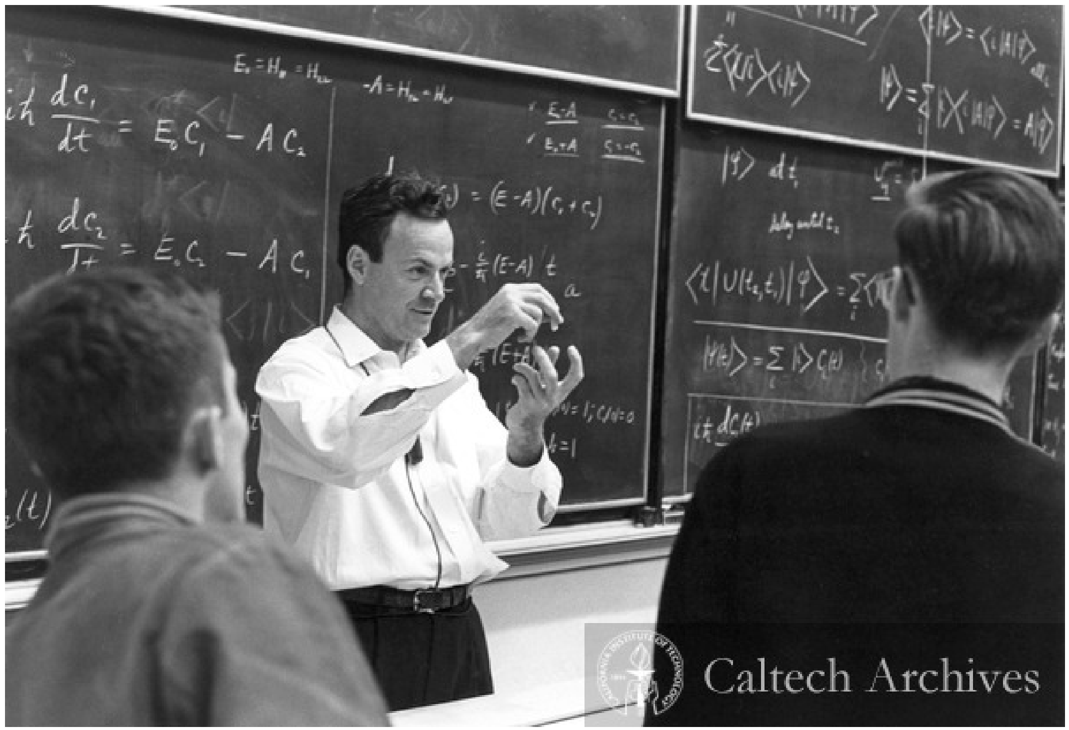
Feynman After 40

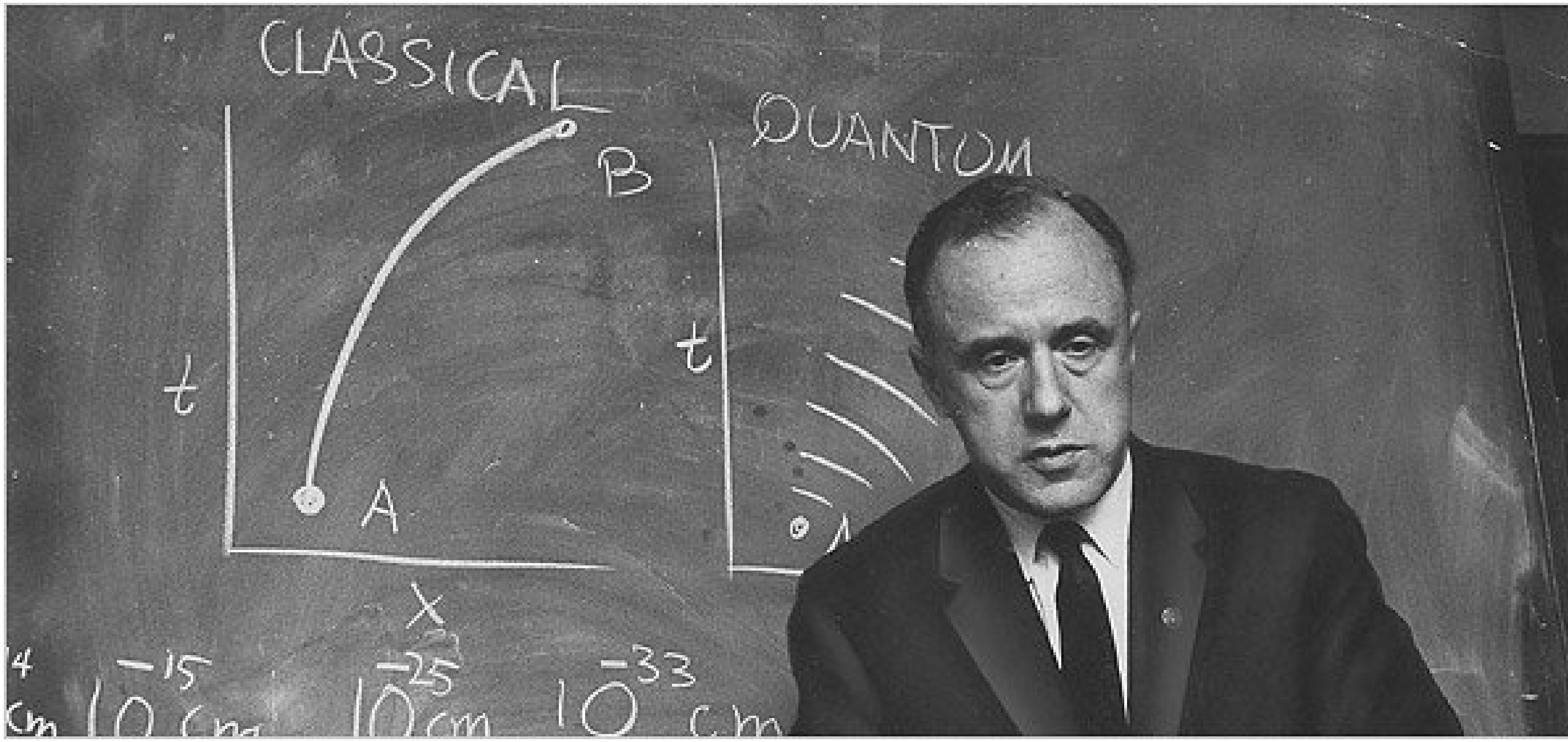


INSTITUTE FOR QUANTUM INFORMATION AND MATTER

John Preskill

APS April Meeting

16 April 2018

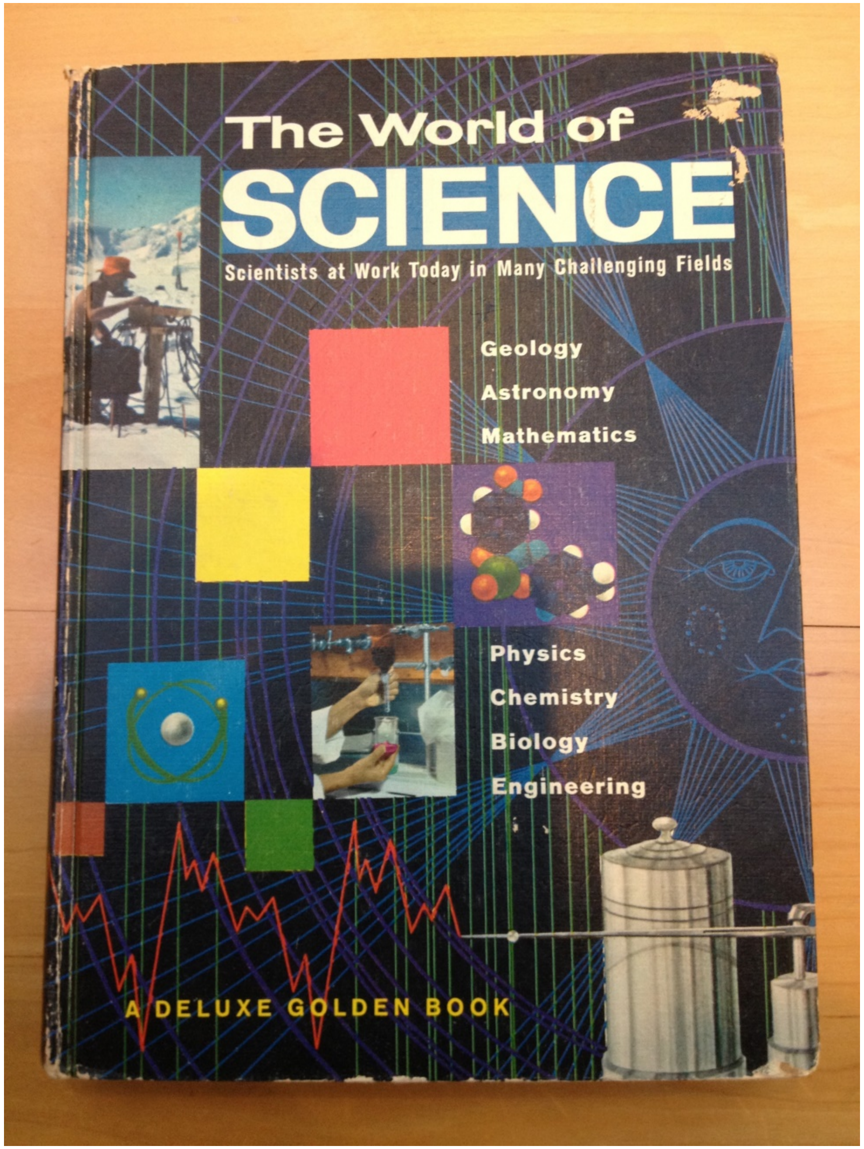


John Wheeler in 1967 [New York Times Photo]

I took a course in Honors Physics from Wheeler when I was a sophomore at Princeton in 1972-73. The very first lecture began by explaining how the least action principle arises from constructive interference in the Feynman sum over histories.

