



Lecture 2: WHAT IS SCIENCE AND RESEARCH ETHICS

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Introduction to PhD studies in astronomy and theoretical physics



What is Science?

What is science?

What does a scientist do?

- Some remarks about careers and some other subjects
- What is science
- Discussion so ask questions
- Idea: make you aware of some things rather than a full course

Very general definition: like to understand and find out how things work

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A typical scientist career

- Getting interested in Science & Technology
- Undergraduate degree: bachelor, master, magister, Diplom,...
- PhD studies ← you are here
- Postdoc 1,2,...
- Assistant professor (4-6 year job)
- Associate professor (lecturer)
- Professor
- really big boss stage

But also: research engineer, positions at laboratories/research institutes, R&D in business/government

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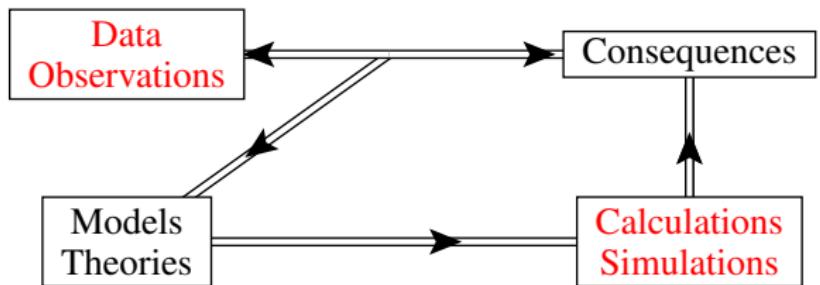
A typical career

- At your stage this looks like the most natural route
- Need to do good research but also
 - is the field popular or not
 - retirements
 - expanding/shrinking of research area

And do not forget:

- There are many other career options (We (seniors) have chosen this path so tend to think of it as THE option)
- Local job market is small: highly specialized
 - luck plays a nonnegligible role (economy up or down)
 - Work for opportunities: no applications ⇒ no job offers
- all our students have found jobs (but it might take a while)

What do we do in astronomy/theoretical physics?



- Where you usually are
- Goal: understanding how things work
- Also science:
 - Finding out how to calculate something
 - Developing a measurement method
 - guessing (inspired) models
- We spend most of our time in details of the various steps (not very grand I know)

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What do we do?

- Learn what others do
- Try a few things
- Get ideas (usually after trying a lot of things)
 ⇒ work them out as much as you can
- Obviously a cycle

Finally some results: now what?

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Paper and presentations

- have some good results
- **Must make others aware of it**
 - presentation at conferences
 - the PAPER itself: preprint+electronic archives
 - submitted, refereed, published
 - letters, review papers, proceedings, . . .
 - Blogs, press releases, . . .
- Just sending a good paper is usually not enough (do YOU read all new papers?)



What happens with a paper?

- Choose a journal:
 - more general or more specialized
 - type: letter or standard article
 - letter: shorter but faster publication (less relevant now with archives)
 - standard paper: longer, so more space to explain things
- It gets to one (or more editors)
 - Is it right for the journal?
 - If yes select referee(s)
 - look at the references to get some thought about possible referees
 - report arrives; editor decides: accept, changes required or reject
 - Sometimes a few cycles

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Why publishing and peer review?

- Journals serve several purposes: dissemination, quality control and long term archiving
- Long term archiving is an unsolved problem
 - paper lives much longer than electronic servers
 - many more paper copies exist (in libraries,...), publishers disappear
- Dissemination: in our fields done by the archives
- Quality control is the main remaining purpose (and the journals keep up the minimum level in the archives)

Quality control

- Refereeing checks:
 - plausibility, not correctness
 - fairness (what he didn't cite me!)
 - is it actually understandable
 - enough new results to be worth publishing
- Note when you read a report:
 - takes lots of UNPAID time
 - not always a real expert
 - report often sloppily written after spending lots of time reading the manuscript
- Anonymous:
 - editors can be known (should be senior scientists)
 - referees should not be: too easy to be influenced, especially if fairly junior and paper by someone very senior
 - Disadvantage: no obvious benefit of doing a good refereeing job
- Similar remarks apply to reviewing of applications

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Richard Feynman

*From a Caltech **commencement** address given in 1974
(what is a commencement) Also in Surely You're Joking, Mr.
Feynman!*

During the Middle Ages there were all kinds of crazy ideas, such as that a piece of rhinoceros horn would increase potency.

