Publish: What? Why Where? How?

These are notes for a lecture given by Alfonso Martinez Arias in a workshop about Responsible Research held at LMU in Munich (Germany) on 24 July 2014 (http://www.responsibleresearch.graduatecenter.uni-muenchen.de/index.html).

The answers to the title of this talk should be obvious. You want to publish your work in the most appropriate journal/place so that people know what you have done, use it in their research and ponder the consequences. As we shall see, like much of the biomedical sciences (and it is this that I shall refer to when talking about publishing), the answers are less straightforward and you should think about every one of them. None of this was a consideration for F Miescher when he discovered nuclein (later known as DNA) and in 1869 wrote a paper which he submitted to the journal of his boss Hoppe Seyler, the Hoppe Seyler Zeitshrift fur physiologische chemie. Hoppe Seyler found the finding of P and N in an organic material so remarkable that refused to publish it until he had observed this himself; now this is proper peer review. Two years later, satisfied with his own experiments, he published the paper –and one of his on the subject to support the observation. Much to ponder here –notice that he did no scoop his pupil- but will leave you to think about it. The delay, the publication, the interactions with Hoppe Seyler did not have much of an impact on the career of Miescher.

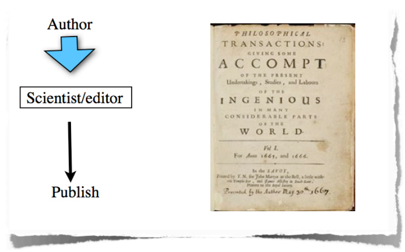
How much things have changed! Today biomedical sciences publishing is, like many other aspects of the field itself, in turmoil; subject to debate not just in the scientific arena but also in magazines and papers. The Economist and the Wall Street Journal, not to mention the New York Times and The Guardian, periodically raise issues about scientific reports, in particular the biomedical sciences. The reasons for this media attention are complicated and you have heard about and discussed some of them earlier in the day but essentially boil down to a complicated tangle that has emerged between publications, jobs and money. Matters like peer review, the process whereby a scientific report is judged to be suitable for publication, have become under scrutiny and perhaps it is not surprising that last year one can find over 1500 and rising peer reviewed (notice the irony) publications on peer review in PubMed (where there were less than 500 ten years ago), with many thousands more in blogs and comments in journals. Why? The main reason is that, as we all know, a publication today is not so much about reporting progress on experiments and findings but about jobs, about grants, about careers, about upmanship. Publications have become, without us realizing it, the token whereby we are judged and ranked. There is a rather poisonous knot here that I shall try to untangle in the second part of the talk. First, let us get some perspective on peer review, the axis of modern science reporting.

It surprises me that we use papers like the famed Watson and Crick manuscript on the structure of DNA in Nature (by the way there were two other papers in that issue, one by some Rosalind

Frankin) as a reference of something wonderful which, if possible, should be imitated. That paper would not have passed the editors desk nowadays and the famed sentence at the end: ‘it has not escaped to our attention…..” would only have served as a ticket for the editorial rejection paraphrased as “it has nor escaped to our attention that while this is an interesting speculation, it would be important to get some experimental results to support it”. Sure, the topic was hot but, under today’s editors, the paper was an speculation on somebody else’s data and would be published, if at all, in the hypothesis section. Whereas in the 1950s science publishing was a small business catering for a small science community, today science is a very large enterprise and, I would say, publishing has not kept pace in the right manner. The fact is that today, the whole publishing business is a tangled web that results from growth without design. What do I mean by this? How do we get here?

There has always been some control over what people wanted to publish, and before the Renaissance, and also afterwards, the catholic church had that power. After all this is what Copernicus and, most famously, Galileo had to endure if their papers, books at that time, were not to the liking of the church. Copernicus was most careful but Galileo was obliged to recant his believes in public under the threat of torture; a rather stern form of rejection. Think about it, today your paper is not published, in those days if the reviewers did not like the paper you could lose your life. There is some progress!. In more enlightened countries where the catholic church was not such an obstacle, Science (Natural Philosophy as it was called) was flourishing and some learned societies emerged to cater for this. Amidst these, the Royal Society of London. More of a gentleman’s club in the beginning than a society, it was a place in which people with a Science inclination got together to discuss what they were finding. People got into the habit of writing up reports and the Royal Society produced a repository for those reports: *Philosophical Transactions of the Royal Society* (1665) which is still being published today. Nothing new here, as at the time there were many different places where people could publish their observations and disquisitions e.g *Acta eroditorum* and *Miscellanea curiosa*. The Philosophical Transactions was another one but one which attracted the best in Europe. For example, it was here where Leeuwenhoek published his letters on what he saw down his lenses. The procedure was simple: you submitted a report, a scientist had a look at it and put it to print to be read by the people who were interested. It was here that, as the number of reports increased, the first hint of peer review appeared in the form of people, associated with the editor, who had a say on what had been submitted; basically making sure that it was not silly. But peer review in the sense of an external person to the journal checking the work was first used by the *Medical Essays and Observations of the Royal Society of Edinburgh* (1731). This kind of light touch review, mainly through the editor or someone very close to the editor of the journal, was adopted by the modern Science journals in the XIX century and this is the way in which Miescher and Mendel published their works. Another way to publish were books and this is, of course, how Darwin published his great book. No, his book was not peer reviewed though he did have what today we would call postpublication peer review. And a lot of it. Can you imagine what would have happened if Darwin would have had to subject himself to word limit, supplementary material and, above all, peer review?

