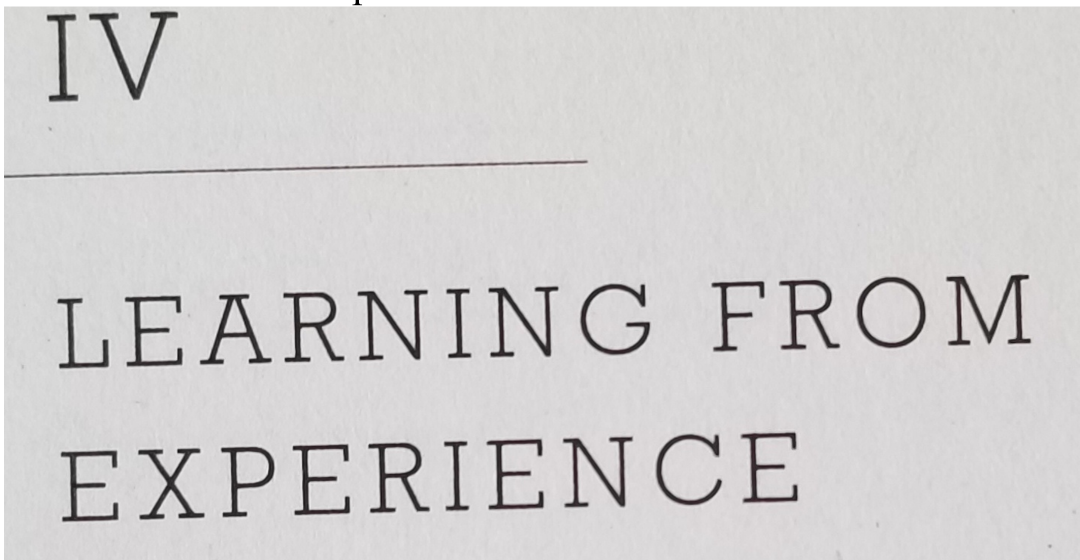
Part I: Before reading the book.

Part II: After reading the book.

Part I was an impulsive reaction to just one page in the book.
Then I found the book and read it.
I believe that the last part of the book



is as important today as it was important twelve years ago (now - maybe even more), and not only for the string theory, or physics, but for science in general, especially for such fields like education and artificial intelligence.
For the last couple of years, I have been writing on Cognisity.How and expressing very similar views on the matter of science, but reading Lee Smolin's book gave me a much broader perspective on the state of the scientific research. So many statements in the book resonated with my own views that I just could not resist to adding them to this post.
Being at risk of accused of copyrights violation, I want to share some excerpts from Chapter IV with my comments to them (but I would strongly recommend everyone to find the book and to read the whole chapter).
1.

What Feyerabend's book said to me was: *Look, kid, stop dreaming! Science is not philosophers sitting in clouds. It is a human activity, as complex and problematic as any other. There is no single method to science and no single criterion for who is a good scientist. Good science is whatever works at a particular moment of history to advance our knowledge. And don't bother me with how to define progress — define it any way you like and this is still true.*

This is what many scientist ignore – science is a human practice and follows the general laws governing any human practice.

2.

Science has succeeded because scientists comprise a community that is defined and maintained by adherence to a shared ethic. It is adherence to an ethic, not adherence to any particular fact or theory, that I believe serves as the fundamental corrective within the scientific community.

There are two tenets of this ethic:

1. If an issue can be decided by people of good faith, applying rational argument to publicly available evidence, then it must be regarded as so decided.
2. If, on the other hand, rational argument from the publicly available evidence does not succeed in bringing people of good faith to agreement on an issue, society must allow and even encourage people to draw diverse conclusions.

Science is not defined by any specific method or methodology (e.g. “science is based on experiments”, “science is based on logic and reason”).
Science is the result of activities of people who decided to follow certain social/ethical rules.

