# NR Path Integrals

## Two formulations of classical mechanics

**Hamiltonian formulation**

**H = T + V**

**=> Schrodinger equation formulation of QM**

**Lagrangian formulation**

**L = T - V**

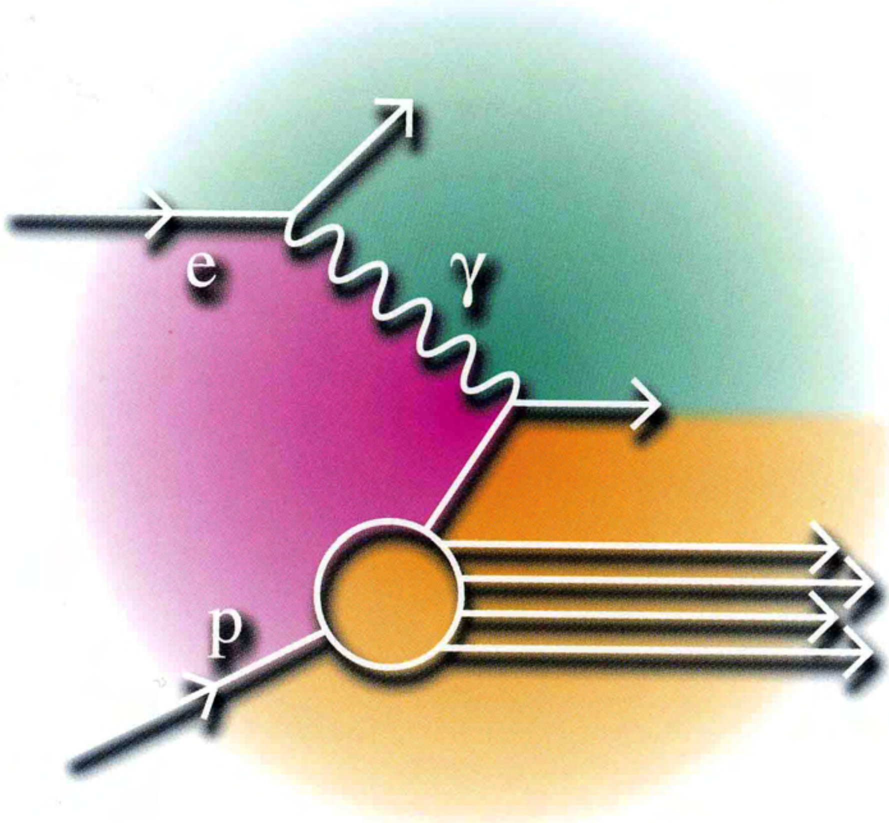
**=> Path integral formulation of QM**

## Ten good things about the path integral formulation

## One bad thing about the path integral formulation

# 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.

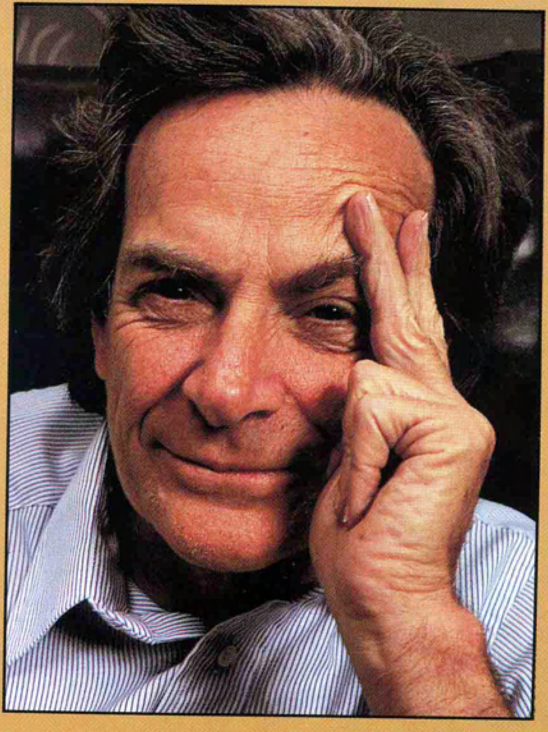
Professor Jehle showed me this; I read it; he explained it to me, and I said, "What does he mean, they are analogous; what does that mean, *analogous?* What is the use of that?" He said, "You Americans! You always want to find a use for everything!" I said that I thought that Dirac must mean that they 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:

# "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."

Feynman was gazing at a rainbow. As if he had never seen one before. Or maybe as if it might be his last.

