Physics 541 **April 14, 2010**

This unit has two topics

Path Integrals

Non-Relativistic QM

Feynman Diagrams

Fully Relativistic QED

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 = KE + PE

=> Schrodinger equation formulation of QM

Lagrangian formulation

L = KE - PE

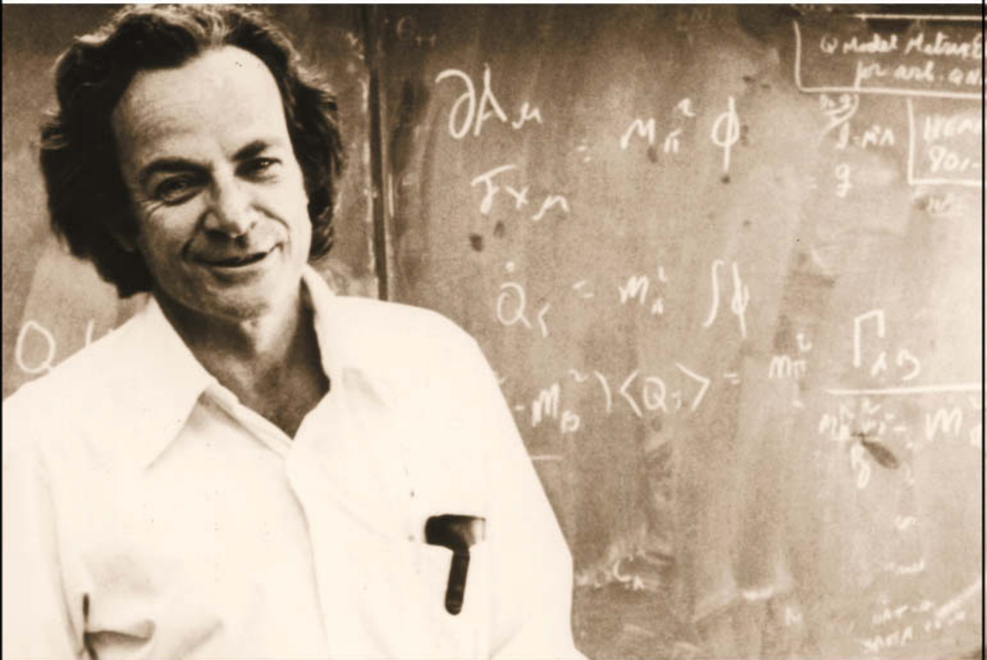
=> Path integral formulation of QM

Operators in a Hilbert Space

versus

No Operators   No Hilbert Space

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

VOLUME 20, NUMBER 2 APRIL, 1948

Space-Time Approach to Non-Relativistic Quantum Mechanics

R. P. FEYNMAN

Cornell University, Ithaca, New York

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.

Quantum Field Theory

IN A NUTSHELL

A. Zee

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

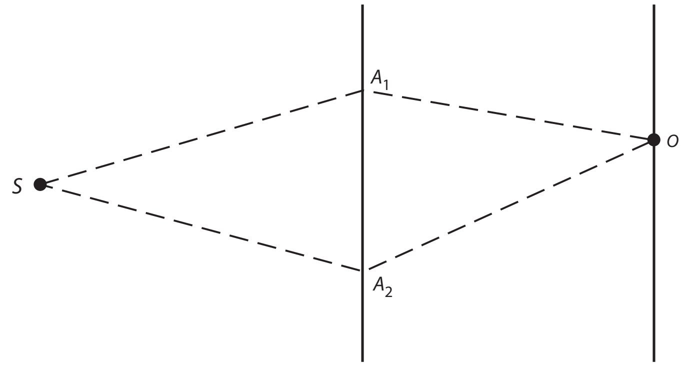


Figure I.2.1

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

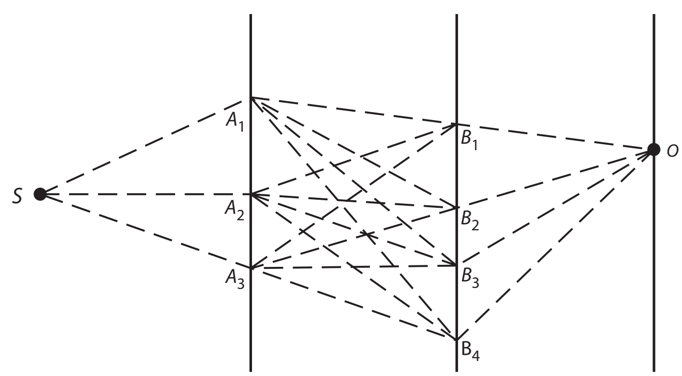


Figure I.2.2

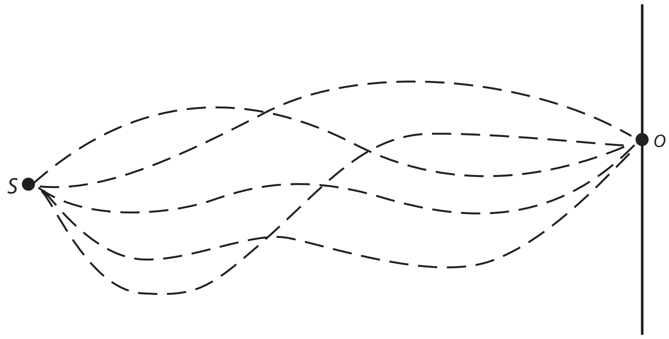


Figure I.2.3

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).

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

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.

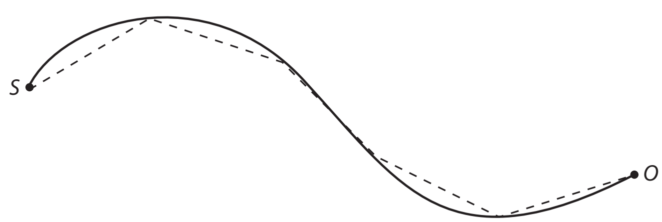


Figure I.2.4

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

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

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

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.

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 ambigu-

[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).

ities 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.

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*

LAWRENCE SCHULMAN
Physics Department, Indiana University, Bloomington, Indiana 47401
(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 original *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

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.

