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a scientist's life

Forget everything you think you know about science and scientists. It’s a bunch of crap. Before we dive into the controversies that surround science, I am going to give you a peek into the making of a typical modern biologist. This is important because without some context of what scientists do, it is hard to understand how they must feel about the current denial and misuse of science to push social and political agendas. I will focus shamelessly on the US for one simple reason: the US is a scientific goliath. It produces about the same number of scientific papers per year as all of Europe combined. Some 63% of the top 1% of all research papers in the biological sciences, measured by citation, are produced in the US. Simply put, the US produces more science that is cited more often by other scientists than the rest of the world. There is no significant biomedical research being published in any language other than English. Good science is being done elsewhere; there is just more of it being done in the States.

In the beginning

A budding biological scientist is faced with a major decision at some point in her undergraduate years: do I want to be a physician or do I go into research? Looking around the classroom and seeing the spreading plague of premeds is an unsettling experience for those who are born to question. Our future scientist finds herself liking the rigor of physics classes and organic chemistry. She loathes the grade whores who crowd professors at the end of class. She might even be tempted to do a little grade whoring herself, but feels uncomfortable with it. But very few labs will take undergraduate freshmen and sophomores in. They are seen as being too young, inexperienced and unreliable. Meantime, they must endure their colleagues’ shallowness and eke out an education in classes designed for premeds.

Scientists are born in the lab. It is there that an undergraduate is expected to learn quickly and accomplish something.

Scientists do not get extra credit for having undergrads in their labs and no one gives them significant grant money to spend their time with undergrads. It is the place where the coddling of high school and the undergraduate classroom disappears. Students who are serious about learning how to do science will succeed in this environment. Those who are just looking to pad their resumes with another line and get themselves a letter of recommendation for medical school will rarely contribute anything significant and typically don't last more than a semester.

There is no golden path of social and economic avarice laid out for scientists. They get the bug for research out of curiosity and a drive to do something that no one else has ever done. While somewhat misguided, the thrill of your first experimental results is like crack to a future scientist. Studying and jockeying for grades will be shed for a passion for experimentation.

Our student has now decided that she has no desire to stitch wounds, prescribe blood pressure medication or provide strippers with bust enhancement surgery. Instead, she finds herself spending more and more time in the lab and decides that getting a PhD in biomedical sciences is her path in life. Instead of six figures' worth of debt, she can get a free education … actually, tuition is free and she will get *paid* to go to graduate school. No annoying patients and no old people complaining about their gout, and you get pocket money instead of debt – where do you sign up? The promises of graduate school sound like an ad for the military, and might be nearly as divisive. 'Pay' is really a bit of an exaggeration for what students are actually getting for their effort. In reality, they will earn less than minimum wage per hour. Their salary is often not enough to pay the bills, requiring student loans or money borrowed from family to make up the difference.

At some institutions, graduate students are the primary source of research results and thus bring in the bulk of the money. Research results equal grants. At many universities and institutions, a great deal of the university rides on the money brought in by biomedical research grants. Few realize that the philosophy, history and other humanities departments at universities don't bring in a lot of dough. When a biomedical researcher brings in a grant, a percentage of the grant goes directly to the university for 'overhead' costs. This can be more than half of the grant, which pretty much sucks. What that means is that science research floats the nonprofitable departments and in many cases is the only reason they can persist. Professors in these departments are often paid as much as biomedical researchers – sometimes more. A close look at many universities reveals that the highest-paid faculty staff are often the ones who have been there the longest and brought in the least money. There is no greater waste of time, money and space than a professor emeritus at a university who is taking a paycheck but not contributing to the department he is in. These professors don't teach much or bring in money, but can draw big salaries. Perhaps they will write a book of the collected wisdom of a career in academia. That's certainly valuable right? Not usually. When these books are written, the income from them goes right into the professors' pockets, so it doesn't subsidize their salary, and the books usually fall somewhere between stuffy and sucky (with a handful of notable exceptions). Advice to university administrators – burn the dead wood.

So, our student applies to grad school and is off to a life as an academic researcher. All is well – her parents can brag that she is curing cancer and she will lead a comfortable life ... Right? *Wrong*! The hurdles are now lined up and chances are she won't make it to the end of the race. First is graduate school. Getting into a good graduate program is key, because a PhD from Podunk U. is virtually useless. To get into a good graduate program, a student will obviously have to have a combination of good grades and admissions test scores. But she will also have to have significant research experience *and* will have to have actually proven that she is competent in the lab. The best way to show an admittance committee that you are competent enough to succeed is to have a recommendation by a respected researcher. Most grad school applicants don't have authorship on research papers, so a written description of the project that they worked on and the recommendation is what they have to rely on. If one does undergraduate research in the lab of a big-time scientist, then more weight will be given to the recommendation. This is not by design – it is just human nature. If those sitting on the committee know the recommender, the recommendation will be trusted and the student will be given a close look. This again shows that the undergraduate institution is key: Dr Big-Time Scientist is unlikely to be working at Podunk's sister school out in rural Nebraska.

Let the games begin

Once in graduate school, the new student will settle in for a year of what is best described as hazing. A typical grad student will have to split her time taking classes, studying for tests, teaching undergraduate classes, and doing research as a rotation student for the first year. It should be noted that this is likely to be the only teaching experience she will ever have before she is actually required to organize and teach lectures to large classrooms of students as an assistant professor. It is completely normal for a first-year student to leave before 8 in the morning and not come home until 11 or 12 at night. As a rotation student (or 'roton'), the student spends several months working in a lab on a project. The purpose of the rotation is similar to that of medical school rotations. They are an opportunity for the student to decide if the particular research projects going on in the lab are best suited for them and whether they will be able to work with the particular personalities in that lab – most importantly the primary investigator, or PI, who is running the lab. At the same time, the rotation serves as a long job interview for the student. The PI and the people in the lab are judging not only how well the student does science, but whether the person's personality jibes well with that of the lab. After all, the student is likely to spend 5–6 years or more working in that lab – the longest and biggest career commitment that the student has ever made.

