WHAT IS WRONG WITH MODERN SCIENCE?

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Abstract. In his argumentation against string theory, Woit shows that the problem is at least in part caused by the modern organization of science. We argue that Woit even underestimates the problem, and that the modern organization of science is a serious danger for freedom of science in general. Especially, Woit's proposals how to change this situation are not sufficient. The status of a modern scientist has to be seriously changed, toward an independence on external "evaluations".

1. Introduction

This article is my reaction to Woit's book "Not even wrong" [1], as well as earlier articles [2], [3]. In these writings, Woit criticizes the current situation in fundamental physics: One particular research direction — string theory — has become the "only game in town". This would not be problematic, if this direction would have been successful — it is, last but not least, what we hope for: The scientific Truth, once found and successfully tested, even should become the "only game in town". But string theory has not been successful as a scientific program. Instead, as Woit argues, it shows the typical signs of a failed research program: It has become more and more ugly and complicate, but, despite this, it has not given even a single new testable physical prediction.

Even this would not be very problematic if this would have been a short time effect: It is not unreasonable that a new proposal will be, some time, in the focus of a scientific community, until it has been established what it is worth. But string theory has its monopoly position in fundamental physics already over twenty years, much longer than usual for a short-time hype, and tens of thousands of articles have been written in this domain in this time — much more than necessary to establish the scientific value of some new proposal. If, despite the scientific failure of a research program, it preserves a monopolistic position over more than twenty years, something has gone seriously wrong.

Now, I do not plan here to argue about the question if string theory has really failed, as a scientific research program, or not. Personally, I simply don't believe in string theory, and the arguments I have heard in favour of it have never been powerful enough to motivate me to learn it. [1] Unfortunately, this prevents me from making professional comments about this domain. But, such is life. As well, my decision not to study theology does not allow me to make professional comments

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[1] The reasons why I have not taken string theory seriously enough to learn it are the following: The main argument in favour of string theory is that it gives a spin two particle $h_{\mu\nu}(x)$ on a Minkowski background $\eta_{\mu\nu}$, which allows to quantize gravity: Such a spin two particle leads to an effective metric $g_{\mu\nu}(x) = \eta_{\mu\nu} + h_{\mu\nu}(x)$. That means, observable relativistic symmetry — the Einstein equivalence principle — appears to be emergent, derived. Given the quantization problems of the alternative, this is quite reasonable. But to introduce, as an unobservable entity, a Minkowski metric — an entity which has been introduced into physics to get rid of unobservable absolute time — is, in my opinion, an absurdity. If we consider relativistic symmetry as emergent, approximate, already the choice between Minkowski spacetime and the Lorentz ether made in the last century was unjustified and has to be rejected.

about the value of modern theology. Despite this, I don't believe that theology is worth to be studied. We all have to make decisions how to spend our time.

Thus, let's remain silent about the scientific value of string theory. Last but not least, the situation in fundamental physics is problematic anyway: This is a highly speculative domain of science, not bounded by experimental constraints. The natural consequence would be a lot of different speculations. Instead, we have de facto only a single speculation. This is not only highly suspect, but even problematic, from point of view of scientific methodology, for string theory itself: The scientific power of a theory, its degree of corroboration, depends on the alternatives which have failed to reach the same results as the given theory. Without the consideration of alternatives, this power is, therefore, not very large.

But, if really, as claimed by Woit, "many physics researchers do not believe in string theory but work on it anyway" [2], then the question of scientific value of string theory itself becomes secondary: What Woit describes here is a situation where scientific freedom is in very serious danger.

Woit also describes the mechanisms which lead to this situation: "Graduate students, post-docs and untenured junior faculty interested in physics beyond the Standard Model are under tremendous pressures in a brutal job market to work on the latest fad in string theory, especially if they are interested in speculative and mathematical research. For them, the idea of starting to work on an untested new idea that may very well fail looks a lot like a quick route to professional suicide" [2], "The current organization of research in physics puts the best young people in a position of needing to quickly prove themselves, to produce results on time-scales of a year or two at most if they want to remain employed. At later stages of their career, even with tenure, the pressure of grant applications continues to discourage many from making the kind of commitment to an unpopular speculative research program that may be needed to make progress" [3].