Henry Oldenburg (1665) Phil Trans. Roy. Soc



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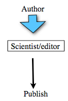

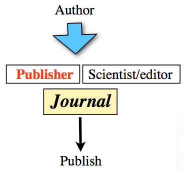

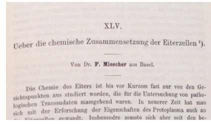
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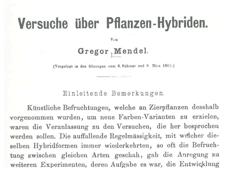
Ueber die chemische Zusammensetzung der Eiterzellen [1]).

Von Dr. F. Miescher aus Basel.

Die Chemie des Eiters ist bis vor Kurzem fast nur von den Gesichtspunkten aus studiert worden, die für die Untersuchung von pathologischen Transsudaten massgebend waren. In neuerer Zeit hat man sich mit der Erforschung der Eigenschaften des Protoplasma auch an


Versuche über Pflanzen-Hybriden.

Von

Gregor Mendel.

(Vorgelegt in den Sitzungen vom 8. Februar und 8. März 1865.)

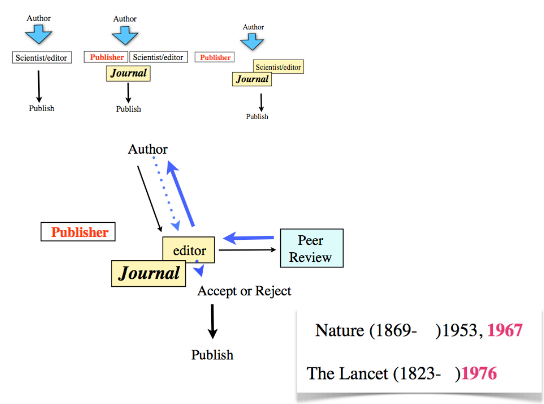
Einleitende Bemerkungen.

Künstliche Befruchtungen, welche an Zierpflanzen desshalb vorgenommen wurden, um neue Farben-Varianten zu erzielen, waren die Veranlassung zu den Versuchen, die her besprochen werden sollen. Die auffallende Regelmässigkeit, mit welcher dieselben Hybridformen immer wiederkehrten, so oft die Befruchtung zwischen gleichen Arten geschah, gab die Anregung zu weiteren Experimenten, deren Aufgabe es war, die Entwicklung

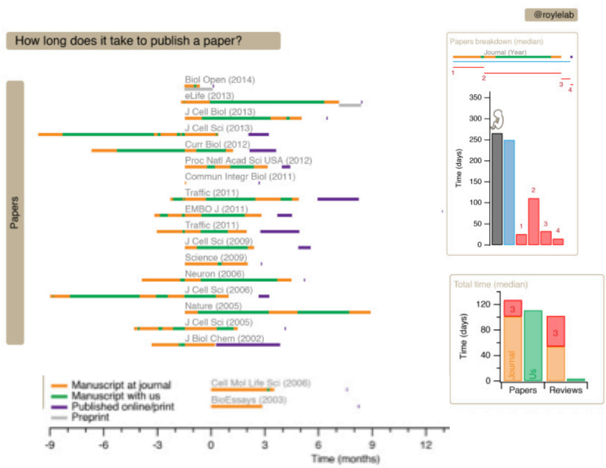
And so, by the turn of the XX century the industry had not changed that much and the small enterprise that was science put out their findings in small journals, run by editors with an interest in science. Nature had been born in 1869 and the Elzevir family began publishing books in the XVI century becoming Elsevier (home to The Lancet and Cell) in 1880 and Science was founded in 1880. The bunching around the late XIX century is not a coincidence, it is a reflection of the rise of journalism and, within this, of the interest to get science to the public. The function of those journals was not to be the arbiters of scientists career, as it has become today, but to get the scientific advances to the people. This is what hides behind that emptiest of lines in a rejection letter from Nature or Science: "your work is not of sufficient general interest….". A vestige of a past in which, unlike now, such journals did sell to the people. In any case, in those days scientists had more specialized forums for discussions amidst themselves. It will not be a surprise to you that the mechanics of a scientific publication was different in those days.