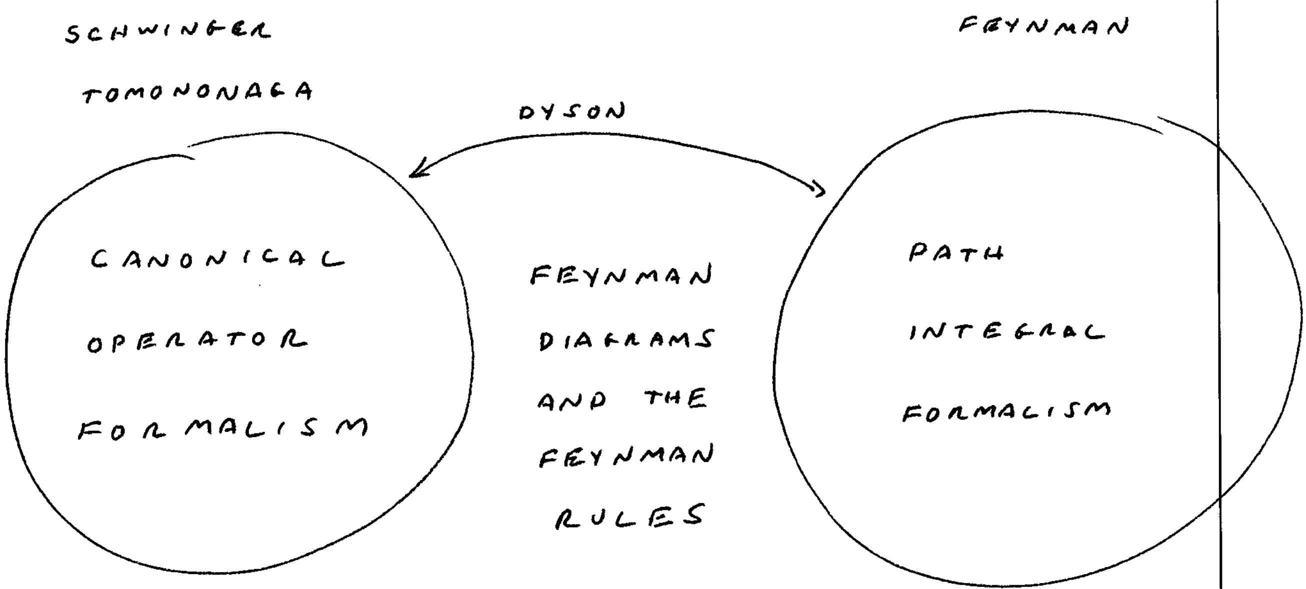
The path-integral approach was revived in the late 1960s, when Faddeev and Popov[4] and De Witt[5] showed how to apply it to non-Abelian gauge theories and general relativity. For most theorists, the turning point came in 1971, when 't Hooft[6] used path-integral methods to derive the Feynman rules for 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 DIAGRAMS ARE UBIQUITOUS!

Why?



UNITARITY OBVIOUS

LORENTZ INVARIANCE IS *NOT* OBVIOUS

LORENTZ INVARIANCE OBVIOUS

UNITARITY IS *NOT* OBVIOUS

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

Feynman Diagrams

Fully Relativistic

Four Papers in Physical Review

The theory of positrons (1949)

Space-Time Approach to Quantum Electrodynamics (1949)

Mathematical formulation of the quantum theory of electromagnetic interaction (1950)

An Operator Calculus Having Applications in Quantum Electrodynamics (1951)

Lecture Notes

Quantum Electrodynamics
Benjamin Press (1961)