- We agree to argue rationally, and in good faith, from shared evidence, to whatever degree of shared conclusions are warranted.
- Each individual scientist is free to develop his or her own conclusions from the evidence. But each scientist is also required to put forward arguments for those conclusions for the consideration of the whole community. These arguments must be rational and based on evidence available to all members. The evidence, the means by which the evidence was obtained, and the logic of the arguments used to deduce conclusions from the evidence must be shared and open to examination by all members.
- The ability of scientists to deduce reliable conclusions from the shared evidence is based on the mastery of tools and procedures developed over many years. They are taught because experience has shown that they often lead to reliable results. Every scientist trained in such a craft is deeply aware of the capacity for error and self-delusion.
- At the same time, each member of the scientific community

recognizes that the eventual goal is to establish consensus. A consensus may emerge quickly, or it may take some time. The ultimate judges of scientific work are future members of the community, at a time sufficiently far in the future that they can better evaluate the evidence objectively. While a scientific program may temporarily succeed in gathering adherents, no program, claim, or point of view can succeed in the long run unless it produces sufficient evidence to persuade the skeptics.

- Membership in the community of science is open to any human being. Considerations of status, age, gender, or any other personal characteristic may not play a role in the consideration of a scientist's evidence and arguments, and may not limit a member's access to the means of dissemination of evidence, argument, and information. Entry to the community is, however, based on two criteria. The first is the mastery of at least one of the crafts of a scientific subfield to the point where you can independently produce work judged by other members to be of high quality. The second criterion is allegiance and continued adherence to the shared ethic.
- While orthodoxies may become established temporarily in a given subfield, the community recognizes that contrary opin-

> ions and research programs are necessary for the community's continued health.
>
> When people join a scientific community, they give up certain childish but universal desires: the need to feel that they are right all the time or the belief that they are in possession of the absolute truth. In exchange, they receive membership

BTW: this is the criterion which allows to separate science from religion.
A true scientist always assumes that he or she may be wrong; a religious person follows dogmatic thinking, assuming he or she is always correct.
3.

> TO REVERSE THE TROUBLING trends in **science** we must first understand what science is — what moves it forward and what holds it back. And to do this, we must define science as something more than the sum of what scientists do.

Scientists are professionals acting in a *scientific field*. But it does not mean yet that the field is a science. **Science is a human practice which mission, goals, purpose, sole existence is providing reliable predictions.** That represents the top stage of the development of a scientific field. For example, there is a field of scientific research on education, but there is not yet science of education.
4.

Thomas Kuhn made a distinction between "normal science" and scientific revolutions. Normal science is based on a paradigm, which is a well-defined practice with a fixed theory and a fixed body of questions, experimental methods, and calculational techniques. A scientific revolution happens when the paradigm breaks down, which is to say, when the theory it is based on fails to predict or explain the results of the experiments.[3] I don't think science always works this way, but there are certainly normal and revolutionary periods, and science is done differently during them. The point is that different kinds of people are important in normal and revolutionary science. In the normal periods, you need only people who, regardless of their degree of imagination (which may well be high), are really good at working with the technical tools — let us call them master craftspeople. During revolutionary periods, you need seers, who can peer ahead into the darkness.

There are periods of a straightforward scientific development, which requires people good at technical work. Drastic changes (needed to overcome long periods of stagnation) require people of a different type – seers.

Science requires a balance between rebellion and respect, so there will always be arguments between radicals and conservatives. But there is no balance in the current academic world. More than at any time in the history of science, the cards are stacked against the revolutionary. Such people are simply not tolerated in the research universities. Little wonder, then, that even when the science clearly calls for one, we can't seem to pull off a revolution.

To keep science healthy, ALL scientists should be hired and promoted based only on their ability, creativity, and independence, without regard to whether they contribute to dominante theory or any other established research program. People who invent and develop their own research programs should even be given priority, so that they can have the intellectual freedom to work on the approach they judge the most promising. The governance of science is always a matter of making choices. To prevent overinvestment in speculative directions that may turn out to be dead ends, ALL departments should ensure that rival research programs and different points of view toward unsolved problems are represented on their faculties — not only because most of the time we cannot predict which views will be right but because the friendly rivalry between smart people working in close proximity is often a source of new ideas and directions.

5.