Over the last few months, in the tangled discussions of Peer Review and as an act of rebellion, a famous incident involving Einstein is often quoted in the context of the negative effects of peer review (for a proper account of the event see Sean Carroll http://www.preposterousuniverse.com/blog/2005/09/16/einstein-vs-physical-review/). Einstein had submitted a paper to Physical Review on gravitational waves and the editor took the then unusual step, of asking for one referee report as he thought, rightly it would appear now, that there might be a problem with the work. When he sent the comments to Einstein for correction, Einsten's reply was harsh:

"We (Mr. Rosen and I) had sent you our manuscript for publication and had not authorized you to show it to specialists before it is printed. I see no reason to address the "in any case erroneous"comments of your anonymous expert. On the basis of this incident I prefer to publish the paper elsewhere"



And this he did. The point of the story is not the one people often quote it for. The point is that Einstein was right at being surprised to have the paper reviewed, because in those days, papers were not reviewed; certainly not in the manner that we do and understand today. Editors needed papers, often commissioned papers, and while they might make editorial comments, they certainly did not share the papers with anybody else. This type of review applied to the Watson and Crick paper and to the plethora of papers that form the basis of molecular and cell biology. However, as the number of scientists and papers increased and the material for publication began to accumulate, together with specialization, we start to see something more like the peer review process of today. Nature introduced a version of it in 1953 and implemented the seeds of what we have today in 1967. But The Lancet only introduced it in 1976! And with this small step, of a fair system to control the quality of what is published a complex machine is set in motion that today is a very complicated business, which can keep your paper going around in circles for up to two years in the high end of the market….the notion of high end of the market is, also a new development….basically, all has gone pear shaped. For a nice account of the life span of an average paper see S. Royle’s blog “some things last a long time” (http://quantixed.wordpress.com/2014/04/15/some-things-last-a-long-time/)



@Roylelab

Many of us complain about the influence that editors have on the fate of the papers that we submit to them for publication. However, if you stop and think about the brief historical perspective I have given, you will appreciate that the relationship now is the same that it was at the end of the XIX century: editors decide what is being published. So, what is the difference? A number of things. The first one is that where the editor needed to find papers, nowadays they want to get rid of papers. The other one is that, in the journals that we could call ‘general interest” where the editors were enlightened individuals with interest and knowledge, today we have basically a whole bunch of poorly trained scientists acting with a lot of power in their hands. The editor scientist has moved to more ‘specialists journals”. Today, more than ever, the generalitsts journals are not means to tell science to the people but represent a career asset, a ticket to a job and fame. But the other ingredient of the equation hat has changed is that where the editor (always in what I want to call the generalist genre) was an informed person making sure that the science got to the people, today is someone with the limitations derived from lack of experience and the size of the field, using more or less consciously a power they have. If you don't believe me, read this post that will tell you how your science is not enough because it needs the editor to help you shape. This is certainly the view of Boyana Konforti (Cell Reports at the time of writing http://www.elsevier.com/connect/10-things-you-need-to-know-about-the-publishing-process) who states clearly that a paper is a collaboration between the scientist and the editor who, believe it or not, will help you tell and see a story. As she tells you: keep it simple! Sell a story! …….how low can we get? Are we writing for the editors? Is the scientific level of the editors what we should aspire to imitate? I guess what this is telling you is that if you want to publish your science, take it to ‘specialists journals’ run by scientists but if you want THE product that will give you a job, take it to them. This is where things begin to go pear shaped, where science departs from its original aim and where we have a disconnect between science and the journals. Particularly as I want to emphasize because those journals were not born to be the arbiters of science but to tell science to the people.

A recent case highlights everything that is wrong and dangerous about the situation and how the relationship between journals and scientists is sailing dangerously close to a storm.

STAP a case in point (the perfect storm)

In January 2014 the journal Nature reported a remarkable finding. Somatic cells subject to a simple treatment (addition of lemon juice, as a friend of mine said), which involved stressing them in controlled conditions, could be reprogrammed to an embryonic state. This, the claim went, represented a huge step forward in the ability to produce embryonic stem cells as this would be natural and not involve the genetic manipulations associated with the Yamanaka cocktail. Furthermore, whereas the iPS cells are pluripotent, STAP (Stress Triggered Activation of

Pluripotency) were totipotent, as they were able to give rise to extraembryonic tissues in addition to embryonic ones. This was a remarkable finding and because of this there was not only excitement but also suspicion. The simplicity of the experiment and the wide availability of cell lines with appropriate markers led to a widespread interest in doing the experiment –we did think of including it in our undergraduate project repertoire-. The papers were published by Nature and they had important and tested names most notably Y Sasai, H Niwa and T. Wakayama amidst the authors. So, what could go wrong? The first author Obokata became a celebrity and Y. Sasai, corresponding author in the main paper, was only too happy with the outcome. Maybe it was all too good to be true.

