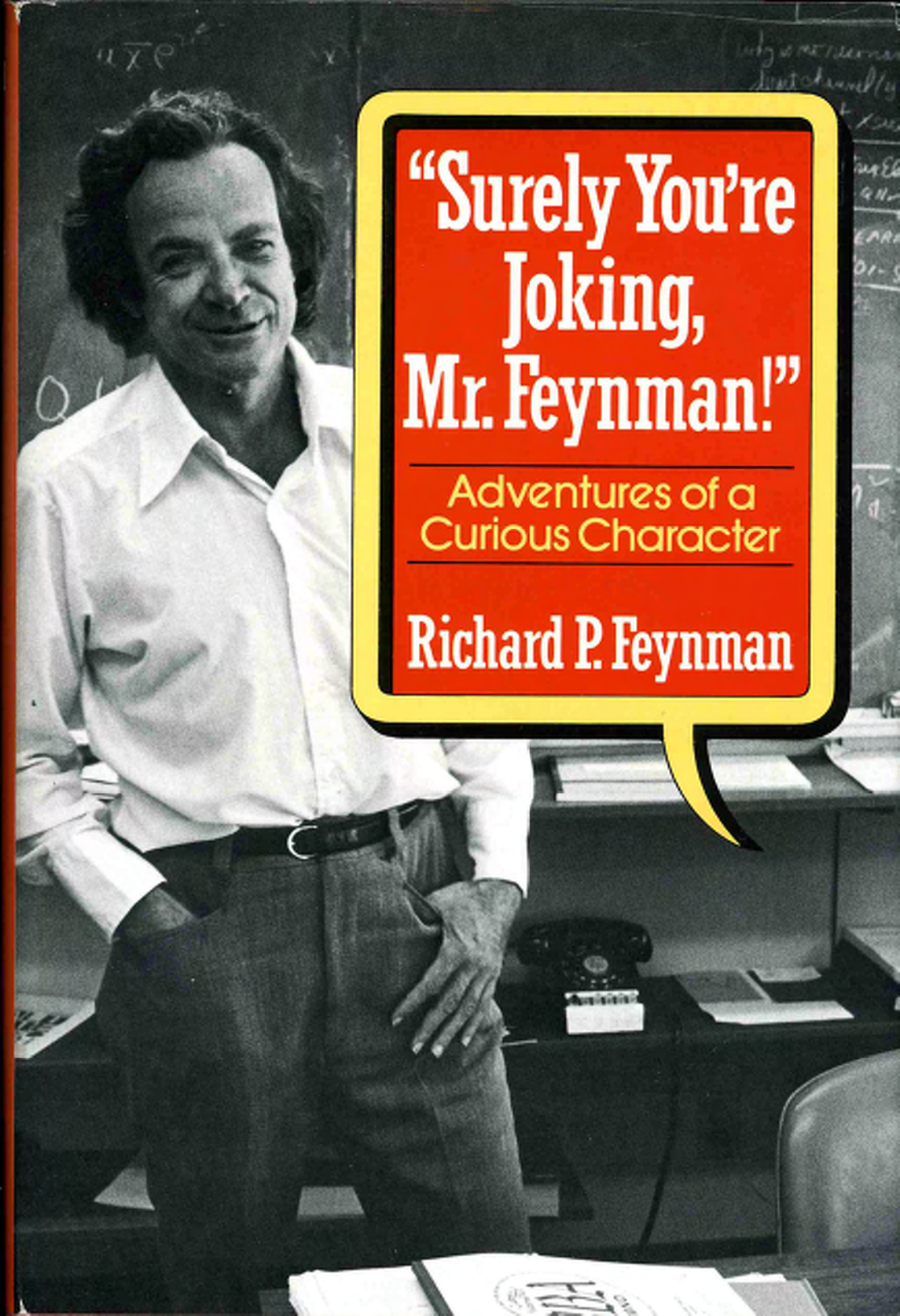
- The configuration space path integral, discussed in Chapter 8.
- The phase space path integral.
- The coherent state path integral.

You will see that the existence of many path integrals is tied to the existence of many resolutions of the identity, i.e., to the existence of many bases.

Following this we will discuss two applications: to the Quantum Hall Effect (QHE) and a recent development called the Berry Phase.

We then turn to imaginary time quantum mechanics and its relation to statistical mechanics (classical and quantum) as well the calculation of tunneling amplitudes by a semiclassical approximation. You will learn about instantons, the transfer matrix formulation, and so on.

Finally, we discuss path integrals for two problems with no classical limit: a spin Hamiltonian and a fermionic oscillator.



"Surely You're Joking, Mr. Feynman!"

Adventures of a Curious Character

Richard P. Feynman

Part 2
The Princeton Years

"*Surely You're Joking, Mr. Feynman!*"

When I was an undergraduate at MIT I loved it. I thought it was a great place, and I wanted to go to graduate school there too, of course. But when I went to Professor Slater and told him of my intentions, he said, "We won't let you in here."

I said, "What?"

Slater said, "Why do you think you should go to graduate school at MIT?"

"Because MIT is the best school for science in the country."

"You *think* that?"

"Yeah."

"That's why you should go to some other school. You should find out how the rest of the world is."

So I decided to go to Princeton. Now Princeton had a certain aspect of elegance. It was an imitation of an English school, partly. So the guys in the fraternity, who knew my rather rough, informal manners, started making remarks like "Wait till they find out who they've got coming to Princeton! Wait till they see the mistake they made!" So I decided to try to be nice when I got to Princeton.

My father took me to Princeton in his car, and I got my room, and he left. I hadn't been there an hour when I was met by a man: "I'm the Mahstah of Residences heah, and I should like to tell you that the Dean is having a Tea this aftanoon, and he should like to have all of you come. Perhaps you would be so kind as to inform your roommate, Mr. Serette."

That was my introduction to the graduate "College" at Princeton, where all the students lived. It was like an imitation Oxford or Cambridge--complete with accents (the master of residences was a professor of "French littrachaw"). There was a porter downstairs, everybody had nice rooms, and we ate all our meals together, wearing academic gowns, in a great hall which had stained-glass windows.

So the very afternoon I arrived in Princeton I'm going to the dean's tea, and I didn't even know what a "tea" was, or why! I had no social abilities whatsoever; I had no experience with this sort of thing.

So I come up to the door, and there's Dean Eisenhart, greeting the new students: "Oh, you're Mr. Feynman," he says. "We're glad to have you." So that helped a little, because he recognized me, somehow.

I go through the door, and there are some ladies, and some girls, too. It's all very formal and I'm thinking about where to sit down and should I sit next to this girl, or not, and how should I behave, when I hear a voice behind me.

"Would you like cream or lemon in your tea, Mr. Feynman?" It's Mrs. Eisenhart, pouring tea.

"I'll have both, thank you," I say, still looking for where I'm going to sit, when suddenly I hear "Heh-heh-heh-heh-heh. Surely you're *joking*, Mr. Feynman."

Joking? Joking? What the hell did I just say? Then I realized what I had done. So that was my first experience with this tea business.

Later on, after I had been at Princeton longer, I got to understand this "Heh-heh-heh-heh-heh." In fact it was at that first tea, as I was leaving, that I realized it meant "You're making a social error." Because the *next* time I heard this same cackle, "Heh-heh-heh-heh-heh," from Mrs. Eisenhart, somebody was kissing her hand as he left.