Woit is not the only one who has observed such pressure. "Younger physics were strongly discouraged from pursuing such questions. Those who persisted generally had difficult careers, and much of the careful thinking about quantum foundations was relegated to departments of philosophy" [4]. "However, it was absolutely impossible at that time to discuss these ideas with colleagues, or even to publish them. An influential Heidelberg Nobel price winner frankly informed me that any further activities on this subject would end my academic career!" [5] "When Alain Aspect formulated his proposal of new experiments on Bell's inequalities, he met Bell to discuss them. Bell's first question was, 'Have you a permanent position?' After Aspect's positive answer, he warmly encouraged and urged him to publish the idea, but warned him that all this was considered by a majority of physicists as a subject for crackpots." [6]

But what I do not find sufficient, and what has motivated me to write this article, are the proposals Woit makes to improve the situation. They seem to be far too modest to change something. At best, they allow to solve the particular problem of fundamental physics with string theory. But the problem is, as I try to show, a more serious one, and not restricted to modern physics. The same mechanisms which have caused the unfortunate situation in fundamental physics — the "publish or perish" way to evaluate scientists, short time working places based on grants, and the continuing pressure of grant applications even at later stages of the career — work in other domains of modern science as well.

The other major argument is that certain terms in perturbation theory become finite. This argument has not impressed me at all: Finite theories can be much easier obtained by lattice regularizations.

What else could motivate me to study string theory? I don't know. That there are no other ideas? Fortunately that's not my problem – I have them.

Therefore, we should expect evenly unwanted effects for scientific freedom in other domains of science too. Now, the string theory speculations are quite harmless for society as a whole. If the universe consists, on the most fundamental level, of strings, loops, or an ether, is certainly interesting, but not really important. But what if a failed research direction obtains a monopoly in the domain of, say, climate prediction, psychiatry, or other domains which, potentially, have a large impact on world economy and politics? Are you sure this has not yet happened?

The prevention of such dangers requires a much more radical and different solution — essentially, a different way to organize science in general. It is, indeed, an absurdity: Everybody acknowledges that freedom and independence of science are important for the whole society. In some states this principle is even constitutional. But scientists have to work in conditions which are extremely unsafe, making them highly dependent on the opinion of various evaluators.

What we need are scientists which are really independent, can decide themself, in a free decision, what are the most promising ideas. But this requires an organization of science where scientists have safe positions, do not depend on grants, evaluations, and other ways to control them. To guarantee freedom of science, there is only one way: The position of a scientist should be as safe as that of a judge.

2. Pressures to conformity

Let's summarize here the various pressures on scientists. Our special interest is how they lead to conformity in the choice of the research direction.

2.1. **Short time jobs.** In the past, the typical job of a scientist was teaching at a university. This was typically a safe job, not only not limited in time, but also with a very low or no risk of being fired in future. Scientific success was not necessary: Even for an unsuccessful scientist, his job as a teacher was safe.

Instead, a job in modern science is, if we think about job security, one of the worst things imaginable. First, the job is often strongly limited in time: A grant is given for one year, two years, three years. After this, the scientist has to look for a new job. Sometimes, this may be a continuation of the old job, but sometimes there is even no hope for such a continuation. If he doesn't find a new job, he has to look for another profession – programmer, taxi driver, whatever else.

In some sense, even working on a daily basis seems more secure: You can expect, at least, that in the average you will find some work some days in a month, thus, you can expect some average income from working in your profession. Instead, the scientist does not know if working as a scientist can give him even a single cent of income next year. If he does not get one of the few jobs in this domain, he has to change his profession.

I do not want to whine here about the poor fate of scientists: It was, after all, their own choice to become a scientist. It was well-known before that the competition will be strong, and scientists who win in this competition are sufficiently well-paid and honoured. Thus, if this type of organization of science would lead to better science, I would not object, as well as I do not argue about job safety in professional sport.

But does this competition for working places lead to better science? What are the consequences of this unsafe situation of the particular scientist for the progress of science?