We live in the era of social media which has empowered people to speak. Paul Knoepfler, a stem cell biologist in California (USA) runs a blog on stem cells (http://www.ipscell.com/) and from the beginning expressed some scepticism on this finding. He set up a crowdsourced section in his Blog where he proposed to report on attempts to reproduce STAP. At the same time he ran a periodic poll on whether people believed in the finding. In February, hope was high and the yes outnumbered the nopes. Slowly, the site was filled with failures to reproduce the finding. What is more, people in websites started to report issues of figure manipulation and text plagiarism which began to raise suspicions on the work and all of a sudden the limelight was on the first author. The web is full of what followed (see sources below) but by the end of May things were not looking good and by the beginning of July the papers were retracted because it became clear that the experiments were fraudulent. The big names put the blame on Obokata, who by now was under a huge amount of pressure (and has ever since) and Nature claimed innocence. There was one most serious consequence of what happened here. The CDB, the host insititution of the researchers, became under close scrutiny and in a surprising development, RIKEN the funding body, .recommended its closure. The ball is still in the air but one hopes that the actions of individuals are not taken against an institution which plays an important role in modern developmental and stem cell biology.

The STAP case represents a collusion of interests jobs, grants and limelight. The details and ramifications of this affair are too complex to dissect here, and I am sure that there will be books and analysis on the matter. Happy to answer questions at the end. There are nonetheless elements that are worth emphasizing. What could have happened here? I am not the only one in thinking that somebody, Obokata perhaps, did see something like STAP at some point. We all have seen black swans in the lab at some point; the problem is that they don't breed, so we move on. In fact TH Morgan in his book on the Genetics of Drosophila has a section on 'non heritable characters' to quote an example. The magnitude of the possibility that harbours the finding and pressures from different sources that ensue then lead to the slippery slope. A group of people see glitter where there is dust and then you get the package: no questioning from above (if you understand Japanese you might be interested in this, and if not and you are interested, get someone to translate this for you: http://headlines.yahoo.co.jp/hl?a=20140416-00000024-wordleaf-sctch), the seal of approval of the big names, the seal of the big journal and, somewhere, the hope that somebody will see it again. I do not think that someone would expose themselves in the manner that Obokata did, unless there is some truth, and I suspect that she thought that somebody would see another black swan. How much Sasai, as corresponding author, knew is difficult to say but the fact remains that he signed as corresponding author. Much to ponder here on the structure and responsibilities in modern science. Ultimately the problem is that what was driving this affair was not science but the limelight. On the other hand, what brought this out was not just the scientists, it was the scientific community through the open discussion provided by social media, which has empowered people. And what responsibility with Nature? More than they are prepared to admit. They claim that the peer review process could not have picked up the problems (http://www.nature.com/news/stap-retracted-1.15488), but it was the science community which picked it up!. Furthermore, the manuscript had been rejected from Cell and Science. Furthermore, a quick standard run of the manuscript through image manipulation software run by EMBO J picked up problems almost instantly (http://onlinelibrary.wiley.com/doi/10.15252/embj.201489076/full). So, what one can conclude is that Nature's peer review process, not THE peer review process, failed to detect the problems. As Nature refuses to provide details of the review process, we do not know what happened, but the refusal to be open (perhaps reasonable but not justifiable) does not help.

Now, this is not the first time that something like this happens in one of the main journals. Nature, Science and Cell have had their share of high profile retractions. In a remarkable one a few years ago, JH Schoen retracted 8 papers from Science and 7 from Nature (not to mention several from prestigious physics journals). This highlights the problem because as has been stated, the issue here is the scale which highlights the depth of the problem. How come that the journals do not take any impact? Have you heard of any editors resigning out of any of these affairs? Have you heard of the journals closing, receiving public scrutiny? Imagine that they were companies trading in the Stock Market. If something like STAP happens, and happens repeatedly, what happens to the shares? what happens to the company? And yet, in these journals nothing happens.

Where are we?

In the end, what we have is a situation in which these generalists journals have a different aim from their original one. Where they (though I should point out that Cell does not fit this mold) meant to be a vehicle to get science to the general public, they have become a, THE, measure of success. In fact, in China this is made clear in terms of outright payment proportional to where you publish (http://scholarlykitchen.sspnet.org/2011/04/07/paying-for-impact-does-the-chinese-model-make-sense/). But do not be surpised, as pointed out earlier, in Europe and the US we offer something much better: a pension!

