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Journal of Unsolved Questions

Quality of Science

Is it Possible to Measure Scientific Performance with the h-Index?

Article:
Smectic LC-Elastomers with NO Shape Change at the Phase Transition

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Dear Reader,

With the very issue you are currently reading, volume 4, issue 1, the fourth year of JUnQ is dawning. The past year started with a transition of editorial responsibility from the 'old veterans' to eager apprentices and it is for you to judge, whether this transition was a successful one. Do you think, we deliver high quality articles? The reader's interest was and is a major indicator for quality in scientific publishing: A subscription based journal only survives, if it is able to acquire a sufficient amount of readers. Your current thought may be "But the pdf I am currently reading on the internet did not cost me anything! (or if you bought the printed version you just balanced the printing costs) and you are right about that! JUnQ is not a subscription-based journal but open access, meaning free to read (and, lucky you, also free to publish in our case). The fact that Open Access journals, which live on author fees, do not depend on the appreciation of their readers for their revenue brings us to an intrinsic enigma of scientific publishing: How do we assure the quality of our merchandise? This is the very question we want to address in the present issue's journalistic part.

THE accepted measure of publication quality is the so-called impact factor, an index which describes the "mean-citedness" of an article. Although widely applied, the impact factor is a rather ill-suited tool for quality assurance, as Prof. Konradin Metze already pointed out in JUnQ almost two years ago.[1] As a matter of fact, the journals with the highest impact factor are also the ones that are best known outside the scientific community. If there are now more and more voices that criticize the main quality indicator of these very journals, a general plight of scientific quality assurance becomes apparent. Days before I was writing this editorial note, one of the 2013 Nobel Price laureates in medicine, Randy Schekman, called for a boycott of "the big brands of publishing", that "accept papers that will make waves because they explore sexy subjects or make challenging claims". According to Schekman such a publishing policy can "encourage the cutting of corners" in extreme cases, meaning it makes the authors prone to submit fraudulent papers to the "big brands".[2] It seems almost like some fly on the wall told Prof. Schekman of our next topic! To put one thing right: We, JUnQ, do not charge the "big brands" with any wrongdoings, it is just quite satisfying how a Nobel Price laureate picks up the main idea of our journal: Science is not always flashy, it also consists of digging for dull-thought diamonds in the junk. Schekman sees "inappropriate incentives" in scientific career paths where "the biggest reward often follows the flashiest work, not the best", while admitting that he himself followed this very incentives out of pure rationality.[2] Although Schekman admits that the "big brands" publish "outstanding research" (they published his own papers after all), he sees not all big brand papers as outstanding and reminds us that there are other "publishers of outstanding research". So in summary, we end up with the call for a new quality benchmark in scientific publishing. Since Mr. Schekman is editor-in-chief of the open access journal eLife (sponsored by the main biomedical funding agencies Howard Hughes Medical Institute (US), Wellcome Trust (UK), and the German Max-Planck Society) his solution is, of course, open access: Since they do not have to promote expensive subscriptions, as Schekman puts it, open access journals could "accept all papers that meet quality standards with no artificial caps".

Although Schekman's model has an undeniably pleasant feel, its mere suggestion does not solve any problems: Even if future journal's are to be "edited by working scientists" as Schekman suggests,[2] these editor's will need a high performing measure of quality in a fast growing publishing business. We want to provide you with further insight into this topic by presenting a possible future of scientific publishing, an article by Prof. Michael Schreiber on the impact factor's younger brother, the "h-index" (page 5), and an interview with Jörg Meidenbauer from the academic publisher Peter Lang Verlag (page V). But we do not want to restrict assurance of quality to science, since it is of equal importance in teaching, a task met by the center for quality control (ZQ) of Mainz University. Its head, Dr. Uwe Schmidt, was interviewed by the editorial board (page XI). In addition Andreas Neidlinger and myself tried to shed shome light on the current publication behavior in our essay "Open Access and Public Peer Review – The Future of Scientific Publishing?" (page III). What may be rather uncontrolled in the German system is what qualities a doctoral candidate has to possess and how he is to be advised, a question addressed from the perspectives of the humanities by Prof. Jörg Meibauer in his essay "How to become a Scholar without a Lighthouse" (page VIII).

Before you, dear reader, miss JUnQ's usual qualities: This issue contains again articles of science, be it on the above mentioned "h-Index" or on the puzzling fact that liquid crystalline elastomers without a shape change at the phase transition do exists (page 1). But the feel of change that, hopefully, was not to apparent last year shall remain part

of JUnQ: With this issue we want to fuse Open Questions and Articles to general scientific articles and introduce a new type of contribution that is thought to address the humanities in particular, but also any researcher that hatches interesting thoughts or opinions on a (controversial) subject. With our new category called "Views on life, the universe and everything" we want to invite YOU, dear reader, to write us about anything that you always wanted to elaborate on. We are looking forward to your submissions!

Enjoy the present issue of JUnQ and have a nice start into 2014!

—Felix Spenkuch

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[1] K. Metze, *JUnQ* **2012**, *2*, 2, XV–XVIII.
[2] http://www.theguardian.com/commentisfree/2013/dec/09/how-journals-nature-science-cell-damage-science (last access on 15.12.2013).

Open Access and Public Peer Review – The Future of Scientific Publishing?

Andreas Neidlinger and Felix Spenkuch

In this issue of JUnQ we set out to illuminate different ways of scientific quality assurance. How do scientists from various fields of research make sure that their work (as well as the work of others) maintains a high standard of quality? How is "quality" measured in the first place?