As a consequence of the situation on the job market, the scientist has to care a lot about his future jobs. This leads to a lot of loss of time — time necessary to write various applications for jobs, grants, and so on. But this is not the main problem. There are more subtle dangers of such short time jobs for scientific freedom: The scientific communities are, in each specialization, not that large. People know each other. Now, if you, as a young scientists, have to think about a future job, it becomes very dangerous for you to argue against mature scientists: May be, your next job depends on their opinion? Now, revolutionaries, in science as well as in other domains, are usually young man, who like and have to fight against the establishment. But who likes to start a scientific revolution, knowing that he has, in one year, to ask his worst enemies in the scientific fight for a job?

Moreover, even if you, nonetheless, try to start such a revolution — the only result will be that you have to look for a new profession next year. Your articles, even if you have succeeded to publish them (which is not easy, because they have to survive peer-review from the established scientists in this domain), may be simply ignored by the establishment.

But this is not the effect which is of interest here. There is another one: It is reasonable to choose, as a specialization, the direction of scientific research with the largest number of job offers. In the case of fundamental physics, this is, today, string theory. The problem is not that it is string theory: If it is, tomorrow, another direction, the situation will not be better — the problem is that young scientists are forced to study the direction with most job offers. After this, once they have invested some of their best years into this direction, they will already continue to study it later, when they are able to make job offers themself.

2.2. **Publish or perish.** A more subtle but evenly fatal role is played by the criteria which are typically used to evaluate scientists. These criteria become especially fatal in combination with the problem of short-time jobs: Once these criteria are used to decide about your job next year, you have to care about them, even if you consider them as nonsensical.

At a first look, the number of publications seems to be a reasonable criterion for the evaluation of a particular scientist. But, of course, any criterion of evaluation, if known by the person which is evaluated, has side effects: The person we want to evaluate tries to optimize the criterion, instead of optimizing what we really want — namely good science. Thus, if we use the number of scientific articles as a criterion to evaluate scientists, we obtain much more scientific articles. And, as may be expected (simply because scientific value is nothing we can produce at will), the average article contains much less scientific value.

More papers with less quality is something very bad. To find the papers which are worth reading, the scientist has to read much more. He has to learn to evaluate the content of a paper, to decide if it is worth reading, at a first glance, without looking at the details. This is much easier in the domain of your specialization, much harder in other domains. Thus, a natural consequence is that you concentrate your readings on the papers in your specialization (last but not least, there remains enough you have to read). The consequence is more specialization than necessary, less understanding between scientists of neighbour domains, and an increasing possibility that worthy papers will be ignored, especially if they are not part of the mainstream.

But this is, again, not the effect we are especially interested here. You know that, to have a chance for a next job, you have to publish some papers. How does this influence your decision about your working direction?

- You certainly will not choose a direction of research which promises many years of hard work without publications, but which, if successfully finished, leads to a scientific revolution. That would be nonsensical – you cannot finish this project anyway, if you loose your job next year.
- To publish a paper is not easy. Many papers will be rejected. But, once rejected, you can try to publish it in another journal – at least if there is another journal interested in papers of your direction. It is, therefore, very

helpful to choose a direction where many journals are available to publish your results.

- If your paper will be published or not, depends on peer review. Reviewers are not perfect. They tend to give better reviews for articles in their own domain of research. That's easy to explain: It's much easier to read and understand articles in your own domain, much less work. But that it is easy to understand the article is an important criterion for publication: Thus, a paper, which is easy (for you) to understand, seems to fit the criteria for publication in a much better way. And this is not the only effect which leads to a preference for articles in the own domain. Last but not least, despite the effects described here, scientists have some remaining freedom in their choice of specialization. Thus, the their direction will be one they consider as more promising. Thus, they will consider articles in their own domain as more promising as well. And even if their own choice was forced by the external pressures we study here, people tend to internalize such things, and to identify themself with the chosen research direction.

  As a consequence of such effects, it will be easier to become published, if your papers will be reviewed by scientists working in your domain. Thus, if you choose a domain with many people working in it, the probability to get reviewers from your domain, and therefore, the probability to receive positive reviews, increases.

The consequence is that it becomes even more reasonable to work in a direction where many other scientists work as well. This essentially increases the probability of successful publication of your results.

2.3. **Increasing the impact.** With modern information technology, it becomes possible to evaluate not only the number of articles of a given scientist, but also how often his articles are quoted by others. Of course, if an article is quoted many times, this indicates that it was a good article. So, why not use this possibility to evaluate the importance of the articles somebody has written?