Yale psychologist Irving Janis, who coined the term in the 1970s, defines groupthink as "a mode of thinking that people engage in when they are deeply involved in a cohesive in-group, when the members' strivings for unanimity override their motivation to realistically appraise alternative courses of action."[18] According to this definition, groupthink occurs only when cohesiveness is high. It requires that members share a strong "we-feeling" of solidarity and a desire to maintain relationships within the group at all costs.

"Groupthink", or "Group thinking", a.k.a. "tribal thinking" is a very common human phenomenon (sport fans, political affiliation, a school, a fraternity, etc.). Lee Smolin points at a very significant fact that scientists – like all humans - also form "tribes" and fall into groupthink. Even in science, very often everyone who thinks differently from the group thinks wrong.

Let me summarize, so we can see where this is taking us. The discussion has brought out seven unusual aspects of the string theory community. **(artificial intelligence; to some extend education)**

1. *Tremendous self-confidence,* leading to a sense of entitlement and of belonging to an elite community of experts.
2. *An unusually monolithic community,* with a strong sense of consensus, whether driven by the evidence or not, and an un-

usual uniformity of views on open questions. These views seem related to the existence of a hierarchical structure in which the ideas of a few leaders dictate the viewpoint, strategy, and direction of the field.

3. In some cases, a *sense of identification with the group,* akin to identification with a religious faith or political platform.
4. A strong sense of *the boundary between the group and other experts.*
5. A *disregard for and disinterest in* the ideas, opinions, and work of experts who are not part of the group, and a preference for talking only with other members of the community.
6. A tendency to *interpret evidence optimistically,* to believe exaggerated or incorrect statements of results, and to disregard the possibility that the theory might be wrong. This is coupled with a tendency to *believe results are true because they are "widely believed,"* even if one has not checked (or even seen) the proof oneself.
7. A lack of appreciation for the extent to which a research program ought to involve risk.

Members of a group may commonly accept assumptions as facts: this is an example

Finiteness is not the only example in string theory of a conjecture that is widely believed but so far unproved.

the fact that key conjectures are believed without being proved is not unusual.[21] No scientist can directly confirm more than a small fraction of the experimental results, calculations, and proofs that form the foundations of their beliefs about their subject; few have the skills, and in contemporary science no one has the time. Thus, when you join a scientific community, you must trust your colleagues to tell the truth about the results in their domains of expertise. This can lead to a conjecture being accepted as a fact, but it happens as often in research programs that are ultimately successful as it does in those that fail.

Positive feedback makes a loop "we want to believe in it – we believe in it!"

[redacted] Optimism is good to a degree, but not when it results in outright misrepresentation. Unfortunately, the picture commonly offered to the general public in books and articles and TV shows — as well as to audiences of scientists — differs substantially from what a straightforward reading of the published results suggests. [redacted]

The roots of such behavior are in human psychology.

There is no way we can address these dysfunctional trends without investigating the sociology that has fostered them. If we physicists have the hubris to try to explain the fundamental laws of nature, certainly we ought to be able to think rationally about the sociology of the academy and the counterproductive decision making that plagues our academic institutions.

> Groupthink members see themselves as part of an in-group working against an outgroup opposed to their goals. You can tell if a group suffers from groupthink if it: **(a.k.a. tribal thinking)**
>
> 1. overestimates its invulnerability or high moral stance,
> 2. collectively rationalizes the decisions it makes,
> 3. demonizes or stereotypes outgroups and their leaders,
> 4. has a culture of uniformity where individuals censor themselves and others so that the facade of group unanimity is maintained, and
> 5. contains members who take it upon themselves to protect the group leader by keeping information, theirs or other group members', from the leader.[20]

For science, to overcome the regime of routine functioning, outsiders need to be welcome to the table. According to Lee Smolin, it is not a case in particle physics. It is also not a case in research on education.