I approached him cautiously and joined him in staring at the rainbow. It wasn't something I normally did—in those days.

"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's mathematical analysis?" he asked.

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

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

—FROM FEYNMAN'S RAINBOW

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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 author 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.

# THE YOUNG FEYNMAN

## John Archibald Wheeler

'"This chap from MIT: Look at his aptitude test ratings in mathematics and physics. Fantastic! Nobody else who's applying here at Princeton comes anywhere near so close to the absolute peak." Someone else on the Graduate Admissions Committee broke in, "He must be a diamond in the rough. We've never let in anyone with scores so low in history and English. But look at the practical experience he's had in chemistry and in working with friction."

These are not the exact words, but they convey the flavor of the committee discussion in the spring of 1939 that brought us 21-year-old Richard Phillips Feynman as a graduate student. How he ever came to be assigned to this 28-year-old assistant professor as grader in an undergraduate junior course in mechanics I will never know, but I am eternally grateful for the fortune that brought us together on more than one fascinating enterprise. As he brought those student papers back—with errors noted and helpful comments offered—there was often occasion to mention the work I was doing and the puzzlements I encountered. Discussions turned into laughter, laughter into jokes and jokes into more to-and-fro and more ideas.

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John Archibald Wheeler is Joseph Henry Professor of Physics, Emeritus, Princeton University, and Ashbel Smith and Jane and Roland Blumberg Professor, Emeritus, University of Texas at Austin.

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

# DICK FEYNMAN—THE GUY IN THE OFFICE DOWN THE HALL


## Murray Gell-Mann


*I hope someday to write a lengthy piece about Richard Feynman as I knew him (for nearly 40 years, 33 of them as his colleague at Caltech), about our conversations on the fundamental laws of physics, and about the significance of the part of his work that bears on those laws. In this brief note, I restrict myself to a few remarks and I hardly touch on the content of our conversations.*

When I think of Richard, I often recall a chilly afternoon in Altadena shortly before his marriage to the charming Gweneth. My late wife, Margaret, and I had returned in September 1960 from a year in Paris, London and East Africa; Richard had greeted me with the news that he was "catching up with me"—he too was to have an English wife and a small brown dog. The wedding soon took place, and it was a delightful occasion. We also met the dog (called Venus, I believe) and found that Richard was going overboard teaching her tricks (leading his mother, Lucille, with her dry wit, to wonder aloud what would become of a

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Murray Gell-Mann is the Robert A. Millikan Professor of Theoretical Physics at the California Institute of Technology.

Thus it would have pleased Richard to know (and perhaps he did know, without my being aware of it) that there are now some indications that his PhD dissertation may have involved a really basic advance in physical theory and not just a formal development. The path integral formulation of quantum mechanics may be more fundamental than the conventional one, in that there is a crucial domain where it may apply and the conventional formulation may fail. That domain is quantum cosmology.

## Seeking Rules for Quantum Gravity

Of all the fields in fundamental physical theory, the gravitational field is picked out as controlling, in Einsteinian fashion, the structure of space-time. This is true even in a unified description of all the fields and all the particles of nature. Today, in superstring theory, we have the first respectable candidate for such a theory, apparently finite in perturbation theory and describing, roughly speaking, an infinite set of local fields, one of which is the gravitational field linked to the metric of space-time. If all the other fields are dropped, the theory becomes an Einsteinian theory of gravitation.

Now the failure of the conventional formulation of quantum mechanics, if it occurs, is connected with the quantum mechanical smearing of space-time that is inevitable in any quantum field theory that includes Einsteinian gravitation.