The general problem of this criterion is similar to that of the number of articles: those who will be evaluated know about this, and try to improve this criterion. This seems hard, but there are, of course, some methods how to manage this. A particular one are personal relations based on the "I cite your papers, so, you cite my papers" principle. Another adverse consequence, similar to the appearance of lots of worthless papers because of "publish and perish", is the appearance of uninteresting, irrelevant citations.

But there is also another influence on science, related with a simple way to increase the probability of being cited: Simply start to work in a domain where many other scientists work too. Each person working in your domain is a person which can possibly read your papers and cite them. To start, instead, a new scientific domain would be professional suicide – however important, your paper will be read and cited only by those few who join your revolution. And even this needs some time. But if, in the current conditions, after ten years people begin to recognize the importance of your revolution and start to cite your paper, you are already working as a taxi driver.

Thus, the evaluation of scientists based on the impact of their papers has similar adverse consequences as the "publish or perish" criterion. And, especially, it also leads to further concentration of the research.

2.4. **Participation in conferences.** Another criterion for scientific success in the participation in various conferences.

This criterion is connected with the “publish or perish” criterion, because giving a talk at a conference is usually related with a publication in the proceedings of this conference. Such a publication does not count as much as the publication in a journal, because there usually is no (or only a less serious) review process involved. Nonetheless, it counts.

But, even if you don’t give a talk in the conference, and do not publish something in the conference proceedings, conferences are a nice way to spend your time: You travel to foreign countries, without having to pay for this. There is usually also some cultural program around the conference. Thus, it is something many scientists like to do.

Conferences are also an important place to establish personal relations with people you may need in future – especially with those who, probably, have to decide about your future jobs. From this point of view, not to participate in conferences is another method for scientific suicide.

Now, if you want to increase the number of participations in conferences, there is a simple way: As the reader will already guess, there are much more conferences in the domains of science where many scientists work. Thus, if your work in such a domain, the list of conferences which may be interesting for you becomes much larger.

2.5. **Getting grants.** If you have succeeded in the heavy competition for a secure, long term, tenured job, you are, nonetheless, not free to do what you like as well. Your position at the university depends on various things, but especially on your possibility to obtain grants for yourself and your university. It depends on these grants how many young scientists you can pay to work for you. Parts of the grant become part of the budget of the university. Therefore, the management of the university is looking carefully how many grants you obtain, and, if you have not enough of them, there are methods to tell you that you are wrong.

Thus, one of your obligations, if you have a tenured position, is to fight for grants. To obtain a grant is, essentially, the same game as to obtain a job, and the consequences are, as you can imagine, quite similar. The danger for you, if you fail to get grants, is not that big — you have, last but not least, a tenured position. Everything else remains unchanged: You have to care about publications, the prestige of the journals of these publications, the number of citations. Now, not only your own publications count, but also those of the people working with you. So, if somebody decides to try to start a scientific revolution, which promises, if successful, a first publication only after ten years, it is in your interest to tell him that he is wrong. Last but not least, it is also helpful to organize or participate in conferences.

And, of course, to get the position you have, it was necessary for you to work in a domain where many other scientist work. You have spend a lot of time learning all that stuff, have already lots of own publication in this particular domain. And, last but not least, you are no longer in the age of a young revoluzzer, you have family.

Given all this, how large is the probability that you throw all that away and start to work on a scientific revolution?

2.6. **Summary.** We have considered the major ways of control of scientist in the modern organization of science. And we have found, that they all have adverse side effects on the scientific process. Especially problematic is that they all lead to a concentration of scientific research in a few number of directions, with large numbers of scientists working in these popular directions. The young scientist is forced to choose such a direction, because it offers more working places, more journals to publish your results, reviewers who evaluate your papers more favourably because they work in the same domain, more scientists who can possibly cite your papers, more conferences, thus, more possibility to give talks and to publish in conference proceedings. If you have a tenured position, the monopolistic direction offers you, as well, more possibilities to get grants.

These are sufficiently strong social and economic pressures: A young scientist has to look for another profession if one does not follow them. There is no place for outcasts in the modern organization of professional science. Those who get a tenured position have already invested most of their best years into the monopolistic direction — too much to change it.