GENIUS

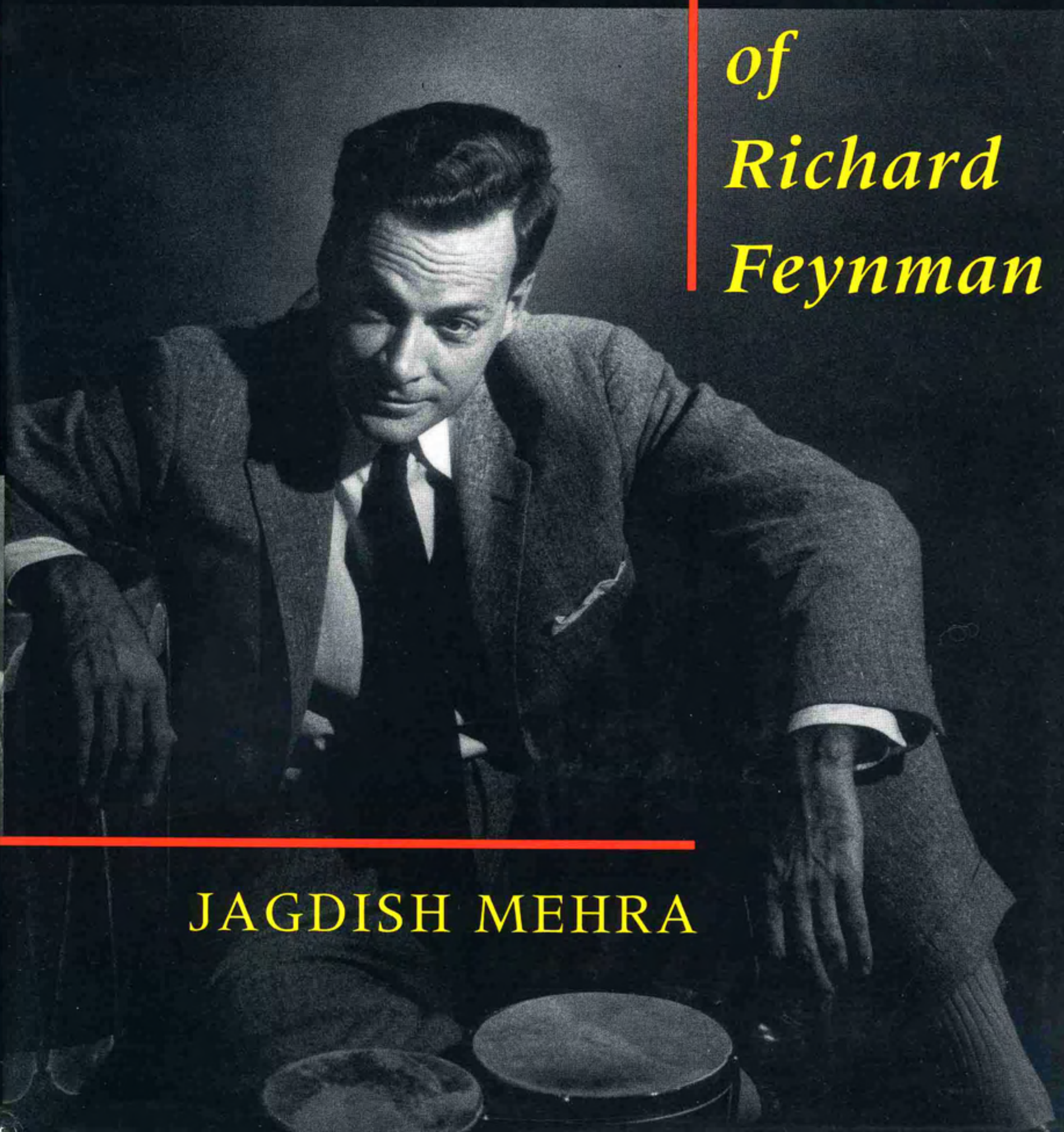
THE LIFE AND SCIENCE OF RICHARD FEYNMAN

JAMES GLEICK

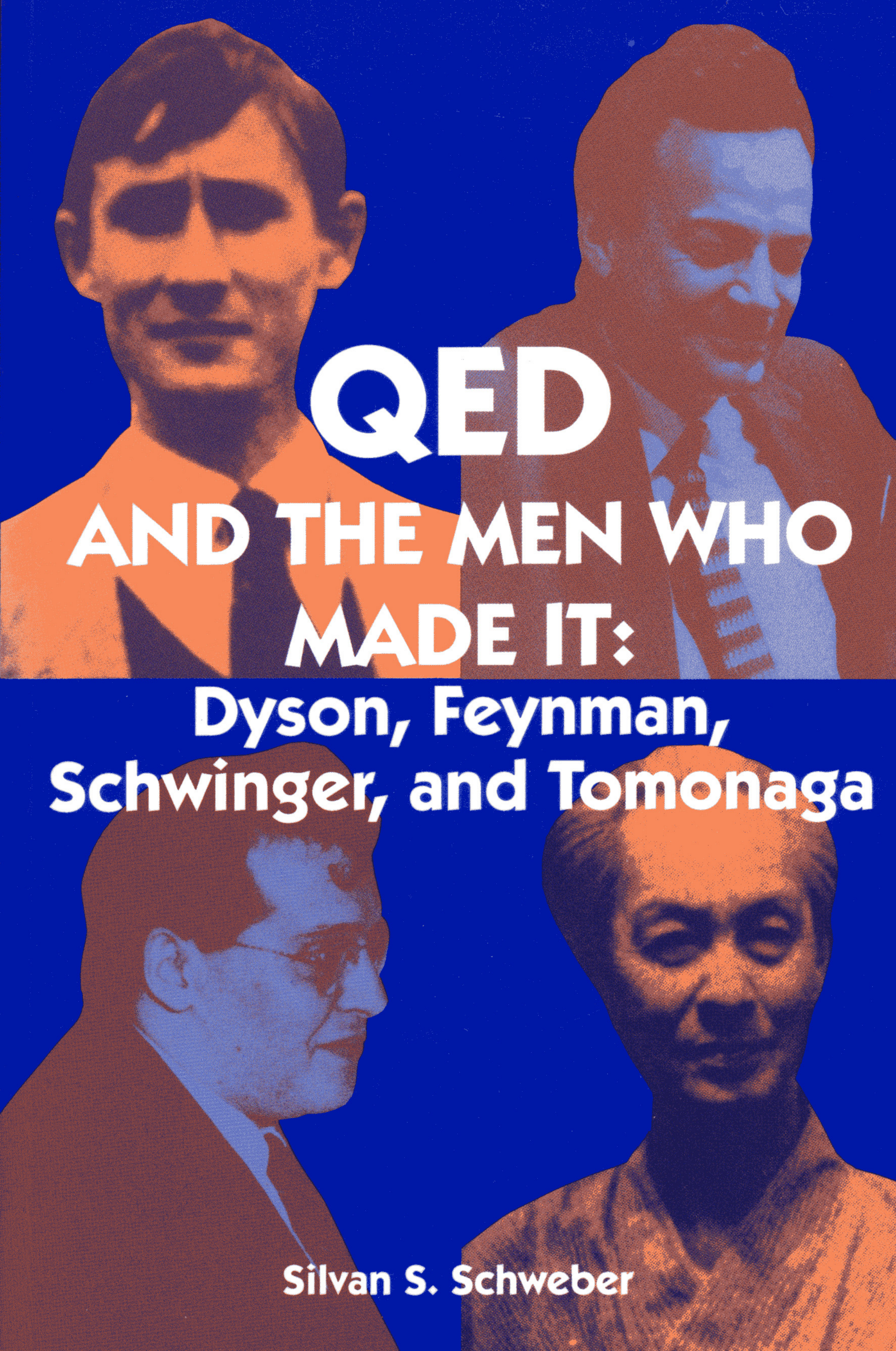
AUTHOR OF CHAOS

THE BEAT OF A DIFFERENT DRUM

The life and science of Richard Feynman



JAGDISH MEHRA



QED AND THE MEN WHO MADE IT:

Dyson, Feynman, Schwinger, and Tomonaga

Silvan S. Schweber

Around a Mental Block

Princeton was celebrating the bicentennial of its founding with a grand explosion of pomp that fall: parties, processions, and a series of formal conferences that drew scholars and dignitaries from long distances. Dirac had agreed to speak on elementary particles as part of a three-day session on the future of nuclear science. Feynman was invited to introduce his one-time hero and lead a discussion afterward.

He disliked Dirac's paper, a restatement of the now-familiar difficulties with quantum electrodynamics. It struck him as backward-looking in its Hamiltonian energy-centered emphasis—a dead end. He made so many nervous jokes that Niels Bohr, who was due to speak later in the day, stood up and criticized him for his lack of seriousness. Feynman made a heartfelt remark about the unsettled state of the theory. "We need an intuitive leap at the mathematical formalism, such as we had in the Dirac electron theory," he said. "We need a stroke of genius."