"An electron wants to go from point A to point B. But how does it know what path to follow? It follows all the paths, and adds them together, weighted by e^{iS}, blah, blah."

Though edifying, this explanation did not help us do the homework (problems from Goldstein, *Classical Mechanics*).



THEORETICAL PHYSICS

WHAT IS THE RULE?

THE LITTLE RED WAGON

BACK ABOUT the time your father was a small boy, there was another little boy who had a red wagon. He liked to carry a ball around in that wagon. As he trotted about with it, stopping and starting, he noticed an interesting thing.

Whenever he started up, the ball in his wagon rolled to the back. Whenever he stopped, the ball rolled to the front of the wagon.

The boy went to his father. He showed him what happened to the ball. "What makes it roll to the back when I start up and to the front when I stop?" he asked.

"People call it inertia," said his father. "The rule is that anything at rest tends to remain at rest, and a thing in motion tends to keep on moving in the same direction at the same speed.

"If you look closely, you will see that when you start the wagon, the ball doesn't really roll backward. It just doesn't start up as quickly as your pull makes the wagon move. The back of the wagon catches up with the ball. When you stop the wagon, the ball keeps on moving. It catches up to the front of the wagon. That is the result of inertia."

"But what *is* inertia?" the boy asked.

"That," the father said slowly, "no one knows."

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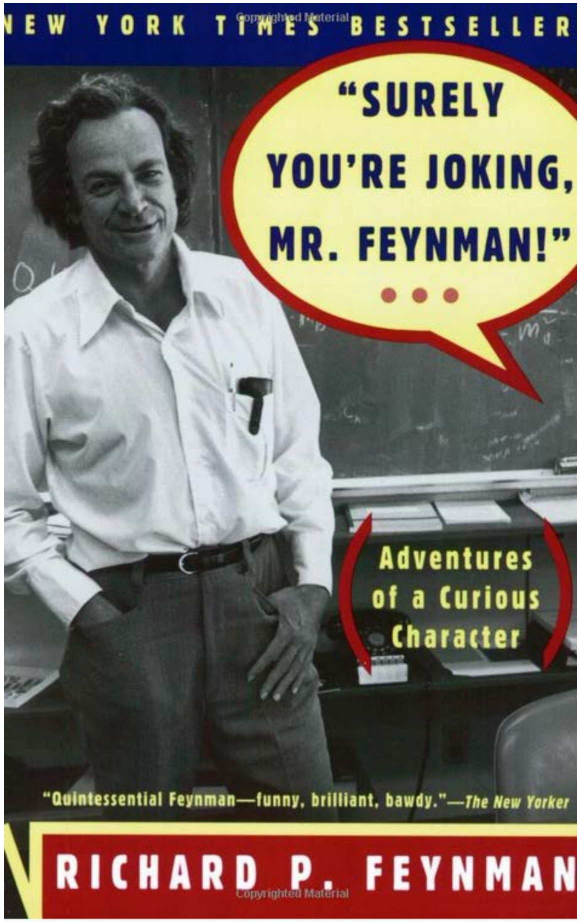
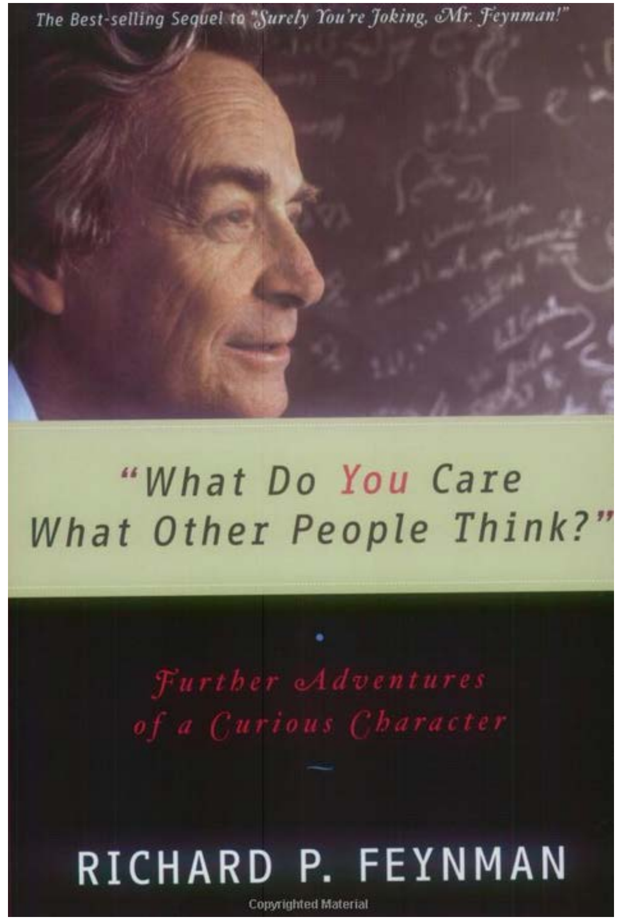
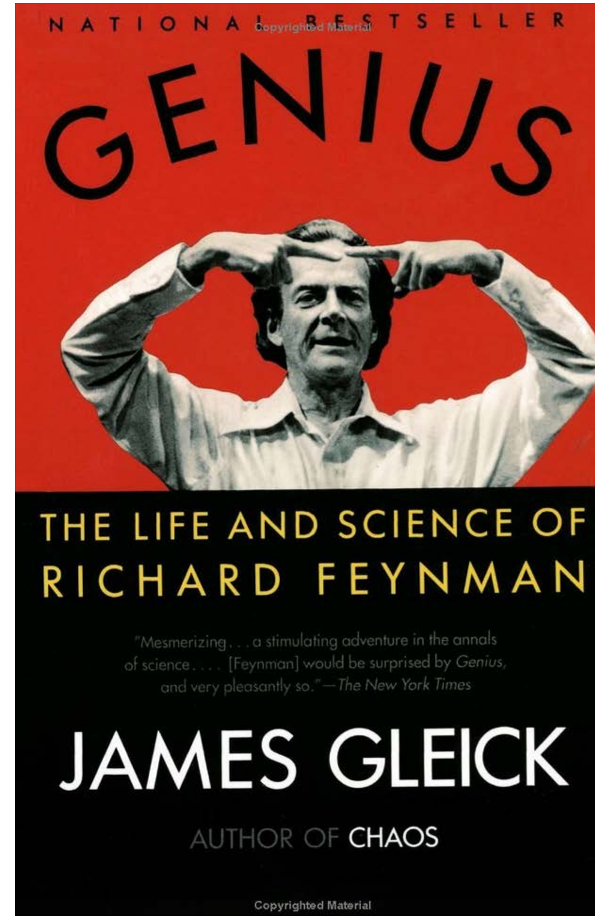
The World of Science, by Jane Werner Watson, published 1958, based on interviews with Caltech faculty. This is my copy, purchased in 1962 when I was 9. The chapter on theoretical physics describes the discovery of parity violation in nuclear beta decay, which I found tremendously exciting. (See *QuantumFrontiers.com*, 2013)