We, a collective we, have used Nature. Science and Cell to create a rock star culture which has eroded into the base of what we do. The main reason for this, I believe, is the fact that the structure of the system we are using is, basically, the same as it was in the XIX century to which we have just added layers without restructuring it. An overloaded camel at the end of a long trip. This, together with a change of emphasis and aims, leads to a machine that churns out papers with a lot of power in its hands and under the control of ill prepared professionals which, basically, follow orders. ***NB I am not discussing here scientists led journals, some of which have a few of the same problems but which certainly do not have the 'impact' that lies at the heart of the malaise.***

Fortunately we are waking up and solutions are on the way. In the final part of this discussion I want, briefly, to address some of them.

Part II: solutions?

The problem is, to a certain degree, clear. Let me recap. What was conceived as a way to communicate between scientists and between scientists and the public has become a measure of success, a ruler of quality and an arbiter of professional development. The change in character has altered what science is, how it is done and how scientists are evaluated; too many papers, difficulty of separating the wheat from the chaff, limited funds….Nowadays to do good science is not good enough. To survive in science one needs skills like being able to 'sell', being a good story teller, being able to chat up editors, being savvy on more than the subject matter, and then combine these skills to get money. Furthermore an increasing blurr between data and thought, between science and accounting creates a climate of confusion in which money talks. Spin and good PR are as important as deep science and enquiry. And an important reason for this is that what we are working with is a XIX century system that has never adapted to the times. What we have, as I have said before is a collection of XVII century tulip bubble about to burs –the glamour journals- sustained by a few. But there is change coming (I think) and we need to support it and make it grow. Here are what in my view are the important elements that are leading this change

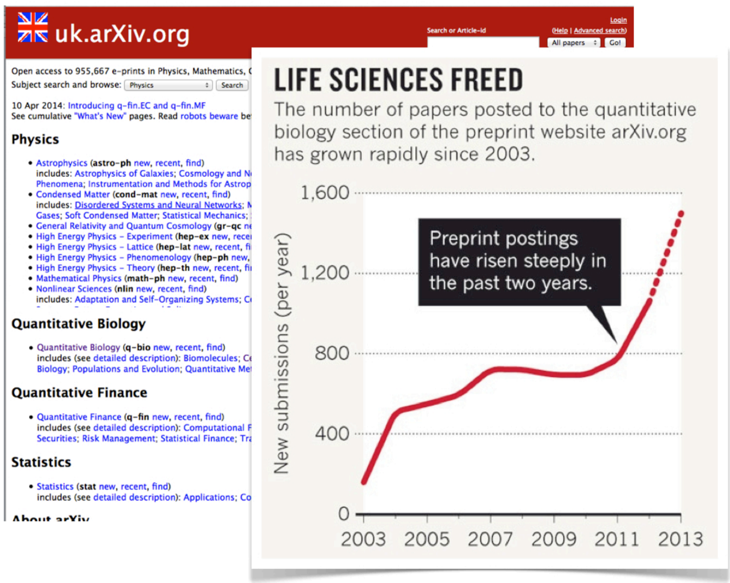
First, ***Open Access.*** This is a most important development and one that for the most part has achieved its goals. You have heard a great deal about this so I shall not dwell on it. Open access is a natural response to the attempt of several publishers to own your/our work, to the fact that science has become a business for the journals but it is our toilings that they work with. Open access is not free publishing but it is rational and sensible publishing and it is good to see that funders of science have rallied to support this move. Everybody should publish Open Acess. While everything is good here, we should not lose sight of the fact that the big publishers have seen the goose of the golden eggs and have rallied to produce their own Open Access journals that take advantage of their brand names to make more money. And this, new journals, takes me to the next all important issue ***New Journals***

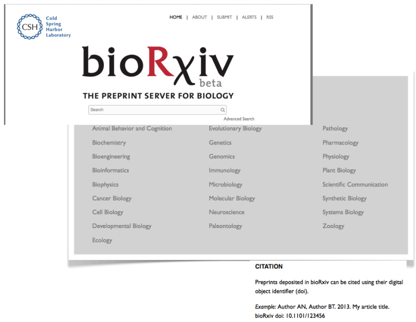
Partly because of the publishing niche opened up by Open Access, partly because of the increasing demand for space to publish driven by an exponentially increasing output, new journals emerge every month. Do we need them? How do we decide where to publish? Are these journals changing anything or are they mere derivatives of what we already know? The latter is often the case and clothed with the mantle of Open Acess there is a barrage of faked and real journals which tempt us with more or less success.