As the day wore on—Robert Wilson speaking about the high-energy scattering of protons, E. O. Lawrence lecturing on his California accelerators—Feynman looked out the window and saw Dirac lolling on a patch of grass and gazing at the sky. He had a question that he had wanted to ask Dirac since before the war. He wandered out and sat down. A remark in a 1933 paper of Dirac's had given Feynman a crucial clue toward his discovery of a quantum-mechanical version of the *action* in classical mechanics. "It is now easy to see what the quantum analogue of all this must be," Dirac had written, but neither he nor anyone else had pursued this clue until Feynman discovered that the "analogue" was, in fact, exactly proportional. There was a rigorous and potentially useful mathematical bond. Now he asked Dirac whether the great man had known all along that the two quantities were proportional.

"Are they?" Dirac said. Feynman said yes, they were. After a silence he walked away.

Feynman's reputation was traveling around the university circuit. Job offers floated his way. They seemed perversely inappropriate and did nothing to help his mood of frustration. Oppenheimer had invited him to California for the spring semester; now he turned the invitation down. Cornell promoted him to associate professor and raised his salary again. The chairman of the University of Pennsylvania's physics department needed a new chief theorist. Here Bethe stepped in paternalistically: he had no intention of

Many years later Feynman and Dirac met one more time. They exchanged a few awkward words---a conversation so remarkable that a physicist within earshot immediately jotted down the Pinteresque dialog he thought drifting his way:

I am Feynman.

I am Dirac.

(Silence)

It must be wonderful to be the discoverer of that equation.

That was a long time ago. (Pause) What are you working on?

Mesons.

Are you trying to discover an equation for them?

It is very hard.

One must try.

and just calculated the integral by means of the Taylor series expansion, thus working out the Schrödinger equation

$$\left(-\frac{\hbar^2}{2m}\frac{\partial^2}{dx^2}+V(x)\right)\psi(x,t)=i\hbar\frac{\partial}{\partial t}\psi(x,t). \tag{6.12}$$

Feynman turned to Jehle, who did not quite follow, and told him that Dirac meant that they were proportional. Herbert Jehle 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!' [22]

In the fall of 1946, Princeton University was celebrating its bicentennial, on the occasion of which numerous festivities, including various series of lectures were organized. In one of these sessions, devoted to science and organized by Eugene Wigner, Feynman was invited to introduce Dirac and, after his lecture, comment upon it. 'It was like the ward-heeler of the 54th district (in New York City) introducing the president of the United States. Dirac sent me his paper, in his own handwriting, to read and I had to comment on it. After Dirac's lecture, I made my comments; I tried to simplify Dirac's very technical talk for the benefit of high school teachers and others who were not familiar with the things that Dirac had talked about. But the other physicists, like Bohr and Weisskopf, who were there did not give a damn about these other people, and they criticized my attempt to 'explain Dirac' in my simplified way. After I had made my criticism, people were standing around and discussing Dirac's paper, and I looked through the window and saw that Dirac was lying on the lawn outside looking up in the sky. I had never really sat and talked to him before then. But there was this question which I very much wanted to ask him, so I walked up to him and said: 'Professor Dirac, you wrote in a paper[23] in which you talk about the analogy between exp $(i\varepsilon L)$ and the difference between two points. He said, "yes." I said, "Did you know that they are not just analogous, they are equal or rather proportional." He said, "Are they?" I said, "Yes." "Oh, that's interesting," was his comment. I wanted to know whether I had discovered something or not, but he had never sat down to find out whether they were equal or proportional. He just said, "No, I didn't know, are they? That's interesting!" That was the first time I talked to him personally.' [1]

In his paper Dirac was not able to complete this line of his investigations on quantum mechanics because his point of view was based on the opinion that the correspondence between the function K and the exponent of the classical action function is only an approximate semiclassical relation. From the very beginning of relativistic quantum mechanics it had been recognized that the expression $\exp[(i/\hbar)S]$ gave the semiclassical approximation to the exact quantum wave function. Therefore Dirac was looking for a proper and exact quantum analog of Hamilton's principal function S, and he found relations between the corresponding exact quantum Hamiltonian wave function and other quantum operators. Another step in this direction was taken by Edmund Whittaker.[24] Up to then this approach seems to have been quite formal and did not lead to any essentially new results. Hence, the crucial formal step to Feynman's new method was to look at the limit when ε goes to zero. In this limit one reaches an exact result for infinitesimal times.

Thus Feynman found the relation between the Lagrangian and quantum mechanics, which was an important result of his dissertation, but still for infinitesimal times. Several days later, when he was lying in bed, he worked out the next fundamental step. Feynman described it as follows: '. . . I'm lying in bed—I can still see the bed. And I can't sleep too well. And the bed was next to the wall. I got my feet up against the wall, leaning my head off on one side of the bed. You know that kind of stuff. And I'm picturing this thing and I'm putting more and more lengthy times, I have to do this again and again, and so I've got this exponential iL times again, times again, integrate it, integrate it. But the product of all the exponentials is the exponential of the sum of the L's, which is the action. So I go, AAAAAHHHHH, and I jumped, "That's the action!" That was a moment of discovery!'[1]

Now Feynman was able, by using N times the formula (6.11), to obtain exactly the right result for the function $K(X, T; x, t)$. He had to construct the expression