Such pressures into the direction of an already existing monopoly are very dangerous for science. They lead, obviously, to a preservation of existing theories, thus, to dogmatism, and prevent scientific revolutions. This is, clearly, not what we want from science. In the extreme, the resulting monopolistic "scientific theory" would be better named religion.

Science should be, instead, organized in a way which minimizes the pressure to conformity.

3. What to do?

What can we do to change the situation? Here are some extracts from the proposals made by Woit [2]:

(1) ... theorists should publicly acknowledge the problems ...
(2) Senior theorists ... should seriously reevaluate their research programs ...
(3) ... theory groups should try and identify young researchers who are working on original ideas ...
(4) Funding agencies should stop supporting theorists who propose to continue working on the same ideas as everyone. ... Research funds should be targeted at providing incentives for people to try something new and ambitious, even if it may take many years of work with a sizable risk of ending up with nothing.

Now, I don't want to argue about the question if string theorists acknowledge the problems in their domain. Some do, some don't, as in every domain of human behaviour. The second and third proposals are proposals to do something against the own interest, as we have already found: For a senior theorist it doesn't make much sense to change a research program which, for his personal career, was a success. Moreover, to change it does not promise anything good – less papers, less grants, and so on, for a long time. As well, for theory groups it does not make sense to invite young researchers who cannot promise to finish several papers in the next two years.

What about the funding agencies? They are, in principle, in a position to support some non-mainstream research. But it is not the funding agency which is taking a risk, but the researcher. The funding agency gives the money, and receives, at best, a few lines of type "this research was supported by ... grant Nr. ..." in the resulting papers. The researcher, instead, has to look for a new job, and, if the research paid by the grant ends with nothing, it is he who probably remains without a job.

In other words, as long as the funding agencies pay only for a few years, the problem remains as worse as before: The scientists who apply for the grant have to care that they are able to publish something during these two or three years, not because the funding agency wants to see some papers, but because the scientists needs a job after the grant.

Thus, the proposals made by Woit, even if realized, change essentially nothing. All of the *institutional* pressures to conformity, which we have found in the previous section, remain unchanged.

4. Possibilities to control scientists

If the standard methods to control scientists have adverse side effects, are there better ways to evaluate the abilities of a particular scientist? Methods, which do not lead to adverse side effects, and which, especially, do not lead to monopolization, do not prevent the consideration of lots of alternative proposals? Unfortunately, there are no such methods, at least the author is unable to see such methods.

The basis of the problem is the general problem of side effects of control: Whatever the criterion used to evaluate people, as long as it is known (and this cannot be prevented in a sufficiently open society) people will try to invent methods to improve their ability to meet the criterion, even if this does not lead to an improvement of what we really want to obtain.

Fortunately, this general human problem does not make control impossible or useless. There are a lot of domains of human behaviour where control works nicely. If we buy something in a shop, we can measure how much we buy – how many pieces, how many bottles, how many kg. And we can, at least, see what we buy, to evaluate the quality.

But how to evaluate the quality of a scientific paper? The problem is that you have, at least, to be a scientist yourself to be able to evaluate it. But even this is not sufficient – the scientific value of the most interesting papers, those which start scientific revolutions, is often recognized only in the next generation. Thus, to evaluate it, we have, at least in principle, to wait for the next generation to see what survives.

But let's, for a moment, forget about this, and assume that other scientists are able to evaluate the value of the scientific contributions of a particular scientist. Then, the ways we use today to evaluate scientists are already fine: We evaluate how other scientists evaluate the contributions of this particular scientist. They do this by giving positive or negative peer reviews for his papers, by inviting or not inviting him to plenary talks at conferences, by citing his papers. And all this is already used today to evaluate a scientist.

The problem is that we cannot use the opinion of other scientists to decide about something important for him, like a job for the next years, without creating a dependence of the evaluated scientist on the opinion of other scientists. This is something which we cannot prevent in principle. It is a simple logical consequence: We decide about his job looking how other scientists about him. It follows, that the opinion of other scientists is very important for his future job, thus, for his whole life.