There are however positive exceptions which are actually trying to move away from traditional models and aims. At the forefront is a new journal called eLIfe; you have heard about it earlier today. It is an on line only journal with much to be commended for. I am particular fond of their reviewing procedure and many technical aspects of how papers are presented, the kind of discussions it posits and the support it has from three heavy weights of research funding (Wellcome, Max Planck and HHMI) which makes it a statement of intent. It is a scientists journal which is trying to carve the future. But…….they have a problem, namely that the people who run the journal are the same people that brought us Cell, the rightly maligned impact factor, who review and publish in Nature, Cell and Science and who control where you publish, how you publish and whether you get a grant or not. How can you implement change with the people who created, and still favour, what you want to change? I guess the answer is with difficulty. Furthermore, they promote openness and yet, many letters of rejection are signed by their chief scientific editor, Randy Shekman, independently of the field, because the editors in charge want to remain anonymous. This is not a good advertisement, But remember what I told you, moist of the people behind the journal are the same ones who have created the problem that eLife claims to want to solve. So, while I applaud what they want to do, they still have work to do. Also, the stated aim of the journal is to compete with the glamour journals, but to do so in a fair manner, by attracting quality. But here we hit another problem and this is the difference between quality and cool and eLife is, inevitably, is so far a mixture of the two with a heavy dose of cool. But I do not want to knock them down because they have much riding for themselves and have an opportunity to do something transformative and help us move forward. Take these comments as a recognition of their toilings and their openness to new ideas. They can succeed but they needs to be bold, really bold and not fall sleep in their coolness. And it is for us to make sure that they deliver.

I mention eLife because is, in my mind, the most interesting project but do not forget others. Most interesting amidst these, the loved and hated PLoS ONE which, as anybody who has published on it knows, is not just a place where you pay and publish (there are many of those). It is a serious journal in the spirit of publishing sound, rather than soundbite, science (notice the irony! I have often heard as a criticism of PLoS ONE that it ONLY wants to publish sound science………). Sure it is a mixed pot but I have never had a paper published there without proper revision and their acceptance rate is around 70%. It is a pioneer. Other journals have emerged as derivatives of main stream products and thus the Company of Biologists have Biology Open and Nature and Cell press continue to expand their portfolio to fill their pockets with Open Access journals. So, the choice today is very large. My advice here is simple: publish where you feel it is more appropriate. Do not waste your time in the lengthy and moral sapping process of peer review in the glamour journals. It is not worth it and certainly not worth your time. And yes, wherever possible use scientist based journals.

One interesting development in terms of 'new publications' is the emergence of preprint servers. This is a notion that comes from the physicists arxiv (http://uk.arxiv.org/), which has been running successfully for over twenty years (founded in 1991). In fact arxiv represents the main avenue for publication of new findings for the physics community and they do not worry too much about impact (the biosciences flavour); what they worry about is precedence and being read and discussed. How does it work? When you have results that amount to a manuscript, you prepare them and post in arxiv; you get a doi and you wait for comments or simply mature the work (you can upload new versions of the ms). In the meantime people can see the paper. Then when you think it is ready for 'official' peer review, you submit it to a journal, and most journals, including Nature and Science, accept papers that have been posted in arxiv (http://en.wikipedia.org/wiki/List_of_academic_journals_by_preprint_policy). There are good reasons for this: Nature and Science are journals where physicists publish, as arxiv is a central element of communication for the physics community, Nature et al have to accept the rules of their game. A lesson here: Nature and Science have to accept the rules of the scientists and notice that Cell press does not like arxiv. There must be a good reason for this: it is a publication. Over the last few years, biologists with a more quantitative inkling have begun to use arxiv, and this has led to a new section in the journal on quantitative biology. As a response to this interest of biologists in using arxiv, and as a way to rally the more traditional biology community, a few months ago BioRxiv (http://biorxiv.org/) was launched, a biological version of arxiv. It is working well, has not yet gathered momentum, but it hails a cultural change and I very much encourage you to use both arxiv and bioRxiv. There are other preprint servers, as they are called e.g PeerJ, Figshare and F1000. So, again, you have a choice.





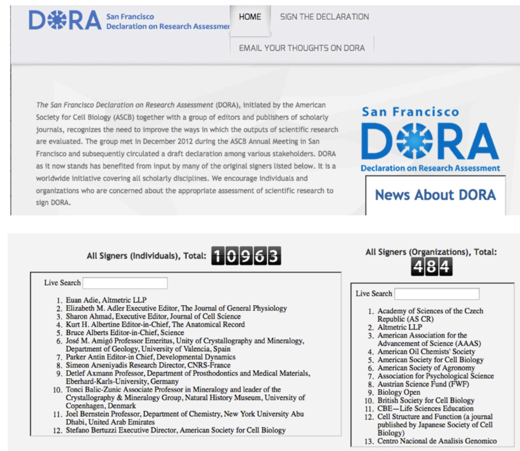
Why use Preprint servers? There are many reasons and I have discussed the matter before ((http://amapress.gen.cam.ac.uk/?p=1239) but here you have two which should be of interest tto you. One, because it gives your research quick visibility and establishes precedence. Two, because it gives you a doi and with it, the ability to refer to it in applications and also in papers. As I say, this is the bread and butter of the physics community and I do not understand why it should not become ours.