Our grad student teaches a laboratory class associated with a larger lecture class, which is in turn taught by a professor. She serves as a teaching assistant for that class, running review sessions, giving practical lectures, grading tests and homework and holding office hours during which premed students will try to suck out information on what will be on the test. The very students that she shared classes with just three months earlier as an undergraduate are now coming to her for information.

During this first year she will also have to take classes. It is pretty normal for grad programs to require their students to get a minimum of a B in any class they take. Sometimes these classes are taken with medical students, who at this point have gone through a welcome transition where they are more comfortable that they are going to become doctors. But our itinerant roton is torn. She feels some responsibility to her grades, but is pressured to spend as much time as she can in her rotation labs doing research, learning new techniques and impressing her rotation PIs. She also feels a responsibility to her own students, but more often than not, if someone is going to suffer, it is the undergrads, who will have little to say about what happens to the roton in the coming months.

The devotion of scientists is rarely in question. What is in question is what they are devoted to. Scientists will suffer at the hands of their own passion and drive for their entire career. Relationships suffer in particular, and the first year of grad school should be an eye opener to budding scientists that something is not right. Relationships come and go in everyone's life, but what is interesting amongst scientists is that devotion to the craft of being a scientist is a primary reason for relationships to crumble. If there is anything wrong with your relationship when you enter grad school, the pressure and long hours will bring it right to the surface. Those students who are not in relationships will often hook up with their fellow students, who can at least empathize with the new lifestyle that they have entered. Still others, despite being expressly forbidden from doing so, wind up dating their undergraduate students. This is actually more common than people would suspect, and there is little that grad schools can do to prevent it. Every graduate student knows a colleague who has dated one of their students – or worse, a professor. This too is strictly forbidden at all universities, but it happens all the time. Some universities have made concessions to situations where a student and faculty member can see each other as long as the faculty member has no say over the student's fate. Here's an interesting example.

In 2003, Arnie Levine, the president of Rockefeller University, was dismissed from his position and lambasted in the *New York Times* for dating a graduate student at the University. Dr Levine is a well-respected member of the research community whose life's work was rewarded with a prestigious position at the University in 2000. What was his crime? He showed up in a public place with his girlfriend, who, by the nature of his position, was someone he should not have had an intimate relationship with. Unofficially, there were a series of events that had led up to his dismissal, but what the *New York Times* failed to report was that this sort of relationship happens at universities *all the time*. They also missed the fact that these relationships are tolerated as long as faculty members and teaching assistants can maintain a reasonable level of plausible denial. Many research institutes have gone so far as to settle harassment cases out of court to save face and preserve the careers of professors who have had inappropriate relationships. In the absence of complaints, it is more typical for universities to turn a blind eye to these indiscretions. Dr Levine removed the element of plausible deniability, thus risking embarrassment to the university, and lost his job because of it.

Once our roton has made it through her first year without engaging in an inappropriate relationship, has found a lab and is starting a project, she will settle into her new life as an indentured servant. Different graduate schools have different policies regarding what is required of a student to receive a PhD. It is fairly common for a student to work on a project for a year and then submit a proposal for a thesis project. At this point the student will put together a committee of professors from the university and perhaps one from an outside university. In effect, the student has a primary advisor (the primary investigator for the lab she is working in) and a committee of other professors who must approve both the student's proposal and ultimately her final thesis. The committee serves two capacities. First, it provides advice to the student on a range of topics, such as experimental design and how to deal with an unreasonable thesis advisor. Second, the committee members serve as advocates for the student so that the advisor cannot hold the student hostage in graduate school for too long or ask her to do anything that they perceive as unreasonable. Sounds like a good system right? Bzzz! Sorry, thank you for playing. We have some nice parting gifts for you.

The committee members do indeed serve as effective advisors for experimental design and will generally give the student good advice on achieving success in the research project. However, when a student has a personal or professional problem with their advisor, the committee is usually powerless to do anything about it. Strike that – the committee isn't actually powerless. It's impotent. The committee would like to do something about an advisor who is being unreasonable, but many times cannot do anything because if they do dare strong-arm the primary investigator, they risk putting themselves in a bad position with a fellow faculty member. This is particularly true of committee members who happen to be junior faculty looking to get tenure in the coming years. There are of course exceptions, but this is usually the case. So our student, in many cases, will feel like she is out there on her own. The system is set up so that students go through a stage where they feel helpless and under-appreciated, and see no end in sight.

There is a honeymoon period in which our young scientist and her new advisor will work well together. Everything is new and potential abounds. It is in the best interests of advisors to get as much as they can from students once they are productive in the lab. This translates into research papers. Most schools require their students to have at least one research publication before they graduate; some require two or three. This is so that the students can be competitive in getting grants as post-doctoral fellows and to make sure that the lab and institution are getting something out of the students they train. What this does in effect is ensure that most students take projects that are likely to succeed and in most cases will be of little consequence to their field. In essence, they do boring work to ensure success.

The situation is quite simple. The currency of biomedical research is the research paper. There is an adage in science: publish or perish. There could be no truer words. Most researchers' salaries are paid from research grants. Some are paid by foundation grants or venture capital money dumped into the little biotech company they have on the side, but the overwhelming majority are paid with taxpayer dollars through the National Institutes of Health, the National Science Foundation or other government-supervised trough. The funding system is set up so that only about 7–12% of research grants submitted are actually funded by the NIH, and this has become tougher over the last couple of years. Writing these grant applications is a time-consuming, frustrating and ultimately unrewarding process. I say unrewarding, because the scientist gets very little scientifically from the process – except money of course. Assuming, that is, that they successfully navigate the process. It can take weeks of solid work to put together an NIH grant, weeks that could be spent more productively if the process wasn't so competitive and if the grants themselves weren't so bureaucratically complicated and unbelievably detailed – 40 pages of written material is not uncommon.