Then a method was discovered for separating the ideas—which was to try one to see if it worked, and if it didn't work, to eliminate it. (Scientific rational revolution) This method became organized, of course, into science. And it developed very well, so that we are now in the scientific age. It is such a scientific age, in fact, that we have difficulty in understanding how witch doctors could ever have existed, when nothing that they proposed ever really worked—or very little of it did.

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But even today I meet lots of people who sooner or later get me into a conversation about UFO's, or astrology, or some form of mysticism, expanded consciousness, new types of awareness, ESP, and so forth. **And I've concluded that it's not a scientific world. (Unfortunately still quite true)**

Most people believe so many wonderful things that I decided to investigate why they did. And what has been referred to as my curiosity for investigation has landed me in a difficulty where I found so much junk that I'm overwhelmed. First I started out by investigating various ideas of mysticism and mystic experiences. I went into isolation tanks and got many hours of hallucinations, so I know something about that. Then I went to Esalen, which is a hotbed of this kind of thought (it's a wonderful place; you should go visit there). Then I became overwhelmed. I didn't realize how MUCH there was.

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At Esalen there are some large baths fed by hot springs situated on a ledge about thirty feet above the ocean. One of my most pleasurable experiences has been to sit in one of those baths and watch the waves crashing onto the rocky slope below, to gaze into the clear blue sky above, and to study a beautiful nude as she quietly appears and settles into the bath with me.

One time I sat down in a bath where there was a beautiful girl sitting with a guy who didn't seem to know her. Right away I began thinking, "Gee! How am I gonna get started talking to this beautiful nude woman?" I'm trying to figure out what to say, when the guy says to her, "I'm, uh, studying massage. Could I practice on you?" "Sure," she says. They get out of the bath and she lies down on a massage table nearby. I think to myself, "What a nifty line! I can never think of anything like that!" He starts to rub her big toe. "I think I feel it," he says. "I feel a kind of dent—is that the pituitary?" I blurt out, "You're a helluva long way from the pituitary, man!" They looked at me, horrified—I had blown my cover—and said, "It's reflexology!" I quickly closed my eyes and appeared to be meditating.

That's just an example of the kind of things that overwhelm me. I also looked into extrasensory perception, and PSI phenomena, and the latest craze there was Uri Geller, a man who is supposed to be able to bend keys by rubbing them with his finger. So I went to his hotel room, on his invitation, to see a demonstration of both mindreading and bending keys. He didn't do any mindreading that succeeded; nobody can read my mind, I guess. And my boy held a key and Geller rubbed it, and nothing happened. Then he told us it works better under water, and so you can picture all of us standing in the bathroom with the water turned on and the key under it, and him rubbing the key with his finger. Nothing happened. So I was unable to investigate that phenomenon.

But then I began to think, what else is there that we believe? (And I thought then about the witch doctors, and how easy it would have been to check on them by noticing that nothing really worked.) So I found things that even more people believe, such as that we have some knowledge of how to educate. There are big schools of reading methods and mathematics methods, and so forth, but if you notice, you'll see the reading scores keep going down—or hardly going up—in spite of the fact that we continually use these same people to improve the methods. There's a witch doctor remedy that doesn't work. It ought to be looked into; how do they know that their method should work? Another example is how to treat criminals. We obviously have made no progress—lots of theory, but no progress—in decreasing the amount of crime by the method that we use to handle criminals.

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Yet these things are said to be scientific. We study them. And I think ordinary people with commonsense ideas are intimidated by this pseudoscience. A teacher who has some good idea of how to teach her children to read is forced by the school system to do it some other way—or is even fooled by the school system into thinking that her method is not necessarily a good one. Or a parent of bad boys, after disciplining them in one way or another, feels guilty for the rest of her life because she didn't do "the right thing," according to the experts. So we really ought to look into theories that don't work, and science that isn't science.

