

For a successful technology, reality must take precedence over public relations, for nature cannot be fooled.

Richard P. Feynman

Report of the Presidential Commission
on the Space Shuttle Challenger Accident (6 June 1986)

Feynman is becoming a real pain in the ass.

William P. Rogers

during a break in a hearing of the Rogers Commission
on the space shuttle Challenger accident, 11 February 1986
(quoted in *Genius: The Life and Science of Richard Feynman* (1992)
by James Gleick, p. 423.)

I took this stuff I got out of your [O-ring] seal and I put it in ice water, and I discovered that when you put some pressure on it for a while and then undo it it doesn't stretch back. It stays the same dimension. In other words, for a few seconds at least, and more seconds than that, there is no resilience in this particular material when it is at a temperature of 32 degrees. I believe that has some significance for our problem.

Richard P. Feynman

statement at hearing by Rogers Commission, 11 February 1986,
Report of the PRESIDENTIAL COMMISSION on the Space Shuttle
Challenger Accident, volume 4, p. 680; also quoted in *Genius: The
Life and Science of Richard Feynman* (1992) by James Gleick, p. 423.

The first principle is that you must not fool yourself, and you are the easiest person to fool.

Richard P. Feynman

from lecture "What is and What Should be the Role of Scientific Culture in Modern Society", given at the Galileo Symposium in Italy (1964)

In general we look for a new law by the following process. First we guess it. Then we compute the consequences of the guess to see what would be implied if this law that we guessed is right. Then we compare the result of the computation to nature, with experiment or experience, compare it directly with observation, to see if it works. If it disagrees with experiment it is wrong. In that simple statement is the key to science. It does not make any difference how beautiful your guess is. It does not make any difference how smart you are, who made the guess, or what his name is — if it disagrees with experiment it is wrong. That is all there is to it.

Richard P. Feynman

The Character of Physical Law (1965)
chapter 7, "Seeking New Laws," p. 156.

The scientist has a lot of experience with ignorance and doubt and uncertainty, and this experience is of very great importance, I think. When a scientist doesn't know the answer to a problem, he is ignorant. When he has a hunch as to what the result is, he is uncertain. And when he is pretty darn sure of what the result is going to be, he is still in some doubt. We have found it of paramount importance that in order to progress we must recognize our ignorance and leave room for doubt. Scientific knowledge is a body of statements of varying degrees of certainty some most unsure, some nearly sure, but none absolutely certain.

Richard P. Feynman

The Feynman Lectures on Physics (1964)

Each piece, or part, of the whole of nature is always merely an approximation to the complete truth, or the complete truth so far as we know it. In fact, everything we know is only some kind of approximation, because we know that we do not know all the laws as yet. Therefore, things must be learned only to be unlearned again or, more likely, to be corrected. . . . The test of all knowledge is experiment. Experiment is the sole judge of scientific "truth".

Richard P. Feynman

The Feynman Lectures on Physics (1964), volume I; lecture 1, "Atoms in Motion"; section 1-1, "Introduction"; p. 1-1

If you have a theory, you must try to explain what's good and what's bad about it equally. In science, you learn a kind of standard integrity and honesty.

Richard P. Feynman

What Do You Care What Other People Think? (1988),
"Afterthoughts," p. 217-218

Since then I never pay attention to anything by “experts”. I calculate everything myself.

Richard P. Feynman

Surely You're Joking, Mr. Feynman! (1985),

After having been led astray on neutron-proton coupling by reports of “beta-decay experts”.

The chance is high that the truth lies in the fashionable direction. But, on the off-chance that it is in another direction — a direction obvious from an unfashionable view of field theory — who will find it? Only someone who has sacrificed himself by teaching himself quantum electrodynamics from a peculiar and unfashionable point of view; one that he may have to invent for himself.

"The Development of the Space-Time View of Quantum Electrodynamics,"
Nobel Lecture (11 December 1965)

The next question was — what makes planets go around the sun? At the time of Kepler some people answered this problem by saying that there were angels behind them beating their wings and pushing the planets around an orbit. As you will see, the answer is not very far from the truth. The only difference is that the angels sit in a different direction and their wings push inward.

Richard P. Feynman

The Character of Physical Law (1965)

chapter 1, "The Law of Gravitation," p. 18: video

Science is the belief in the ignorance of experts.

Science alone of all the subjects contains within itself the lesson of the danger of belief in the infallibility of the greatest teachers of the preceding generation.

address "What is Science?", presented at the fifteenth annual meeting of the National Science Teachers Association, in New York City (1966), published in *The Physics Teacher*, volume 7, issue 6 (1969), p. 313-320

There is one feature I notice that is generally missing in cargo cult science. . . . It's a kind of scientific integrity, a principle of scientific thought that corresponds to a kind of utter honesty . . . a kind of leaning over backwards. For example, if you're doing an experiment, you should report everything that you think might make it invalid . . . not only what you think is right about it; other causes that could possibly explain your results; and things you thought of that you've eliminated by some other experiment, and how they worked . . . to make sure the other fellow can tell they have been eliminated.