What scientists are actually doing is justifying their research over and over again through the course of their career. They repeatedly go through this process of peer review. It is an unbelievable system of checks and balances that is supposed to keep people on track and make sure that grant money is not being spent frivolously. Researchers can't just go out willy-nilly and do whatever they like. The supposed freedom of academics that people opposed to research funding tout as a reason to reduce NIH spending is a complete fallacy, and no one has bought into it more than the researchers themselves.

Our student works out her project with her advisor, gets some preliminary results that indicate that the project will work and she meets with her committee. They evaluate her proposal and let her move on. If she had done poorly and her project had looked like it would bomb or she had displayed a lack of knowledge about her project, she would have failed her proposal and would have been kicked out of school. Getting kicked out of graduate school would seem like a really bad thing. It isn't. If our student makes it to her thesis proposal and doesn't know what she is doing, or fails classes or exams multiple times, then it is best if she gets out and does something else with her life. There's always medical school. In the end, whether a student is kicked out is really at the discretion of the advisor. I have never heard of a single case where a student was kicked out after failing the proposal if the advisor still supported the student's efforts. Here is where the student's ability is really not the issue so much as the advisor's willingness to support her.

Let's jump ahead. Our student passes her proposal and has immersed herself in her project. She is now dating a fellow grad student; wisely, he doesn't even work on the same floor as her. Her mother still thinks she is curing cancer, which is fine for the time being. She is focused on achieving the goals set out in her proposal and more importantly, getting published. Somewhere in the next couple of years it is likely that her relationship with her advisor will break down. They will stop speaking or even become hostile towards each other. This is especially the case when the student reaches the point where her work does not seem to be progressing well and she is looking for guidance. The guidance she is seeking is not so much advice, but more handholding. This is an extremely difficult period, where her advisor is looking for her to push through her difficulties and resolve problems on her own. Eventually, the two of them will overcome their personal difficulties. Not surprisingly, the relationship will improve as she works through her project hurdles and gets closer to the time of writing her first paper.

As her results mount and the two of them try to assess the appropriate journal for the work to get submitted to, she will also start to think more about 'getting out.' If she is in her fourth or fifth year, her colleagues, friends and family will start to ask her when she is going to graduate. Her answer will change over and over as she works her way through writing and submitting her first paper and thinking about asking if she can graduate. The green light for this will have to wait until her next committee meeting, but will consume her for months or years before that happens.

Publish or perish

Once her paper is written, polished and submitted to a journal, it enters what is, to her, a black hole. When the paper arrives at a journal, an editor, who is either a full-time professional editor, as is the case for most top biology journals, or an editor who is also a researcher, usually at a lower tier journal, will assess the novelty of the work and the quality of the data presented in it and decide whether or not it is going to be sent out to peer review. The great majority of papers are rejected by top journals without being sent for review by scientists in the community. Up to 80% of the papers never make it past the editor. The leading journals typically reject close to 90% of all submitted papers.

Professional editors are former scientists with PhDs who have left the research lab to take on a career in publishing. These editors actually play an important part in science, even though they will, most of the time, never actually do another experiment for the rest of their careers. The editor must be able to assess science from a wide range of fields and determine whether the work presented in the paper is a significant advance or not. This is easier said than done, and an editor relies heavily upon the peer review process to make the more difficult decisions. Peer review is the process whereby scientists not involved with the work presented in the paper anonymously evaluate the quality and significance of the work. A research paper that is sent out to review is typically seen by three referees, who provide both confidential comments to the editors and comments for the authors of the paper. It takes several hours to write a full review of a paper. The goal of the review is for referees to provide advice on whether the scientist has presented enough data to support the conclusions provided at the end of the paper and to let the editors know whether they think the work is novel enough to publish in a prestigious journal or whether it was an incremental advance; simple enough. But here's the kicker: the reviewers do it for free. In addition to running their lab, writing grant applications and research papers, teaching, counseling their lab members, attending faculty meetings, and traveling to science conferences, researchers give their time to the peer review process. It is widely understood that reviewing research papers and grants is an integral and important part of the scientific process.

Like everything in science, not all is as it would appear. The peer review system can be corrupted by researchers holding a grudge against an author, who are friends with an author, or who are competing with the author. This is why there are multiple reviewers, but in the end the voice of the pickiest reviewer is often heard over the protests of the authors. It's not a perfect system, but no one has really come up with a better one that promotes competition and protects the authors.

Research papers are rarely accepted for publication after a single review. More often than not, the scientists are asked to do further experiments and significantly revise their text before publication. After the reviews are returned to the editor, he or she will confer with other editorial colleagues and decide whether the paper is going to be rejected or accepted, or whether the researcher will be given the opportunity to revise the manuscript and resubmit it. But what is to keep reviewers from making claims or asking for unreasonable amounts of work?

Analogy time: let's say that you are a scientist reviewing a paper and realize that the results are very similar to results that your lab has produced, but which you have not yet published. Let's say that the authors of the paper have figured out a detail that you have not. Do you have your student or post-doc do the key experiments with the knowledge you have gained so you can avoid being scooped? Do you ask for experiments that would delay publication until you can catch up? Do you ask for them to do an experiment that would actually require them to collaborate with you? Do you make such harsh comments that the paper might get rejected? After all, delaying their publication could seriously help you. The reality is that scientists in this position could excuse themselves when they realize that there is a conflict of interest, but many do not. Remember: these are anonymous reviews, so you can say what you like as long as it appears legit. The idea of having three reviewers is to have a balanced view of the paper, so no single reviewer can be unreasonable. The only thing keeping reviewers honest is their own integrity (and sometimes a diligent editor), but it really is a lot to ask of someone who is by nature highly competitive. There is little that anyone could do in any system to completely prevent unscrupulous behavior.