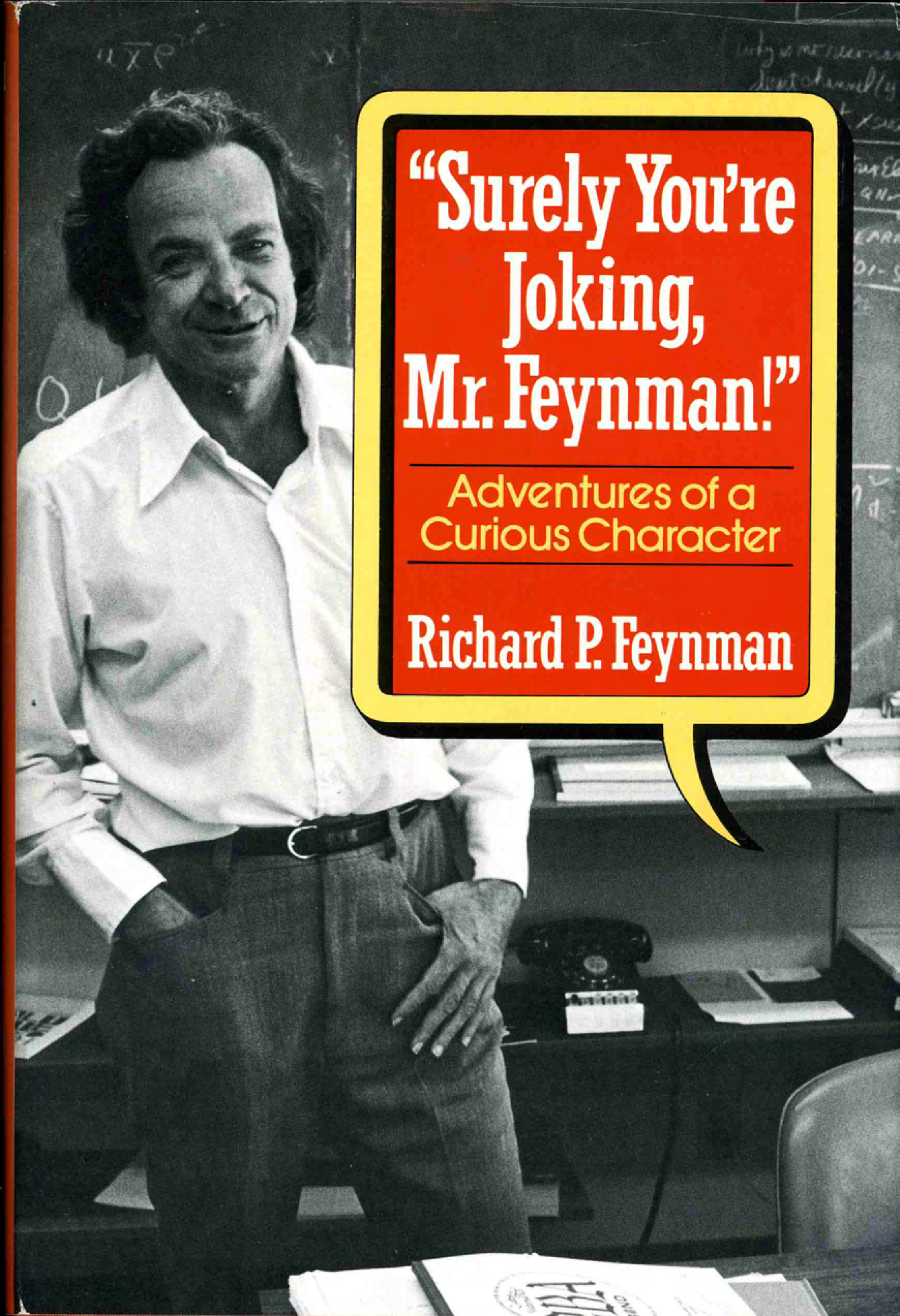
$$\int \cdots \int \exp\left((i/\hbar) \sum_{i=0}^{N-1} L[(x_{i+1}-x_i)/(t_{i+1}-t_i), x_{i+1}]\,(t_{i+1}-t_i) \right) \frac{dx_N}{A_N} \cdots \frac{dx_1}{A_1}, \tag{6.13}$$

where $t=0, t_1, t_2, \ldots, t_{N-1}, t_N = T$ are certain instants of time, which divide the time interval from the initial instant t to the final instant T into a large number of small intervals from t_1 to t_{i+1} of duration ε ($i=1, 2, \ldots, N$), such that $t_i = t + i\varepsilon$. Then, in the limit when ε goes to zero, we reach the exact quantum function K. In this limit, the expression in the exponent in equation (6.13) resembles Riemann's integral for the classical action functional:

$$A = \lim_{\varepsilon \to 0} \left(\sum_{i=0}^{N-1} L[(x_{i+1}-x_i)/(t_{i+1}-t_i), x_{i+1}]\,(t_{i+1}-t_i) \right). \tag{6.14}$$

Feynman's conclusion was that equation (6.11) 'is equivalent to Schrödinger's differential equation for the wave function ψ. Thus, given a classical system described by a Lagrangian, which is a function of velocities and coordinates only, a quantum mechanical description of an analogous system may be written down directly, without working out a Hamiltonian.'[25]

This approach thus promised to solve the main problem, which Feynman was trying to attack in his thesis: that is, the quantization of a classical system without knowing its Hamiltonian. In addition, it turned out that he obtained a


"Surely You're Joking, Mr. Feynman!"
Adventures of a Curious Character
Richard P. Feynman

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?"

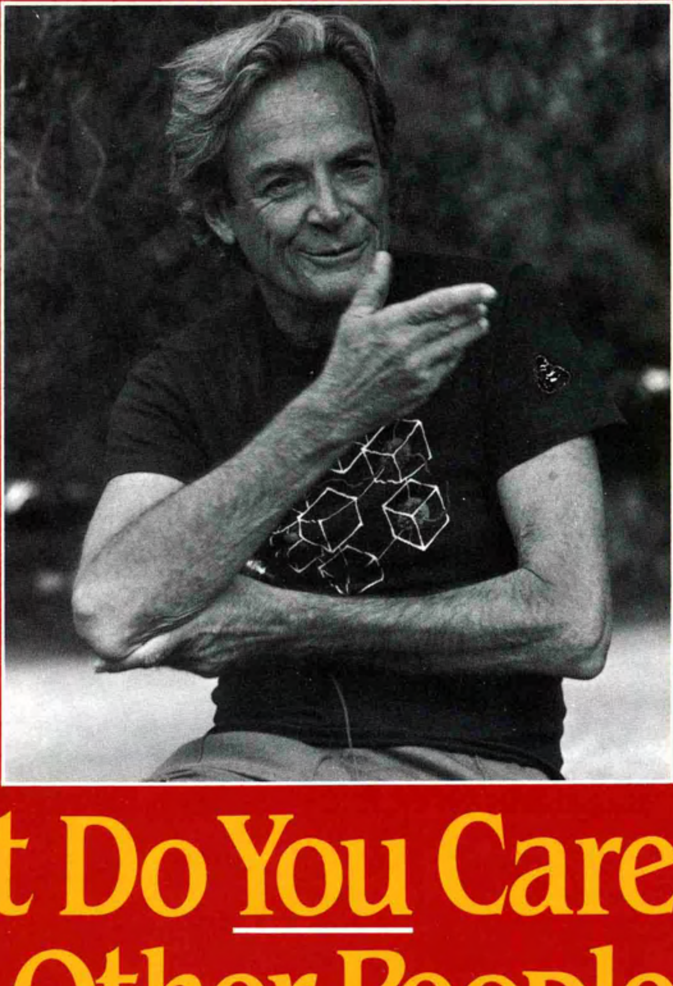
"Tea," I said, "thank you."