Another time, perhaps a year later, at another tea, I was talking to Professor Wildt, an astronomer who had worked out some theory about the clouds on Venus. They were supposed to be formaldehyde (it's wonderful to know what we once worried about) and he had it all figured out, how the formaldehyde was precipitating, and so on. It was extremely interesting. We were talking about all this stuff, when a little lady came up and said, "Mr. Feynman, Mrs. Eisenhart would like to see you."

"OK, just a minute . . ." and I kept talking to Wildt.

The little lady came back again and said, "Mr. *Feynman*, Mrs. Eisenhart would like to see you."

"OK, OK!" and I go over to Mrs. Eisenhart, who's pouring tea.

"Would you like to have some coffee or tea, Mr. Feynman?"

"Mrs. So-and-so says you wanted to talk to me."

"Heh-heh-heh-heh-heh. Would you like to have *coffee*, or *tea*, Mr. Feynman?"

"Tea," I said, "thank you."

A few moments later Mrs. Eisenhart's daughter and a schoolmate came over, and we were introduced to each other. The whole idea of *this* "heh-heh-heh" was: Mrs. Eisenhart didn't want to talk to me, she wanted me over there getting tea when her daughter and friend came over, so they would have someone to talk to. That's the way it worked. By that time I knew what to do when I heard "Heh-heh-heh-heh-heh." I didn't say, "What do you mean, 'Heh-heh-heh-heh-heh'?"; I knew the "heh-heh-heh" meant "error," and I'd better get it straightened out.

Every night we wore academic gowns to dinner. The first night it scared the life out of me, because I didn't like formality. But I soon realized that the gowns were a great advantage. Guys who were out playing tennis could rush into their room, grab their academic gown, and put it on. They didn't have to take time off to change their clothes or take a shower. So underneath the gowns there were bare arms, T-shirts, everything. Furthermore,

there was a rule that you never cleaned the gown, so you could tell a first-year man from a second-year man, from a third-year man, from a pig! You never cleaned the gown and you never repaired it, so the first-year men had very nice, relatively clean gowns, but by the time you got to the third year or so, it was nothing but some kind of cardboard thing on your shoulders with tatters hanging down from it.

So when I got to Princeton, I went to that tea on Sunday afternoon and had dinner that evening in an academic gown at the "College." But on Monday, the first thing I wanted to do was to see the cyclotron.

MIT had built a new cyclotron while I was a student there, and it was just *beautiful*! The cyclotron itself was in one room, with the controls in another room. It was beautifully engineered. The wires ran from the control room to the cyclotron underneath in conduits, and there was a whole console of buttons and meters. It was what I would call a gold-plated cyclotron.

Now I had read a lot of papers on cyclotron experiments, and there weren't many from MIT. Maybe they were just starting. But there were lots of results from places like Cornell, and Berkeley, and above all, Princeton. Therefore what I really wanted to see, what I was looking forward to, was the PRINCETON CYCLOTRON. That must be *something!*

So first thing on Monday, I go into the physics building and ask, "Where is the cyclotron – which building?"

"It's downstairs, in the basement – at the end of the hall."

In the *basement*? It was an old building. There was no room in the basement for a cyclotron. I walked down to the end of the hall, went through the door, and in ten seconds I learned why Princeton was right for me – the best place for me to go to school. In this room there were wires strung *all over the place!* Switches were hanging from the wires, cooling water was dripping from the valves, the room was *full* of stuff, all out in the open. Tables piled with tools were everywhere; it was the most godawful mess you ever saw. The whole cyclotron was there in one room, and it was complete, absolute chaos!

It reminded me of my lab at home. Nothing at MIT had ever reminded me of my lab at home. I suddenly realized why Princeton was getting results. They were working with the instrument. They *built* the instrument; they knew where everything was, they knew how everything worked, there was no engineer involved, except maybe he was working there too. It was much smaller than the cyclotron at MIT, and "gold-plated"?--it was the exact opposite. When they wanted to fix a vacuum, they'd drip glyptal on it, so there were drops of glyptal on the floor. It was wonderful! Because they *worked* with it. They didn't have to sit in another room and push buttons! (Incidentally, they had a fire in that room, because of all the chaotic mess that they had--too many wires--and it destroyed the cyclotron. But I'd better not tell about that!)

(When I got to Cornell I went to look at the cyclotron there. This cyclotron hardly required a room: It was about a yard across--the diameter of the whole thing. It was the world's smallest cyclotron, hut they had got fantastic results. They had all kinds of special techniques and tricks. If they wanted to change something in the "D's"--the D-shaped half circles that the particles go around–they'd take a screwdriver, and remove the D's by hand, fix them, and put them back. At Princeton it was a lot harder, and at MIT you had to take a crane that came rolling across the ceiling, lower the hooks, and it was a *hellllll* of a job.)

I learned a lot of different things from different schools. MIT is a *very* good place; I'm not trying to put it down. I was just in love with it. It has developed for itself a spirit, so that every member of the whole place thinks that it's the most wonderful place in the world--it's the *center*, somehow, of scientific and technological development in the United States, if not the world. It's like a New Yorker's view of New York: they forget the rest of the country. And while you don't get a good sense of proportion there, you do get an excellent sense of being *with* it and *in* it, and having motivation and desire to keep on--that you're specially chosen, and lucky to be there.

So MIT was good, hut Slater was right to warn me to go to another school for my graduate work. And I often advise my students the same way. Learn what the rest of the world is like. The variety is worthwhile.

I once did an experiment in the cyclotron laboratory at Princeton that had some startling results. There was a problem in a hydrodynamics book that was being discussed by all the physics students. The problem is this: You have an S-shaped lawn sprinkler – an S-shaped pipe on a pivot--and the water squirts out at right angles to the axis and makes it spin in a certain direction. Everybody knows which way it goes around; it backs away from the outgoing water. Now the question is this: If you had a lake, or swimming pool--a big supply of water--and you put the sprinkler completely under water, and sucked the water in, instead of squirting it out, which way would it turn? Would it turn the same way as it does when you squirt water out into the air, or would it turn the other way?

The answer is perfectly clear at first sight. The trouble was, some guy would think it was perfectly clear one way, and another guy would think it was perfectly clear the other way. So everybody was discussing it. I remember at one particular seminar, or tea, somebody went nip to Prof John Wheeler and said, "Which way do *you* think it goes around?"

Wheeler said, "Yesterday, Feynman convinced me that it went backwards. Today, he's convinced me equally well that it goes around the other way. I don't know *what* he'll convince me of tomorrow!"

I'll tell you an argument that will make you think it's one way, and another argument that will make you think it's the other way, OK?

One argument is that when you're sucking water in, you're sort of pulling the water with the nozzle, so it will go forward, towards the incoming water.

But then another guy comes along and says, "Suppose we hold it still and ask what kind of a torque we need to hold it still. In the case of the water going out, we all know you have to hold it on the outside of the curve, because of the centrifugal force of the water going around the curve, Now, when the water goes around the same curve the *other* way, it still makes the same centrifugal force toward the outside of the curve. Therefore the two cases are the same, and the sprinkler will go around the same way, whether you're squirting water out or sucking it in."

After some thought, I finally made up my mind what the answer was, and in order to demonstrate it, I wanted to do an experiment.

In the Princeton cyclotron lab they had a big carboy – a monster bottle of water. I thought this was just great for the experiment. I got a piece of copper tubing and bent it into an S-shape. Then in the middle I drilled a hole, stuck in a piece of rubber hose, and led it up through a hole in a cork I had put in the top of the bottle. The cork had another hole, into which I put another piece of rubber hose, and connected it to the air pressure supply

of the lab. By blowing air into the bottle, I could force water into the copper tubing exactly as if I were sucking it in. Now, the S-shaped tubing wouldn't turn around, but it would twist (because of the flexible rubber hose), and I was going to measure the speed of the water flow by measuring how far it squirted out of the top of the bottle.