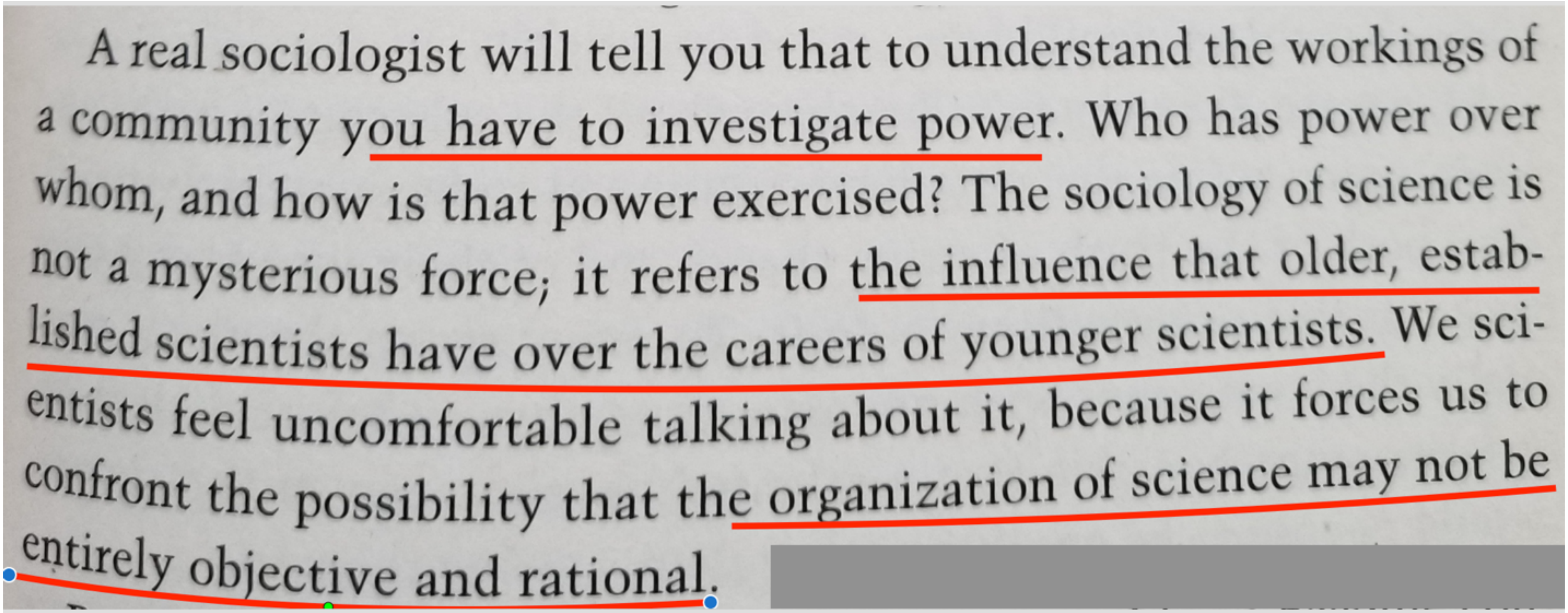
> I have concluded that we must do two things. We must recognize and fight the symptoms of groupthink, and we must open the doors to a wide range of independent thinkers, being sure to make room for the peculiar characters needed to make a revolution.

An openly critical and candid attitude should be encouraged. People should be penalized for doing superficial work that ignores hard problems and rewarded for attacking the long-standing open conjectures, even if progress takes many years. More room could be made for people who think deeply and carefully about the foundational issues weird, the seers

Of course, there is a real risk. Some of them will not discover anything. I am talking in terms of a real lifetime contribution to science. But then most academic scientists, though they succeed in career terms — get grants, publish a lot of papers, go to a lot of conferences, and so on — contribute only incrementally to science. At least half our colleagues in theoretical physics fail to make a unique or genuinely lasting contribution. There is a difference between a good career and an essential career. Had they done something else with their lives, science would have gone on much the same. So it's a risk either way.

The nature and costs of different kinds of risk are issues that businesspeople understand better than academic administrators. It is much easier to have a useful and honest conversation about this with a businessperson than with an academic.

6.