Quality assurance in publishing is of special importance in present times, where we experience a paradigm shift in publishing: In contrast to traditional journals, which acquire money via subscriptions, more and more journals are *Open Access*, which means readable for free. The whole thing started with the arXiv server for preprints in Mathematics and Physics in 1991 and now, two decades later, ca. 25% of all articles are freely available online immediately after acceptance,[1] just like in JUnQ. In biomedical research open access got a boost eight years after the start of arXiv, when the director of the US National Institute of Health (NIH, a main source of funding in US biomedical research) proposed an archive of free biomedical papers in 1999, which led to the founding of PubMedCentral in 2000.[1] PubMedCentral was an immediate and exceptional success, resulting in a call for boycott of journals that did not deposit their papers on PubMedCentral six months after publication. To date more than 50% of all published articles are open access at least twelve months after publication.[1]

Although nobody can object to free accessibility of papers there is a pitfall in this development: Unlike JUnQ – we are a nonprofit journal – or arXiv, which is funded on voluntary basis, many other open access journals, need to make money to survive. Since open access journals get no subscription fee from the reader they usually acquire publication fees directly from the author. This business model results in a dramatic shift of the journals main priority: While subscribers and thereby readers increase the revenue of "classic" subscription based journals, submissions are the only important figure for open access journals. Consequently, critics of open access point out that quality control is of lesser importance for open access journals, since, in terms of revenue, it does not matter much whether the articles are valued enough to attract readers or not. As one of the authors of the text in hand is RNA biochemist by profession the main journal of his field, Nucleic Acids Research, published by Oxford University Press, is an open access journal since 2005. It does not, at least in the author's opinion, stand out by publishing low-quality content, however. The second general journal of the RNA field is "RNA" published by the RNA society. RNA articles are open access twelve months after publication, which makes articles younger than that a valued merchandise, if you get access to it. Obviously, open access combines advantages concerning availability with disadvantages, while it does not result in low-quality publishing automatically. arXiv for example could function for more than a decade without any quality control except moderators that may re-categorize submissions as off-topic. From 2004 on publication on arXiv requires the consent of an "established" arXiv author.[2] It could be shown by Davis and Fromerth in 2007 that deposition on arXiv resulted in a decrease of downloads from the actual publisher's website, demonstrating the rivalry of open access and conventional publishing.[3]

So open access publishing is a successful, widely accepted approach, but it harbors a severe secondary effect: The amount of journals increases with exceptional speed, since it does neither require much expertise nor money to set up an online open access journal and as direct consequence the publication volume is exploding with a new paper published every 20 seconds (*whatever you want to publish – you will find a journal that will*).[1] The development of reader's tools, which are quality control and quality ranking or assessment are lacking behind in comparison. At least the author based in biochemistry tries to hold onto well-known journals and well known authors to survive the "publishing maelstrom" of the current time, where no functional compass for scientific quality seems to exist anymore that would help in finding papers worth the time required to read them. Quality control or rather the lacking of it in open access journals was tested by John Bohannon recently, who submitted over 300 versions of a manuscript dealing with a promising anticancer agent extracted from lichen.[4] More than half the journals accepted the paper, while the revisions, if any at all, were mostly concerning the format of references or the abstract. The disaster in this story is that the studies were completely made up and so full of flaws that, according to Bohannon, "any reviewer with more than a high-school knowledge of chemistry and the ability to understand a basic data plot should have spotted the paper's short-comings immediately".[4] While quality control was neglected, the financial aspect was treated with due care, resulting in timely requests to pay the author's fee after submission from the editors.

If the traditional way of publishing is changing, what does this mean for the way that guarantees the quality of these papers? The usual, classic way to guarantee quality in scientific publishing is *Peer Review*. The manuscripts submitted to JUnQ are subjected to a double peer review process, where the article is sent to two experts in the respective field of research to be confidentially checked for errors and/or (unintended) ambiguity. Most other journals do the same. The manuscript is not published until the referee's (and the editor's) remarks are revised to satisfaction. At first sight it is a good idea to ask other researchers to review manuscripts within their area of study. However, one problem about this way of quality assurance is the anonymity that is granted to the reviewers. Taking scientists from the same field of research as referees of scientific output is ac-

tually like asking the head of software development of the Microsoft Windows platform to evaluate the work of the competing brand Apple or vice versa. Scientists from the same field are mostly nothing less than strong competitors, a rivalry which can, in extreme cases, result in more or less arbitrary or overly critical referee reports. Thus, the rivalry between author and referee might abolish an effective quality control.

A different approach to manuscript review is *Open/Public Peer Review*, which allows immediate publication of manuscripts after a cursory quality check avoiding thereby that many months pass between submission and final publication. Still experts from the same or adjacent fields of research are addressed to review the manuscript, but peer review is done after publication, where stealing of data is no longer possible. The reviewers are selected and need to post their names and affiliations alongside with comments. It is not surprising that this new approach to quality control that deprived the reviewers of their anonymity was launched in a comparably small research discipline that is more driven by cooperation than by rivalry: Since 2001 the journal "Atmospheric Chemistry and Physics" (ACP) applies post-publication public peer review with great success: The "Anticipation of public peer review and discussion deters authors from submitting low-quality manuscripts and, thus, relieves editors and referees from spending too much time on deficient submissions", while the reviewers have to identify themselves only to the authors directly.[5]

It seems fitting that the just cited review on ACP was published in a journal of the "Frontiers" series founded in 2007 at the Swiss Federal Institute of Technology in Lausanne. According to the editor in chief of "Frontiers in Neuroscience", Idan Segev, the group of journals originated from the urge to change the "rejection attitude" of referees that reviewed papers with the sole aim to find fundamental flaws culminating in rejection. The idea behind "Frontiers" was that reviewers and authors should be united rather by the idea to improve the paper than working against each other. When the paper passed a "threshold of excellence" in this "independent review phase" it enters an "interactive review", as Segev stated, that consists of discussions between all authors, the still anonymous reviewers and editors using an online forum. Frontiers calls their review process the "fastest review system in publishing" and sees it as a revised peer-review process. The so-called "Frontiers Evaluation System" may then elevate a respective paper to a new tier: Based on the reading activities in the first three months after publication and on scores supplied by the "Frontiers community" articles can qualify for a second peer review step which eventually may transform them into review style articles that are targeted to a broader audience. The Frontiers Series also features special ad-hoc publications called "General Commentaries" that are subjected to post-publication review and a community network that increased article views and downloads by 30%.[6,7]