I got it all set up, turned on the air supply, and it went "*Puup!*" The air pressure blew the cork out of the bottle. I wired it in very well, so it wouldn't jump out. Now the experiment was going pretty good. The water was coming out, and the hose was twisting, so I put a little more pressure on it, because with a higher speed, the measurements would be more accurate. I measured the angle very carefully, and measured the distance, and increased the pressure again, and suddenly the whole thing just blew glass and water in all directions throughout the laboratory. A guy who had come to watch got all wet and had to go home and change his clothes (it's a miracle he didn't get cut by the glass), and lots of cloud chamber pictures that had been taken patiently using the cyclotron were all wet, but for some reason I was far enough away, or in some such position that I didn't get very wet. But I'll always remember how the great Professor Del Sasso, who was in charge of the cyclotron, came over to me and said sternly, "The freshman experiments should be done in the freshman laboratory!"

The Busted Bottle

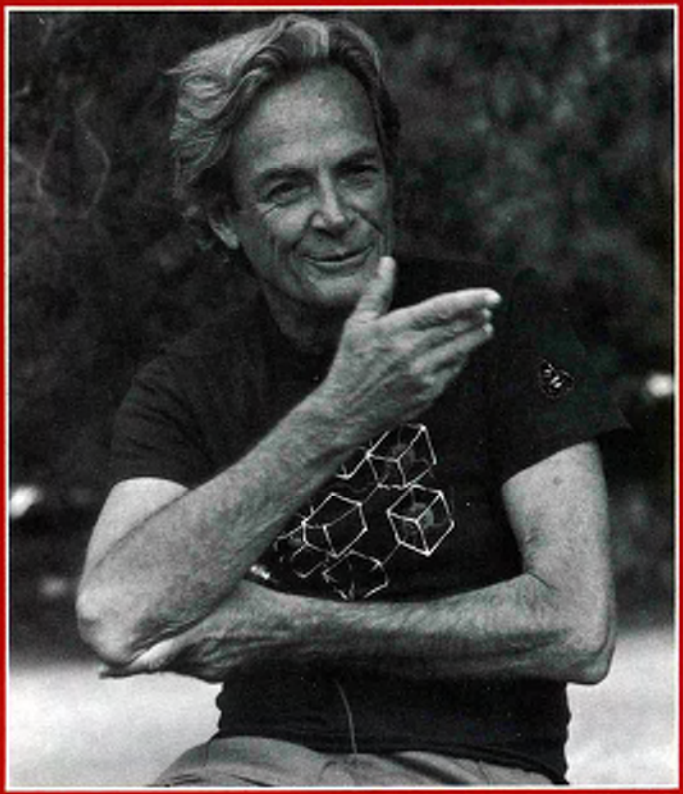
One day our discussions led to Mach's principle. We knew of the inspiration Einstein had found in thinking of inertia as originating in acceleration—not relative to Newton's absolute space, but relative to Mach's faraway stars. Was it a problem in the junior course in mechanics that started us thinking about the familiar lawn sprinkler? Shaped like a swastika, it shoots out four jets of water. The recoil drives the sprinkler arms round and round. But where does the recoil act? Doesn't it act at the point where the stream of water suddenly changes direction from straight out to straight transverse? But suppose the arm sucks water in instead of squirting it out. Surely, we said to each other, there is an identical change in direction and therefore an identical reaction. Surely the sprinkler will again turn round when water in the arms is sucked in rather than being shot out. Oh no, it won't. Oh yes, it will. We had a great time trying out both sides of this question on our colleagues. As the days went by, more and more colleagues up and down the corridors took positions. The debate grew more animated. No argument of theory was strong enough to still the disagreements. The situation called for an experiment.

Feynman made a six-inch miniature lawn sprinkler out of glass tubing and hung it from a flexible tube of rubber. He checked that it worked OK as a sprinkler. Then he wangled the whole dangling gadget through the throat of a great glass carboy filled with water. He got this outfit set up on the floor of the cyclotron lab, where there was a handy compressed-air outlet. He ran the compressed air in through a second hole in the cork at the top of the carboy. Ha! A little tremor as the pressure was first applied, as water first began to run backward through the miniature lawn sprinkler. But, as the flow continued there was no reaction. Then increase the air pressure. Get more backward flow of water. Again a momentary tremor at the start of this maneuver but no continuing torque. OK, more pressure. And more! Boom! The glass container exploded. Water and fragments of glass went all over the cyclotron room. From that time onward Feynman was banished from the lab.

Everything as Scattering

I enlisted Feynman's help on one of the ever-expanding problems I had brought back to Chapel Hill and then to Princeton from my postdoc days. At the great Rutherford-centered October 1934 London-

RICHARD P. FEYNMAN



"What Do You Care What Other People Think?"

Further Adventures of a Curious Character

We went along a little bit farther, and the guide said, "This is a lounge for the various delegates, where they often hold informal discussions." There were some small, square windows in the doors to the lounge that you could look through, so people looked in. There were a few men sitting there talking.

I looked through the windows and saw Igor Tamm, a physicist from Russia that I know. "Oh!" I said. "I know that guy!" and I started through the door.

The guide screamed, "No, no! Don't go in there!" By this time he was *sure* he had a maniac on his hands, but he couldn't chase me because he wasn't allowed to go through the door himself!

Tamm's face lit up when he recognized me, and we talked a little bit. The guide was relieved and continued the tour without me, and I had to run to catch up.

At the Physical Society meeting my good friend Bob Bacher said to me, "Listen: it's going to be hard to get a room when that Atoms for Peace Conference is going on. Why don't you have the State Department arrange a room for you, if you haven't already made a reservation?"

"Naw!" I said. "I'm not gonna have the State Department do a damn thing for me! I'll do it myself."

When I returned to my hotel I told them that I would be leaving in a week, but I'd be coming back at the end of summer: "Could I make a reservation now for that time?"

"Certainly! When will you be returning?"

"The second week in September. . ."

"Oh, we're terribly sorry, Professor Feynman; we are already completely booked for that time."

So I wandered off, from one hotel to another, and found they were all booked solid, six weeks ahead of time!

Then I remembered a trick I used once when I was with a physicist friend of mine, a quiet and dignified English fellow.

We were going across the United States by car, and when we got just beyond Tulsa, Oklahoma, there were supposed to be big floods up ahead. We came into this little town and we saw cars parked everywhere, with people and families in them, trying to sleep. He says, "We had better stop here. It's clear we can go no further.'

"Aw, come on!" I say. "How do you know? Let's see if we

can *do* it: maybe by the time we get there, the water will be down.

"We shouldn't waste time," he replies. "Perhaps we can find a room in a hotel if we look for it now."

"Aw, don't worry about it!" I say. "Let's go!"

We drive out of town about ten or twelve miles and come to an arroyo. Yes, even for me, there's too much water. There's no question: we aren't going to try to get through *that*.

We turn around: my friend's muttering about how we'll have no chance of finding a room in a hotel now, and I tell him not to worry.

Back in town, it's absolutely *blocked* with people sleeping in their cars, obviously because there are no more rooms. All the hotels must be packed. I see a small sign over a door: it says "HOTEL." It was the kind of hotel I was familiar with in Albuquerque, when I would wander around town looking at things, waiting to see my wife at the hospital: you have to go up a flight of stairs and the office is on the first landing.