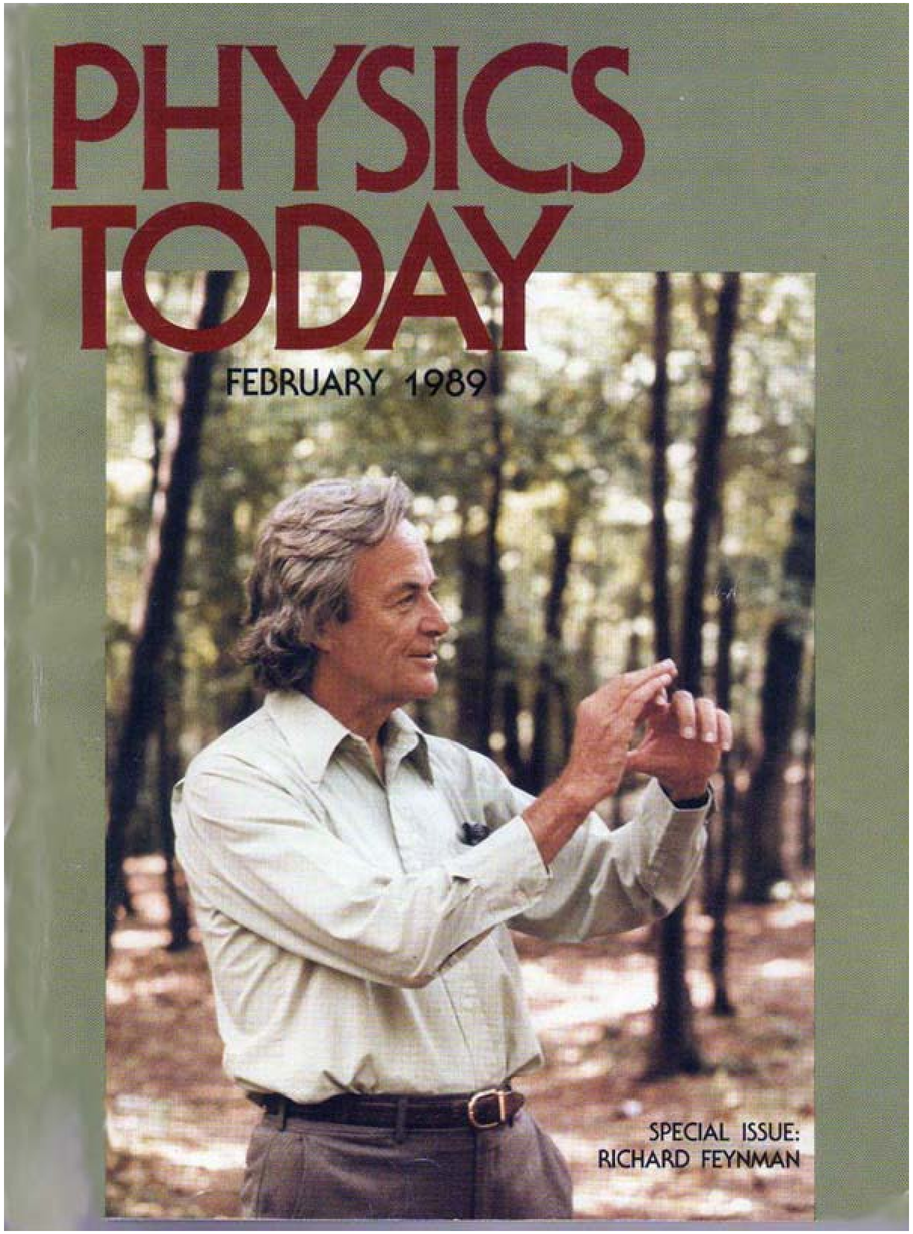


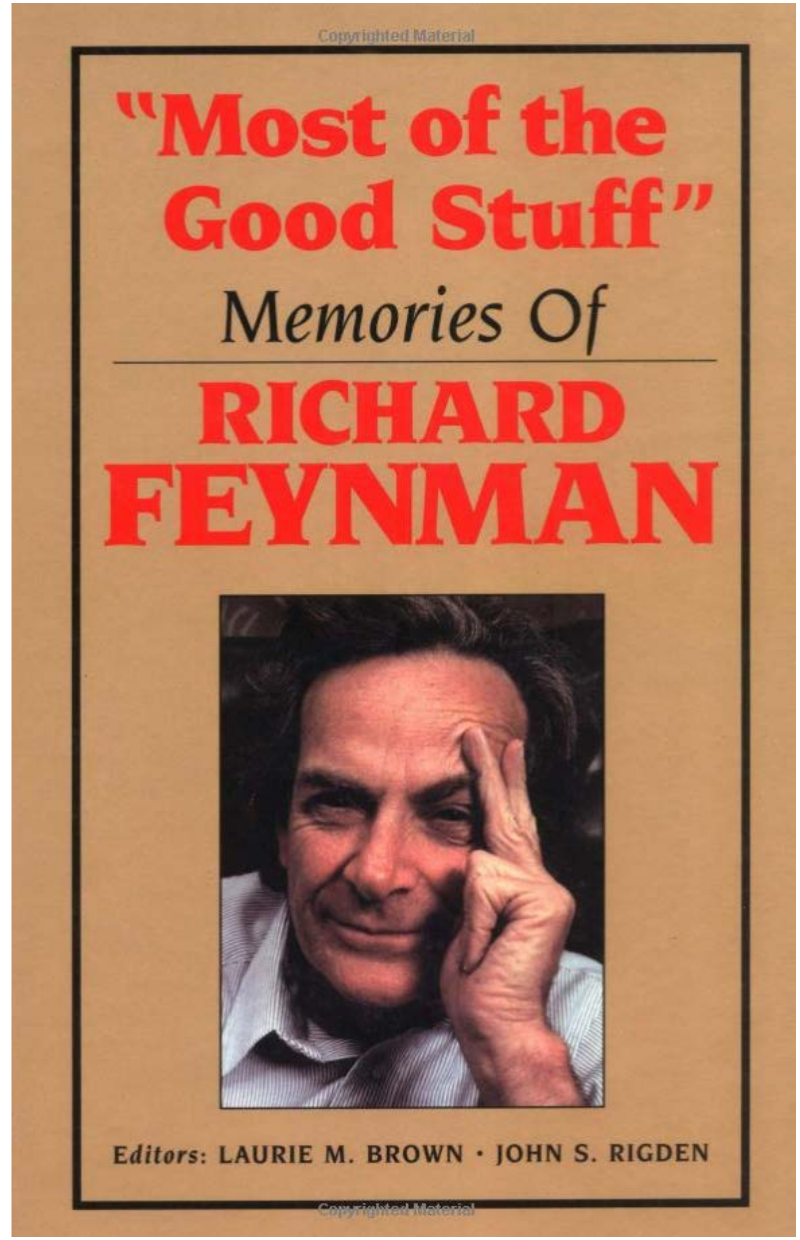




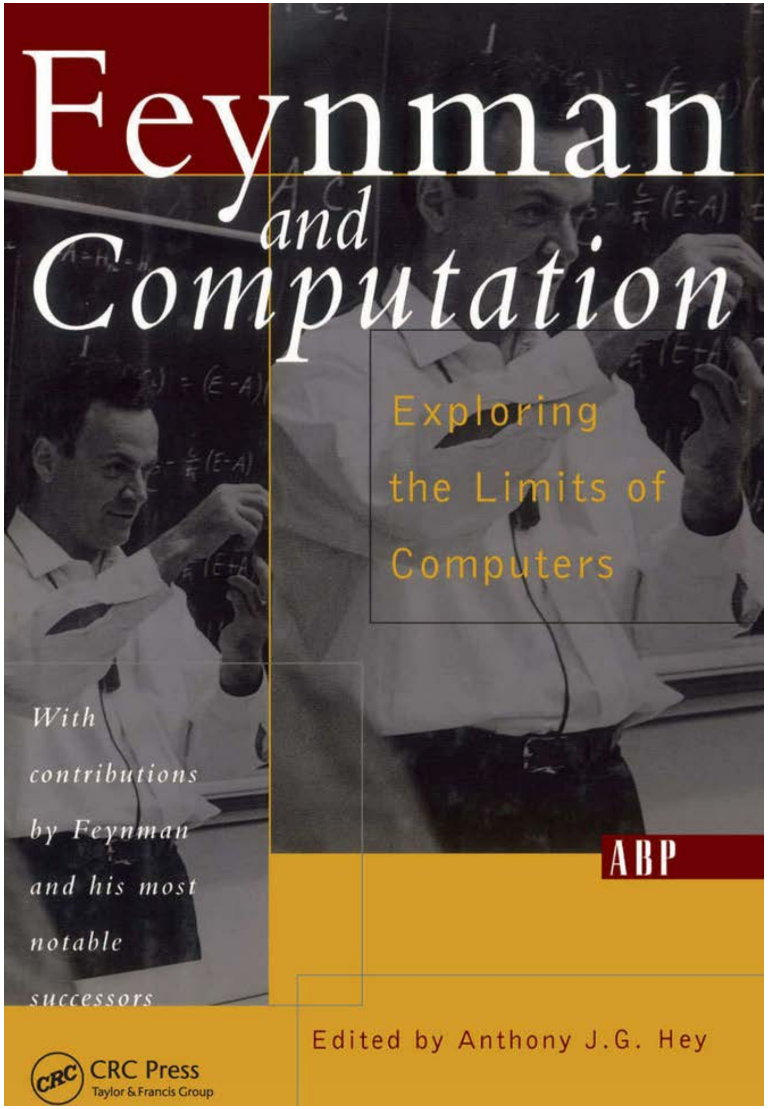
Feynman stories, just the way Feynman told them.

Many interviews
(see end notes for sources).

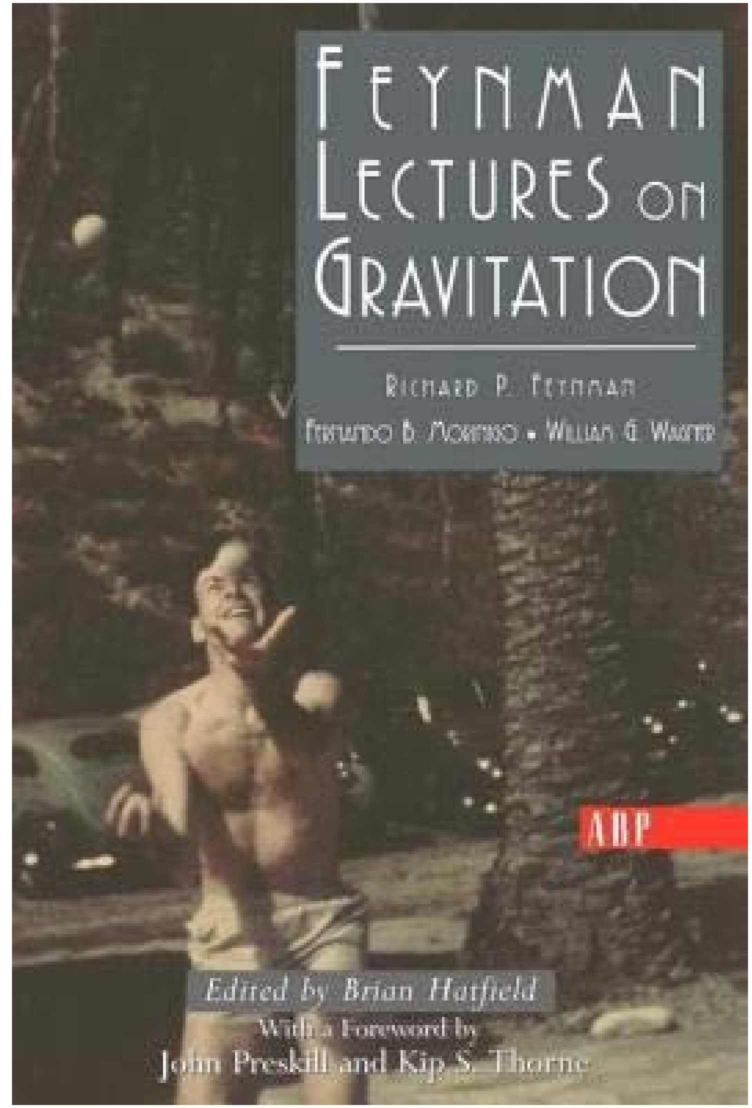




Feynman remembered by Wheeler, Bethe, Dyson, Brown, Schwinger, Gell-Mann, Bjorken, Pines, Goodstein, Cohen, Goldberger, Hillis, Rigden, Telegdi, Joan Feynman.