Post-publication peer review may leave its niche eventually, since it was taken up by Vitek Tracz, chairman of a conglomerate called the Science Navigation Group and one of the most important publishers of the last decade. Tracz launched a journal called F1000Research, which is, of course, open access, but also applies a new post-publication public peer review.[8] Furthermore, all gathered data from the study must be included in unprocessed form in the submission, allowing other researchers to dissect the manuscript down to the bone of raw data. As Tracz puts it, one does not "just want the narrative of what you think you found, but what you actually found".[8]

Public or not: The scientist doing literature research is still faced with a steadily and fast increasing number of journals and publications and thereby in a never more urgent need of a powerful quality compass. How to evaluate science is a question that was reflected already by Prof. Konradin Metze in JunQ ca. 18 months ago in JUnQ's 2nd issue.[10] The best known criterion to date is the impact factor generated by the Thomson Reuters database "Web of Science", which tries to estimate the "mean citedness" of an article published in a specific journal. Metze sees the impact factor as detrimental to science and as "nonsense (...) to measure something" and even calls for caution if one uses the citation count of the very article under investigation to evaluate its impact, since especially pure "methodological papers" may get high citation counts if compared to revolutionary and paradigm changing papers of e.g. Einstein or Watson and Crick. Metze attests "counting citations (...) to be the best proxy available at the moment (...) [but that it] should be used with great caution".[1] Even if we do not use the impact factor to direct our reading habits or decide where we publish, many will admit that they categorize journals in a more or less subjective "ranking system". Brembs *et al.* recently wrote about "unintended consequences of journal rank" in, where else could it be, a Frontiers journal.[9] The authors state that journal rank is a weak to moderate predictor of perceived importance, meaning that only few papers that are published in high ranked journals get highly cited, while others don't although published at high rank. In addition Brembs *et al.* see journal rank as a "moderate to strong predictor of intentional as well as unintentional scientific unreliability" that is expensive, delays science and frustrates researchers. Similar to Konradin Metze[10] Brembs *et al.* criticize that the impact factor violates "most basic scientific standards" while generating a "subjective judgment of journal quality". The surprising conclusion of the review: A return of scholarly communication "back to the research institutions", culminating in a transformation of scientific publishing into an "archival publication system" that would be run by librarians. Professional editors could have their place in this new system by being paid for selecting especially important work post-publication. The authors envision a publishing system where the "products of our [the scientist's] labor is back under our own control", where open evaluation, metrics and social networks control quality, but see that "almost anything appears superior to the

[1] Editorial note: For another approach on rating scientific output take a look at Michael Schreiber's text about the Hirsch index on page 5 in this issue of JUnQ.

status quo". The work of Brembs *et al.* clearly demonstrates how remote from the current system visions on scholarly publishing are already, alarming us to take action for the better.[9]

Thinking of a future in publishing consisting of open access and public peer review – everyone is thrilled, everything is working. Of course, as it always is in scientific publishing, the next endeavor awaits: Reproducibility of experiments and the "publish or perish" vicious cycle are most likely to be the next construction sites. As usual, final answers and definite truths cannot be provided here. Just one thing remains true: One has to be aware of the strengths and weaknesses of the current (and always changing) publication strategies and must not trust ratings blindly.

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Publishing in the Humanities – Interview with Jörg Meidenbauer

Dr. Jörg Meidenbauer[2]
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The Frankfurt-based Peter Lang GmbH is part of the Peter Lang International Academic Publishing Group, which is domiciled in Berne/Switzerland. The company has been engaged in academic publishing for more than 40 years, focusing primarily on the humanities and social sciences. Some 1,200 works are published in Frankfurt each year in electronic and hard copy format, together with some academic journals. To find out more about the view of publishers on quality, JUnQ editoral board member David Huesmann sat down with Dr. Jörg Meidenbauer – CEO of Peter Lang GmbH – to discuss the role of publishers in the quality assurance process.

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JUnQ: Let us start with a controversial question: Why do we need publishers in times of the world wide web? Can we not just upload all our research ourselves and make it available for everyone?

Meidenbauer: Well, of course everyone can simply upload her or his research onto some server, and at the end of the day we will see if it is then visible for everyone or rather lost in the depths of the internet's ocean of information. But seriously, I think the role of publishing companies has not changed, even if the environment that they operate in has been changing dramatically. The role of publishers has always been to make content visible, to put it into proper context and to make it accessible. A publishing company operates in five dimensions – and I think this is true for all disciplines:

1. It creates products (different from content), which today means books or journals in printed and electronic forms.
2. It adapts contents, e.g. for databases. This field is growing rapidly, as databases are becoming more and more important in the social sciences and in the humanities.
3. A central task of publishing is quality assurance. A publisher should check the formal quality of a scholarly work, whereas peer experts can deal with the quality of its content. I am a historian by training, but I wrote my dissertation a long time ago. I still am familiar with the methods of the discipline, but I do not know what the current issues are. So I can check if the formal quality of the content is ok, but I

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cannot tell whether the author reflects actual discussions, even when it comes to the topic of my own work. That is why – as a publisher – I have to focus on formal quality.

4. As I said, one of our tasks is to put content into context. This means building up a program that stands for certain disciplines or topics. We do not want to be a platform for just anything (like the internet). In the humanities, series with a specific focus are very important.

5. Marketing is also a big issue: Making content visible in databases, in aggregators' catalogues for booksellers and library systems – especially when it comes to electronic books. And there is also the issue of distribution. In the age of Gutenberg, distribution was separate from marketing. You had advertising for and selling of books. These were completely different processes. When it comes to electronic content, distribution comes closer and closer to marketing. Nowadays, you see content in a database, you click on it to get more information, and with the next click you buy it.