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.

RICHARD P. FEYNMAN



"What Do You Care What Other People Think?"

Further Adventures of a Curious Character

crossed Queens and Brooklyn, then went to Staten Island on the ferry—that was our romantic boat ride—and drove to the city hall for the borough of Richmond to get married.

We went up the stairs, slowly, into the office. The guy there was very nice. He did everything right away. He said, "You don't have any witnesses," so he called the bookkeeper and an accountant from another room, and we were married according to the laws of the state of New York. Then we were very happy, and we smiled at each other, holding hands.

The bookkeeper says to me, "You're married now. You should kiss the bride!"

So the bashful character kissed his bride lightly on the cheek.

I gave everyone a tip and we thanked them very much. We got back in the car, and drove to Deborah Hospital.

Every weekend I'd go down from Princeton to visit Arlene. One time the bus was late, and I couldn't get into the hospital. There weren't any hotels nearby, but I had my old sheepskin coat on (so I was warm enough), and I looked for an empty lot to sleep in. I was a little worried what it might look like in the morning when people looked out of their windows, so I found a place that was far enough away from houses.

The next morning I woke up and discovered I'd been sleeping in a garbage dump—a landfill! I felt foolish, and laughed.

Arlene's doctor was very nice, but he would get upset when I brought in a war bond for $18 every month. He could see we didn't have much money, and kept insisting we shouldn't contribute to the hospital, but I did it anyway.

One time, at Princeton, I received a box of pencils in the mail. They were dark green, and in gold letters were the words "RICHARD DARLING, I LOVE YOU! PUTSY." It was Arlene (I called her Putsy).

Well, that was nice, and I love her, too, but—you know how you absentmindedly drop pencils around: you're showing Professor Wigner a formula, or something, and leave the pencil on his desk.

In those days we didn't have extra stuff, so I didn't want to waste the pencils. I got a razor blade from the bathroom and cut off the writing on one of them to see if I could use them.

The next morning, I get a letter in the mail. It starts out, "WHAT'S THE IDEA OF TRYING TO CUT THE NAME OFF THE PENCILS?"

It continues: "Aren't you proud of the fact that I love you?" Then: "WHAT DO YOU CARE WHAT OTHER PEOPLE THINK?"

Then came poetry: "If you're ashamed of me, dah dah, then Pecans to you! Pecans to you!" The next verse was the same kind of stuff, with the last line, "Almonds to you! Almonds to you!" Each one was "Nuts to you!" in a different form.

So I had to use the pencils with the names on them. What else could I do?

It wasn't long before I had to go to Los Alamos. Robert Oppenheimer, who was in charge of the project, arranged for Arlene to stay in the nearest hospital, in Albuquerque, about a hundred miles away. I had time off every weekend to see her, so I would hitchhike down on a Saturday, see Arlene in the afternoon, and stay overnight in a hotel there in Albuquerque. Then on Sunday morning I would see Arlene again, and hitchhike back to Los Alamos in the afternoon.

During the week I would often get letters from her. Some of them, like the one written on a jigsaw-puzzle blank and then taken apart and sent in a sack, resulted in little notes from the army censor, such as "Please tell your wife we don't have time to play games around here." I didn't tell her anything. I *liked* her to play games—even though she

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.

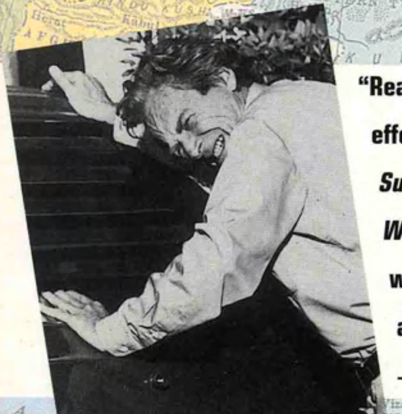
"Let me ask you something. Think back to when you were a kid. For you, that isn't going too far back. When you were a kid, did you love science? Was it your passion?"

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

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

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

Tuva and ask how I can visit? As much fun as it was to find out more about Tuva, our real goal was to get to Kyzyl, and so far we hadn't done anything about that.

I contacted Mary Fleming Zirin, a woman I had bummed rides off of when I was a student at UCLA, where she was working on her Ph.D. in Russian. Mary remembered me, and agreed to translate a short letter to "Teacher" at Shkola No. 2 in Kyzyl. For good measure I sent a similar letter to Shkola No. 1, Kyzyl, Tuva, USSR.

In the spring, after the high school swimming season and its coaching responsibilities were over, I went to the library at the University of Southern California and searched through immigration records of 1900–1950 to see if anyone had come from Tuva to America. While there was no specific category for Tuva, several Mongolians and "others" had come to the United States in any given year.

Just in case one of those "others" was from Tuva and had ended up in Los Angeles, I obtained a personalized license plate and mounted it in a do-it-yourself frame with the words "MONGOL MOTORS" and "KYZYL" flanking "TOUVA" above and below. At the very least, a stamp collector might recognize the spelling and honk if he loved Tuvan postage stamps.

An article I found at the same library at USC claimed that Kyzyl was the USSR's "Atom City"—the center of Soviet atomic weapons development—because Tuva is isolated and surrounded by mountains rich in uranium. Another article, in the *Christian Science Monitor* (September 15, 1966), said:

> According to the official version, Tannu Tuva . . . asked for admission into the Soviet Union. Its "petition was granted," just as four years earlier those of the three Baltic republics had been granted.
>
> In the case of Tannu Tuva the discovery of a large uranium deposit, the first to be found in the Soviet Union on the

threshold of the atomic age, seems to have caused the change of status.

If Kyzyl is the USSR's Los Alamos, I thought, then the KGB will never believe that Richard Feynman wants to visit the place because of how it is spelled!

In the summer of 1978, after competing in the First Annual Southern California Clown Diving Championships in Los Angeles, I flew to Europe for a camping trip in the Balkans. Meanwhile, Richard went to the doctor complaining of abdominal pains. He soon underwent surgery. The doctor removed a fourteen-pound mass of cancer the size of a football that had crushed his kidney and spleen. Richard needed the remainder of the summer to recover.

When I returned from Europe, there was no reply from my fellow teachers at Shkola No. 2 or Shkola No. 1.