Feynman remembered by Hey, Hopfield, Mead, Sussman.



Foreword by Thorne and JP, informed by interviews/correspondence with: Bardeen, Deser, DeWitt, Fowler, Frautschi, Hartle, Iben, Kraichnan, Misner, Peebles, Sandage.

Capturing the Wisdom of Feynman

The sole survivor of the three authors who brought *The Feynman Lectures on Physics* to life describes how his hopes of introducing modern physics to undergraduate students were realized beyond his dreams.

Matthew Sands

Physics Today April 2005

My arrival at Caltech in August 1983

Someone is drumming on the wall while walking down the hallway. I realize it must be Feynman, and stumble out of my office, hoping to say hello.

Helen Tuck: Dr. Feynman, this is Dr. Preskill, our new faculty member!

RPF: What group?

JP: Um … particle theory.

RPF: People who say they do particle theory do many different things. What do you do?

JP: Blah, Blah …. Early Universe … Blah, Blah.
And lately, I have been working, without much success, on models in which quarks and leptons are composite.

RPF: [*pause*]. Well, your lack of success has been shared by many others. [*RPF pivots, enters his office*.]

Steve Weinberg, Times Higher Education 1998

"Years ago, when I was an assistant professor of physics at Berkeley [1960-66], I used to be invited down to Caltech about once a year to give a talk. It was usually the low point of my year. In the audience at Caltech were two leaders of modern physics, Murray Gell-Mann and Richard Feynman, who interrupted with frequent questions, ruthlessly probing to see if I really knew what I was talking about and had anything new to say. Of the two, Feynman was the more frightening. Gell-Mann was mostly interested in finding out whether there was anything in my talk that he should know about, so he was no problem if I did have anything worth while to say. Feynman was having fun."

I gave my first seminar at Caltech in 1981

This was 15-20 years later than Weinberg's experience, and the treatment I received was not nearly as rough. (Coincidentally, part of what I talked about was joint work with Weinberg on chiral symmetry breaking.)

By then, one could play Feynman and Gell-Mann against one another. When Dick attacked, Murray defended me. And when Murray raised an objection, Dick would be on my side.

Gerard 't Hooft was also there, which made it even more interesting.

It was an eventful seminar, but not "the low point of my year."

Talking Physics with Feynman

In the mid-1980s, Feynman was very interested in two topics I happened to know something about: Quark confinement and chiral anomalies.

We had frequent discussions about both. I would sometimes impress Feynman with an idea or calculation I had learned from reading the literature. I would tell him the source, but RPF was more interested in the ideas than in the references.

Once I overheard Feynman (he had a booming voice) tell Helen as he returned to his office after our discussion: "He's like an encyclopedia. No, he's *better* than the encyclopedia!"

Made my day.

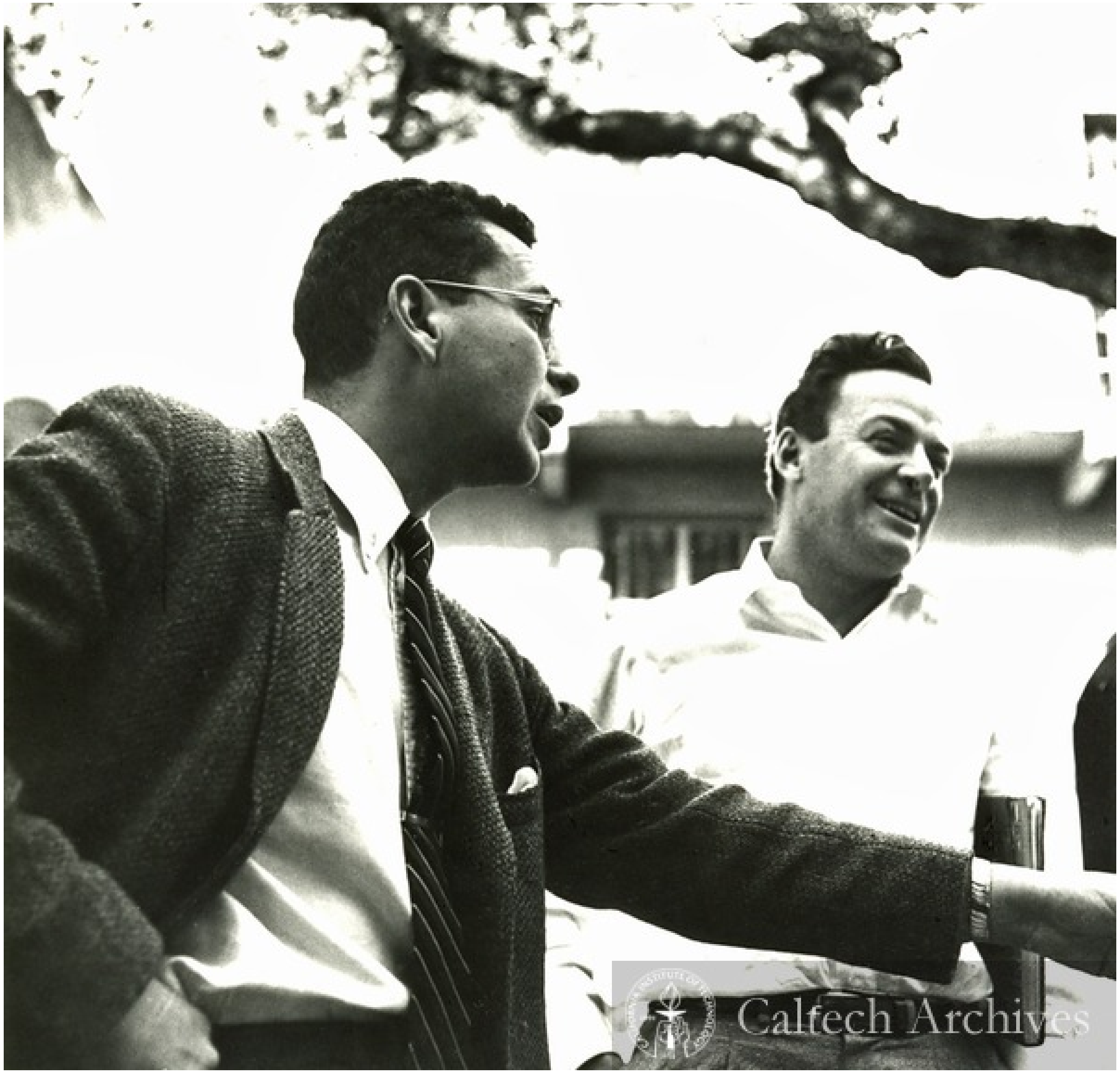
But sometimes I wondered whether Feynman knew my name, as he sometimes seemed to confuse "Preskill" and "Peskin" --- with whom, I presumed, he had discussed similar things.