So if the publisher – as an expert for academic communication – takes proper care of all these dimensions, it will be more effective than just uploading something to the internet.

JUnQ: What are quality criteria that you look for in works that you publish?

Meidenbauer: In the humanities, quality is defined by the sustainability of the content, and how sustainable it is only becomes apparent in the course of the scholarly discourse. Life-cycles of academic works in the humanities are quite long – maybe decades – and so it's not easy to have an objective measure of whether content is of high quality or not. Of course you have hints. You can see, for instance, whether the author worked with proper methods, especially when it comes to the more empiric approaches of social sciences. But, as I have stated, a publisher in the humanities, can only check objectively if formal quality is good. That includes language, orthography, citation etc.

JUnQ: Do you look for different criteria in empirical work and in work that is more theoretical, like say philosophy?

Meidenbauer: A philosopher might appear as the prototype of a person sitting in her or his ivory tower, writing just for him- or herself. It is very, very difficult to have an objective measure of whether a philosopher is right or wrong. You could even start by asking what is right or wrong? I think the more it comes to classical humanities, the more difficult it is to state objective criteria for quality. From the humanities' point of view, you will never win a debate on whether you are objective or not. Lots of people would say an objective view on anything in the world simply does not exist – and so there are no objective criteria for quality.

JUnQ: Would you say, things like H-indices or impact factors, that appear to be objective, provide a good way to measure the quality of scientific work?

Meidenbauer: Even in the humanities, everybody knows the stories, appearing at least twice a year, of big fakes published in well-known STM-journals using objective methods. So of course there is a discussion, whether quantitative criteria are suitable or not. I personally think that quantifying methods can be proper approaches to evaluate quality – but they cannot guarantee it.

JUnQ: So why are these criteria used anyway? What makes them so appealing?

Meidenbauer: What makes them attractive is that numbers can very easily be linked e.g. to funding money as well as career development.

JUnQ: If you could decide, what criteria would you choose to measure quality in science?

Meidenbauer: It depends on the discipline, of course. Generally speaking, there are indicators like the observance of all relevant material or literature. Does the author know the relevant literature and does he take it into account? Other indicators are methods. And you can, of course, always look at the conclusiveness of the argument, is it logical or not? But, as you can see, with every indicator I mention, a new field for discussion opens up.

JUnQ: Peer-review is often used to assess the quality of scientific work. What does peer-review mean? Does Peer-Review mean the same thing to everyone?

Meidenbauer: To start with your second question: certainly not! I think what is important when it comes to peer-review is that there is no common tradition of peer-reviewing, neither in a cultural context, nor in the context of a certain discipline. We had very intense discussions about this within our publishing company as well as with external series editors. In a certain phase of this discussion I have collected different forms of peer-review and I found more than ten. There is open, blind (single-blind, double-blind), internal or external peer-review only to state a few. In a proper sense, "peer-review" can mean any way of examination by colleagues.

JUnQ: How independent are the reviewers in this peer-review process?

Meidenbauer: It is often stated, at least in the humanities, that the fields of research are so specialized that those three to four people who deal intensively with a subject simply cannot peer-review each other blindly. They even recognize each others style of writing. One might think of possibilities where independence is compromised, for example when the reviewer knows the author and the author is on a board that

decides about funding the reviewers' research. It would be very difficult to stay objective under these circumstances. So I would very clearly state that you get indicators for quality, but no guarantee for absolute truth.

JUnQ: When confronted with over interpreting data, scientists often claim that editors pressured them to make their results more interesting.[3] Can a publisher be very successful when he is only looking at quality or is there also a need for exciting and controversial content?

Meidenbauer: In the humanities, with very long life cycles, you never know exactly what is on the way out, or what will be in the future, because you simply do not know the questions of future scholars. That is the first thing. Another aspect: Doing your research you might find out that you focused on questions that cannot be answered satisfactory at all. An historian, for example, might have an idea, go into the archives to look at different sources and after some time find out that there are no sources on the subject he intended to work on. Usually, he would not publish that fact; he adjusts his question a bit to make it worth dealing with. So the issue here is: Is there a worthwhile question to answer or not? If the answer is negative, and you did not put too much time into finding out, no problem will arise. But if you invested a lot of work and money into your research, just to find out that there is nothing there, you will be asked: What did you spend your time and money on? Efforts to avoid such discussions might turn the interpretation of results in a certain direction, but this is not that common in the humanities. Naturally, everybody involved in the scientific process prefers results that are worth disseminating – researchers like to enhance their reputations, and publishers need to boost book sales. But I do not think publishers in the humanities put a lot of pressure on their authors.

JUnQ: Why do you think a lot of cases of plagiarism were found in the theses of German politicians in the last years? Do you think the cases of plagiarism really increase or are they just easier to find nowadays?

Meidenbauer: I think this is a political issue in itself. It is quite interesting, that when plagiarism became an issue of public debate, a very prominent politicial figure was involved first,[4] and then a number of other important politicians had to step down. When you think of the latest findings,[5] it was not such a big deal anymore. So, while I think that there is misuse of the whole system of academic titles, which has much to do with the reputation coming along with an academic title in Germany, I also think that the time of scandals in this area might well be over. Topics of political scandals have always been changing, and it seems now everybody is waiting for a new big issue. In the end I would say that today's possibilities were not available at the time these works were published, and that might put things into a different perspective. All in all, the plagiarism scandal had a lot of impact, since there is a stronger focus on plagiarism now and more people are aware of the problem.

JUnQ: So there might actually be something good coming out of this scandal for the publishing industry?

Meidenbauer: Yes, and for academia, because everybody is getting more aware of the potential for fraud that the system offers. And I think that is a good outcome, even if not all of these scandals or pseudo-scandals were helpful.

—David Huesmann

[3] http://www.sciencemag.org/content/342/6154/68.full

[4] Editorial note: Karl-Theodor zu Guttenberg, former German Minister of Defense, stepped down in 2011 after plagiarism in his doctoral thesis became known.