For many scientists, receiving a rejection letter is an enraging experience. This is particularly true when the reason given is that the work does not represent a significant conceptual advance or that it is not novel enough. Talk about a big ol' slice of humble pie. An editor or reviewer doesn't think your work is interesting. After working your ass off for several years, your work is deemed insignificant. The reaction to this can range from rage to depression, but is rarely quiet acceptance. Often a scientist will appeal the decision. However, a paper is rarely published after it has been rejected, and the appeal letter often only serves as a means to vent frustration or anger rather than actually achieving anything.

The pressure to publish can be so great that it leaves a young scientist to do the unspeakable: commit fraud. Like speaking Voldemort's name, scientists cringe at the mere mention of someone forging results. It is widely considered the worst thing a scientist can do. A survey came out in 2003 that indicated that the majority of scientists knew of a colleague who had done something that they would consider unscrupulous. Yet the incidence of reporting scientific fraud is very low. The reason? It is extremely difficult to prove that someone has committed fraud unless they do something very stupid or confess when confronted. However, it is so horrible to imply that a scientist has committed fraud that it is exceedingly rare to have someone be confronted and then confess. Not only that, there is virtually no oversight by the NIH to try to detect it. There are of course cases where people do bad Photoshop jobs to try to forge a result, but mostly the data is just manufactured wholesale and this type of fraud is next to impossible to detect. Since science works by piling results on top of one another, building a foundation from which to get a larger picture of (say) biology, researchers who forge data are destined to be discovered – unless, for some reason, they happen to be correct in the story they made up. But by the nature of science research, data that cannot be reproduced could just mean that the experiment was done wrongly or differently, which makes it even harder to detect fraud.

So, why do some graduate students and post-doctoral fellows forge data? The answer is unfortunately predictable: the pressure to succeed is too high. For the same reason that some premedical students cheat in undergraduate classes, some scientists will resort to making up their own success to meet the demands of an advisor who is putting pressure on them to get results that fit into their model or a committee that is putting pressure on them to get results in order to graduate, or to avoid the fate of having their funding run out with no papers to show for their efforts. The sense of helplessness that all scientists eventually face is almost never addressed. Forging data is seen as the domain of the unscrupulous and weak.

Let's say that our graduate student has to go back and do some more experiments. She resubmits her paper and it is accepted for publication: the ideal situation. She gets the green light to write her thesis. At the same time, she is deciding if and where to do a post-doctoral fellowship. Once you have your PhD, you are considered inexperienced and unqualified to run your own lab. A post-doctoral position is held up by most scientists as your only reasonable option. It is only recently that students have been exposed to alternative careers after graduate school. The fact is that only around 14% of all PhDs in the biological sciences will ever run their own academic labs. This statistic is never revealed to people applying for graduate school or incoming graduate students; not because faculty are unaware of the fact that most don't get faculty positions, but that it would seem to be part of the frame of mind required to be a scientist. This means that most of her colleagues will not make it to the coveted faculty position. Instead, they will remain post-docs indefinitely, become teachers, or take a job in the biotech industry.

For a long time (until as recently as the early 1990s) it was considered selling out if a scientist took a job in the biotech industry. The change corresponded directly with the glut of post-docs who just couldn't find faculty jobs and the growth of the biotech industry. However, in the eyes of university professors it meant that you had failed – you weren't good enough to make it. This seems ludicrous today. Now there is sometimes as much competition for a good industry position as there are for faculty positions. Increasingly, there is a large number of scientists going into 'alternative careers,' becoming editors, intellectual property attorneys, policy makers, and biotech business consultants. Indeed, it is now common to have people giving seminars on other career options in the same institutions where professors consider their graduate students to be failures if they don't become a post-doc. This reflects a more realistic trend, and in the end it will benefit all. If there is not a significant increase in the available faculty positions, then fewer and fewer people will go into science as the dirty little secret about job availability becomes more widely known amongst undergraduates. The dedication that it takes to become faculty at a research institute is so strong that their students and post-docs not getting what they aim for really doesn't occur to many professors.

Our student is one of these people who does not consider the possibility of failure. Graduation and the thesis defense go smoothly, and after revising the text she can officially be called 'doctor'. Now she cleans up some loose ends and is off to become a post-doc. Becoming a scientist is a more difficult process than most would have imagined. So what! Wait until you hear what a post-doc goes through. Most correctly estimate that getting a PhD is hard – you are earning a doctorate after all. But grad school is child's play compared to doing a post-doc. This is where despair and depression are layered like a birthday cake topped with a generous icing of pressure and doubt. Yummy!

Post-docs are usually required to get funding to support their projects. If they don't have any first author papers from graduate school, then they are pretty well screwed. Also, if the new lab they are working in has not yet had publications in the area they are proposing to do work in, they will have a hard time. Many lab heads will require that their post-doc comes in with independent funding before they start in the lab. This means that students start writing grant applications as soon as they are done with their PhD, or even while they are still writing their papers and thesis required for graduation.

It is widely assumed from early in the education of scientists that they don't have to be proficient, clear writers to succeed. The truth could not be any different. While there certainly are a lot of scientists who are as awkward with the English language as they are with the opposite sex, one of the most important tools that scientists can have is the ability to communicate what they have achieved and how they achieved it. The language of science around the world is English, and foreign researchers are well aware of it. What is remarkable is the fact that few graduate programs ever teach their students how to write a grant application or research paper.

Let's assume that our budding scientist has been able to navigate this process and get a grant. (We have thus far assumed that she is really good, so why stop now?) The fact is that the NIH awards post-doc grants to approximately 30% of applicants. Other agencies are a little better or worse, but 30% is normal, necessitating application for several grants to secure funding. Our post-doc will now be getting about $28,000 a year to do science. This is the standard salary paid by an NIH grant to a first-year post-doc. On average, she will spend about five years as a post-doc and typically switch labs and institutions once. That means that she will have spent approximately 11 years becoming a scientist before she is applying for jobs at universities to run her own lab.