In the fall a new school year began, this time without the coaching responsibilities. Another change: along with four math classes, I was permitted to teach one class of world geography. Of course my students eventually learned about a little lost country called Tannu Tuva, but there were more important things to discuss: the horrors of the Khmer Rouge regime in Cambodia were becoming known to the outside world; Iran was in turmoil, with the Shah's regime threatened by the exiled Muslim leader Ayatollah Khomeini; and Pope John Paul I had died after thirty-three days in office and was succeeded by Karol Cardinal Wojtyla of Poland, the first non-Italian pope in four hundred years. In the Middle East, Moammar Kadafi was angry with Anwar Sadat for signing the Camp David Accords with Menachem Begin. (I therefore had to explain why the geography book, written in the 1960s, said that Libya and Egypt were allies against Israel.)

Although there was no ballet to work on in 1978, Richard and I continued drumming together. When we discussed

Hail to the Chief!

4

We were in shock. Tuva, isolated in the center of Asia—that little lost land of enchanting postage stamps—had transcended our wildest dreams. The sounds on the record were stunning: how could two notes be produced simultaneously by a single singer? At first the higher "voice" sounded like a flute, several octaves higher than the fundamental tone. Then came even stranger styles of *höömei,* the most bizarre of which was the "rattling" style, which sounded like a long-winded frog.

It took several days for us to recover. Finally, I sent the mysterious sounds to all the Friends of Tuva, including Mary Zirin, who suggested I send a copy to Mario Casetta, the charismatic deejay of ethnic music on KPFK, the local independent radio station.

To Mr. Casetta, I simply wrote, "Guess what this is, and where it comes from—Ralph Leighton, 577-8882." (The hint was to see which letters on the telephone correspond to 8882.)

Casetta responded right away. "It sounds like something I have on a record from Mongolia," he said enthusiastically. (Indeed, his record contained some *höömei* from western Mongolia, where several thousand Tuvans live.)

I told him the mysterious sounds were from the land once known as Tannu Tuva.

"Tannu Tuva—you mean the place with those beautiful postage stamps?" (Mario, too, had collected Tuva's distinctive stamps as a boy.) "We'll have to do a show—just give me some time to rummage around the attic and find my collection."

At the end of October Richard had some medical tests done at UCLA. The results were "interesting" from Richard's point of view; they were disastrous from everyone else's perspective: the cancer in his abdomen that supposedly had been removed three years before had now spread in a complicated pattern around his intestines.

Dr. Donald Morton of UCLA's John Wayne Cancer Clinic was called in to operate. "I believe in cutting away an inch of good tissue around every place I find cancer," said the surgeon. "I usually don't stop until I can see the operating table underneath."

"What are the odds in an operation like that?" Richard asked.

"Well, I've had a dozen patients, and I haven't lost one yet—but I still don't know what my limitations are."

Richard took radiation therapy to soften up the cancerous tissue, and then underwent what was to be a ten-hour operation. As he was being sewn up, an artery close to his heart burst. He required eighty pints of blood before it was over. Coincidentally, there had been two other patients at UCLA with similar needs that day, so the blood bank was running

RECIPE

~~[illegible]~~

(1) CONSTRUCT ALL PATHS

(2) CALCULATE THE ACTION $S[x(t)]$ FOR EACH PATH

$$S[x(t)] = \int_{t_1}^{t_2} L \, dt$$

L ← LAGRANGIAN

(3) ~~(INTEGRATE)~~ SUM OVER ALL PATHS TO OBTAIN THE PROPAGATOR

$$U(x_2, t_2; x_1, t_1) = A \sum_{\text{all paths}} e^{i S[x(t)]/\hbar}$$

(4) USE THE PROPAGATOR

$$\psi(x_2, t_2) = \int U(x_2, t_2; x_1, t_1) \, \psi(x_1, t_1) \, dx_1$$

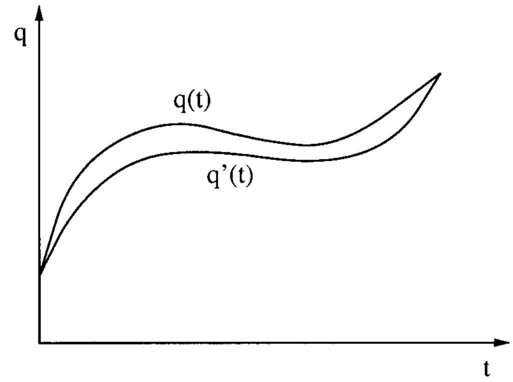


Figure 2: Two neighbouring paths.

However, this argument must be rethought for one exceptional path: that which extremizes the action, *i.e.*, the classical path, $q_c(t)$. For this path, $S[q_c + \eta] = S[q_c] + o(\eta^2)$. Thus the classical path and a very close neighbour will have actions which differ by much less than two randomly-chosen but equally close paths (Figure 3). This means that for fixed closeness

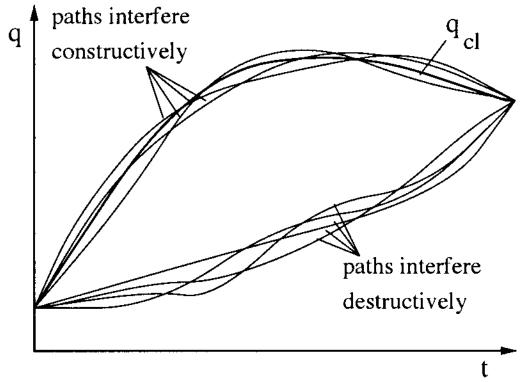
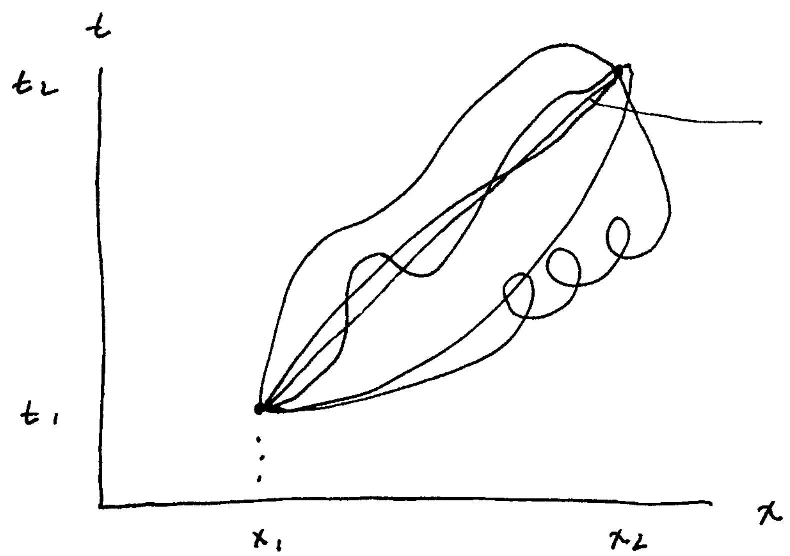


Figure 3: Paths near the classical path interfere constructively.

of two paths (I leave it as an exercise to make this precise!) and for fixed $\hbar$, paths near the classical path will on average interfere constructively (small phase difference) whereas for random paths the interference will be on average destructive.