[5] Editorial note: Frank-Walter Steinmeier (former German Vice Chancellor) and Norbert Lammert (Chairmen of the Bundestag (Lower House of German Parliament)) were both accused of plagiarism in their dissertations, but were found not guilty by their universities.

How to Become a Scholar without a Lighthouse

Prof. Dr. Jörg Meibauer[6]
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Jörg Meibauer attained his Ph.D. at the University of Cologne in German linguistics in 1985. He habilitated in Tübingen in 1993 and is a full professor at the Department of German Studies at the Johannes Gutenberg-Universität Mainz since 1998.

1 In the Lighthouse

Albert Einstein, in a famous speech in the London Royal Albert Hall, proposed that young scholars who want to think a problem through should be given a job as a lighthouse keeper. That was in October 1934, and what he had in mind then were job opportunities for emigrated scholars. But ever since, the idea that someone did her Ph.D. in the splendid isolation of a lighthouse has lost nothing of its fascination. The picture of a young woman working hard on a scientific problem, not only illuminating passageways for the ships, but also enlightening society, fits well our romantic ideas of academic work.

Taking the picture more seriously, we may ask two questions: Whether Ph.D. students need a job, and whether they need isolation. The first question can be answered easily. Ph.D. students need a living, be it on the basis of a scholarship or a grant, be it through a job as a research assistant. Scholarships may ensure that one can fully concentrate on one's work; however, they are restricted in time, and it is often unclear whether a Ph.D. can be achieved within, say, 2 1/2 years. Hence jobs as a research assistant appear to be a good alternative, all the more since the young researcher is embedded into a research team or the chair's respective work. I assume that working at a supermarket or at the gas station, while valuable and enlightening in other regards, is not very helpful when you are to describe which constituents may fill in the German prefield (i.e., the space before the finite verb in a German sentence).

This leads us to our second question: The isolation of the lighthouse, according to the romantic idea, helps the Ph.D. student in focusing on her own thinking or on the development of that thinking. Indeed, there are students who need and enjoy the "lonely" work on a project. However, there are students who need contact to friends, spouses, and family; hence the lighthouse's isolation would be detrimental to their work. So it depends. What is true, however, is that there is no success in the creation of academic work without the chance to focus on that work. We can then ask what might be helpful for this focusing and what hinders it.

In the following paragraphs, I would like to point out some factors that enhance academic success, at least with regard to my own field of research, i.e., German linguistics. We all know, of course that there are differences between the humanities and sciences, yet linguistics is a field that has to do with both areas: it is concerned with historical and social aspects of the language, as well as the functioning of language in the brain.

Recently, there is a certain skeptical attitude towards the individual relation between a supervisor (traditionally called "doctor father" in German) and her Ph.D. candidate that is prominent in the arts faculty. The skeptics hold that, as is the case in the hard sciences, Ph.D. students should work in classes or schools, being looked after by a group of academics. What I like to point out in the following paragraph is that there are several factors that support the student's progress, and other that are hindering in this respect. These ideas are not particularly new; however, it is worthwhile to discuss them in the light of new developments in our academic system of education.

2 See the Light

Of the many factors contributing to academic success, I would like to single out knowledge, trust, and motivation. These are factors that are important in all stages of academic maturation, yet have to be parameterized according to the respective developmental stage of a Ph.D. student.

2.1 Knowledge

Being keen on outstanding, excellent, cutting-edge, pioneering work (the correct prose here is sometimes bordering on bullshitting)[1] we often forget that ordinary BA and MA studies lay the ground for later success. So the question is not so much how new little Einsteins can be created, but rather what we can do to raise the average quality of every single student. This holds all the more when the relation between academic staff and the number of students is taken into account. Many colleagues from abroad are absolutely flabbergasted when I tell them that we have around 2.800 students at the German department, yet only 3 full-time linguistics professors to teach them.

In our department, we teach linguistics on a broad basis, i.e., the core fields of linguistics (phonology, morphology, syntax, semantics, and pragmatics) as well as fields like language acquisition and language change. We also invested much energy into the creation of introductions into German linguistics, i.e., *Einführung in die germanistische Linguistik* (Introduction into German linguistics),[2] *Schnittstellen der germanistischen Linguistik* (Interfaces

[6]e-mail: meibauer@uni-mainz.de

of German linguistics),[3] and the book series *Kurze Einführungen in die germanistische Linguistik* (Short introductions into German linguistics) edited by Meibauer and Steinbach.[4] These works are used as textbooks during BA- and MA-studies and have proven to be very helpful in establishing a fundament for all educational purposes, i.e., teacher education as well as education of future researchers. Rather often, I overhear remarks of colleagues who speak in a derogatory tone about "mere textbook writers". I object to such statements. In contrast, I would like to propose that textbook writing should be considered as one important qualifying property of professors. Naturally, when it is more attractive and lucrative to be engaged in activities aimed at funding, text book writers are down-ranked in the academic hierarchy.

Students, on the other hand, are lucky to have clear-written, up-to-date introductions with exercises and solutions to those exercises. And it helps when Ph.D. students have a solid scholarly fundament to build upon.

Admittedly, when it comes to more specific requirements for a successful Ph.D. dissertation, we face more problems. I would like to point out two aspects. The first is the ability to read a lot. The second is the ability to use statistical and experimental methods.

As insiders are well aware of, there is a growing amount of scientific output with a tendency of scholars to specialize in topics and theoretical approaches. Yes, one should create a law saying that every researcher is allowed to publish only one article each year! The insight into this amazing scientific productiveness comes as a shock for every Ph.D. student. How the hell is it possible to read and understand all this stuff? If someone works at a paper for 3 years, and this paper is finally published in a well-renowned journal like *Language*, do you really think you can read and understand that paper within 2 days? Two weeks would be a better estimate. This is a problem that is similar for young researchers and established academic staff. But it has to be solved within the limits of the dissertation. Try to read only the relevant papers, try to integrate what you have read and understood into your own writings, but read!