If there is a dominant background metric for space-time, especially a Minkowskian metric, and one is treating the behavior of small quantum fluctuations about the background (for example, the scattering of gravitons by gravitons), then the deep questions about space-time in quantum mechanics do not come to the fore.

Dick played a major part in working out the rules of quantum gravity in that approximation. It so happened that I was peripherally involved in the story of that research. We first discussed it when I visited Caltech during the Christmas vacation of 1954–55 and he was my host. (I was offered a job within a few days—such things would take longer now.) I had been interested in a similar approach, sidestepping the difficult cosmological issues, and when I found that he had made considerable progress I encouraged him to continue, to calculate one-loop effects and to find out whether quantum gravity was really a divergent theory to that order. He was always very suspicious of unrenormalizability as a criterion for rejecting theories, but he did pursue the re-

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.

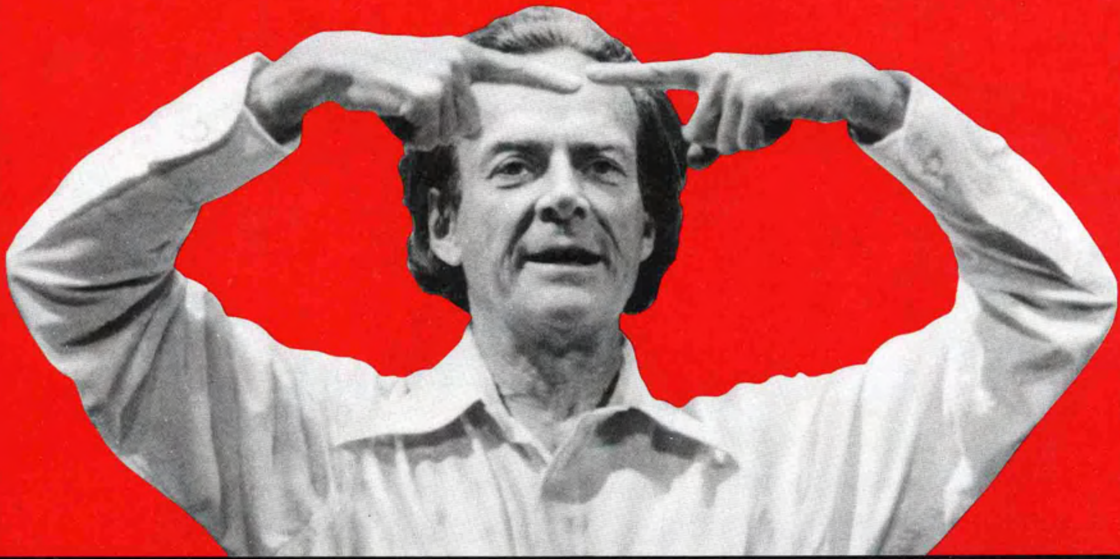
## Turning Things Around

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

# GENIUS



## THE LIFE AND SCIENCE OF RICHARD FEYNMAN

JAMES GLEICK

AUTHOR OF CHAOS

## *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.**

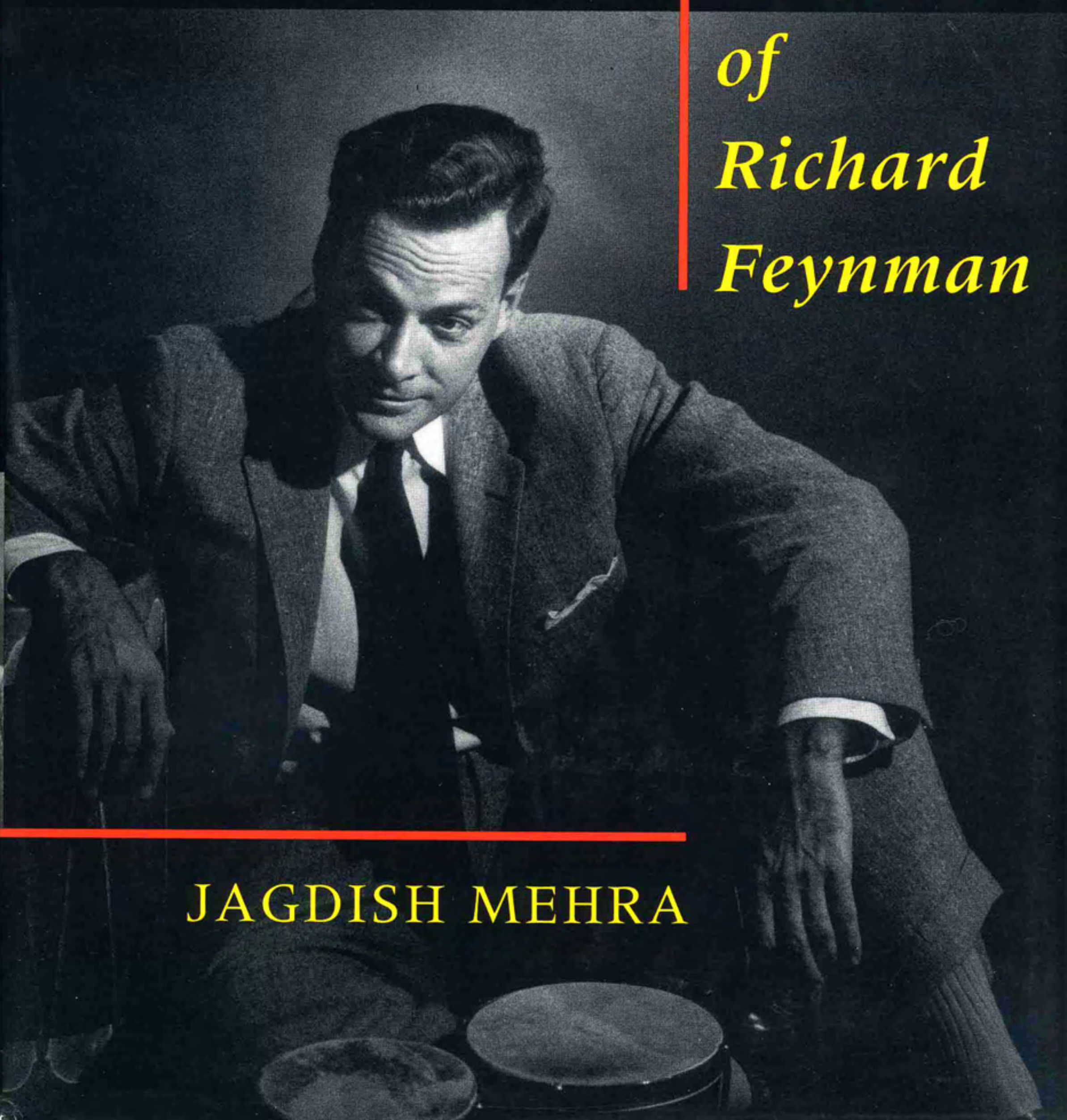
**Are you trying to discover an equation for them?**

**It is very hard.**

**One must try.**

# THE BEAT OF A DIFFERENT DRUM

*The life and science of Richard Feynman*



JAGDISH MEHRA

Playing the bongo drums. (Courtesy: California Institute of Technology, Pasadena, California.)

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 $\varepsilon$ 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