Feynman and Gell-Mann

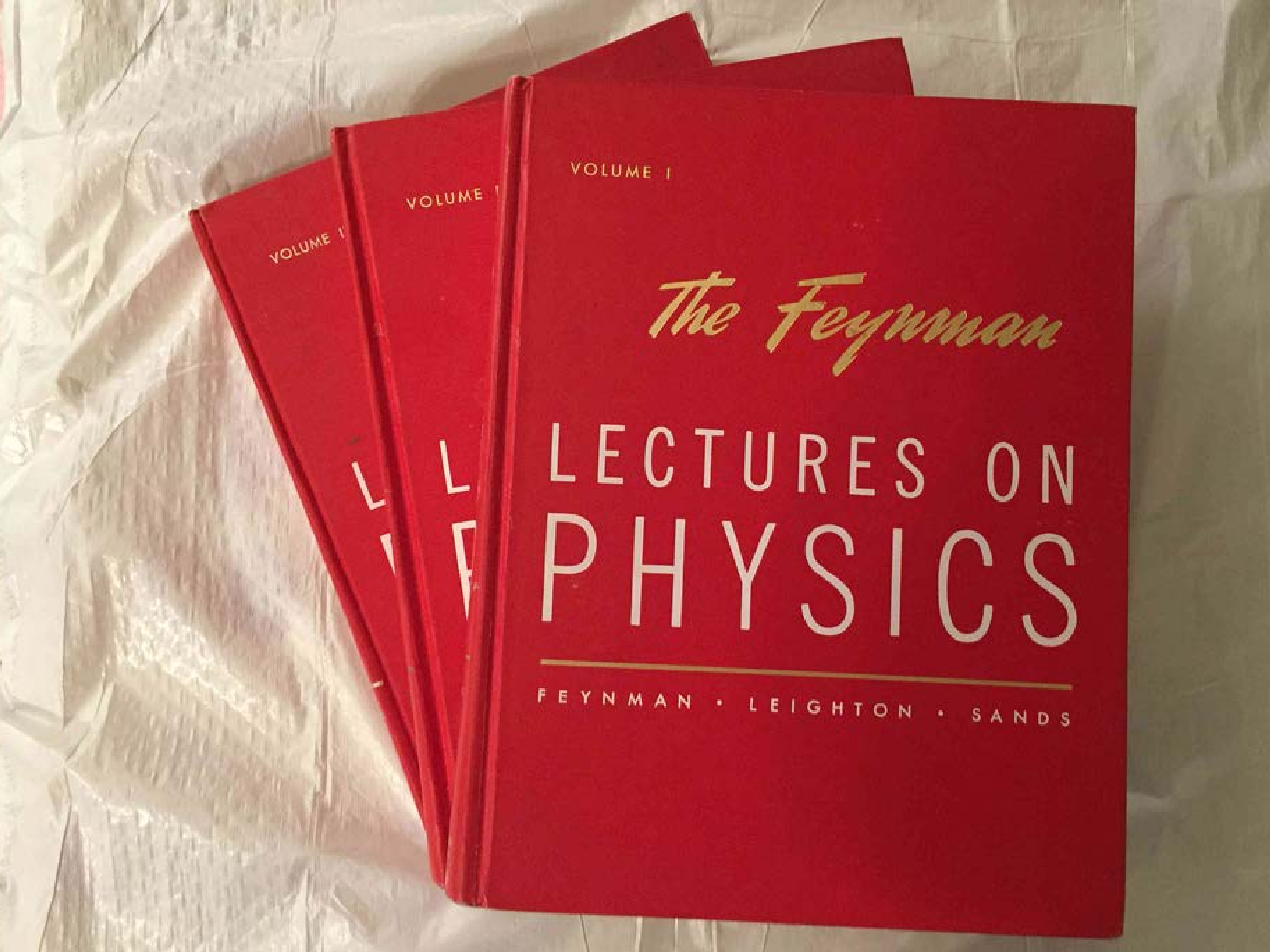
Feynman and Gell-Mann had once been quite close, but by the time I arrived their relationship was visibly tense. Around 1986 I asked both of them what had gone wrong.

I expected to hear about the 1958 V-A paper they co-authored, because we all knew that Murray was upset about how it had been described in *Surely You're Joking* (then recently published).

But both RPF and MGM said they had gotten along well until around 1969, when Feynman was working on the parton model. MGM referred sarcastically to "Feynman's put-ons," resenting that Feynman refused to call them "quarks." RPF complained that MGM had ridiculed the idea that quarks would behave as nearly free particles inside a proton. What started out as a scientific disagreement had become increasingly personal and hostile.



Gell-Mann and Feynman, 1959 (Caltech Archives)


VOLUME I
The Feynman
LECTURES ON
PHYSICS
FEYNMAN · LEIGHTON · SANDS

The Feynman Lectures on Physics, 1961-63

Matt Sands (Physics Today 2005):

After discussions with a few colleagues, I proposed to Robert Bacher, then head of the physics department, that we start a program to reform the introductory course. His initial response was not very encouraging … Bacher appointed a small task force to lead the program: Robert Leighton as chairman, H. Victor Neher, and me …

It soon became clear that a common ground would not be easily found. I viewed Leighton's approach as a rehash of physics course content that had been in vogue for 60 years … I didn't see how Leighton and I could ever agree on a syllabus … Then one day I had an inspiration: Why not ask Feynman to give the lectures for the course.

[Feynman] was not immediately enthusiastic … Finally he asked me: "Has there ever been a great physicist who has presented a course to freshmen?" I told him that I didn't think so. His response: "I'll do it."

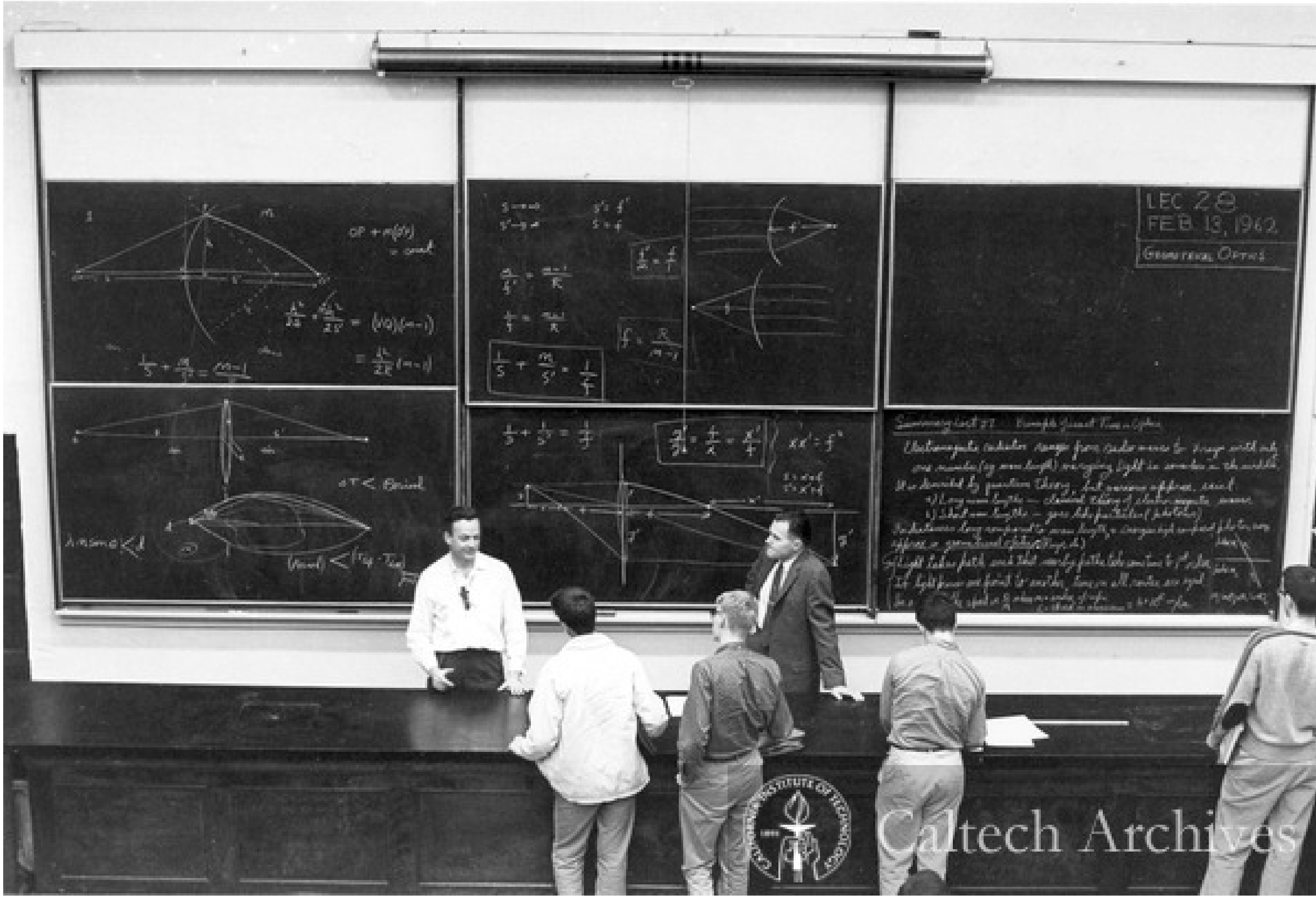
The Feynman Lectures on Physics, 1961-63

Matt Sands (Physics Today 2005):

Witnessing the lectures: [Feynman] would take out of his shirt pocket one or two small pieces of paper --- perhaps 5 X 9 inches --- unfold them, and smooth them out at the center of the lecture bench in the front of the lecture hall. Those were his notes for his lecture, although he rarely referred to them … And his timing was most impressive. Only very rarely would he finish more than a fraction of a minute before or after the end of the hour. Even his use of the chalkboards at the front of the lecture hall appeared to be carefully choreographed.

Authorship: [Feynman] had a violent reaction to the proposed authors: "Why should your names be there? You were only doing the work of a stenographer!" I disagreed and pointed out that, without the efforts of Leighton and me, the lectures would never have come to be a book … I returned to the discussion some days later and we came up with a compromise: *The Feynman Lectures on Physics* by Feynman, Leighton, and Sands.

The Preface: I … wondered whether he would like to provide a preface. The idea interested him, but he was short of time. I suggested that he dictate his preface into the Dictaphone on my desk. So, still depressed over the average grade [65%], he recorded the first draft of "Feynman's Preface" which you will find in each volume of the *Lectures*. In it he says, "I don't think I did very well by the students." I have often regretted that I had arranged for him to make a preface in this way, because I do not think it was a very considered judgment.



Feynman and Robert Leighton with students, 1962 (Caltech Archives)

Students respond to Richard Feynman (far right) during one of his lectures in 1963. (Photo from Realites/Courtesy of Caltech Archives.)

Women undergrads were not admitted to Caltech until 1970.

The Feynman Lectures on Physics, 1961-63

Remembering Richard P. Feynman - Reunion Weekend - 5/14/2015
https://www.youtube.com/watch?v=S0Q80twy11Q

Robbie Vogt (Talk to the Caltech class of '65 at their 50th reunion, 2015. Robbie became the "Feynman Lecturer" in fall of 1963.)