But this is already a dangerous dependence, a dependence which is the root of the most dangerous adverse side effects:

- Scientists tend to think better about those who work in the same direction as they do. The reasons we have already mentioned considering peer review: First, simply because they usually think that their own direction is the better one. Second, because they better understand results in their own research direction, and humans naturally prefer what they understand. This is an excusable human weakness, but if it is combined with a dependence of young scientists on the opinion of other scientists, it leads to the problematic concentration of the research in a few domains with many scientists working in it.

- Moreover, the established scientists do not think very good about revolutionaries. That's not necessarily bad – often enough, this opinion is justified, and the scientific revolution appears to be a scientific failure, for reasons the established scientists have been able to see. Not every scientific revolt becomes a successful scientific revolution. But it is important that young scientists have the possibility to start scientific revolutions, with the consequence that established scientists will not love them.

Of course, for a scientist it is much better to depend on the opinion of other scientists than on stupid decisions of dictators. But for science itself, the difference is not that large. Whatever the direction which obtains a monopoly, not because of its scientific value, but because of institutional mechanisms, it will be, with high probability, wrong, simply because the number of possible scientific theories is very large, and, without testing the alternatives, the choice will be, essentially, accidental.

4.1. **The factor of time.** The other big problem of control of scientists is the problem of the time scale.

It consists of several parts: First, the most interesting, important ideas, the ideas which make scientific revolutions, need a sufficient large time to be worked out into viable theories. This time scale is larger than the one to three years of a grant. It may be, as well, ten or twenty years. For really independent scientists, it is quite typical that they spend their whole life studying the approach they think is the most promising one.

Then there is the time one needs to learn. In different professions, the time we need to learn how to do things we should do is very different. Some jobs can be done by unskilled workers, other jobs need special education for one or two years, others require five years at a university. The job of the scientist needs even more – learning during your whole life. The university gives only a base. What you need in the domain of your specialization you learn later, in your working time. In principle, every important paper in your domain is new stuff you have to learn.

This makes short time jobs much more problematic. Much of the time in a three-year job is spend for learning things which are important for the very special problem you try to solve in these three years. If, after this, you have to do something very different, simply because you have another job, the time for learning these things has been simply wasted. Of course, much of the time of life of a scientist will be wasted anyway for learning things he will never use: The scientist cannot know before, what exactly becomes useful in his later life. Moreover, sometimes the seemingly wasted time appears useful in an unexpected way: It allows the scientist to see similarities between very different domains, similarities which allow to apply techniques from one domain in the other one. But to enforce the waste of time for learning a new domain by an institutional mechanism, which forces scientists to change their job every two or three years, is clearly nonsensical.

The time you need to learn something increases also the problem of evaluation. The problem is that, at first, you have to learn. And, as long as you learn, you are usually unable to find new results. Some people need more time to learn, but, as the result, know the things much better. They may be, later, able to find something deep, really new. Others are able to do some things without understanding them deeply. They will be able to produce some papers in much shorter time, but the scientific value of their results will be low. In other words, a reasonable evaluation during the time of learning is nonsensical.

Last but not least, there is the time which is necessary for the evaluation and acceptance of the results by the scientific community. This time is short, if the result is part of the mainstream. But, with the distance from the mainstream, it increases. For a true scientific revolution, this time may be quite long as well – possibly, the current generation of scientists will die without accepting it, and only the next generation will accept it. This time scale is certainly too long for your next application for a new job.

4.2. **Scientific honour – the better way of control.** Fortunately, there is another method to motivate scientists to do what society really wants. This is a very old, and very successful method: Scientific honour.

An important part of the scientific education consists of learning the names of those who have made important contributions in the past. These names are used to name theorems, theories, functions, physical effects, and whatever else in science seems worth to be named. The naming usually follows scientific priority. Questions of priority are considered very careful, and with great interest. Why all this? The aim is obvious – to give honour to those scientists who have made important contributions to science.

To find something new, which will later be associated with the own name – that's the dream of many scientists. Essentially, a human weakness – the love for honour – is used here to motivate the scientist to do the right things.

What is the difference between this way of motivating scientists and the current "publish or perish" science? That's quite obvious: Uninteresting papers will never give you scientific honour. This will be given only to really useful contributions. Then, there is almost no problem with the time scale: Scientific honour lasts for centuries, and it doesn't matter that much if a scientist receives this honour only very late. The incentive is the right one: A scientist interested in honour is ready to spend many years of intense research, if he thinks that the result can give him such honour. Especially, he will not be afraid to start a scientific revolution – instead, starting such a revolution is the thing which promises, if successful, the largest possible honour.