A real sociologist will tell you that to understand the workings of a community you have to investigate power. Who has power over whom, and how is that power exercised? The sociology of science is not a mysterious force; it refers to the influence that older, established scientists have over the careers of younger scientists. We scientists feel uncomfortable talking about it, because it forces us to confront the possibility that the organization of science may not be entirely objective and rational.

The organization of science is the result of the activities of the people organizing science – managers, administrators, officials. Since science has become a large industry, the quality of the management defines the scientific progress.

venture capitalist said that if more than 10 percent of the companies he funded made money, he knew he was not taking on enough risk. What these people understand, and live with, is that you get overall a maximal return, which maps to a maximal rate of technological progress, when 90 percent of new companies fail.

I wish I could have an honest conversation about risk with the National Science Foundation. Because I'm sure that 90 percent of the grants they give in my field fail, when measured against the real standard: Do those grants lead to progress in science that would not have occurred if the person funded did not work in the field?

As every good businessperson knows, there is a difference between low-risk/low-payoff and high-risk/high-payoff strategies, starting with the fact that they are designed with different goals in mind.

In recent decades, the business world has learned that hierarchy is too costly and has moved to give **"weird"** people more power

The leaders of hi-tech companies know that if you want to hire the best young engineers, you need young managers. The same holds for other creative fields, such as the music business. I'm sure that some jazz musicians and old rock 'n' roll guys appreciate hip-hop and techno, but the music industry does not let sixty-year-old former stars choose which young musicians get signed to recording contracts. Innovation in music proceeds at such a hectic, vibrant pace because young musicians can find ways to connect to audiences and other musicians quickly, in clubs and on the radio, without having to ask the permission of established artists with their own agendas.

I once worked on a project with a retired general who had headed a college for military officers and then become a business consultant. He talked about his frustrations in trying to work with universities. I asked him what he perceived the problem to be. He said, "There is a simple but essential thing we teach to every Marine officer, that no university administrator I've met seems to know: *There is a big difference between management and leadership.* You can manage the procurement of supplies, but you must lead soldiers into battle." I agree with him. In my time in universities, I've seen much more management than leadership.

The problem is of course not confined to science. The pace of innovation in curriculum planning and teaching methods is positively medieval. Any proposal for change has to be approved by the faculty, and in general most professors see nothing wrong with how they have been teaching for decades.

In my terms, what Lee Smolin calls a "manager" I would call, a bureaucrat". A bureaucrat will never support a seer.

People with impressive technical skills and no ideas are chosen over people with ideas of their own partly because there is simply no way to rank people who think for themselves. The system is set up not just to do normal science *but to ensure that normal science is what is done.* **(and nothing else - because it is a safe bet)**

The problem is far wider than string theory; it involves the values and attitudes fostered by the physics community as a whole. Put simply, the physics community is structured in such a way that large research programs that promote themselves aggressively have an advantage over smaller programs that make more cautious claims. Therefore, young academic scientists have the best chance of succeeding if they impress older scientists with technically sweet solutions to long-standing problems posed by dominant research programs. To do the opposite — to think deeply and independently and try to formulate one's own ideas — is a poor strategy for success.

Physics thus finds itself unable to solve its key problems. It is time to reverse course — to encourage small, risky new research programs and discourage the entrenched approaches. We ought to be giving the advantage to the Einsteins — people who think for themselves and ignore the established ideas of powerful senior scientists.

It goes without saying that people who are good at asking genuinely novel but relevant questions are rare, and that the ability to look at the state of a technical field and see a hidden assumption or a new avenue of research is a skill quite distinct from the workaday skills that are a prerequisite for joining the physics community. It is one thing to be a craftsperson, highly skilled in the practice of one's craft. It is quite another thing to be a seer.

This distinction does not mean that the seer is not a highly trained scientist. The seer must know the subject thoroughly, be able to work with the tools of the trade, and communicate convincingly in its language. Yet the seer need not be the most technically proficient of physicists. History demonstrates that the kind of person who becomes a seer is sometimes mediocre when compared with the mathematically clever scientists who excel at problem solving. The prime example is Einstein, who apparently couldn't get a decent job as a scientist when he was young. He was slow in argument, easily confused; others were much better at mathematics.