We go up the stairs to the office and I say to the manager, "We'd like a room."

"Certainly, sir. We have one with two beds on the third floor."

My friend is amazed: The town is packed with people sleeping in cars, and here's a hotel that has room!

We go up to our room, and gradually it becomes clear to him: there's no door on the room, only a hanging cloth in the doorway. The room was fairly clean, it had a sink; it wasn't so bad. We get ready for bed.

He says, "I've got to pee."

"The bathroom is down the hall."

We hear girls giggling and walking back and forth in the hall outside, and he's nervous. He doesn't want to go out there.

"That's all right; just pee in the sink," I say.

"But that's unsanitary."

"Naw, it's okay; you just turn the water on."

"I can't pee in the sink," he says.

We're both tired, so we lie down. It's so hot that we don't use any covers, and my friend can't get to sleep because of the noises in the place. I kind of fall asleep a little bit.

A little later I hear a creaking of the floor nearby, and I open one eye slightly. There he is, in the dark, quietly stepping over to the sink.

* * *

Anyway, I knew a little hotel in Geneva called the Hotel City, which was one of those places with just a doorway on the street and a flight of stairs leading up to the office. There were usually some rooms available, and nobody made reservations.

I went up the stairs to the office and told the desk clerk that I'd be back in Geneva in six weeks, and I'd like to stay in their hotel: "Could I make a reservation?"

"Certainly, sir. Of course!"

The clerk wrote my name on a piece of paper—they hadn't any book to write reservations in—and I remember the clerk trying to find a hook to put the paper on, to remember. So I had my "reservation," and everything was fine.

I came back to Geneva six weeks later, went to the Hotel City, and they *did* have the room ready for me; it was on the top floor. Although the place was cheap, it was clean. (It's Switzerland; it was *clean*!) There were a few holes in the bedspread, but it was a clean bedspread. In the morning they served a European breakfast in my room; they were rather delighted to have this guest who had made a reservation six weeks in advance.

Then I went over to the U.N. for the first day of the Atoms for Peace Conference. There was quite a line at the reception desk, where everyone was checking in: a woman was taking down everybody's address and phone number so they could be reached in case there were any messages.

"Where are you staying, Professor Feynman?" she asks.

"At the Hotel City."

"Oh, you must mean the Hotel Cité."

"No, it's called 'City': C-I-T-Y." (Why not? We would call it "Cité" here in America, so they called it "City" in Geneva, because it sounded foreign.)

"But it isn't on our list of hotels. Are you *sure* it's 'City'?"

"Look in the telephone book for the number. You'll find it."

"Oh!" she said, after checking the phone book. "My list is incomplete! Some people are still looking for a room, so perhaps I can recommend the Hotel City to them."

She must have got the word about the Hotel City from someone, because nobody else from the conference ended up staying there. Once in a while the people at the Hotel City would receive telephone calls for me from the U.N., and would *run up* the two flights of stairs from the office to tell me, with some awe and excitement, to come down and answer the phone.

There's an amusing scene I remember from the Hotel City. One night I was looking through my window out into the courtyard. Something, in a building across the courtyard, caught the corner of my eye: it looked like an upside-down bowl on the windowsill. I thought it had moved, so I watched it for a while, but it didn't move any. Then, after a bit, it moved a little to one side. I couldn't figure out what this thing was.

After a while I figured it out: it was a man with a pair of binoculars that he had against the windowsill for support, looking across the courtyard to the floor below me!

There's another scene at the Hotel City which I'll always remember, that I'd love to be able to paint: I was returning one night from the conference and opened the door at the bottom of the stairway. There was the proprietor, standing there, trying to look nonchalant with a cigar in one hand while he pushed something up the stairs with the other. Farther up, the woman who brought me breakfast was pulling on this same heavy object with both hands. And at the top of the stairs, at the landing, there *she* was, with her fake furs on, bust sticking out, hand on her hip, *imperiously waiting*. Her customer was a bit drunk, and was not very capable of walking up the steps. I don't know whether the proprietor knew that *I* knew what this was all about; I just walked past everything. He was ashamed of his hotel, but, of course, to me, it was delightful.

FEYNMAN'S RAINBOW

A SEARCH FOR BEAUTY IN PHYSICS AND IN LIFE

LEONARD MLODINOW

a cigarette. It seemed to bring him such deep satisfaction. "Let me know if you want to learn lattices," he said. "I'll promise you one thing . . . you won't have to sleep under a table of spiders—or strings."

With that we kept on toward the physics building. Then I spotted Feynman in the distance. I had spent the last couple of days on the lookout for Feynman, hoping to manufacture a natural way to bump into him and see if he would still talk to me. I told Constantine I'd see him later. I walked over toward Feynman.

When I got to him, Feynman was gazing at a rainbow. He had an intense look on his face, as if he were concentrating. As if he had never seen one before. Or maybe as if it might be his last.

I approached him cautiously.

"Professor Feynman. Hi," I said.

"Look, a rainbow," he said without looking at me. I was relieved that I didn't detect any residual annoyance in his voice.

I joined him in staring at the rainbow. It appeared pretty impressive, if you stopped to look at it. It wasn't something I normally did—in those days.

"I wonder what the ancients thought of rainbows," I mused. There were many myths based on the stars, but I thought rainbows must have seemed equally mysterious.

"That's a question for Murray," he said. I eventually tested Feynman's theory on this and asked Murray. Sure enough, I discovered that Murray was an encyclopedia when it came to native and ancient cultures. He even collected artifacts. I learned from him that

the Navajo people saw the rainbow as a sign of good fortune, whereas some other Indians saw the rainbow as a bridge between the living and the dead. I didn't quite get the names of those Indians because Murray pronounced them in a manner that was so authentic it was unintelligible.

"All I know," Feynman continued, "is that according to one legend angels put gold at its ends and only a nude man can reach it. As if a nude man doesn't have better things to do," he said with a sly smile.

"Do you know who first explained the true origin of the rainbow?" I asked.

"It was Descartes," he said. After a moment he looked me in the eye.

"And what do you think was the salient feature of the rainbow that inspired Descartes' mathematical analysis?" he asked.

"Well, the rainbow is actually a section of a cone that appears as an arc of the colors of the spectrum when drops of water are illuminated by sunlight behind the observer."

"And?"

"I suppose his inspiration was the realization that the problem could be analyzed by considering a single drop, and the geometry of the situation."

"You're overlooking a key feature of the phenomenon," he said.

"Okay, I give up. What would you say inspired his theory?"

"I would say his inspiration was that he thought rainbows were beautiful."

I looked at him sheepishly. He looked at me.

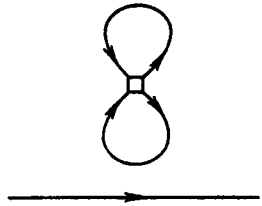
"How's your work coming?" he asked.

I shrugged. "It's not really coming." I wished I was like Constantine. It all came so easily to him.

"Let me ask you something. Think back to when you were a kid. For you, that isn't going too far back. When you were a kid, did you love science? Was it your passion?"

I nodded. "As long as I can remember."

"Me, too," he said. "Remember, it's supposed to be fun." And he walked on.