Last but not least, we should not forget that the payment in form of honour is very cheap. It is not completely without costs. Society has to spend a lot of time – and time is money – to establish priority correctly. That's not always easy, and there is a lot of disagreement and argumentation about priority in the scientific community. But in comparison with the costs for the other forms of evaluation of scientists, especially the hidden costs related with the various adverse side effects, establishing priority is really cheap.

If we think about the question how to motivate scientists to do what society needs, we should not forget about another very important motivation: The interest of the scientist to understand our world, to find out some truth about Nature. This is a very important motivation. It may be even more important than the motivation by scientific honour: I would guess that a scientist without such motivation will never be successful.

Thus, there are two strong incentives for scientists to do what society really needs: The personal interest of the scientist to find out Truth about Nature in itself, and the motivation given by scientific honour.

4.3. **Psychological effects of competition based on fear.** I'm not a psychologist, but common sense is sufficient to distinguish two major types of competition: Those with a positive motivation – the winner gets something, the loser gets nothing – and those with negative motivation – the winner preserves his position, the loser really looses something. In the second type of competition, the main motivation is fear. And common sense psychology tells us as well that motivation by fear is much worse. Worse not only for those who compete – to experience fear is not nice, much worse than to experience hope to win. But worse also if we consider the results. Slavery, where the motivation to work is based on fear, is inefficient in comparison with paid labour. And our common sense psychology also tells that this effect increases with the intellectual requirements of the job. With slaves, only stupid, monotonous work can be done. The typical job in the industry cannot be done by slaves. And we can expect that the difference is the greatest for the jobs with the highest intellectual requirements – like those in science.

Now, the competition to find scientific Truth, or to receive scientific honour, is a positive one. Instead, the competition for a new job next year, where the loser has to look for a new profession, is a negative one. The conclusion is clear: That's not the type of competition which leads to high intellectual achievements.

5. My proposal

I'm a libertarian, and reject the state. This is, clearly, a minority position. Therefore I make two proposals here: A libertarian one, and an alternative for those who believe the state is necessary to pay for science.

The libertarian proposal would be a quite simple one: No tax money for scientific jobs.

The consequence would be a return to the old way to do science: Scientists work as teachers in universities and do science as a hobby, to increase their reputation. Universities, who live from the money paid by students, will support this – if their teachers are well-known, successful scientists, this increases the reputation of the university, so that they can increase their prices or their number of students. But not all university teachers have to be good scientists – some have to be good teachers. If one does not have success in science, one will not be fired, but has to care more about teaching.

The proposal to improve state science is also a very simple one: Let's transform all short time jobs in science into permanent jobs. Point.

Is this sufficient to make scientist independent? It is. Without the fear of unemployment, scientists will do what they really like – to work in the direction which they find most promising. Even if the job is not well-paid, and there is some competition for better paid jobs, it is not likely that the results are dangerous for the freedom of science. If, for getting these better paid jobs, he has to do things he doesn't like – to work in a direction which he considers to be hopeless, to give up his own research which, he hopes, will lead to a scientific revolution, to remain silent if authorities talk nonsense – will he do it? The situation of the scientist is, in this point, different from that of a typical worker: The average worker is doing things he does not like very much, things he would not do without payment. Instead, the independent scientist is doing exactly what he likes to do, he is doing this even without getting paid, in his free time. This is nothing one easily agrees to give up for a little more money.

Thus, with a safe job as a background, the positive motivations of the scientist – to find scientific truth, to receive scientific honour, and, last but not least, simply to do what he likes most – seem strong enough to protect freedom of science.

5.1. **The case of Soviet science.** A small but safe salary was, essentially, what scientists in the Soviet Union have received. Jobs have been safe in all domains, but for ideological reasons (leading role of the working class) scientific workers (being part of the intelligence) have received even lower payments than average workers. What was the result?

In most domains of economy, the result of such safe, low paid working places was fatal. The economy of Soviet union was, in most parts, that of a Third World country.