As for knowledge in statistics and experimental methods, this is something which our (in a way still philological) curriculum does not contain, because it used to not belong to the field of German philology. With linguistics becoming a more empirical and experimental discipline, things are changing. Here, Ph.D. students need the help of experts in order to keep in touch with the newest developments in methodology. Still, seemingly old-fashioned methods of close reading and interpretations will not vanish as academic virtue.

2.2 Motivation

In a famous textbook on semantics the authors claimed that they wrote the book because they wanted to be rich and famous. Alas, I admit I also want to be the George Clooney of linguistics, but this is not a very realistic goal. So what should be the motives for Ph.D. students to spend important years of their lives at the writing desk or in the laboratory? When I answer "Because of the fun", I usually earn raised eyebrows. Yet, I think that the fun of academic work is often underestimated.

In the humanities, it is common knowledge that attractive jobs inside and outside the university are hard to get. Because there is not much room for extrinsic motivation (becoming rich and famous is impossible, and if you are paid at all in the end for what you studied, you are lucky), intrinsic motivation is more apt. Fun arises when you have mastered a difficult problem, when you find mistakes in other's works, when you have a "good idea", when you detect structures in experimental results or corpus data, when you find an elegant way to introduce into your topic, and so on. Numerous occasions to have fun are waiting for you. And, believe it or not, writing this article is fun for me!

I acknowledge, however, that many students are scared. To have to write so much, to have to be better than many competing researchers is, admittedly, a high stake. I do not know of any hard-and-fast medicine against frustrations coming along with academic work, and I doubt there is one. (O.K., JunQ is an antidote!) So what remains as a scholarly motivation is the serious wish to research and to write an own book that will be useful for the community and the society as a whole!

2.3 Trust

Being well equipped with a solid basic knowledge and a good portion of motivation, what is still needed for the Ph.D. student is trust – trust in her own abilities as well as trust in the will of her supervisor to do a good job. Trust has to be balanced: The one who is too critical against her own abilities will face problems, and the one who is too naïve in this respect will also have problems. And a partnership, as is the relation between a Ph.D. supervisor and the supervised Ph.D. student, that is influenced by mutual distrust, is in danger just as much as if the supervisor was the student's superhero, or the student the future genius in the supervisor's wild fantasies. It's all about balancing things out.

Let me add the truism that a supporting family and friends are also important for success. We all like to read about this in acknowledgments and prefaces – the cat, the pizza deliverer, and the coach from the fitness center. Yes, they all have their share in a high-quality dissertation, and, face it, in the end the cat might as well be more important than the supervisor!

3 Two proposals

Already Albert Einstein knew that the pressure on students to produce more and more papers leads to sort of superficial work and has exhaustive effects.

"An academic career puts a young man into a kind of embarrassing position by requiring him to produce scientific publications in impressive quantity – a seduction into superficiality, which only strong characters are able

to withstand."[5] ("Denn die akademische Laufbahn versetzt einen jungen Menschen in eine Art Zwangslage, wissenschaftliche Schriften in impressiver Menge zu produzieren – eine Verführung zur Oberflächlichkeit, der nur starke Charaktere zu widerstehen vermögen.") Quality, so much is clear to everyone interested in scientific progress, ranges above quantity. Yet quantity is important with respect to all systems that measure so-called "impact".[6] We should fight against the prejudice that the mere amount of output is evidence for hard work and academic success.

In order to foster good quality in academic writings, I have two proposals. The first proposal is one that has to do with failure in academic enterprises. In fact, I heard this proposal years ago from my Mainz colleague Carola Lentz, an ethnologist. She said frankly that some Ph.D. students do fail, for a range of reasons, among them illnesses, birth of children, the difficulty of a task, and so on. Now failing is certainly not something which is appreciated in our society that builds on competition and in which universities are more and more managed as if they were banks. However, what is really the problem is this: If someone works on a problem, say for 2 years, and then s/he decides to skip that work, why not give him/her a sort of testimony explaining that he engaged in this and that scientific work and that the results of this work are useful to others? That would be a human reaction to academic "failure" that would reduce much stress and would contribute to harmony in the academia.

The second proposal relates to the formats in which Ph.D. students and supervisors can learn from each other in an optimal way. There are many formats on the market: seminars and colloquia, workshops, private consultations, permanent e-mail correspondence, etc. All of them are valuable, but each has also weaknesses. Too much competition with fellow students, a topic being not well understood or similarly important for each member of the group, etc. are such factors that make regular cooperation difficult. Often, students will not have time to meet, or they cannot attend meetings because they do not live close to the university at all.

Since discussing monographs and papers is, at least in the arts, an important life of the academia, my idea was to write reviews together with a group of engaged students. In writing a review, one has to carefully represent another's work, and all criticism has to be justified. So, by taking responsibility for the review and creating a real academic product, we learned a lot from each other, and had a lot of academic fun. The results are [7] and [8]. (Let me add that it is by no means common that students of German write in English. Some even avoid reading English texts.) This was hard work, yet I wish I had more time to do it. But I do not really know when or where to do it in our current BA and MA curriculum.

In contrast to practices in other fields, co-productions in the humanities are not very popular. They are time-consuming and the risk of not really understanding the ideas and ambitions of other researchers is high. Yet I believe that such co-operations are very important for a number of reasons. They are important for the whole discipline,[9] they are important in order to learn from each other in a systematic and controlled way, they are important in order to appreciate the abilities of others and to relativize one's own. In recent years, I wrote papers together with Ph.D. students, with assistants, and with colleagues, and I think we profited a lot from each other. Note that it was close to impossible to write a paper together with a professor when I was young. In sum, the times, they are a-changing, sometimes in a bad direction, sometimes, as I have also made clear throughout this essay, in a good direction.