And publications rolls on to the next topic, an all important modern classic, ***Peer review,*** which echoes much of what I have said in the first part, so I shall be brief. Few people would disagree that peer review in the biomedical sciences is in crisis and changing it should be the next target of our community after Open Access,. Peer review it is not doing the job that it is supposed to do. At the moment, the main role of peer review is to make it difficult for you to publish and the degree of difficulty is proportional to the perceived glamour of the journal and, in the journals where this index is high, inversely proportional to your interactions with editors and members of the editorial board, remember the remarkable advice from the Cell Press editor I mentioned earlier. Anybody who believes that peer review does a good job is dreaming. Please do not think that I do not want peer review. Nothing further from the truth but, I do not want a suurealistic process which has so much element of chance and clickishness.

There are no easy fixes here. The peer review is the community. We do need peer revew, but we need a system which avoids the extended essays and legal arguments which bedevil the system at the moment. The longer you are in this business, the less you trust the system. These days the collection of reviewer's comments and replies can be longer than paper itself! Some journals, like EMBO J and eLife have interesting leads that should become common practice: one, and only one, round of review (both of them and extending) and, in the case of eLife, comments reduced to 500 words (one page, if you want to be generous). There is no reason why one needs more space to assess a paper. A review is not about the paper the reviewer would like to write with the data and resources of the reviewee, but simply a comment on the paper. You will soon see what publishing a paper in a glamour journal means and you will not like it. Perhaps the worst thing about the process is the number or unnecessary experiments and their cost which do not advance the paper (on this see the almost classic : http://www.nature.com/news/2011/110427/full/472391a.html). A few comments and an editorial decisions is what should happen. As we have seen this is what it used to be and maybe we should go back to go forwards. I wished the examples of EMBO J and eLife would be followed.

Then there is pre- and post- publication peer review. This is, like anything to do with this issue, a huge subject so I shall summarize. Many people advocate for postpublication peer review and to a large degree this goes on, in private, at lab meetings, tea rooms, discussions at meetings. But there is no will to do this openly. Most journals have now, routinely, places for comments which are not used. As we heard earlier: why are happy to write an impromptu review of a restaurant, a hotel or a book but we cannot do it with a piece of scientific work? what is wrong with bioscience? The only public comments that are allowed are positive. It seems to me that the notion and escalation of anonymous peer review has much to do with this situation. What we have is fear of backlash from criticism. The STAP case is a good, positive example of the cleansing power or open discussion of results. There is also a case for prepublication peer review but for these there are venues: preprint servers. As usual, for all the talking what happens is less than what one might expect. Preprint servers are not bursting at the seams. Lots of talk, but less action. The reason is because, for the most part, the scientists that make this tick are sensible and to prepare a manuscript takes time and care. The so far limited use of those servers, and the limited interest of bioscientists in them, should also be food for thought but, in any case I repeat my advide: use them!

And, of course, we need peer review. But we need is to recover some sanity. We need responsible editors who do nor fall prey to the endless sequence of reviews which cost money and careers, and we also need sensible reviewers who understand what reviewing a paper is and, specially, that it is not a way to block a piece of science seeing the light. We need to remember what a scientific article is and remember that it is the work that matters, and if you want to see more of the future let me tell you about another important development: ***San Francisco Declaration on Research Assesment*** (SFDORA or DORA for short).



In 2012, at the annual meeting of the American Society for Cell Biology, a group of publishers and funders signed a declaration, SF DORA, in which there is a explicit statement for “a pressing need to improve the ways in which the output of scientific research is evaluated by funding agencies, academic institutions, and other parties” (http://am.ascb.org/dora/). The main point that DORA wants to draw home is that people should be judged by what they publish and not where they publish. To date the declaration has been signed by over 10,000 individuals and over 400 institutions. My advice is sign and, more importantly, enforce it. Make sure that those who have signed it, abide by it. What DORA says is obvious and it is surprising that it needs to be said.

Time consuming….recently I saw a colelcri of poor talks dor an important job…..the candiates had been selected beacsie of where the pubclihs…..