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I think the educational and psychological studies I mentioned are examples of what I would like to call **cargo cult science**. In the South Seas there is a cargo cult of people. During the war they saw airplanes with lots of good materials, and they want the same thing to happen now. So they've arranged to make things like runways, to put fires along the sides of the runways, to make a wooden hut for a man to sit in, with two wooden pieces on his head to headphones and bars of bamboo sticking out like antennas—he's the controller—and they wait for the airplanes to land. They're doing everything right. The form is perfect. It looks exactly the way it looked before. But it doesn't work. No airplanes land. So I call these things cargo cult science, because they follow all the apparent precepts and forms of scientific investigation, but they're missing something essential, because the planes don't land.

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Now it behooves me, of course, to tell you what they're missing. But it would be just about as difficult to explain to the South Sea islanders how they have to arrange things so that they get some wealth in their system. It is not something simple like telling them how to improve the shapes of the earphones. But there is one feature I notice that is generally missing in cargo cult science. That is the idea that we all hope you have learned in studying science in school—we never say explicitly what this is, but just hope that you catch on by all the examples of scientific investigation. It is interesting, therefore, to bring it out now and speak of it explicitly. 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.

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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;

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but that the finished theory makes something else come out right, in addition.

In summary, the idea is 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.

The easiest way to explain this idea is to **contrast it, for example, with advertising**. Last night I heard that Wesson oil doesn't soak through food. Well, that's true. It's not dishonest; but the thing I'm talking about is not just a matter of not being dishonest; it's a matter of scientific integrity, which is another level. The fact that should be added to that advertising statement is that no oils soak through food, if operated at a certain temperature. If operated at another temperature, they all will—including Wesson oil.

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So it's the implication which has been conveyed, not the fact, which is true, and the difference is what we have to deal with. 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.

A great deal of their difficulty is, of course, the difficulty of the subject and the inapplicability of the scientific method to the subject. Nevertheless, it should be remarked that this is not the only difficulty. That's why the planes don't land—but they don't land.

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We have learned a lot from experience about how to handle some of the ways we fool ourselves. One example: Millikan measured the charge on an electron by an experiment with falling oil drops, and got an answer which we now know not to be quite right. It's a little bit off because he had the incorrect value for the viscosity of air. It's interesting to look at the history of measurements of the charge of an electron, after Millikan. If you plot them as a function of time, you find that one is a little bit bigger than Millikan's, and the next one's a little bit bigger than that, and the next one's a little bit bigger than that, until finally they settle down to a number which is higher.

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Why didn't they discover the new number was higher right away? It's a thing that scientists are ashamed of—this history—because it's apparent that people did things like this: When they got a number that was too high above Millikan's, they thought something must be wrong—and they would look for and find a reason why something might be wrong. When they got a number close to Millikan's value they didn't look so hard. And so they eliminated the numbers that were too far off, and did other things like that. We've learned those tricks nowadays, and **now we don't have that kind of a disease.**
(opinions whether this true?)

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But this long history of learning how to not fool ourselves—of having utter scientific integrity—is, I'm sorry to say, something that we haven't specifically included in any particular course that I know of. We just hope you've caught on by osmosis. The first principle is that you must not fool yourself—and you are the easiest person to fool. So you have to be very careful about that. After you've not fooled yourself, it's easy not to fool other scientists. You just have to be honest in a conventional way after that.

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I would like to add something that's not essential to the science, but something I kind of believe, which is that you should not fool the layman when you're talking as a scientist. I am not trying to tell you what to do about cheating on your wife, or fooling your girlfriend, or something like that, when you're not trying to be a scientist, but just trying to be an ordinary human being. We'll leave those problems up to you and your rabbi. I'm talking about a specific, extra type of integrity that is not lying, but bending over backwards to show how you're maybe wrong, that you ought to have when acting as a scientist. And this is our responsibility as scientists, certainly to other scientists, and I think to laymen.