After the fire

Amongst scientists there is an often spoken, but little addressed, problem with post-doc life – call it the 'post-doc's lament'. They are overworked, underpaid and often unappreciated. Graduate students produce papers for a lab, but post-docs are the real powerhouse of science. They are expected to perform, but little is done to protect them. Graduate students at least have a thesis committee to protect them from an unreasonable advisor, but post-docs very rarely have a mechanism to protect themselves from their bosses. Policies regarding the treatment of post-docs are developed *ad hoc* and are institute- or even lab-specific. Nightmarish stories are told of post-docs who are pitted against each other in the same lab on the same project. This is done to drive the project, but it results in strife and frustration. Abusive treatment is common and there is little they can do about it. Even at institutions where there are offices of post-doc affairs, little can really be done if a PI is a jerk. With these issues recognized, the National Academy of Sciences put out recommendations to change things in 2000. There is no evidence that there has been widespread adoption of their recommendations though. In 2002, the national post-doc association was established, but again there is little evidence that it has been effective in creating any widespread change in the current state of affairs. The weird thing is that less prestigious institutions seem to be more progressive in reforming the rights of post-doctoral fellows. There is a great need for transparent, consistent policies regarding the treatment of post-docs, but this will not happen unless the NIH and NSF require it of institutions.

Our post-doc doesn't have any problems in this area though. She chose her advisor wisely and will be treated with dignity and respect. After entering her new lab, getting funding and starting her project, she publishes a few really high-profile papers; one in *Nature* and another in *Science*. She has married a post-doc who works in the lab two floors below. He has been there for five years and has published two papers, but they have appeared in lesser journals and he is well aware of the fact that he will not be getting a job any time soon. His initial funding ran out two years ago and despite the fact that he is very good at his job, his project did not turn out to be as exciting as he would have liked. He was therefore forced to move on to another lab and try something new. Eventually he had to move on and take a position in another city. They will be apart for about a year and a half.

She, on the other hand, got lucky with her project. Her advisor has been very supportive, but she only has one year of funding left. Rather than try to secure more funding, she scans the back pages of journals for jobs to apply for. After picking a short list of 47 jobs to apply for, she writes up a description of what direction her work is going in, prepares a CV, writes her 'statement on teaching,' collects letters of recommendation and sends out her application. She finds writing her statement on teaching to be somewhat amusing, because she has not taught a class since her first year in graduate school and generally is speaking from ideology rather than raw experience. She is virtually untrained as a teacher. Each of the jobs she applies to has 300–500 other people applying for them, but because our scientist is so good she gets four interviews. At her interviews she meets lots of faculty, gives a pretty good talk about her work and heads home to wait. She continues to do research and has not heard from the institutions she interviewed at for two months.

Finally word comes and she gets two job offers. This is exceptional. It is more common to get a single offer and be forced to take it no matter where it is or what it entails. The option of waiting a year to apply again, in the hope that she will have another publication to show that her work is still progressing, is more depressing than taking a job that is less than ideal. One offer is in a small department at state school in the mid-west. She didn't like the facilities there and there is a heavy teaching load, but it is a tenure track position. The other is at a more prestigious university on the east coast, in a large department with lots of people that she can collaborate with, but this position isn't tenure track and pays a lot less. The average salary for a starting primary investigator depends largely on the institution and the standard of living in the area. A salary of $50–80,000 is normal, with women making less than men in wide-cast surveys, but our researcher is just thankful that she has offers. Most of her colleagues do not. It annoys her that the other two places she interviewed at never let her know she didn't get the job, but she is well aware of the fact that universities usually don't bother to do this. Callousness is standard. After discussing it with her husband, they decide that she should take the tenure track position despite the fact that the other job sounded more exciting.

Some universities rarely offer tenure track positions. They just can't afford to have any more dead wood than is already floating around the department. This would seem to be a reasonable solution to the problem – get rid of those who are not productive and hire new faculty who will bring in grant money. The result is an active department, but it adds another level of insecurity to the lives of their scientists. They are either unaware of, or just don't consider, an alternate plan. Cold Spring Harbor Laboratory, where I did my PhD work, has an unusual system where instead of giving tenure to faculty, they award something called 'rolling five.' Briefly, when a faculty member becomes rolling five, it means that if they go through a period of low productivity (meaning that they have not brought in enough grant money to keep them in the black), they are given five years to find a position at another university or institute. This seems like a nice compromise compared with getting rid of tenure completely.

In talking with her husband, they decide that they would like to have children in the next five years. She is 35 now and just doesn't want to risk a non-tenure position. At the same time, there is a better chance of her husband finding a faculty position at the same university rather than at the highly competitive non-tenure job. Co-appointments are hard to get. Many post-doc couples try to get them, but more often than not they just find it harder to find a job when a university is faced with hiring both or neither.

There is an interesting phenomenon amongst scientific faculty today that has never really been discussed. Their parents, if alive, are usually still married, and if they are divorced have either remarried or divorced once their children were already adults. If you think about it, this actually makes sense. It takes a lot of fortitude to go through the process of becoming a scientist. One needs to have a pretty strong constitution, and perhaps more importantly you have to have a pretty strong support system to fall back on. Could the results reflect the fact that more people with a higher education have parents that are still married? Perhaps – but it is interesting nonetheless that the situation exists. It certainly isn't an indication that scientists are more grounded than your average Joe.