X

IT WAS HARD TO GET a handle on the articles Schwarz handed me, but at least I found myself finally able to focus. I discovered that, despite the dubious reputation of Schwarz and his theory, and his lack of any collaborators on the faculty, he had four or five graduate students working under him, more than any other professor in the department. I spoke with a couple of them when I had questions. They seemed capable. They seemed sane. Didn't they realize that 99.9 percent of physics "experts" thought they were all crackpots?

And why did the rest of the faculty allow so many students to go "astray" like this? Someone, I thought, must be a supporter. Could it be Feynman?

It was a Saturday and the campus was as quiet as the city at daybreak. But this was well past noon, and I was hungry for breakfast. The problem was, though most students lived on campus, on weekends both the Greasy and the Athenaeum were closed. I figured they must eat somewhere, so I went walking outside

in search of road kill, or maybe a vending machine. I spotted Feynman a short distance away. I couldn't imagine why he was there, but I took the opportunity to "bump" into him.

"Make any discoveries yet?" he said.

"Right now I'm trying to discover some food. You know where I can eat?"

"I know where," he said. "The difficulty lies in the 'when.' Weekends, the usual places on campus are closed."

We were heading in the direction of the Athenaeum. There seemed to be something going on there. We didn't talk for a while.

"Let me ask you something," I finally said. "Do you think it would be wise to work on a theory that almost everyone else thinks is nonsense?"

"Only under one condition," he said.

"And what is that?"

"That *you* don't think it is nonsense."

"I'm not sure I know enough to tell."

He chuckled. "Maybe if you knew enough to tell, you wouldn't work on it, either."

"You mean maybe I'm too dumb to know better."

"Not necessarily. Maybe you just don't know enough, or haven't known it long enough, to be *spoiled* by what you know. Too much education can cause trouble."

It is true that many of the greatest discoveries in physics were made by "kids" who were roughly the age I was. It was the age at which Newton invented calculus, Einstein discovered relativity, and Feynman developed his diagram technique. Plenty of other ad-

vances were made by older physicists, but the most revolutionary seemed to be made by the young. It had been understood among us graduate students that, for mathematical and theoretical physics brainpower, our minds were at their peak. But Feynman seemed to be seeing it differently, as if we go downhill not because of mental decline, but due to some kind of brainwashing. Maybe that's why he avoided learning new things from books or research papers; he was famous for always insisting on deriving new results himself, on understanding them his way. To him, to stay young meant to retain a beginner's outlook. He had clearly succeeded.

"Look," he said. "You've found food."

There was a big buffet in the Athenaeum courtyard. There seemed to be a wedding reception going on. We stopped and gazed at the crowd in their elegant dresses, suits, and ties.

"Yeah, but unfortunately we're not invited."

"I see you are an expert in etiquette."

"What do you mean?"

"I mean, if you are not invited, does it mean you aren't welcome?"

I shrugged. "I usually assume that."

"Then I guess you aren't that hungry."

I thought about it for a moment.

"Well, we're not exactly dressed for it." He had on a dress shirt and slacks. I was clad in shorts and a T-shirt.

"Of course we aren't. What scientist goes to work looking like he's dressed for a wedding? Well, other than Murray." He laughed.

"You'll come with me?" I said.

He grinned. We headed to the buffet. He looked on as I started loading my plate. At first no one seemed to pay much attention to us, but then a man in a tuxedo came up behind us in line.

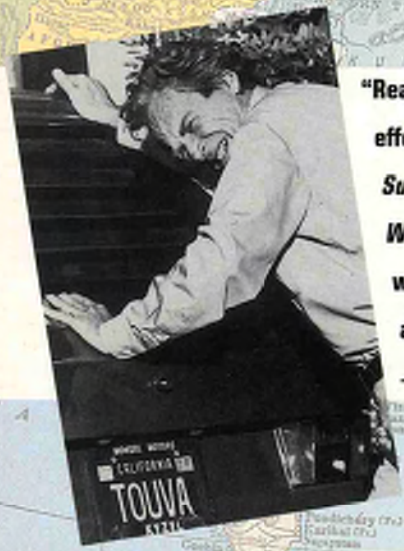
"Bride's side or groom's?" the man asked.

"Neither," said Feynman. The man looked us up and down. My mind raced, searching for a lie that might minimize my embarrassment. Then Feynman said, "We represent the physics department."

The man smiled, took some salad, and walked away, seemingly unbothered by either the answer or our attire.

TUVA OR BUST!

RICHARD FEYNMAN'S LAST JOURNEY

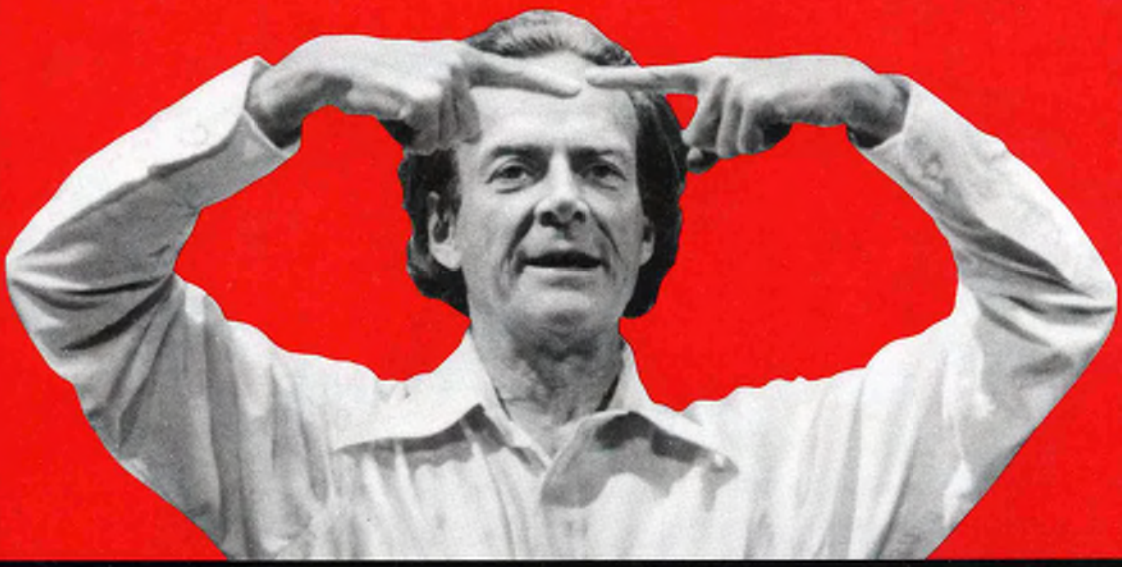


"Readers who enjoyed the collaborative efforts of Feynman and Leighton in *Surely You're Joking, Mr. Feynman!*, and *What Do You Care What Other People Think?* will undoubtedly cherish this poignant account of Feynman's last escapade."

— *Library Journal*

RALPH LEIGHTON

GENIUS



THE LIFE AND SCIENCE OF RICHARD FEYNMAN

JAMES GLEICK

AUTHOR OF CHAOS

tron; it'll be proportional to the charge on the other electron."

What bothered me was, I thought he must have *done* the calculation. I only realized later that a man like Wheeler could immediately *see* all that stuff when you give him the problem. I had to calculate, but he could see.

Then he said, "And it'll be delayed—the wave returns late—so all you've described is reflected light."

"Oh! Of course," I said.

"But wait," he said. "Let's suppose it returns by advanced waves—reactions backward in time—so it comes back at the right time. We saw the effect varied inversely as the square of the distance, but suppose there are a lot of electrons, all over space: the number is proportional to the square of the distance. So maybe we can make it all compensate."