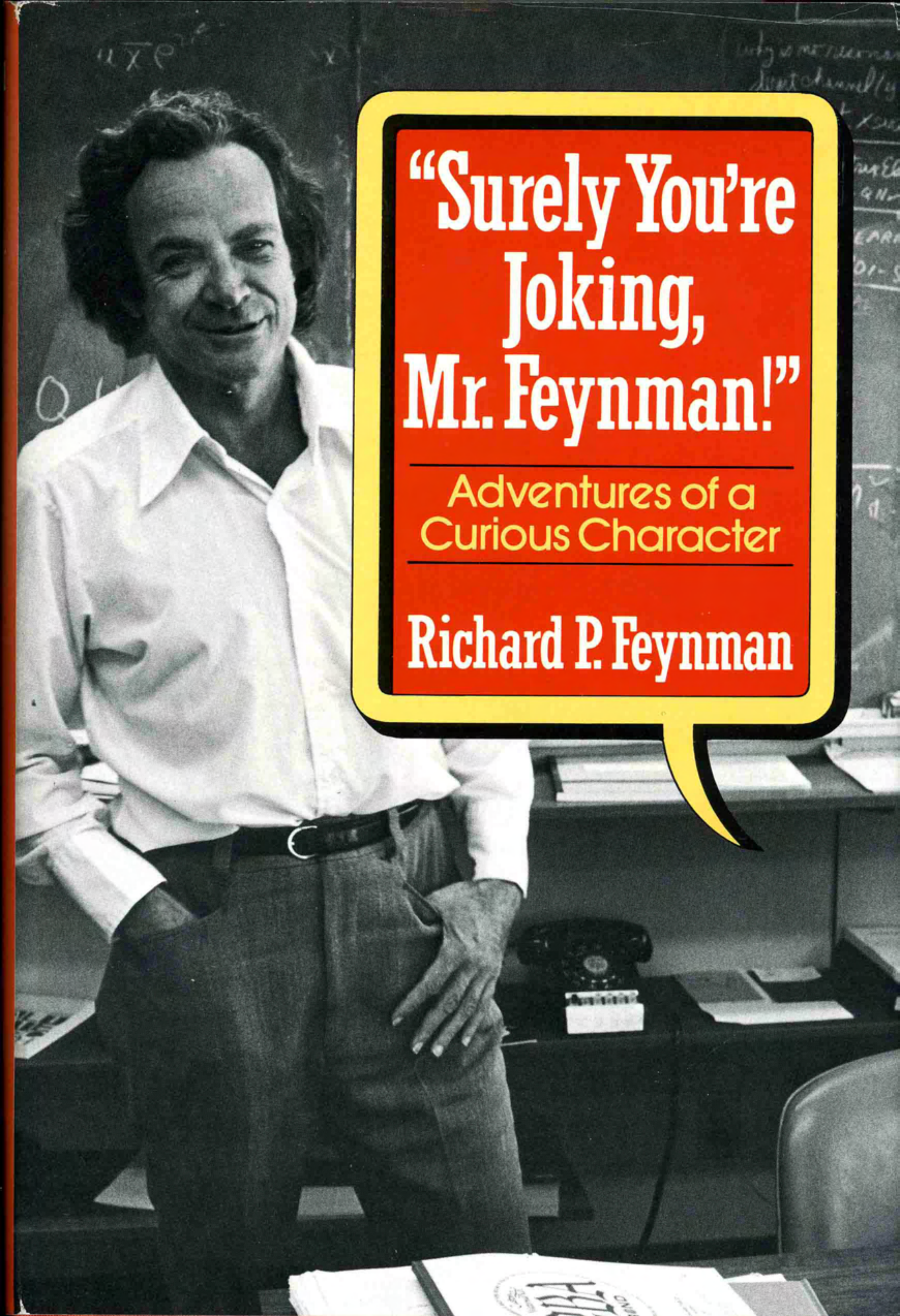
$$\int \cdots \int \exp\left( (i/\hbar) \sum_{i=0}^{N-1} L[(x_{i+1}-x_i)/(t_{i+1}-t_i), x_{i+1}]\,(t_{i+1}-t_i) \right) \frac{dx_N}{A_N} \cdots \frac{dx_1}{A_1}, \tag{6.13}$$

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 $\varepsilon$ ($i=1, 2, \ldots, N$), such that $t_i = t + i\varepsilon$. Then, in the limit when $\varepsilon$ 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:

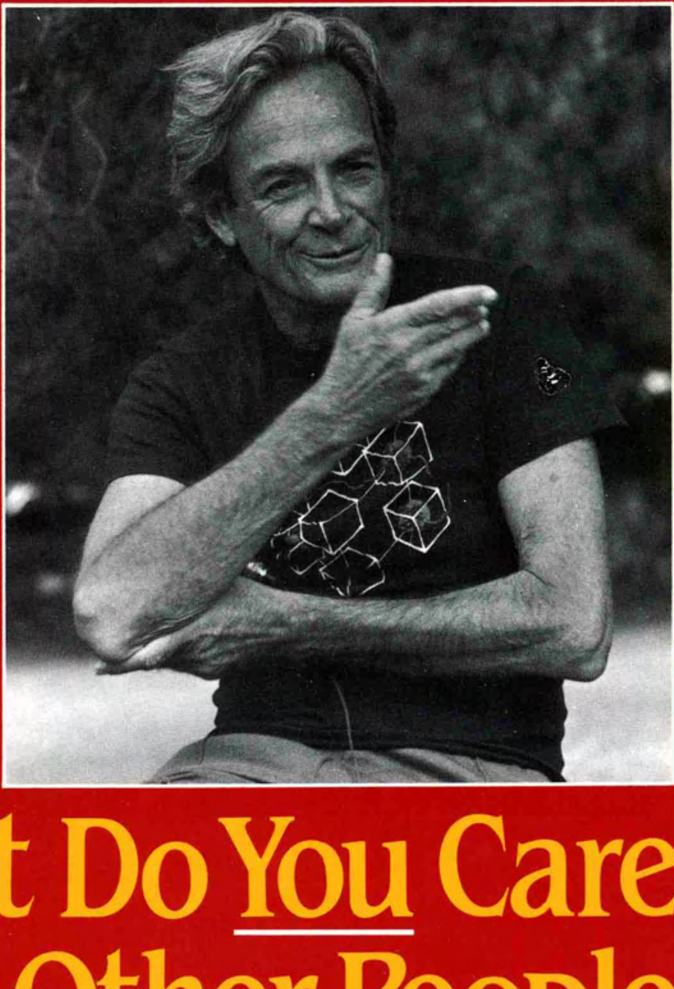
$$A = \lim_{\varepsilon \to 0} \left( \sum_{i=0}^{N-1} L[(x_{i+1}-x_i)/(t_{i+1}-t_i), x_{i+1}]\,(t_{i+1}-t_i) \right). \tag{6.14}$$

Feynman's conclusion was that equation (6.11) 'is equivalent to Schrödinger's differential equation for the wave function $\psi$. 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

# RICHARD P. FEYNMAN

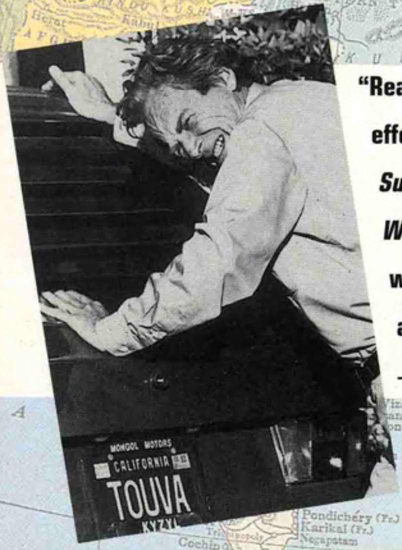


# "What Do You Care What Other People Think?"

## Further Adventures of a Curious Character

# 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

# Feynman's Rainbow

A Search for Beauty in Physics and in Life

Leonard Mlodinow

# Principles of Quantum Mechanics

Ramamurti Shankar

*Yale University*
*New Haven, Connecticut*

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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 $\mathbf{r}_{\mathrm{CM}}$ and vice versa, because $\mathscr{L}(\mathbf{r}, \dot{\mathbf{r}}; \mathbf{r}_{\mathrm{CM}}, \dot{\mathbf{r}}_{\mathrm{CM}}) = \mathscr{L}(\mathbf{r}, \dot{\mathbf{r}}) + \mathscr{L}(\mathbf{r}_{\mathrm{CM}}, \dot{\mathbf{r}}_{\mathrm{CM}})$.

(2) The first fictitious particle is the CM, of mass $M = m_1 + m_2$. Since $\mathbf{r}_{\mathrm{CM}}$ is a cyclic variable, the momentum $\mathbf{p}_{\mathrm{CM}} = M\dot{\mathbf{r}}_{\mathrm{CM}}$ (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 $\dot{\mathbf{r}}_{\mathrm{CM}} = 0$, 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 $x_{\mathrm{cl}}(t)$ 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

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

## 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]

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[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*

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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 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 approxima-



tion). 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 $\sigma$-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?