After accepting the job as an assistant professor – the order of advancement usually goes something like assistant professor to associate professor to tenured full professor – the faculty at her new institute offers her husband a position as well, but not a tenure track position. Instead they offer him a very small lab space as a research fellow (not a professor), but he will have to secure his own funding before he can start. Typically, a university will give new faculty a breather by giving them some start-up money. This is designed to pay their salary and float the lab while it is being established. The money is usually designed to last a few years and is absolutely required to buy lab equipment, computers and chemicals and to pay salary lines for technicians and other support staff. During that time, our scientist is trying to get as much done as she can so she can apply for research grants. The NIH gives some wiggle room to new investigators and requires only a minimum of new data compared with established faculty and requires them to have fewer publications to get a grant. Luckily, she is only required to write the curriculum for, and teach, one class in undergraduate genetics the first semester she is there and to give a single lecture on her work to the graduate students. Just as when she was a graduate student, her teaching responsibility will have to take a back seat to her other responsibilities. Undergraduate students will again pay the price of overworking a scientist.

Our young assistant professor hires a technician to do some of the more mundane tasks in the new lab. She has been thinking about what she is going to do first science-wise, but has not been able to focus on it for several months. As luck would have it, her former post-doctoral advisor was gracious enough to let her take a lot of the materials she developed in his lab with her. This is not always the case. Often, scientists will decide that the project that their post-doc developed was so interesting (and lucrative grant- and research paper-wise) that they compete against them. A sort of bargaining process takes place as a post-doc prepares to leave the lab to forge a path of their own. A new faculty member will certainly have more on her plate than she can reasonably handle. She will be asked to divide her time between organizing her research projects, attending faculty meetings, teaching, writing grant applications, taking on and training graduate rotation students, hiring technicians, attending science conferences and, if she is lucky, taking on her own post-doc. Most starting investigators don't get to take on a post-doc until their second or third year. And oh yeah – lest we forget about her competition outside of her old advisor – she will need to keep up with the mountain of new papers that are coming out in her field as well. If someone publishes results similar to the results she is getting in her lab, then she is said to have been 'scooped'. This means that she will have to come up with even more data and will probably have to publish them in a much lower ranked journal. This is widely regarded as an embarrassment and really hurts your chances of getting a grant funded.

Once our scientist publishes her results in a journal, she is required to share all of the reagents that she made to get those results with anyone in the scientific community that wants them, her competition in particular. This is another system of checks and balances that allows others to repeat and confirm her experimental results. At the same time, it removes some of the advantage she has over her competition. The tradition has existed for over a century, but it works on the honor system. Journals require it, but I have never heard of a case in which someone has experienced any repercussions for not sharing their reagents with another lab. You are basically considered a scumbag if you don't share them, but many scientists openly refuse to do so. They claim that too much is invested in the production of the reagents to share them, but in the end they do know it is wrong. Journals could play a role in policing the policy by noting on their website that the authors have refused to share reagents, in effect marking them with a scarlet letter of embarrassment, but they have taken no action. Sharing reagents after publication is good for science as it allows more rapid progress, prevents duplication of efforts and makes sure that published results are real, but it also adds another degree of insecurity.

Currency exchange

While she is struggling to manufacture her career, let's consider how the hell she is going to get a grant. If research papers are the currency of science, then grants are the lifeblood. You need the currency to afford the blood. The system has been set up such that the projects with the greatest chance of success are funded. Sounds great right? Let's consider what low risk means in science.

You are a researcher on an NIH grant committee and you have been given the monumental task of reading 30 research grant applications and reporting back what you think of them. After you do that, you go to the NIH and sit in a room with several other researchers who have done the same thing. You will give scores on these applications based on how likely they are to succeed, the degree of innovation, history of success, the institution's ability to support the project and your feeling regarding the applicant's ability to achieve the goals set out in the grant. In the pile there are a handful of applications that have little or no data in them, but a really interesting idea that, if correct, would change the way the field views a particular biological problem. Another set has a series of well-thought-out experiments which progress logically, have a little preliminary data and the researcher has just published a paper related to the topic of the grant in a prestigious research journal. Finally, there is a third set of applications from researchers who have a logical progression of well-designed experiments, have published recently in prestigious journals, and have preliminary data that suggest every single thing that they propose will work out. In addition, at the last minute, they send in an addendum to their application showing that the first of the three objectives they have proposed has actually been achieved. Which application gets funded? The third set, of course. They are going to succeed. They've already proven it. There is such a high chance of success that almost all risk is eliminated.

What does this do to innovation? Novel ideas? It crushes them in a vise of pressure. Very little risk is ever taken in science funding. Recently, the NIH started an initiative that encourages risky experiments, but even here risk has been minimized. The program is a bureaucratic oxymoron. Only well-established scientists are considered for funding of risky projects. These scientists are already well funded and thus are not as inhibited in the projects they can take on to maintain themselves. It all boils down very simply: research labs have to be run like small farms. Farms run on a very small profit margin and thus little risk can be taken in planting new crops. It is safe to plant corn, tomatoes and carrots. They can be sold. If the farmer plants some broccoli rabe or arugala across a large percentage of his fields, he is risking not being able to sell the fruits of his labor. Larger wealthier farms can take a larger risk. They have the money to gamble on risky crops. Why would you give them an extra monetary incentive to do what they already have the means to do? In the lab, if you have too many people working on risky projects, the papers might not come out at a pace that will allow it to sustain itself. Novel, innovative projects with any degree of risk can make a scientist's career, but more often will end badly.

This pressure has fostered an environment where a lot of shitty science is published in shitty journals. New journals are cropping up all the time. Does the scientific community need them? *No!* There are so many journals out there that even the poorest designed, sloppiest work can eventually make it to press. Rather than having a system where peer review holds the work up to a high standard for all journals, reviewers and editors for crappy journals just let lame science step over a much lower bar. Scientists generally use these journals as a dumping ground that allows them to have another publication on their CV. But that is not the half of it. The pressure to publish is so great that even when a scientist has a fantastic story, what they often do is slice it up like deli meat and try to figure out what is the least amount of data they can put in and still get a high-quality publication – the so-called 'minimum publishable unit.' Ideally, a scientist would be judged on the merit of the work they do, not the number of publications they have. In biological research, there exists a holy trinity of publications: *Cell, Science* and

Nature. If you publish your results in these journals, you are virtually guaranteed to get a grant refunded, and the companies that run the journals know it.