We found out we could do that. It came out very nicely, and fit very well. It was a classical theory that could be right, even though it differed from Maxwell's standard, or Lorentz's standard theory. It didn't have any trouble with the infinity of self action, and it was ingenious. It had actions and delays, forwards and backwards in time—we called it "half-advanced and half-retarded potentials."

Wheeler and I thought the next problem was to turn to the quantum theory of electrodynamics, which had difficulties (I thought) with the self-action of the electron. We figured if we could get rid of the difficulty first in classical physics, and then make a quantum theory out of that, we could straighten out the quantum theory as well.

Now that we had got the classical theory right, Wheeler said, "Feynman, you're a young fella—you should give a seminar on this. You need experience in giving talks. Meanwhile, I'll work out the quantum theory part and give a seminar on that later."

So it was to be my first technical talk, and Wheeler made arrangements with Eugene Wigner to put it on the regular seminar schedule.

A day or two before the talk I saw Wigner in the hall. "Feynman," he said, "I think that work you're doing with Wheeler is very interesting, so I've invited Russell to the seminar." Henry Norris Russell, the famous, great astronomer of the day, was coming to the lecture!

Wigner went on. "I think Professor von Neumann would also

be interested." Johnny Von Neumann was the greatest mathematician around. "And Professor Pauli is visiting from Switzerland, it so happens, so I've invited Professor Pauli to come"—Pauli was a very famous physicist—and by this time, I'm turning yellow. Finally, Wigner said, "Professor Einstein only rarely comes to our weekly seminars, but your work is so interesting that I've invited him specially, so he's coming, too."

By this time I must have turned green, because Wigner said, "No, no! Don't worry! I'll just warn you, though: If Professor Russell falls asleep—and he will undoubtedly fall asleep—it doesn't mean that the seminar is bad; he falls asleep in all the seminars. On the other hand, if Professor Pauli is nodding all the time, and seems to be in agreement as the seminar goes along, pay no attention. Professor Pauli has palsy."

I went back to Wheeler and named all the big, famous people who were coming to the talk he got me to give, and told him I was uneasy about it.

"It's all right," he said. "Don't worry. I'll answer all the questions."

So I prepared the talk, and when the day came, I went in and did something that young men who have had no experience in giving talks often do—I put too many equations up on the blackboard. You see, a young fella doesn't know how to say, "Of course, that varies inversely, and this goes this way . . ." because everybody listening already knows; they can see it. But *he* doesn't know. He can only make it come out by actually doing the algebra—and therefore the reams of equations.

As I was writing these equations all over the blackboard ahead of time, Einstein came in and said pleasantly, "Hello, I'm coming to your seminar. But first, where is the tea?"

I told him, and continued writing the equations.

Then the time came to give the talk, and here are these *monster minds* in front of me, waiting! My first technical talk, and I have this audience! I mean they would put me through the wringer! I remember very clearly seeing my hands shaking as they were pulling out my notes from a brown envelope.

But then a miracle occurred, as it has occurred again and again in my life, and it's very lucky for me: the moment I start to think about the physics, and have to concentrate on what I'm explaining, nothing else occupies my mind—I'm completely im-

mune to being nervous. So after I started to go, I just didn't know who was in the room. I was only explaining this idea, that's all.

But then the end of the seminar came, and it was time for questions. First off, Pauli, who was sitting next to Einstein, gets up and says, "I do not sink dis teory can be right, because of dis, and dis, and dis," and he turns to Einstein and says, "Don't you agree, Professor Einstein?"

Einstein says, "Nooooooooooooo," a nice, German-sounding "No,"—very polite. "I find only that it would be very difficult to make a corresponding theory for gravitational interaction." He meant for the general theory of relativity, which was his baby. He continued: "Since we have at this time not a great deal of experimental evidence, I am not absolutely sure of the correct gravitational theory." Einstein appreciated that things might be different from what his theory stated; he was very tolerant of other ideas.

I wish I had remembered what Pauli said, because I discovered years later that the theory was not satisfactory when it came to making the quantum theory. It's possible that that great man noticed the difficulty immediately and explained it to me in the question, but I was so relieved at not having to answer the questions that I didn't really listen to them carefully. I do remember walking up the steps of Palmer Library with Pauli, who said to me, "What is Wheeler going to say about the quantum theory when he gives his talk?"

I said, "I don't know. He hasn't told me. He's working it out himself."

"Oh?" he said. "The man works and doesn't tell his assistant what he's doing on the quantum theory?" He came closer to me and said in a low, secretive voice, "Wheeler will never give that seminar."

And it's true. Wheeler didn't give the seminar. He thought it would be easy to work out the quantum part; he thought he had it, almost. But he didn't. And by the time the seminar came around, he realized he didn't know how to do it, and therefore didn't have anything to say.

I never solved it, either—a quantum theory of half-advanced, half-retarded potentials—and I worked on it for years.

Mixing Paints

THE REASON why I say I'm "uncultured" or "anti-intellectual" probably goes all the way back to the time when I was in high school. I was always worried about being a sissy; I didn't want to be too delicate. To me, no *real* man ever paid any attention to poetry and such things. How poetry ever got *written*—that never struck me! So I developed a negative attitude toward the guy who studies French literature, or studies too much music or poetry—all those "fancy" things. I admired better the steelworker, the welder, or the machine shop man. I always thought the guy who worked in the machine shop and could make things, now *he* was a *real guy!* That was my attitude. To be a practical man was, to me, always somehow a positive virtue, and to be "cultured" or "intellectual" was not. The first was right, of course, but the second was crazy.

I still had this feeling when I was doing my graduate study at Princeton, as you'll see. I used to eat often in a nice little restaurant called Papa's Place. One day, while I was eating there, a painter in his painting clothes came down from an upstairs room he'd been painting, and sat near me. Somehow we struck up a conversation and he started talking about how you've got to learn a lot to be in the painting business. "For example," he said, "in this restaurant, what colors would you use to paint the walls, if *you* had the job to do?"

I said I didn't know, and he said, "You have a dark band up to such-and-such a height, because, you see, people who sit at the tables rub their elbows against the walls, so you don't want a nice, white wall

Nature uses only the longest threads to weave her patterns, so that each small piece of her fabric reveals the organization of the entire tapestry.

"Is no one inspired by our present picture of the universe? This value of science remains unsung by singers, you are reduced to hearing not a song or poem, but an evening lecture about it. This is not yet a scientific age."