The situation was surprisingly different in those domains of science, like physics and mathematics, which have not been fatally influenced by Marxist ideology. In these domains, Soviet science was able to compete with the leading scientific powers of the world. Many important scientific results are associated with Russian names. And, after the crash of Soviet Union, many former Soviet scientists have been able to find good jobs on the Western scientific job market.

5.2. **How to identify the best scientists?** The purpose of the current permanent evaluations is not only the motivation of scientists to do good science. It is as well to find out those with the best abilities to do science. What happens with this purpose of the control?

Now, the abilities do not change very much in time. Thus, they can be established as well, with sufficient accuracy, at the university. Those who finish the university with the best results, those who are able to win the first competition for the life-long working places, are sufficiently able to do science.

Of course, this initial evaluation has its problems as well. What is measured is the ability to learn, not the ability to do science. These are different things. Nonetheless, what is measured in later evaluations is also not the ability to do science, but the ability to publish a lot of papers in short time. As we have argued above, this is also not the same.

Last but not least, there will be, anyway, another competition: Those for jobs with better payment. Such a competition is not dangerous: Nor from psychological point of view – it is a competition for something positive – nor from point of view of the other arguments considered here. Good scientists usually don't care that much about money. If they have a possibility to do what they like most – research in the domain they like – they will not apply for another job, if that means they have to do something they don't like, even if it is paid better. Thus, this type of competition for better jobs, with a safe job in the background for all participants, will not lead to a deformation of science.

5.3. **The costs.** What about the costs of our proposal?

It costs nothing. In the libertarian variant, the taxpayer has to pay nothing at all for science. But the etatist proposal does not cost much too. Instead, it even allows for some economy. Increasing the time of a job from two years to forty years does not mean that the costs increase by a factor of twenty. The costs depend on the overall number of jobs in science at a given moment, and it doesn't matter if these jobs are for two years or forever. But there are several points which can lead to economy:

- For the typical scientist, job security is of high value. He does not like to care about getting a new job – he is already doing something he really likes to do, namely, working on scientific problems. Typically, he prefers to spend his time doing science. In principle, this additional value for the scientists can be compensated with lower payment, thus, can be used for economy.
- Job security is not all the scientist obtains. Scientific independence is itself a value, for each particular scientist. Especially, he is now free to choose the direction of research which he considers to be the most interesting and promising one. Thus, in the average, he will study problems he likes much more than the problems he has to study today. This is a value to him (which, again, may be compensated by lower payment), but also a value for society: The success in science is closely related with the personal involvement. Thus, if scientists do things they really like, the probability of success increases.

- The costs of evaluating the applicants for jobs will be reduced. Evaluators have to evaluate, and they don't do this for free. Even if they don't receive extra payments, they do all this in their working hours. And the scientists who are evaluated also spend a lot of time to prepare themself for the evaluation. And for this time they also get paid. Not? Maybe they do all this outside their working hours? Maybe. But in this case you can be sure that, without control, they will spend the same time, outside their working hours, for doing science instead. Because this is what they really like to do, much more than reading or writing documents for various evaluations. Here, increasing the time of a job from two years to forty years can give, indeed, a heavy reduction factor of the costs, close to twenty times.
- The hidden costs for the adverse side effects of "publish or perish" science will be reduced. The number of uninteresting papers will be reduced, thus, less time will be lost for writing them, reviewing them, publishing them, and reading them.

Thus, to obtain something of very high value for society as a whole – to obtain really independent scientists – we don't have to pay more than today. Instead, we can have the same number of scientists, in much better quality (simply because they are now independent), working much more efficient (no time lost for evaluations), and all this for even less money.

5.4. **The chances for realization.** But, in fact, I don't believe that the etatist proposal may be realized. States like to control, and they do not like to give up control which they have already obtained. Why would those who control science today give up their power?

If I don't believe that it may be realized, why do I make such a proposal?

In fact, it is an illustration that states are unable to do something useful. Money given to the state are lost money. They do not lead to progress, even if spend for science. The "science" of Marxism-Leninism was the best example. The case of string theory is, in comparison, a quite harmless one – string theory is, at least in part, beautiful and interesting mathematics.

It is much more probable that the effects of state-controlled science are much worse. I leave it to the reader to think about other domains of actual, modern science, which can be used as examples.

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