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For example, I was a little surprised when I was talking to a friend who was going to go on the radio. He does work on cosmology and astronomy, and he wondered how he would explain what the applications of his work were. "Well," I said, "there aren't any." He said, "Yes, but then we won't get support for more research of this kind." I think that's kind of dishonest. If you're representing yourself as a scientist, then you should explain to the layman what you're doing— and if they don't support you under those circumstances, then that's their decision.



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One example of the principle is this: If you've made up your mind to test a theory, or you want to explain some idea, you should always decide to publish it whichever way it comes out. If we only publish results of a certain kind, we can make the argument look good. We must publish BOTH kinds of results. (negative results too often not published)

I say that's also important in giving certain types of government advice. Supposing a senator asked you for advice about whether drilling a hole should be done in his state; and you decide it would be better in some other state. If you don't publish such a result, it seems to me you're not giving scientific advice. You're being used. If your answer happens to come out in the direction the government or the politicians like, they can use it as an argument in their favor; if it comes out the other way, they don't publish at all. That's not giving scientific advice.

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Other kinds of errors are more characteristic of poor science.

When I was at Cornell, I often talked to the people in the psychology department. One of the students told me she wanted to do an experiment that went something like this—it had been found by others that under certain circumstances, X, rats did something, A. She was curious as to whether, if she changed the circumstances to Y, they would still do A. So her proposal was to do the experiment under circumstances Y and see if they still did A.

I explained to her that it was necessary first to repeat in her laboratory the experiment of the other person—to do it under condition X to see if she could also get result A, and then change to Y and see if A changed. Then she would know the the real difference was the thing she thought she had under control.

She was very delighted with this new idea, and went to her professor. And his reply was, no, you cannot do that, because the experiment has already been done and you would be wasting time. This was in about 1947 or so, and it seems to have been the general policy then to not try to repeat psychological experiments, but only to change the conditions and see what happened.

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Nowadays, there's a certain danger of the same thing happening, even in the famous field of physics. I was shocked to hear of an experiment being done at the big accelerator at the National Accelerator Laboratory, where a person used deuterium. In order to compare his heavy hydrogen results to what might happen with light hydrogen, he had to use data from someone else's experiment on light hydrogen, which was done on different apparatus. When asked why, he said it was because he couldn't get time on the program (because there's so little time and it's such expensive apparatus) to do the experiment with light hydrogen on this apparatus because there wouldn't be any new result. And so the men in charge of programs at NAL are so anxious for new results, in order to get more money to keep the thing going for public relations purposes, they are destroying—possibly—the value of the experiments themselves, which is the whole purpose of the

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It is often hard for the experimenters there to complete their work as their scientific integrity demands. (remember this, most large scale experiments/observations/medical studies are very hard to repeat)

All experiments in psychology are not of this type, however. For example, there have been many experiments running rats through all kinds of mazes, and so on—with little clear result. But in 1937 a man named Young did a very interesting one. He had a long corridor with doors all along one side where the rats came in, and doors along the other side where the food was. He wanted to see if he could train the rats to go in at the third door down from wherever he started them off. No. The rats went immediately to the door where the food had been the time before.

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The question was, how did the rats know, because the corridor was so beautifully built and so uniform, that this was the same door as before? Obviously there was something about the door that was different from the other doors. So he painted the doors very carefully, arranging the textures on the faces of the doors exactly the same. Still the rats could tell. Then he thought maybe the rats were smelling the food, so he used chemicals to change the smell after each run. Still the rats could tell. Then he realized the rats might be able to tell by seeing the lights and the arrangement in the laboratory like any commonsense person. So he covered the corridor, and still the rats could tell.

He finally found that they could tell by the way the floor sounded when they ran over it. And he could only fix that by putting his corridor in sand. So he covered one after another of all possible clues and finally was able to fool the rats so that they didn't run away in the third.

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If he relaxed any of his conditions, the rats could tell.

Now, from a scientific standpoint, that is an A-number-one experiment. That is the experiment that makes rat-running experiments sensible, because it uncovers that clues that the rat is really using— not what you think it's using. And that is the experiment that tells exactly what conditions you have to use in order to be careful and control everything in an experiment with rat-running.