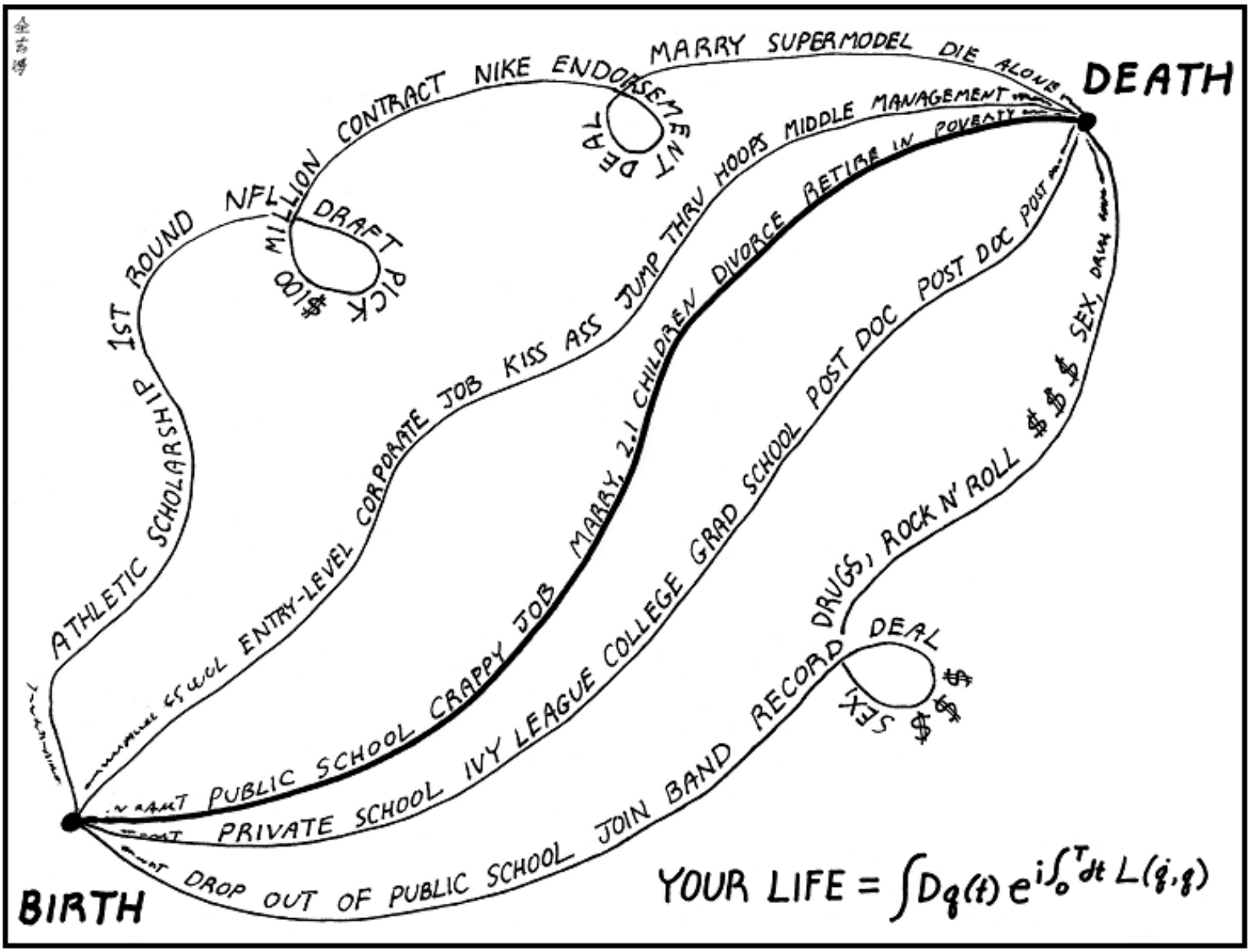
Poets say science takes away from the beauty of the stars — mere globs of gas atoms. Nothing is "mere". I too can see the stars on a desert night, and feel them. But do I see less or more? The vastness of the heavens stretches my imagination — stuck on this carousel my little eye can catch one-million-year-old light. A vast pattern — of which I am a part. What is the pattern or the meaning or the why? It does not do harm to the mystery to know a little more about it. For far more marvelous is the truth than any artists of the past imagined it. Why do the poets of the present not speak of it? What men are poets who can speak of Jupiter [Roman God] if he were a man, but if he is an immense spinning sphere of methane and ammonia must be silent?

The Universe in a Glass of Wine

A poet once said, "The whole universe is in a glass of wine." We will probably never know in what sense he said that, for poets do not write to be understood. But it is true that if we look in glass of wine closely enough we see the entire universe.

There are the things of physics: the twisting liquid which evaporates depending on the wind and weather, the reflections in the glass, and our imagination adds the atoms. The glass is a distillation of the earth's rocks, and in its composition we see the secrets of the universe's age, and the evolution of the stars. What strange array of chemicals are in the wine? How did they come to be? There are the ferments, the enzymes, the substrates, and the products. There in wine is found the great generalization: all life is fermentation. Nobody can discover the chemistry of wine without discovering the cause of much disease. How vivid is the claret, pressing its existence into the consciousness that watches it!

If in our small minds, for some convenience, divide this glass of wine, this universe, into parts - physics, biology, geology, astronomy, psychology, and so on - remember that nature does not know it! So let us put it all back together, not forgetting ultimately what it is for. Let us give one more final pleasure: drink it and forget it all!

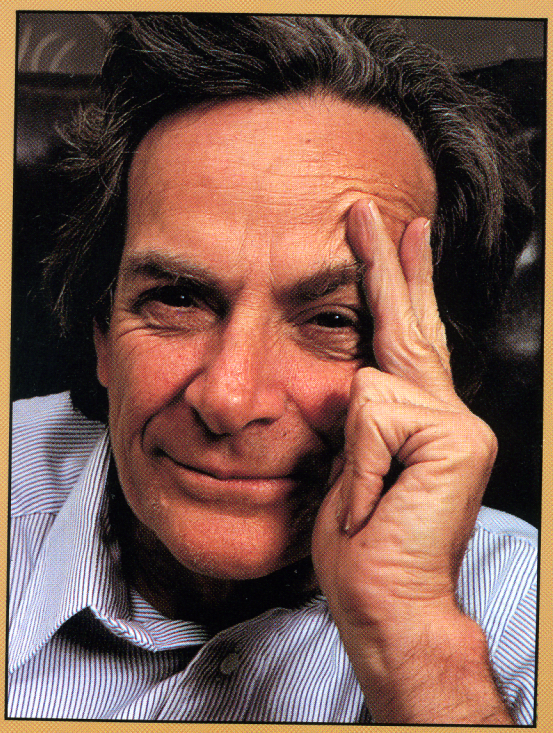


The Path Integral Formulation of Your Life

"Most of the Good Stuff"

Memories Of

RICHARD FEYNMAN



Editors: LAURIE M. BROWN · JOHN S. RIGDEN

RICHARD FEYNMAN AT LA CAÑADA HIGH SCHOOL: FEYNMAN'S LAST PUBLIC PERFORMANCE

John S. Rigden

The only time I talked with Richard Feynman was on November 14, 1987—three months plus one day before he died. On that occasion, he was obviously ill, yet his spirit animated the high school auditorium where our panel sat.

For me, it started in late September or early October. I was scheduled to give a colloquium at California Polytechnic State University, San Luis Obispo on Thursday, November 12. Sometime around October 1, I got a call from a member of the physics department at San Luis Obispo. I shall never forget the question posed to me: would I be willing to stay an extra couple of days, go to a meeting in the Los Angeles area, serve on a panel at the meeting, and "fill in for Feynman!" I laughed and said, "Right. I'll fill in for Feynman. You've got to be kidding." The explanation followed: Feynman had agreed to serve on this panel, but he was too ill and had to withdraw. In typical Feynman fashion, he did not want his name to appear on the program so only a few people knew of his potential participation. When I learned that the subject to be discussed by the panel was, "What High School Physics

Should Include," I agreed to be a panel member. Besides, I thought, someday I can tell my grandchildren: I once filled in for Richard Feynman.

On November 12 I was in San Luis Obispo. Sometime during that day, a telephone call came to the physics department informing my hosts that Feynman was feeling better and would participate in the meeting on Saturday (November 14). On hearing this, I immediately offered to withdraw from the panel and happily listen to Feynman and the other panel members from the audience. "No," I was told, "your name is on the program. We shall simply add another chair."