—Prof. Dr. Jörg Meibauer

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Assuring Quality in Science – Interview with Uwe Schmidt

Dr. Uwe Schmidt[7]
Center for Quality Assurance and Development, University of Mainz, Forum universitatis 4, 55099 Mainz, Germany

Maintaining a certain level of quality in research and lectures and measuring this level has become increasingly important in recent years. Due to the diversity of scientific research fields, a general approach is difficult. At the University of Mainz, the *Zentrum für Qualitätssicherung und -entwicklung* (ZQ, Center for Quality Assurance and Development), takes most important organization of this kind. We talked to Dr. Uwe Schmidt, head of the ZQ, about the role of evaluation and quality management in natural sciences and the humanities, the Bologna Process, and the principles of scientometrics.

JUnQ: From your perspective as head of the Center for Quality Assurance and Development (ZQ), by which means does the ZQ assure quality at the university?

Schmidt: There are different areas in the field of quality assurance. First, we are the first university in Germany that is system-accredited, i.e. we have the ability to accredit our study program internally. Every five to seven years we ensure that the programs fulfill certain standards and, in close cooperation with the lecturers, assess whether further improvement is necessary. Additionally, we conduct surveys among graduate and undergraduate students. The second field is more related to organizational and managerial questions. Here we develop strategies to optimize management processes.

In the third area we look into matters of quality assurance in research. Typically, this is accomplished by peer review process and a few other indicators, like third-party funding and the number of publications.

JUnQ: Could you say a few words about the system accreditation.

Schmidt: Since the introduction of the bachelor/master degrees every study program has to be accredited. For this purpose there is a national Accreditation Council, which approves certain Accreditation Agencies, which in turn, accredit the study programs. Therefore, in 2006, we developed a model allowing Accreditation Agencies to accredit the whole university and its internal accreditation of individual study programs, which is done within the university. In 2011 we became the first university that was system accredited in Germany, providing us with the possibility, not only to accredit study programs, but also to raise a discussion about the crucial criteria.

JUnQ: Since when does the ZQ exist and why was it founded?

Schmidt: The ZQ exists, with predecessors, since 1992 under the name "Project for the Promotion of Study and Teaching". Within this project we accompanied over 200 innovative teaching projects. Since the mid-90's we focus on the internal and external evaluation of institutes, departments and whole universities, whereby an internal evaluation is followed by an evaluation of external referees. The ZQ itself was founded in 1999. In contrast to other universities, the ZQ is a scientific institution, residing between the departments and the directorate of the university, which is quite an unusual constellation in Germany.

JUnQ: Most students only know the questionnaires at the end of the semester concerning the lecture courses. What are the differences between the evaluation of lectures and the evaluation of research?

Schmidt: The evaluation of research arises out of particular occasions, e.g. to choose specific referees or refereeing methods. In contrast to the evaluation of lecture courses, these measures are non-permanent.

JUnQ: Regarding the questionnaires, what are the actions resulting from this type of evaluation?

Schmidt: The evaluation of the lecture courses takes place regularly every three semesters. After having analyzed the

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data, we offer various consulting services, such as individual coaching for the lecturers. Direct feedback alone, as it is known from research, does not necessarily lead to a quality improvement; it is important to interpose an advisory process. Obviously no lecturer is aiming to give poor lecture courses. They just have not learned the proper way to do that. Therefore we need these individual coaching offers.

JUnQ: Which criteria are used for evaluating the quality of research?

Schmidt: In the area of lectures, as well as in research, we refer to the so-called AGIL-scheme from Talcott-Parsons. It states that you need to consider different dimensions and harmonize them. For example, not only do you need resources to have good lecture courses, but you also need good didactics and vice versa. For research this states that not only the high output is important, but also its quality. The four dimensions in this scheme are, of course simplified, the resource dimension, the dimension of results, the dimension of integration and the dimension of scientific culture. Based on this scheme we developed a model, where you can only achieve 25 points for each dimension, leading to a balance, e.g. between publications and scientific culture. The idea is to relativize the momentum of these research indicators. Furthermore, the model allows us to differentiate not only subject-specific, but also age-specifically. A senior researcher would gain a lot more credit points in this system, due to his work in various boards and committees, but might not need to acquire that much third-party funding.

JUnQ: Which measures are taken when criteria are not fulfilled?

Schmidt: As stated before, we offer individual and group coaching lessons. Furthermore, the results are not only presented to the respective lecturer, but also to the dean of the department, who might initiate a collegial discussion. The internal discussion between colleagues is the most promising approach, since it is more effective, than external interventions.

JUnQ: Do you also work on external projects?

Schmidt: We are mostly funded by external projects, as we evaluate several nation-wide projects. To name justone example: we currently evaluate a national project from the Federal Ministry of Education and Research, fundingmore than 170 universities, which is supposed to ascertain the quality in teaching. Our advantage arising from these external projects is that we gain a lot external knowledge, even from non-university projects. This helps us to gain a broader focus and improve our work.

JUnQ: Besides the assurance of quality, you are also dealing with evolution of quality. What does this mean exactly?

Schmidt: The development of quality is the transition from measuring quality towards distinct measures and procedures, e.g. the previously mentioned coaching offers. Normally we try to pool the evaluation and the development of quality, especially if we are working with whole departments. We try to integrate these measures for quality evaluation into the process, including discussions and consulting offers, as many persons know their problems but do not want to openly communicate them.

JUnQ: Do you see differences in the quality assurance in scientific research, with respect to the different departments?

Schmidt: Definitely. - Especially when looking at criteria and research processes. When looking at interdiscplinarity and network structures, traditions differ immensely between natural sciences and the humanities. In natural sciences the concept of the post-doc supervising PhD students is well known. In humanities, however, this concept is just on the onset of being established. But to be fair, the humanities also lacked the resources to do so. Furthermore, theoretical research in the humanities does not necessarily need that much external funding and one has to think about how to evaluate this work.