Cell Press and the Nature Publishing Group have launched a series of more specialist journals over the last 15 years that, since they have the 'Cell' or 'Nature' name associated with them, are thought to be pretty damn good; and largely they are. They also make a lot of money and serve the community well. They make money in three basic ways – personal subscriptions, institutional subscriptions and advertising. In other words you have to pay to get access to what is printed in the journal and, more importantly, get access online. It is a pretty good system. The journals provide a service to the community by overseeing the peer review process, editing papers and providing news on new findings and science policy around the world. Readers pay for it and rightly so. The information is valuable. The money for these subscriptions generally comes from grant money. In other words, researchers use tax dollars in the form of grants to pay to view research results that were obtained with public funding. Not only that, they pay to publish papers in these journals in the form of fees for color figures and page charges.

Recently, there has been a growing movement in the scientific community for free access to this material. The argument is that the companies running these journals are profiting off of an essential component of the scientific process and that the public doesn't have access to research results that they paid for unless they pay for it again. The companies that run these journals respond by saying that they provide an excellent product that serves the community well and that they should be rewarded for it. They are, after all, operating for profit. They also argue that the profit they make goes into supporting less profitable publishing ventures that also serve the community well. This is only partially true though. They do funnel money into publishing ventures that cost rather than make money, but these ventures are intended to be profitable in the end. Otherwise they wouldn't do it.

Some enterprising scientists have now spearheaded the launch of a series of journals that are free … sort of. Anyone, even non-scientists, can view the content. In addition, some other journals make their content free after a period of time, generally a few months after publication. They have argued that their publishing model is the future and have even obtained support from a congressman, who was thinking of putting forth a bill requiring that all publicly funded research be freely available to anyone who wants to read it. The only problem is that the business model for these journals will not work if widely applied. The funding model for these free journals falls apart pretty quickly when one considers how to publish very specialized journals that serve a small subset of the research community.

Take for example one of these ventures, the Public Library of Science (PLoS). When this venture was launched they hired some seasoned and respected editors to launch a series of journals, starting with *PloS Biology*. They obtained a $9 million private donation to do so and presented their publishing model to the community. Researchers pay $2500 to have their paper published in the journal (assuming it passes a rigorous peer review). The journal saves money by having its content primarily online and will provide subscriptions to a print edition at cost. Therefore, for their business model to work, they will need to publish a certain number of papers per month to cover their overhead. This shouldn't be a problem, because biology is a very general topic and there is a lot of great research to publish. What if it were a more specialized field, like genetics? We'll soon find out, since *PLoS Genetics* was launched in the summer of 2005. Our young scientist, in a small department, would have to take a large ideologically driven risk with her and her post-doc or grad student's career to do so. It is an experiment in publishing that could, like many science experiments, fall apart.

Zealots within the movement are quick to demonize any company that makes money in science publishing, but if pressed would be forced to admit that their own model would not work on a wider scale. In any other business, the idea that profit is bad would be laughable, but scientists are an idealistic bunch. Time will tell, but it is highly unlikely that for-profit science publishing is going to disappear unless a ridiculous amount of money is funneled into the idealistic model – public money that is.

The Bush administration has now initiated a policy that if publicly funded scientific results are to be published, then the NIH 'requests and strongly encourages' scientists to make the final edited text publicly available. To do this they have launched a program where scientists submit their papers to an NIH database that the public can access.

What is the best they can hope for: access to science results that less than one per cent of Americans can understand? I know it sounds elitist to say that the public will not benefit from free access to research they are paying for, but it is the truth. Most Americans don't know the difference between DNA and protein. How the hell are they going to wrap their heads around science papers that are complicated for people with years of training? The idea that this initiative will serve some lofty public good is completely naïve.

The new policy has opened several other cans of worms. First, there is no oversight in place to prevent multiple versions of research papers appearing. While the NIH 'encourages' scientists to submit the final edited version of their text, it is a certainty that this process will frequently get screwed up. The policy also ignores the inherent value that publishers play in editing text and the fact that it costs them money to get those papers into shape for publishing. Thus there is no real regulation of what scientists can write in the paper that they submit to the public database. It also allows researchers to make claims that their data does not justify and put it in the public domain. These claims are usually removed during the peer review and editing process, but who is to stop a researcher saying whatever they want in the version submitted to the NIH database? Personally, I can't wait for scientists to start submitting their papers with statements about how the administration has shown contempt for science and do not understand how science works.

In the case where multiple versions of the paper appear publicly, serious intellectual property issues could arise regarding disputes over the significance of research. Universities and companies will have to shell out lots of money to square these problems. Imagine the arguments over which version is valid. What a total mess.

The policy is actually a watered down version of the original proposal that would have *required* that publicly funded science be submitted to the database, but small publishing companies and scientific societies who publish journals and rely on subscription fees to remain solvent placed a lot of pressure on the NIH, claiming that a mandatory database would put them in serious danger of going out of business. Also, since some of these companies retain copyright in the finished product, placing them in the public domain could cause legal issues. In the end, the policy is a eunuch. It is just a suggestion, and few scientists will actually view it as valuable and thus will not participate in it. None of these cold realities has stopped the administration from claiming that the database is an actual archive of NIH-funded research, that it will be valuable for scientists trying to search for research papers, that it will allow the NIH to manage its research portfolio, that it will actually provide scientists with higher visibility for their work, that it will improve the public understanding of biomedical research, and that it will strengthen the impact of research findings. None of this is true and the NIH, their scientists and the greater scientific community know it. I cannot for the life of me think of a single previous instance in the entire history of the National Institutes of Health where they actively tried to deceive the public.