"It is unfortunate that Feynman came to kind of a pessimistic conclusion that he wrote in his preface … There are other people who say the Feynman books are not suitable as textbooks. They are wrong. If you do it right, you let them have Feynman, and then you help them to digest it."

"I loved Feynman's way of introducing quantum mechanics. Now, I had been taught quantum mechanics by no one less than Fermi. And that was good. But Feynman was better. Because, he said once, he only believed he understood physics if he could teach it to freshmen. And he tried to teach them the beauty of quantum mechanics even if you were a biology major, or a humanist, or anything like that."

Vogt , on why the Feynman course was eventually abandoned at Caltech (email 2018):
"There had been rumblings for a number of years that the Feynman Lectures were too hard, and not suitable as a textbook. I have no quarrel with the students complaining that the Feynman course was hard; it should not be a cake-walk, and I had set them a high bar; that's what made them Techers."

The Feynman Lectures on Physics

FLP has sold over 1.5 million copies in English, and many more copies in foreign language editions.

Mike Gottlieb and Rudi Pfeiffer converted FLP to LaTeX (correcting hundreds of errata), and all figures were redrawn at scalable vector graphics, paving the way for publication of the "New Millenium Edition" in 2011.

Since 2013, a free html version has been available online at http://www.feynmanlectures.caltech.edu/

Mike Gottlieb and friend at Caltech (2013)

(QuantumFrontiers.com, 2013): "[Mike Gottlieb] would love to have a role in solving the great open problems in physics, in particular the problem of reconciling general relativity with quantum mechanics, but feels it is beyond his ability to solve those problems himself. Instead, Mike feels he can best facilitate progress in physics by inspiring other very talented young people to become physicists and work on the most important problems. In Mike's view, there is no better way of inspiring students to pursue physics than broadening access to the Feynman Lectures on Physics!"

Feynman's "Physics X" class for Caltech freshman

David Goodstein (Physics Today 1989): "For many years – at least 17, but there is no written record to check ."

Paul Steinhardt (email 2018):
"There were rules that Dick imposed from the outset: No questions about coursework. No questions about so-and-so's paper. No questions to explain what so-and-so's equation meant or so-and-so's theorem meant. Questions had to be about trying to understand something. And they could be about literally ANYTHING. And the discussion could go ANYWHERE. I remember asking him a question about the color of shadows, and they brought us to shadows of all kinds, earthshine, etc. On and on. Part of the fun is that, every now and then, a question would spark him to tell us one of the famous stories eventually recounted in his books, such as all about safe-cracking techniques. Actually, much more than appeared in his books.

"The important thing for me was that everything was considered interesting -- every corner of science. And that was a huge influence on my outlook on physics and on my career. "

Feynman teaching Physics X, 1976 (Caltech Archives)

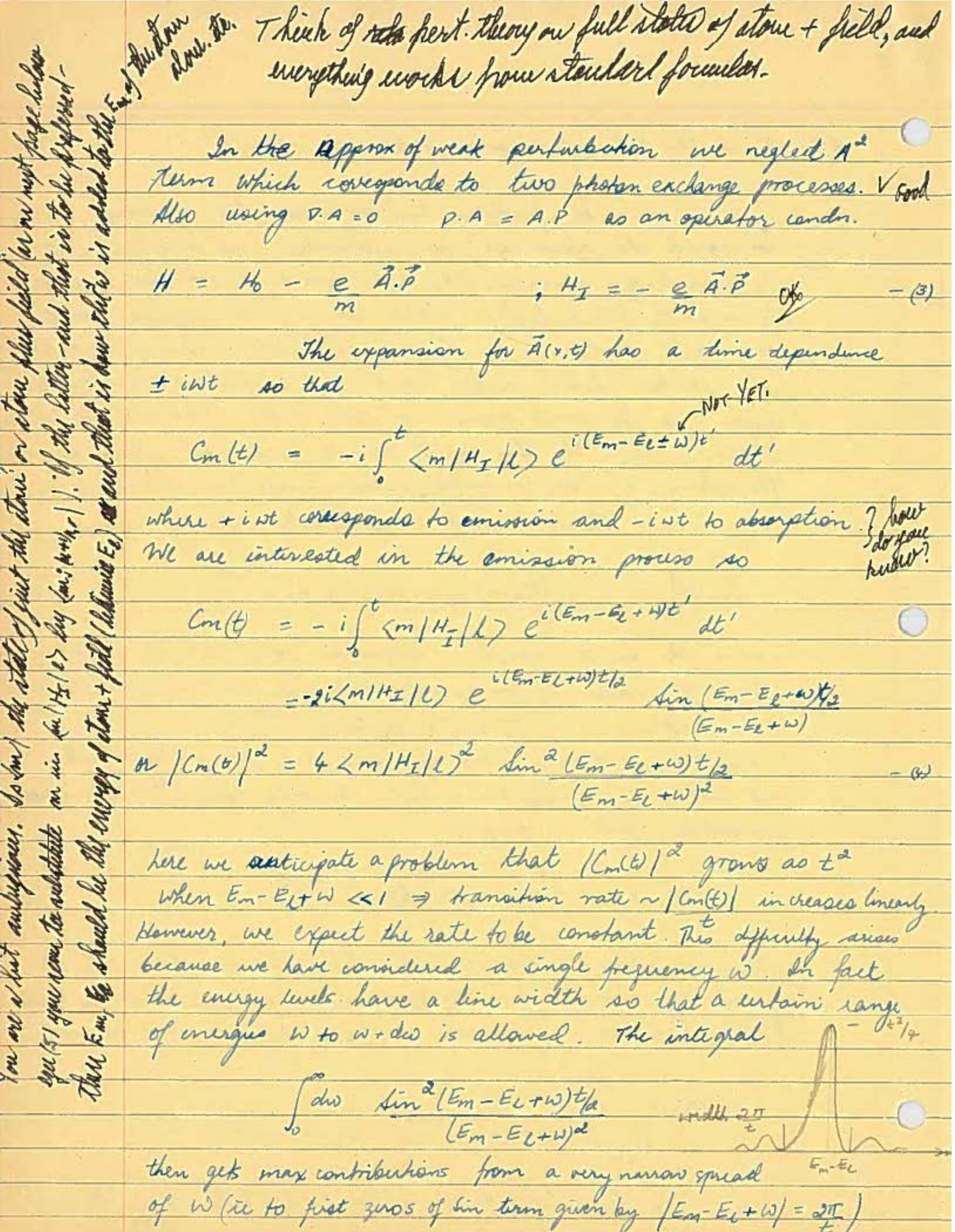
Feynman recalled by a Caltech undergrad

Michael Turner (email 2018):

I was an UG at Caltech from Fall 1967 to June 1971. Everyone took the Feynman course — for 2 years. It was inspiring to all but frustrating to many when it came to doing problems … I like to say that all the physics I learned was from the Feynman lectures — an exaggeration to be sure, but it wouldn't be an exaggeration to say that those lectures taught me how to think about physics.

My sophomore year I did a one-on-one, hour/week tutorial with Feynman … It was an adventure … By a series of games he had me play on my own (no books). The moment I most remember was when Feynman told me he was envious of my ignorance! I believe he meant knowing too much makes you less open to new ideas.

In those days Feynman was everyone's hero: the students and the faculty. I am not sure who worshipped him more.


Think of ~~rela~~ pert. theory on full states of atom + field, and everything works from standard formulas.

You are a bit ambiguous. Is ⟨m| the state of just the atom or atom plus field (as in next page below eqn (5) you seem to substitute m in ⟨m|H_I|e⟩ by ⟨m; k, n_k|). If the latter — and that is to be preferred — then E_m should be the energy of atom + field (otherwise E_l) and that is how the ω is added to the E_m of the atom alone etc.

In the approx of weak perturbation we neglect A^2 terms which corresponds to two photon exchange processes. ✓ good

Also using $\nabla \cdot A = 0$ $\quad p \cdot A = A \cdot P$ as an operator condn.

$$H = H_0 - \frac{e}{m}\vec{A}\cdot\vec{P} \quad ; \quad H_I = -\frac{e}{m}\vec{A}\cdot\vec{P} \qquad \text{OK} \qquad -(3)$$

The expansion for $\vec{A}(r,t)$ has a time dependence $\pm i\omega t$ so that

$$C_m(t) = -i\int_0^t \langle m|H_I|l\rangle\, e^{i(E_m - E_l \pm \omega)t'}\, dt'$$

NOT YET.

where $+i\omega t$ corresponds to emission and $-i\omega t$ to absorption ? how do you know?

We are interested in the emission process so

$$C_m(t) = -i\int_0^t \langle m|H_I|l\rangle\, e^{i(E_m - E_l + \omega)t'}\, dt'$$

$$= -2i\langle m|H_I|l\rangle\, e^{i(E_m-E_l+\omega)t/2}\, \frac{\sin(E_m-E_l+\omega)t/2}{(E_m-E_l+\omega)}$$

or

$$|C_m(t)|^2 = 4\langle m|H_I|l\rangle^2 \frac{\sin^2(E_m-E_l+\omega)t/2}{(E_m-E_l+\omega)^2} \qquad -(4)$$

here we anticipate a problem that $|C_m(t)|^2$ grows as t^2 when $E_m - E_l + \omega \ll 1 \Rightarrow$ transition rate $\sim \frac{|C_m(t)|}{t}$ increases linearly. However, we expect the rate to be constant. This difficulty arises because we have considered a single frequency ω. In fact the energy levels have a line width so that a certain range of energies ω to ω + dω is allowed. The integral

$$\int_0^\infty d\omega\, \frac{\sin^2(E_m-E_l+\omega)t/2}{(E_m-E_l+\omega)^2}$$

$t^2/4$

width $\frac{2\pi}{t}$

$E_m - E_l$

then gets max contributions from a very narrow spread of ω (ie to first zeros of sin term given by $|E_m - E_l + \omega| = \frac{2\pi}{t}$)

A page from Rajan Gupta's homework for Feynman's Field Theory course (1977), graded by Feynman.