Einstein himself is said to have remarked, "It's not that I'm so smart. It's just that I stay with problems longer."[1] Niels Bohr was an even more extreme case. Mara Beller, a historian who has studied his work in detail, points out that there was not a single calculation in his research notebooks, which were all verbal argument and pictures.[2] Louis de Broglie made the astounding suggestion that if light is a particle as well as a wave, perhaps electrons and other particles also behave as waves. He proposed this in a 1924 PhD thesis that did not impress his examiners and would have failed without the endorsement of Einstein. As far as I know, he never did anything nearly as influential in physics again. There is only one person I can think of who was both a visionary and the best mathematician of his day: Isaac Newton; indeed, almost everything about Newton is singular and inexplicable.

7.

But what is equally important, and sobering, is how often we fool ourselves. And we fool ourselves not only individually but *en masse.* The tendency of a group of human beings to quickly come to believe something that its individual members will later see as obviously false is truly amazing. Some of the worst tragedies of the last century happened because well-meaning people fell for easy solutions proposed by bad leaders. But arriving at a consensus is part of who we are, for it is essential if a band of hunters is to succeed or a tribe is to flee approaching danger.

The progress of science is solely based on the views of scientist about how the progress of science should be managed.

The goal, then, is not to hire the scientist most likely to do good science but the scientist whose acquisition will optimize the status of the department

People with impressive technical skills and no ideas are chosen over people with ideas of their own partly because there is simply no way to rank people who think for themselves. The system is set up not just to do normal science *but to ensure that normal science is what is done.* **(and nothing else - because it is a safe bet)**

We must encourage the opposite, which is to disagree as much as the evidence permits. Given how much humans need to be liked, to fit in, to be part of the winning team, we must make it clear that when we succumb to these needs, we are letting science down.

There are other reasons that a healthy scientific community should encourage disagreement. Science moves forward when we are forced to agree with something unexpected. If we think we *know* the answer, we will try to make every result fit that preconceived idea. It is controversy that keeps science alive, keeps it moving.

As every good businessperson knows, there is a difference between low-risk/low-payoff and high-risk/high-payoff strategies, starting with the fact that they are designed with different goals in mind. When you want to run an airline or a bus system or make soap, you want the first. When you want to develop new technologies, you cannot succeed without the second.

What I wouldn't give to get university administrators to think in these terms. They set up the criteria for hiring, promotion, and tenure as if there were only normal scientists. Nothing should be simpler than just changing the criteria a bit to recognize that there are different kinds of scientists, with different kinds of talents. Do you want a revolution in science? Do what businesspeople do when they want a technological revolution: Just change the rules a bit.

In recent decades, the business world has learned that hierarchy is too costly and has moved to give **"weird"** people more power

I have served on many committees for hiring, tenure, and promotion, but I have never been instructed, as jurors are instructed, on how to best weigh the evidence.

Once at a dinner party I asked people in other lines of work if they were trained in such matters. Everyone who was not an academic but who had responsibility for hiring or supervising other people had been given several days of training in recognizing and combating signs of unfairness or prejudice, in discounting the effects of hierarchy, and in encouraging diversity and independence of thought.

If lawyers, bankers, television producers, and newspaper editors are assumed to need guidance in how to make wise and fair personnel decisions, why do we academic scientists assume we can do it automatically?

This is a very good point. In academia, people automatically assume that they good at managing and teaching, even if they have never had any specific training in the field. When they need to fix a broken car or tooth they go to a professional. But if they need to do something within the walls of their university they just don't think they may not have enough expertise in the matter. Despite the facts proving that very often they are not.

There is affirmative action for people who are visibly different, like women and blacks. But what about people who just *think* differently — who reject mainstream approaches in favor of their own ideas? Should there be affirmative action for them, too?

Everything I've said about hiring is true also of the panels that evaluate grant applications. It also holds for tenure evaluations. These matters are related, because you cannot get tenure in science at a U.S. research university if you haven't been successful in getting grants, and you can't get hired unless there is a likelihood that you will get grants.