And so to the future

The point of this lecture was to discuss publishing in the biosciences, why you might want to publish, what would like to publish, how would you like to publish it and also where to pubish. We have seen that answering these questions is more complicated than you think at first sight. There are two main reasons for this, the first one is that publishing today is not just a way to reveal and share our work but rather a complex action which will impinge on our future and job prospects, and one that is not straightforward. Together with this there is the fact that, in contrast with the science itself, the structure of scientific publishing has not changed much over the last hundred years and that therefore we are working with a system which is no only overloaded, in terms of supply and demand, but also, simply, not fit for purpose. As a consequence of this and, really, because of the impact that where you publish has on your career prospects, there is a very open debate and scrutiny of the publications system and associated issues. There are many ideas around as to how to change it, and change is happening –slowly- but is happening. So, where is all this going? To predict the future is always difficult but to close I would like to enumerate a number of elements of the tangle which need to be addressed for change to happen

1.The system of assessment of our work by the journals has to change. The way papers are reviewed by editors and reviewers and the roots of high rejection rates need to be reviewed. Some creativity and leadership is needed here and I would suggest that eLife and EMBO Press have made interesting steps in the right direction about this.

In this context if a reviewer wants to have an extended discussion with the author i.e exceed a certain length in the review, and suggest new experiments (which mean money), the identity should be revealed and the author of a manuscript should be able to have a proper discussion of what is being suggested to see what is reasonable. Like many I suspect that having to reveal the identity will change the tone and the content of the review.

2. Journals should implement seriously their policies of 'conflict of interest' and at this, they should make sure that their editorial boards do not have members that are also in multiple editorial boards of competing journals. This will ensure a commitment of the editorial board members to the particular journal and will, also, expand the group of people that make decisions about the content of those journals.

3. The journals have to adapt to the times. Supplementary Material was another added on to the operating system and as could have been predicted, it has got out of hand. Journals like Nature and Science now publish incomprehensible short papers where the actual science is in the supplementary. The reason why the text of many of these papers is gibberish is because it has been fragmented in the reviewing and publication process. Journals need to find new ways of conveying the science and a way to normalize the size and content of a paper i.e they need to adapt to the times.

4. Science is made by the scientists and it is for the scientists. There is a trend, promoted in particular by Cell Press, of dumbing down the science, of favouring impact and headline over content. Of favouring certain authors who frequent meetings and editors. Pieces of research become stories and journals rather than reporting advances, tell stories about genes and proteins and it is how you tell a story, rather than the content, that begins to matter. This could not happen in Physics but biology lends itself to this (and if you don't believe it remember R Kipling's 'Just so stories" some of which, with added materials and methods, could work well in Cell Press.

Journals need to take us seriously, we need to take the journals seriously.

5. If journals, particularly the generalists, want to keep their clout, they have to be more stringent on how they choose and train their editors. Teaching how to be an editor rather than learning it on the trot should be compulsory. In an increasingly professional world it is surprising that the main qualification to work on those journals (Nature, Science, Cell) is not to like, be bore with or having shown inability in the subject matter they are going to decide on. Imagine that the main reason to become a Cheff is that you are bored with cooking or do not have a palate…… There should be a form of professional qualification for scientific editors e.g a master, on the subject (maybe there are). The consequences of the wway editors are selected are clear for all to see. This can and should change.

But in the end, and for change to happen, WE have to make choices. At the moment we have chosen to emphasize form over content and science has become a routine mixture of salesmanship and upmanship, a power game where a few have an advantage. There are lots of meetings and lots of papers, but (with some exceptions) very little discussion and certainly opemn discussion. In public, all papers are great, all findings are exciting and there are lots of breakthroughs. In private, the picture is different. Part of the reason for this is that we have substituted questions and science for data presentation and therefore there is little conceptual to talk about. There is also the problem, allow me to emphasize, that the journals that drive the most visible biosciences are run by people that are inexperienced at both science and publishing. My advice is always, to avoid those journals. As long as we continue to search for their endorsement as a proxy for scientific quality, we shall be delaying change and we shall continue in a path towards forgetting the heart of the scientific quest.

The future belongs to you, so: make sure that you see it before it catches up with you. Nowadays the form of science is as important as its content and for this reason you should make sure that you shape it, that you don't let the shaping to others. Open Access has become normal, let us make sure that other aspects that need change also become normal. Move away from glamour journals and towards journals that are led by scientists, use preprint servers, be open, careful and mindful when you review papers and grants (don't do a review that you don't want to see yourself), implement DORA. Let us make sure that science gets back to a normality adapted to the times.

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On the Schoen affair, take as a start: http://en.wikipedia.org/wiki/Sch%C3%B6n_scandal but there is much more on the web.
Very readable, interesting and informative history of Nature magazine:
http://www.nature.com/nature/history/timeline_1860s.html
On Elsevier, with its long and distinguished history:
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