Gupta (then a grad student): Feynman went through each homework line by line. Even when the calculation was right, he did not shy away from pointing out it was uninspired. "What have you learned? When can you apply this?" He wanted clarity, depth, and connections at each step.

Feynman writes: "You are a bit ambiguous. Is ⟨m| the state of just the atom, or atom plus field? … Think of perturbation theory on full states of atom + field, and everything works from standard formulas."

Sidney Coleman on Feynman [quoted in *Genius* by James Gleick]

“There are lots of people who are too original for their own good, and had Feynman not been as smart as he was, I think he would have been too original for his own good.

There was always an element of showboating in his character. He was like the guy that climbs Mt. Blanc barefoot just to show it could be done. A lot of things he did were to show, you didn’t have to do it that way, you can do it this other way. And the other way, in fact, was not as good as the first way, but it showed he was different.

I suspect that Einstein had some of the same character. I’m sure Dick thought of that as a virtue, as noble. I don’t think it’s so. I think it’s kidding yourself. Those other guys are not all a collection of yo-yos. Sometimes it would be better to take the recent machinery they have built and not try to rebuild it, like reinventing the wheel.

I know people who are in fact very original and not cranky but have not done as good physics as they could have done because they were more concerned at a certain juncture with being original than with being right. Dick could get away with a lot because he was so goddamn smart. He really could climb Mont Blanc barefoot.”

There's Plenty of Room at the Bottom

Transcript of a talk at the APS Annual Meeting, Caltech 1959
(Google Scholar ~ 3200 citations)

Future computers: If they had millions of times as many elements, they could make judgements … In many ways, they would have new qualitative features … There is nothing that I can see in the physical laws that says computer elements cannot be made enormously smaller than they are now. In fact, there may be certain advantages.

What biologists want from physicists: "What you should do in order for *us* to make more rapid progress is to make the electron microscope 100 times better."

Medicine (attributed to Al Hibbs): It would be interesting in surgery if you could swallow the surgeon.

Atom-by-atom assembly: All our devices can be mass produced so that they are absolutely perfect copies of one another … The principles of physics, as far as I can see, do not speak against the possibility of maneuvering things atom by atom.

Simulating Physics with Computers

Transcript of a talk at the Conference on the Physics of Computation, MIT 1981

Google Scholar ~ 6300 citations

The goal: The rule of simulation that I would like to have is that the number of computer elements required to simulate a large physical system is only to be proportional to the space-time volume of the physical system.

Complexity: Now I explicitly go to the question of how we can simulate with a computer … the quantum mechanical effects … But the full description of quantum mechanics for a large system with R particles is given by a function which we call the amplitude to find the particles at $x_1, \ldots, x_R$, and therefore because it has too many variables, it *cannot be simulated* with a normal computer.

Quantum computing: Can you do it with a new kind of computer --- a quantum computer? Now it turns out, as far as I can tell, that you can simulate this with a quantum system, with quantum computer elements. It's not a Turing machine, but a machine of a different kind.

Universality: I present that as another interesting problem: To work out the classes of different kinds of quantum mechanical systems which are really intersimulatable --- which are equivalent --- as has been done in the case of classical computers.

Simulating Physics with Computers

Transcript of a talk at the Conference on the Physics of Computation, MIT 1981

Google Scholar ~ 6300 citations

About half of the talk is taken up by the section: Can Quantum Systems be Probabilistically Simulated by a Classical Computer?

"If you take the computer to be the classical kind I've described so far (not the quantum kind described in the last section) and there're no changes in any laws, and there's no hocus-pocus, the answer is certainly, "No!" This is called the hidden variable problem: It is impossible to represent the results of quantum mechanics with a classical universal device."

There follows a lucid discussion of Bell inequalities and the experimental evidence that they are violated. There are no references, and Bell is never mentioned!

Feynman also speculates on how the conclusion might somehow be avoided, and mentions in particular the "free-will loophole":

"We have an illusion that we can do any experiment that we want. We all, however, come from the same universe, have evolved with it, and don't really have any "real" freedom … All I was doing was hoping that the computer-type of thinking would give us some new ideas, if any are really needed. I don't know, maybe physics is absolutely okay the way it is."

Simulating Physics with Computers

Transcript of a talk at the Conference on the Physics of Computation, MIT 1981

Google Scholar ~ 6300 citations

"Might I say immediately, so that you know where I intend to go, that we always have had (secret, secret, close the doors!) we always have had a great difficulty in understanding the world view that quantum mechanics represents. At least I do, because I'm an old enough man that I haven't got to the point that this stuff is obvious to me. Okay, I still get nervous with it. And therefore, some of the younger students … you know how it always is, every new idea, it takes a generation or two until it becomes obvious that there's no real problem. It has not yet become obvious to me that there's no real problem. I cannot define the real problem, therefore I suspect there's no real problem, but I'm not sure there's no real problem. So that's why I like to investigate things. Can I learn anything from asking this question about computers --- about this may or may not be mystery as to what the world view of quantum mechanics is?"

Note: When pressed, Feynman would support the Everett viewpoint, that all phenomena (including measurement) are encompassed by unitary evolution alone. According to Gell-Mann, both he and Feynman already held this view by the early 1960s, without being aware of Everett's work. However, in 1981 Feynman says of the many-worlds picture: "It's possible, but I'm not very happy with it."

The talk concludes: “Nature isn’t classical, dammit, and if you want to make a simulation of Nature, you’d better make it quantum mechanical, and by golly it’s a wonderful problem because it doesn’t look so easy.”

Strangely, though, John Hopfield, who co-taught with Feynman in 1983 a course on the Physics of Computation, has this to say (in *Feynman and Computation* 1999):

“While he is often given credit for helping originate ideas of quantum computation, my recollection of the many conversations with him on the subject contain no notion of his that quantum computers could in some sense of N scaling be better than classical computing machines. He only emphasized that the physical scale and speed of computers were not limited by the classical world, since conceptually they could be built of reversible components at the atomic level. The insight that quantum computers were really different came only later, and to others, not to Feynman.”

From the 1981 talk, we know this is clearly wrong. Yet it is true that Feynman does not mention the quantum computer’s advantage for quantum simulation in his 1984 CLEO/IQEC talk, or in *The Feynman Lectures on Computation*. Why not?

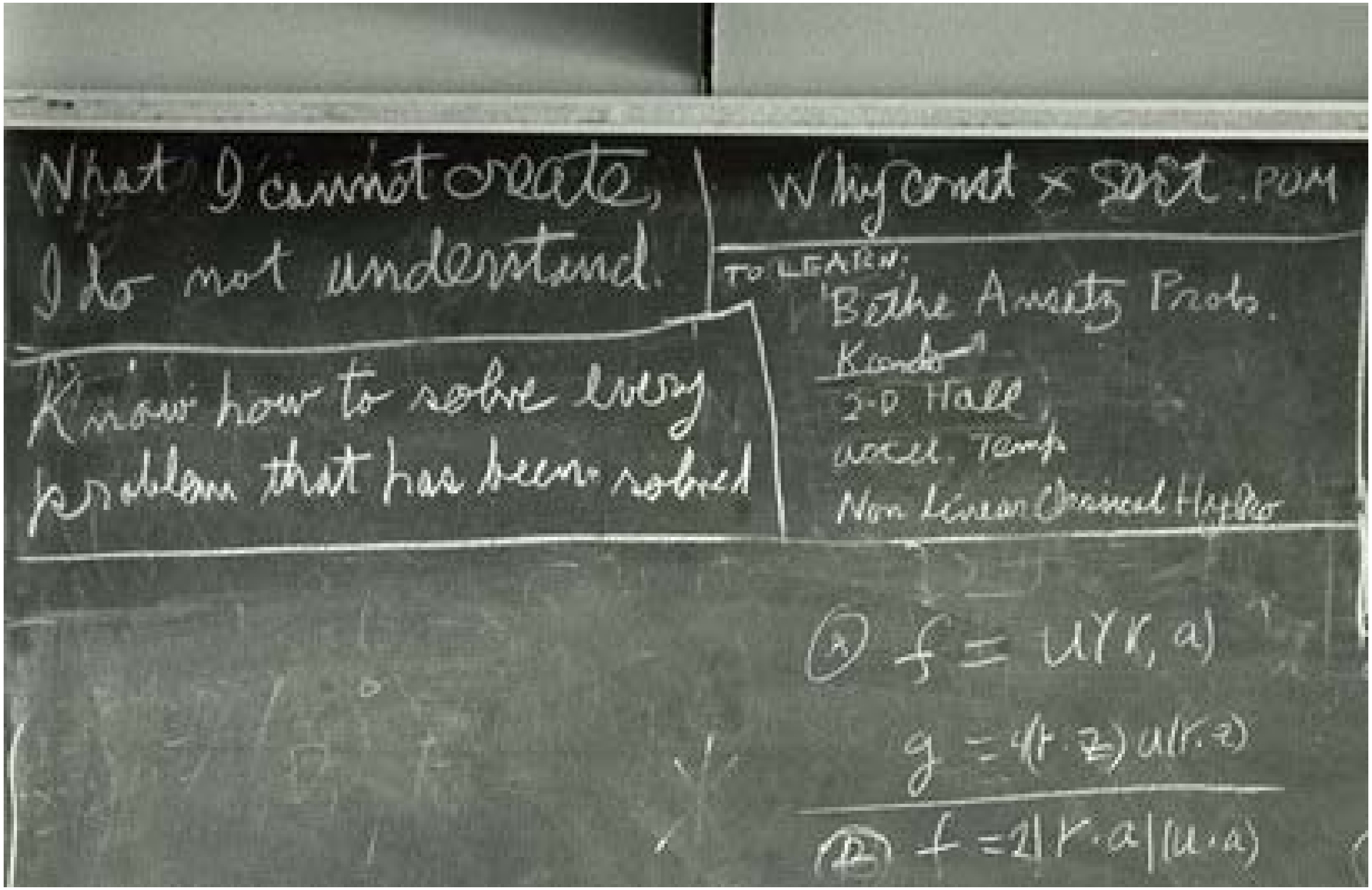
Among those who attended the Feynman Lectures on Computation were Gerry Sussman (who was on leave from MIT, and helped Feynman plan the course), Eric Mjolsness (the TA), and Mike Douglas (a student in the class).