Thus heuristically, we conclude that if the problem is classical (action $\gg \hbar$), the most important contribution to the PI comes from the region around the path which extremizes the PI. In other words, the particle's motion is governed by the principle that the action is stationary. This, of course, is none other than the Principle of Least Action from which the Euler-Lagrange equations of classical mechanics are derived.

(1) CONSTRUCT ALL PATHS



(2) CALCULATE THE ACTION

$$L(x, \dot{x}, t) = T - V$$

CLASSICAL PATH HAS MINIMUM ACTION

PHYSICS IS WHERE THE LEAST ACTION IS

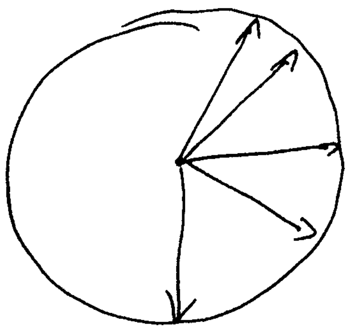
EXTREME ACTION IS

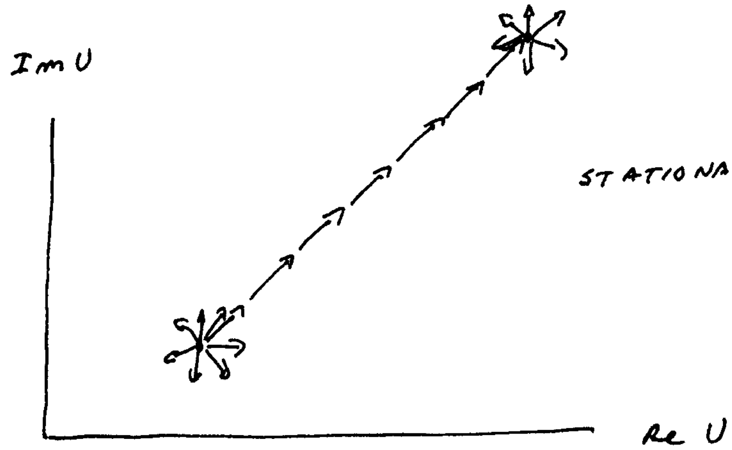
11

(3) CALCULATE THE PROPAGATOR

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

UNIT CIRCLE





STATIONARY PHASE

PHASE COHERENCE

$$\frac{|S_{\text{classical}} - S|}{\hbar} \leq \pi$$

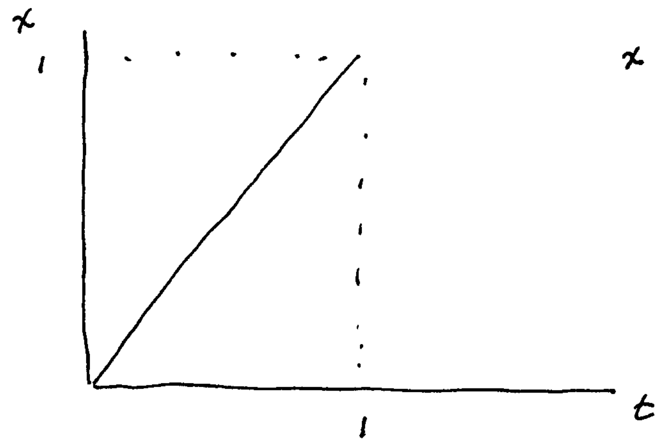
$\Delta S \leq \pi \hbar$

Q: Why do baseballs follow classical trajectories while electrons diffract, go thru both slits, etc.?

LAGRANGIAN for baseball and electron

$$L = T - V = \frac{1}{2} m v^2 - V(x) = \frac{1}{2} m v^2$$

THE CLASSICAL PATH



$x = v t \Rightarrow x = t$

CALCULATE THE ACTION FOR THE CLASSICAL PATH

$$S_{CL} = \int_0^1 L \, dt$$

$$= \int_0^1 \frac{1}{2} m v^2 \, dt$$

$$= \int_0^1 \frac{1}{2} m (1)^2 \, dt$$

$$S_{CL} = \frac{1}{2} m$$

NOW LET'S DO A NON CLASSICAL PATH

$$x = t^2$$

$$v = \frac{dx}{dt} = 2t$$

$$S_{NC} = \int_0^1 L \, dt = \int_0^1 \frac{1}{2} m (2t)^2 \, dt = \frac{4}{3} \left(\frac{1}{2} m \right)$$

$$S_{NC} = \frac{4}{3} S_{CL}$$ 14

THE DIFFERENCE IS THE MASS!

BASEBALL $m \sim 200g$

$$S_{CL} = \frac{1}{2}(200g)(1\ cm/s)^2(1s)$$

$$= 100\ erg \cdot sec \approx 10^{29}\hbar$$

$$\Delta S = \frac{1}{3} S_{CL} = 3 \times 10^{28}\hbar \ggg \pi\hbar$$

SO, BASEBALL MUST STAY EXTREMELY CLOSE TO THE CLASSICAL PATH!

ELECTRON $m \sim 10^{-27} g$

$$S_{CL} = \frac{1}{2}(10^{-27})(1)^2(1)$$

$$= 5 \times 10^{-28}\ erg \cdot sec$$

~~$\frac{1}{2}\hbar < \pi\hbar$~~

$$\Delta S = \frac{1}{3} S_{CL} = \frac{1}{6}\hbar \ll \pi\hbar$$

SO, ELECTRON WILL FOLLOW THE NON CLASSICAL PATH! 15