While publishing provides the bread and butter for a scientist, there is another component to science that provides a healthy dollop of drive (or at least it used to): the scientific conference. It used to be that scientists would get together with their colleagues from around the world to discuss their latest results, forge collaborative efforts on scientific problems to which each side could contribute results or expertise and get a bird's-eye view of what's going on in their field. The importance of the scientific meeting cannot be underestimated even today. But things have changed a bit. The speed with which science happens is at once a crawl and a race. It takes a long time to put together a story that makes a significant conceptual advance in biology, but if you miss a step, someone will beat you to the punch. Over the past few decades, the scientific meeting has changed dramatically, and it is now more common for scientists to publicly discuss results that are already or are about to be published. Time after time, scientists have been burned by discussing their results publicly because a competitor, who has similar results, is sitting in the audience. Thus the original spirit of the scientific meeting is dead.

Let's say that our young investigator is presenting the work from her new lab at a science meeting. She is eager to show that she is really working independently of her former advisor and shows all of her latest data. Here are three scenarios that she will have to consider. Her competitor is sitting in the audience, fearing that he is about to get scooped. He immediately submits similar results to a journal for publication and hopes it is not too late. If they are friendly, they might get together and try to publish two papers back to back in a journal, but revealing that you have similar results has its downside as well. What if they don't want to cooperate?

Scenario number two occurs if the competitor has conflicting results. Rather than sparking a lively discussion that advances the field, it will likely result in a race to publish. It is very hard for scientists to admit they are wrong, even when it is pretty well proven. So, out into the public domain go two results that are in complete conflict and the parties involved often know it. Publication usually takes priority over clarity in the field and if conflicting results are already out there, she will have a much steeper hill to climb to get her results published, having first the task of proving that her competitor was indeed wrong.

Still worse is the third scenario, in which the competitor goes back to his lab, does the same experiments and publishes while our scientist is trying to polish the story. There aren't a lot – percentage-wise – of these scumbags, but they are out there and it only takes one to sour an entire field. After thinking it over, she prudently bows out and shows some of the work she published the year before and speaks of her current work in the vaguest terms.

If you want to hear the latest results from someone's lab, it is best to talk to them in the bar after their presentation is over. In fact, some of the most productive discussions regarding science take place in bars and restaurants in the evenings after the scheduled sessions are over. Most scientists can tell you a story about how they figured out a problem, designed an experiment or forged a fantastic collaboration after tipping a few too many. Ethanol definitely lubes up the noggin. Luckily, our scientist is fond of the drink. Realize that grant money – your taxes – pays for scientists to attend these meetings. There is no available estimate of how much money is spent on airfare, hotel rooms, food and attendance fees for scientific meetings, but when you consider that there are companies that specialize in running them, you can guess that there is some significant money being spent.

With the road to becoming a scientist being paved with so many potholes, one might think that it would be hard to find people to take on these challenges. This is simply not true. Part of the reason is that it is not made clear to students entering graduate school that they will probably not make it to a full professorship. In fact, there have been a number of reports that indicate that we are not producing enough scientists in the US. The problem is that the original report from the National Science Foundation predicting a shortfall of scientists in the US was based on an expected wave of retirement that never happened. The number of unemployed scientists in this country is rising. Even with a doubling of the NIH budget during the Clinton era (something that President Bush has implied he had something to do with on several occasions), the number of new jobs for scientists did not keep up with the number of scientists on the market. Not only that, there is an increase in the number of foreign-born scientists competing for jobs in the US. The number of years that one spends as a post-doc is increasing. Simply put, post-docs are wondering when this 'shortfall' is going to result in a job for them.

With a 2001 report on national security reporting 'current trends of supply and demand ... may seriously threaten our long-term prosperity, national security and quality of life,' there is a suggestion of a crisis that is simply not there. The claim that mismanagement of science and education poses a danger that is 'second only to weapons of mass destruction detonating in an American city,' is merely a scare tactic; what's more, if this is really a threat little is being done about it. A closer look reveals that the predicted shortfall is based on numbers that reflected a drop in the rate at which graduates were being produced, rather than on an estimate of the number of jobs that would go unfilled. With several hundred applicants for a typical faculty position, it would seem that students are being grossly misled about their prospects for employment and that the government is continuing to scream that we don't have enough scientists. It is a nauseating display of misguided intentions, and smart talented people are paying the price.

On her 37th birthday, our scientist realizes that the ticking sound that has been keeping her up at night for the month leading up to her birthday is her biological clock telling her that she really wants kids ... *now*! She and her husband have discussed it over and over again, ending each conversation with the same rational conclusion: that they will start a family when things 'settle down.' Her career has kept her from having kids up until now, and reality has finally set in. She has been all too aware that, as she gets older, the chance of her getting pregnant and having an uneventful pregnancy decreases. She has students, post-docs and technicians who rely on her to run the show every day. There is no good time to have a child. There is no real maternity leave for scientists either. While officially universities will allow professors to take leave, the reality of it is that you are more likely to find an infant in a professor's office than a professor out on full maternity leave. Either way, she will work even if it is from home.

If you are considering having children as a post-doc, you risk falling out of favor with your boss. Some scientists used to flat-out ask women post-doctoral candidates if they were planning on having children in the immediate future. A few lawsuits later they are more savvy than that, instead trying to beat around the bush to the same end. This aspect of science is not unlike the discrimination of women that is seen in the business world.

I have given you a taste of what it takes to become a scientist, and the less than glamorous life a scientist leads. With all of the complications associated with becoming a research scientist: why do they do it? Scientists pretty much make their own hours – though that usually means that they work 60–80 hour weeks. In the end, the reality is that scientists are members of a cultish band of men and women with an unusual thirst for discovery. Let's now consider some of the most controversial issues that scientists face today: the wide misunderstanding of, and uneducated ideological opposition to, what they do – and the misrepresentation of what they are achieving.

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