I looked up the subsequent history of this research. The next experiment, and the one after that, never referred to Mr. Young. They never used any of his criteria of putting the corridor on sand, or being very careful. They just went right on running the rats in the same old way, and paid no attention to the great discoveries of Mr. Young, and his papers are not referred to, because he didn't discover anything about the rats.

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In fact, he discovered all the things you have to do to discover something about rats. But not paying attention to experiments like that is a characteristic example of cargo cult science.

Another example is the ESP experiments of Mr. Rhine, and other people. As various people have made criticisms—and they themselves have made criticisms of their own experiments—they improve the techniques so that the effects are smaller, and smaller, and smaller until they gradually disappear. All the para-psychologists are looking for some experiment that can be repeated—that you can do again and get the same effect—statistically, even. They run a million rats—no, it's people this time—they do a lot of things and get a certain statistical effect. Next time they try it they don't get it any more. And now you find a man saying that is an irrelevant demand to expect a repeatable experiment. This is science?

This man also speaks about a new institution, in a talk in which he was resigning as Director of the Institute of Parapsychology. And, in telling people what to do next, he says that one of things they have to do is be sure the only train students who have shown their ability to get PSI results to an acceptable extent—not to waste their time on those ambitious and interested students who get only chance results. It is very dangerous to have such a policy in teaching—to teach students only how to get certain results, rather than how to do an experiment with scientific integrity.

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So I have just one wish for you—the good luck to be somewhere where you are free to maintain the kind of integrity I have described, and where you do not feel forced by a need to maintain your position in the organization, or financial support, or so on, to lose your integrity. May you have that freedom.
(it might not help much but integrity earns respect in the long run)



Fred Wilson: what is science?

(page 1) More precisely, the essential characteristics of science are:

- ① It is guided by natural law;
 - ② It has to be explanatory by reference to natural law;
 - ③ It is testable against the empirical world;
 - ④ Its conclusions are tentative, i.e., are not necessarily the final word; and
 - ⑤ It is falsifiable.
- (p2) The humanist is taught that science is a large collection of facts; and if this is true, then the only scientists need do is to see the facts.
 - (p2) I define science as the organization of our knowledge in such a way that it commands more of the hidden potential in nature. **(and the process of finding it)**

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- (p3) And this is why in turn practical discoveries are not made only by practical people.
- (p4) All science is the search for unity in hidden likenesses.
- (p5) But now they miss the crux of the story. For what struck the young Newton at the sight was not the thought that the apple must be drawn to Earth by gravity; that conception was older than Newton. What struck him was the conjecture that the same force of gravity, which reaches to the top of the tree, might go on reaching out beyond Earth and its air, endlessly into space. Gravity might reach the moon:

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The Semmelweiss story:

- (p6) Asking Why
- (p7) Offering explanations: observation and generalization

Scientific method: (p8-9)

- ① First, you must collect observations about some facet of nature (observation).
- ② Second, you must organize these observations into an orderly array (correlation). The organization does not alter them but merely makes them easier to handle. This is plain in the game of bridge, for instance, where arranging the hand in suits and order of value does not change the cards or show the best course of play, but makes it easier to arrive at the logical plays.
- ③ Third, you must derive from your orderly array of observations some principle that summarizes the observations(generalization).
- ④ And finally, the principle must be put to the test along with a set of basic standards for judging the merits of experiments designed (experimentation).
- ⑤ each of these is split in many parts

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(p10) Yet modern science need not be so complete a mystery to nonscientists. Much could be accomplished toward bridging the gap if scientists accepted the responsibility of communication - explaining their own fields of work as simply and to as many as possible - and if nonscientists, for their part, accepted the responsibility of listening. To gain a satisfactory appreciation of the developments in a field of science, it is not essential to have a total understanding of the science. After all, no one feels that one must be capable of writing a great work of literature in order to appreciate Shakespeare.



Final thoughts

- science
- knowledge
- exact sciences
- technology
- social sciences

Is history science?

What is
Science and
Research
Ethics

Johan Bijnens

Introduction

Careers

What do we
do?

Papers etc

Cargo Cult
Science

What is
science?

20 Big questions

What is
Science and
Research
Ethics

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do?

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What is
science?

Discuss the 20 big questions