SCHWINGER
TOMONONAGA

FEYNMAN

DYSON

CANONICAL
OPERATOR
FORMALISM

FEYNMAN
DIAGRAMS
AND THE
FEYNMAN
RULES

PATH
INTEGRAL
FORMALISM

UNITARITY
OBVIOUS

LORENTZ INVARIANCE
IS NOT OBVIOUS

LORENTZ INVARIANCE
OBVIOUS

UNITARITY IS NOT
OBVIOUS

# 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**

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

**S is the classical action**

# 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

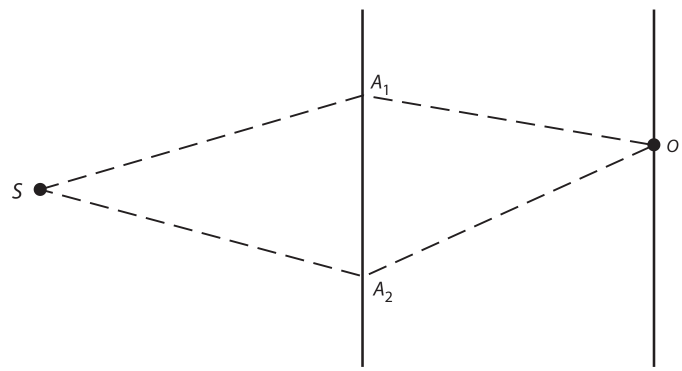


**Figure I.2.1**

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

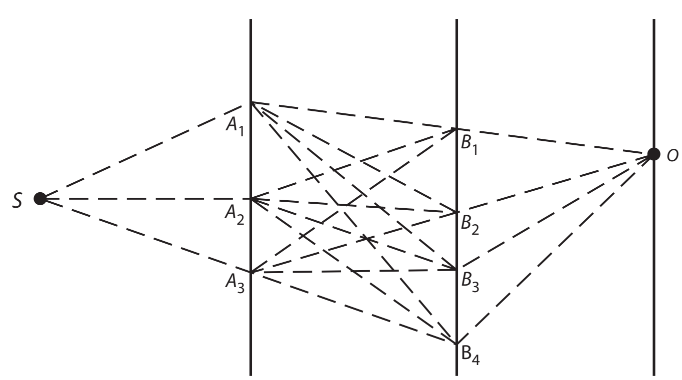


**Figure I.2.2**

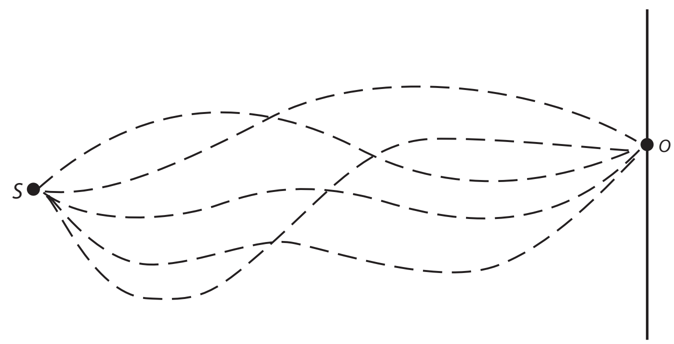


**Figure I.2.3**

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).

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

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.

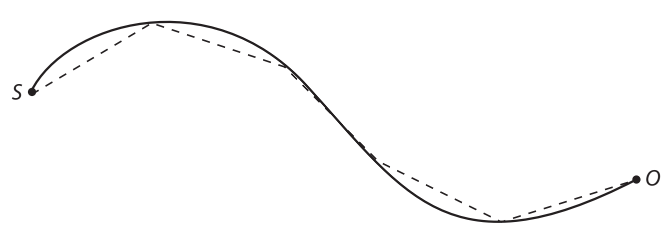


**Figure I.2.4**

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

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

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