Douglas (email 2018): I did find a relevant page of my notes from his lectures on quantum computing. It does not mention the question "are quantum computers more powerful than classical," and as I recall, that wasn't a focus of his lectures on quantum computing … But I believe that he did ask the question either in the lectures, in our lunchtime conversations, or both …

Indeed one of my term papers in 83-84 was precisely on the question of whether a quantum computer could solve the SAT problem and thus other NP hard problems faster than a classical computer … I don't recall thinking that my asking this question was a brilliant leap beyond what he had told us.

Mjolsness (email 2018): I don't clearly remember whether Feynman explicitly asked in class whether there are things a quantum computer can do that a classical one can't – but I'd be very surprised if he didn't, as it is something we discussed at the lunches Mike refers to.

Sussman (email 2018): Gee, I actually don't remember if the issue was explicitly discussed. But I do remember that Richard said, at a lunch, that quantum computing methods would help us simulate quantum systems more effectively.



Feynman’s final blackboard (Caltech Archives)

In 1987, Feynman led a small group of beginning graduate students who studied integrable models

Sandip Trivedi (Interview 2018):

Feynman believed that a deeper understanding of exactly solved models could lead to a better grasp of the soft physics in QCD. He told the students: "We gotta know how to solve every problem that has been solved."

They met every Wednesday in Feynman's office, and sometimes the meetings would last all afternoon. On a few occasions Feynman took the students out to dinner afterward.

Though ill, Feynman was "incredibly enthusiastic, and extremely patient."

He described the 6-vertex model and told the students to try to solve it, without looking up any references. ("What I cannot create, I do not understand.") This went on for some weeks, without notable progress, until Feynman unveiled his own solution.

They proceeded to the 8-vertex model. The students couldn't solve that one either, and neither could Feynman! (Baxter won 1987 APS Heineman Prize for his solution.)

During his final illness, Feynman instructed Helen Tuck to share his notes with the students. Trivedi now has a copy, and marvels at how meticulous and detailed they are. He shows the notes to his own students to inspire them.

ca. 15 February 1988. Photo by Patricia Schwarz.

ca. 15 February 1988. Photo by Patricia Schwarz.

Thanks!

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Additional Slides

(omitted to save time)

Feynman in *QED* (1985):

"The shell game that we play [is] called "renormalization." But no matter how clever the word, it is what I would call a dippy process. Having to resort to such hocus-pocus has prevented us from proving that quantum electrodynamics is mathematically self-consistent, It's surprising that the theory still has not been proved self-consistent by one method or another by now; I suspect that renormalization is not mathematically legitimate. What *is* certain is that we do not have a good mathematical way to describe the theory of quantum electrodynamics."

I spoke to Feynman a number of times about renormalization theory during the mid-80s (I arrived at Caltech in 1983 and he died in 1988). I was surprised on a few occasions how the effective field theory viewpoint did not come naturally to him.

One time he told me that no one could explain why the action of Yang-Mills is $\mathrm{tr}(F^2)$ instead of something more complicated. When I replied that higher dimensional operators would be irrelevant he understood quickly, but acted like this idea was new to him.

Along the same lines, Feynman briefly discusses in his lectures on gravitation (1962) why there are no higher derivative terms in the Einstein action, saying this is the "simplest" theory, not mentioning that higher derivative terms would be suppressed at low energy by more powers of the Planck length.

Feynman in a letter to Fermi, 19 December 1951:
"Don't believe any calculation in meson theory that uses a Feynman diagram."

Feynman and I once talked about pion dynamics. Mark Wise had given a talk about chiral Lagrangians, and it turned out Feynman had misunderstood it --- he thought Mark was foolishly doing perturbation theory in the pion-nucleon coupling, which was completely unjustified.

I explained that the calculation was really an expansion in the pion momentum, and therefore justified when the pions are sufficiently soft. Again, he understood quickly, but I was surprised he needed my prompting to catch on.

It was funny in a way. I would tell Feynman things that I knew were common lore, and he would be receptive, but he seemed unfamiliar with the ideas (though I suspected he had his own way of understanding similar things). This only served to accentuate further his overblown impression of my intelligence.

Feynman's "Physics X" class for Caltech freshman

Joe Polchinski (arXiv:1708.09093):

"If we ran out of questions he would talk about some of his ideas. One example of this was, how do you take the square root of the Fourier transformation, so that acting on a function twice with the operation would be the same as the Fourier transform … This kind of happy creativity was fascinating to see. Another question was, what is a negative probability?"

"Unfortunately, my main contribution to the class was falling asleep one day, in the front row, which has delighted some of my classmates to this day. "

Bill Zajc (email 2018):

"My strongest memory from Physics X is that one time Joe sat in the first row and feel asleep in a very visible way, with his neck across the back of his seat and his face pointed towards the ceiling. Feynman took no note of this."

"Feynman sometimes seeded the questions, or gave us something to think about for next time … . In one of the first sessions I attended, the "assignment" was to calculate (1/2)! … Feynman [also] challenged us to figure out how to compute half-derivatives … The general lesson Feynman was trying to impart (he said this explicitly) was a sense of play. "

Yuri Manin (b. 1937), *Computable and Uncomputable* (1980)

Translated from the Russian by Victor Albert

"These objects [quantum automata] may show us mathematical models of deterministic processes with highly unusual features. One of the reasons for this is because the quantum phase space is much bigger than classical: where classical space has N discrete levels, a quantum system allowing their superposition will have c^N Planck cells. In a union of two classical systems, their sizes N_1 and N_2 multiply, but in the quantum case we have c^{N1+N2}.

"These heuristic calculations point to a much larger potential complexity of the behavior of a quantum system when compared to its classical imitator. … The model of evolution is unitary rotation in a finite Hilbert space, and the model of dividing into subsystems is cutting up the system into tensor factors. Somewhere in this picture one needs to introduce interactions, described traditionally by Hermitian operators and probabilities."

Paul Benioff (b. 1930), *J. Stat. Phys. 22, 563-591* (1980)

"These considerations suggest that it may be impossible even in principle to construct a quantum mechanical Hamiltonian model of the computation process. The reason is that any such model evolves as an isolated system with a constant total energy. The point of this paper is to suggest, by construction of such models, that this may not be the case."

Note: Unlike Manin, Benioff was not concerned with quantum *complexity*. Rather, he mainly focused on the question whether a quantum computer can operate *without dissipation* (as did Feynman in his 1984 CLEO/IQEC talk on "Quantum Mechanical Computers").

Feynman and Lattice QCD

Rajan Gupta (email 2018):
Feynman expected that lattice QCD would be a great application of (Euclidean) path integral methods, but he was more interested in achieving a deeper understanding of QCD using analytic methods, or a combination of numerics and analytical methods.

He was optimistic about the long-term potential of massively parallel computation (and he worked out how to do floating-point arithmetic on the Connection Machine with applications to QCD in mind --- Hillis 1989), but felt that it would be a long time before computing power would suffice for realistic QCD calculations.

Feynman at the Wangerooge Workshop, September 1987:
"But the fact of the matter is that the lattices are not big enough and they are not understanding everything and there aren't any numbers coming out. So the idea that the lattice calculations are successful in dealing with this matter of the hadron energies and properties is false at the present time. It's true that if we could make the machines that we use sufficiently much larger, then technically, in principle, we would be able to do it … I think it's going to take a long time to get much out."

Difficulties in applying the variation principle to quantum field theories

Transcript of a talk at the Wangerooge Workshop, September 1987

Google Scholar ~ 25 citations

A failure: Feynman identifies three reasons why his attempts to do variational calculations in QCD did not succeed: (1) Short-distance physics dominates the energy. (2) The variational ansatz is nearly Gaussian. (3) It's too hard to compute the functional integral. What to do instead?

"Maybe there is some way to surround the object, or the region where we want to calculate things, by a surface and describe what things are coming in across the surface. It tells us everything that's going on outside."

"I think it should be possible some day to describe field theory in some other way than with wave functions and amplitudes. It might be something like the density matrices where you concentrate on quantities in a given locality."

Feynman seems to be groping for a tensor network description of variational trial wavefunctions. See: J. Haegeman et al., "Applying the variational principle to (1+1) dimensional relativistic quantum field theories" (2011) for applications of matrix product states to relativistic QFT, and a discussion of how to overcome Feynman's reservations.