Details that could throw doubt on your interpretation must be given, if you know them. You must do the best you can if you know anything at all wrong, or possibly wrong to explain it. If you make a theory, for example, and advertise it, or put it out, then you must also put down all the facts that disagree with it, as well as those that agree with it. There is also a more subtle problem. When you have put a lot of ideas together to make an elaborate theory, you want to make sure, when explaining what it fits, that those things it fits are not just the things that gave you the idea for the theory; but that the finished theory makes something else come out right, in addition.

In summary, the idea is to try to give all of the information to help others to judge the value of your contribution; not just the information that leads to judgement in one particular direction or another.

We've learned from experience that the truth will come out. Other experimenters will repeat your experiment and find out whether you were wrong or right. Nature's phenomena will agree or they'll disagree with your theory. And, although you may gain some temporary fame and excitement, you will not gain a good reputation as a scientist if you haven't tried to be very careful in this kind of work. And it's this type of integrity, this kind of care not to fool yourself, that is missing to a large extent in much of the research in cargo cult science.

“Cargo Cult Science,” adapted from a 1974 Caltech commencement address; also published in *Surely You're Joking, Mr. Feynman!*, p. 342.

To those who do not know mathematics it is difficult to get across a real feeling as to the beauty, the deepest beauty, of nature. . . . If you want to learn about nature, to appreciate nature, it is necessary to understand the language that she speaks in.

The Character of Physical Law (1965)

chapter 2, "The Relation of Mathematics to Physics," p. 58

The real problem in speech is not precise language. The problem is clear language. The desire is to have the idea clearly communicated to the other person. It is only necessary to be precise when there is some doubt as to the meaning of a phrase, and then the precision should be put in the place where the doubt exists. It is really quite impossible to say anything with absolute precision, unless that thing is so abstracted from the real world as to not represent any real thing.

Pure mathematics is just such an abstraction from the real world, and pure mathematics does have a special precise language for dealing with its own special and technical subjects. But this precise language is not precise in any sense if you deal with real objects of the world, and it is only pedantic and quite confusing to use it unless there are some special subtleties which have to be carefully distinguished.

“New Textbooks for the “New” Mathematics,” *Engineering and Science*, vol 28, no 6 (March 1965), pp. 9-15 at p. 14.

Do not keep saying to yourself, if you can possibly avoid it, "But how can it be like that?" because you will get "down the drain," into a blind alley from which nobody has yet escaped. Nobody knows how it can be like that.

The Character of Physical Law (1965), ch 6, "Probability and Uncertainty — the Quantum Mechanical View of Nature," p. 129.

We always have had . . . a great deal of 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.

"Simulating Physics with Computers," International Journal of Theoretical Physics, vol 21, 1982, p. 467–488, at p. 471.

One of the first interesting experiences I had in this project at Princeton was meeting great men. I had never met very many great men before. But there was an evaluation committee that had to try to help us along, and help us ultimately decide which way we were going to separate the uranium. This committee had men like Compton and Tolman and Smyth and Urey and Rabi and Oppenheimer on it. I would sit in because I understood the theory of how our process of separating isotopes worked, and so they'd ask me questions and talk about it. In these discussions one man would make a point. Then Compton, for example, would explain a different point of view. He would say it should be this way, and he was perfectly right. Another guy would say, well, maybe, but there's this other possibility we have to consider against it.

So everybody is disagreeing, all around the table. I am surprised and disturbed that Compton doesn't repeat and emphasize his point. Finally at the end, Tolman, who's the chairman, would say, "Well, having heard all these arguments, I guess it's true that Compton's argument is the best of all, and now we have to go ahead."

It was such a shock to me to see that a committee of men could present a whole lot of ideas, each one thinking of a new facet, while remembering what the other fella said, so that, at the end, the decision is made as to which idea was the best — summing it all up — without having to say it three times. These were very great men indeed.

from the First Annual Santa Barbara Lectures on Science and Society,
University of California at Santa Barbara (1975)

I do feel strongly that this is nonsense! ... So perhaps I could entertain future historians by saying I think all this superstring stuff is crazy and is in the wrong direction. I think all this superstring stuff is crazy and is in the wrong direction. ... I don't like it that they're not calculating anything. ... why are the masses of the various particles such as quarks what they are? All these numbers ... have no explanations in these string theories — absolutely none! ... I don't like that they don't check their ideas. I don't like that for anything that disagrees with an experiment, they cook up an explanation — a fix-up — to say, "Well, it might be true."

For example, the theory requires ten dimensions. Well, maybe there's a way of wrapping up six of the dimensions. Yes, that's all possible mathematically, but why not seven? When they write their equation, the equation should decide how many of these things get wrapped up, not the desire to agree with experiment. In other words, there's no reason whatsoever in superstring theory that it isn't eight out of the ten dimensions that get wrapped up and that the result is only two dimensions, which would be completely in disagreement with experience. So the fact that it might disagree with experience is very tenuous, it doesn't produce anything.

interview published in *Superstrings: A Theory of Everything?* (1988)
edited by Paul C. W. Davies and Julian R. Brown, pp. 193-194.

The theoretical broadening which comes from having many humanities subjects on the campus is offset by the general dopiness of the people who study these things.

letter to Robert Bacher (6 April 1950), quoted in *Genius: The Life and Science of Richard Feynman* (1992) by James Gleick, p. 278.