Two days later, on November 14, I stood in the foyer of the auditorium of La Cañada High School in La Cañada, California. About 30 minutes before the meeting was to begin, I saw David Goodstein, a Cal Tech physicist, and Richard Feynman coming up the sidewalk and they entered the foyer. (As I looked at Feynman I was shocked. I had attended a lecture Feynman gave in 1983 and the change in his appearance from that earlier time was jolting.) David saw me and, since we know each other, he walked to me with Feynman at his side. "Dick Feynman," David said, "this is John Rigden." And with that, David walked away.

I remember the thoughts that raced through my mind as I stood there alone with Feynman: "What do I say to this guy? What do I call him? I can't call him Dick, I won't call him Professor Feynman." In a nervous way I blurted out,

"Feynman, what are you doing here? I'm taking your place."

"Ohhhh," said Feynman, "you're taking my place. Then I'll leave."

"No, no, Feynman," I said quickly, still coping with my nervousness, "You stay. You might say something useful." At that, we both laughed. That was how my only conversation with Richard Feynman began.

My friends who knew Feynman tell me that my irreverent remarks were probably a good way to start our conversation. They may be correct. In any event, Feynman and I had a free flowing discussion for the next 30 minutes. At one point I said,

"That was a nice letter you wrote to David Mermin."

Feynman looked at me and said, "Who's Mermin?"

Who's David Mermin? A little background explanation is needed. Mermin is a physicist at Cornell University. But that's not so important. The significant fact is that Mermin wrote a paper entitled "Bringing Home the Atomic World: Quantum Mysteries for Anybody." This paper was published in the *American Journal of Physics* in October 1981 during my tenure as editor. It was a wonderful paper. With only arith-

degeneration of the physics. So I think anytime you try to teach the subject without teachers who love the subject, it is doomed to failure and is a foolish thing to do."

"I was on the [mathematics] curriculum committee some years ago and the State had to look at everything that anybody presents—it's kind of a democratic law. And so all kinds of little plans for how to teach elementary arithmetic were sent in and they were all wonderful. One used matchsticks, another teaches base 2, another makes little crossword puzzles with numbers. And the wonderful thing was that every one of these methods were successful. In every case there was evidence of this—they tried it in a class and it worked well. The only trouble is, we are not sure if it would work in a class when we don't know if the ideas are communicated to someone whose expertise is not in this area, or they hadn't invented the idea or had no enthusiasm for it. It was always the one who invented it—who loved the subject and had special students or even ordinary students but who had a special attitude and was going to try a new way to teach it. There was a certain enthusiasm and a special relationship between the teacher and the students which was a kind of excitement, and unless that excitement exists between the teacher and student then I don't think education is worthwhile and it's better not to try education under these circumstances."

Another question posed to the panel was, "How are we going to get more people to take physics?" Feynman began his response by saying that students should not be *required* to take physics.

If they are required, "...not only will we have a lot of physics teachers who don't know physics, but we will have... students in their classrooms who are not interested in the subject and when you have a very large number of students who are not interested in the subject, the whole flavor of the class disappears."

Feynman went on:

"I want to add one other thing. I hear a good deal about

> teaching physics, that it's a difficult subject. I think if you look at it a little bit differently it's the same thing. What is physics?" Feynman asked. Then he answered his question: "It's supposed to be a description of the physical world. Now if you think, 'I'm *not* going to be teaching physics, rather I'm going to be telling [students] about the physical world.' Does that give you a different idea? What is the physical world like? Does that mean we...start [the course] and spend 2/3 of our time with falling bodies...?"

Again Feynman asked a question:

> "What tells you as much as possible about the physical world? We can make a list of things about the physical world that are...delightful. One of the things about the physical world is...that an object falls in uniform acceleration. Let's look at [this] a little bit...you know with the torques and angular momentum. That's not so delightful as the fact that everything is made out of atoms. [Knowing about atoms] you can understand what evaporation is and freezing and when it evaporates its becomes cooler because the fast ones leave and so forth. And in one picture, [the atomic picture] you get a whole lot of ideas. Maybe if we would think in terms of telling what the physical world is like so that they can understand it better."

Feynman went on talking about teaching physics:

> "...its a way of thinking about teaching: what you're trying to describe is the wonders and the way things actually are and later on you can talk about the falling bodies after a bit. You don't need to know a hell of a lot—just a little [about the] elasticity of colliding molecules—because they have no friction they never stop, they go on forever. Put a few molecules together that they attract each other. [In this view] you have a tremendous amount of understanding of the world. It might be possible to concentrate on those things and it won't be quite as difficult for the students."

trical phenomena. It's kind of mysterious how a television works."

When the panel discussion ended, people from the audience swarmed to the stage of the auditorium and surrounded Feynman. They started asking Feynman questions, physics questions. As I watched, I realized I was witnessing something extraordinary. Feynman's energies grew as he responded to question after question. The outside corners of his eyes were creased by the smiles that played over his face as he talked about physics. His hands and arms cut through the air with increasing vigor as their motions served to complement, even demonstrate, *his* explanations.

"I have a question," a man said and, as he positioned himself in front of Feynman. He held a long copper tube in his left hand and two metal cylinders in his right hand.

"All right," said Feynman, "I'll answer, but if there's a trick, I might miss it."

"No trick," said the physicist and with that he released one of the metal cylinders and it fell rapidly through the copper tube and onto the floor. Then he released the second metal cylinder: it fell s*l*o*w*l*y through the tube and the questioner dramatically moved his right hand to the bottom of the copper tube and caught the cylinder as it emerged.

"It's a magnet," said Feynman.

"That's right," said the physicist holding the tube, "but that's not the question. Suppose the tube were a superconductor. With no i^2R losses, how do you account for the energetics of the falling magnet?" It was a great question, Feynman was challenged, and his virtuoso performance continued.

I was not surprised by Feynman's deftness as I watched him—his reputation in such impromptu situations was well known. It was the enjoyment he exuded as he stood there talking physics with an eager, receptive group of physics teachers that moved me. It was an enjoyment I could feel. When the session ended and Feynman, along with David Goodstein, walked out of the La Cañada High School auditorium, I had the feeling that I was standing on holy ground.

Earlier, in the foyer before the panel session began, I had told Feynman that I thought he had cheated the public in his book *Surely You're Joking*.

"Who are you," said Feynman in response, "to tell an au-

> thor that you don't like his book and that he cheated the reader?"
>
> "If there is one thing about your life that holds it together," I said, "it is your love of physics and that doesn't come through your book."
>
> Immediately, Feynman shot back, "That was deliberate."
>
> "That's how you cheated the public," I said.
>
> After a just-discernable pause, Feynman said, "That's the next book."

When I heard of Feynman's death, early in the morning on February 16, I experienced an eerie sense of personal loss and, with sadness, I thought of the "next book," a book that Feynman would never write.

The panel session at La Cañada High School was Feynman's last public appearance. During the months prior to this meeting, Feynman experienced a succession of physical heights and depths which took him from feeling relatively good to feeling very bad. He had canceled his agreement to appear on the panel during one of his bad times, but a few days before the meeting he was once again feeling well enough to reaffirm his commitment. So he had his secretary, Helen Tuck, call the organizer of the panel session to indicate his willingness to participate. In spite of his deteriorating health, high school physics was a subject Feynman would *choose* to discuss and La Cañada High School was a place Feynman would *choose* to come. It is somehow fitting that Feynman's last public appearance was in a high school auditorium discussing what should be taught in high school physics.