But also with respect to only one research field, there can be quite large discrepancies. For example when comparing literary studies and linguistics - two fields that overlap in many instances. In contrast to literature studies, linguistics is often more empirically oriented, makes use of peer reviewed journals and impact factors. Therefore, you have very different reference systems.

JUnQ: On a more critical note: It seems that a lot of money is spent on the evaluation of research, instead of funding the actual research. What is your opinion on that?

Schmidt: I think that we have to guarantee that these evaluation systems are not evolving in a way where they are completely decoupled from their initial purpose, namely, to support research and lecturing. Then again, since more and more work tasks and functions are outsourced from the ministries to the universities, and due to the rising complexity and size of the universities, more resources are needed to maintain and evolve quality. A very good example is the Bologna process. In this process many resources were needed; not only for the assurance of quality, but for the organization of study programs. Since exams take place more frequently – contrasting to the previous system – the bureaucratic effort has risen tremendously, due to fact that all conditions have to be legally suitable. Furthermore, the documentation of the students' achievements has changed as a result. These are typical side effects of improved automatic administration of the universities at an organisational level, which has been massively underestimated.

Another recent example for a needed increase in monetary overhead is the external funding. Very successful universities, i.e. universities that get a lot of external funding, are

having structural deficits because the administration of external funding becomes quite expensive.
Overall I would say that in general the funds for quality assurance are not very high, especially with regard to evaluation of EU projects. In this case, normally one to two percent of the total amount are spent on quality assurance and evaluation, which is a lot more than our budget. Particularly in Mainz, most of the ZQ employees are externally funded and not directly employed by the university. Nevertheless, we always have to take care that quality assurance brings additional benefit and further improvement to teaching and research, and does not become a habit without generating new insights.

– Kristina Klinker, Robert Lindner

Questions of the Week

The Journal of Unsolved Question presents a "Question of the Week" on its homepage every week. Set up and formulated by the members of the editorial board, the main purpose of the "Question of the Week" consists in intriguing the reader by presenting topics of ongoing research. "Questions of the Week" published so far cover a wide variety of scientific fields, but share the feature to be of certain importance to several disciplines.
In the following, we present selected "Questions of the Week" from the last six months.

Why Do We Blush?

by David Huesmann

Being embarrassed is certainly one of the more uncomfortable emotions. But, on top of all, do other people really need to see that we are embarrassed? Well we show them, whether we like it or not, through blushing.

How blushing works is understood quite well. Adrenaline is released increasing the heart rate, dilating blood vessels and improving oxygen transport. In humans, the veins of the face (and only the face) react to the adrenalin rush causing blushing.

While the "how" seems to be quite clear, the "why" is not so well understood. There are different theories, the most popular one revolving around the assumption that blushing is a way of convincing people we have understood that we have a done something socially unacceptable. Since blushing is involuntary people seem to be more sympathetic with a blushing person, because they do not expect manipulation. So the next time you get red in the face, be a little more grateful for this response. It might just have saved your reputation.

Read more:

[1] http://science.howstuffworks.com/life/blush.htm
[2] http://mentalfloss.com/article/51299/why-do-people-blush
[3] http://www.youtube.com/watch?v=-qDi7IbYGVY
[4] http://www.scientificamerican.com/article.cfm?id=why-we-blush-social-embarrassment

Do Bonobos Blush?

by Wolter Seuntjens

> "Man is the Only Animal that Blushes. Or needs to.[1]

Right at the opening of the thirteenth chapter of "The Expression of the Emotions in Man and Animals" Charles Darwin wrote:
BLUSHING is the most peculiar and the most human of all expressions. Monkeys redden from passion, but it would require an overwhelming amount of evidence to make us believe that any animal could blush.[2]
Though Darwin affirmed that monkeys may redden from passion, he denied that they could blush in any human way. Maybe it comes as a surprise that even today, almost 150 years after Darwin stated the above-cited, human blushing remains quite mysterious. Concerning the human blush there are still many questions very open. In fact, the blush is ubiquitous yet scarcely understood.[3]
Recently, however, one age–old question concerning blushing – Do young women blush in the dark?[4] – was answered positively.[5] The essential apparatus used in this experiment was, of course, a thermographic camera.
Is blushing indeed the most human of all expressions? The present Question of the Week therefore becomes: Are there no other animals that blush? If we find one other blushing animal, then Darwin got it wrong. And likewise Frans de Waal, who wrote: "The only uniquely human expression, as Darwin already noted, is blushing. I don't know of any instant face reddening in other primates."[6]

Bonobo (*Pan paniscus*).[8]

The other animal most likely to blush – or most likely needing to blush – is arguably the bonobo. Thus remains the crucial problem: How to make a bonobo blush?
Bonobos prove to be societal and cultural animals. A great part of society and culture is concerned with morals. Morals always involve rules and boundaries. Breaking these rules and crossing these boundaries constitute a moral breach. In humans such a breach might produce a feeling of shame.

[8] Downloaded from http://upload.wikimedia.org/wikipedia/commons/a/a1/Bonobo_009.jpg

According to Darwin, blushing is associated predominantly with the feeling of shame. The test whether bonobos blush might consequently require (A) a naturalistic observation of one individual crossing a moral boundary while one or more others are present or (B) an artificial experiment in which one bonobo would be forced to commit a moral breach while being observed by other bonobos. The theoretical ground for this test has already been cleared: trichromatic primates can discriminate color changes in the skin of the face and the rump of conspecifics.[7] As bonobos may have more facial hair than humans and as their skin is rather dark blushing may not easily be noticed by a human observer. Here the aforementioned thermographic camera, already successful in solving one question, may come in handy again.

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Does Our Language Influence the Way We Think?

by Felix Spenkuch

About two years ago Thomas Jagau posted the question "Why do some languages not distinguish between blue and green?". The possible answer was: Due to defects in color perception.

Whatever the actual answer is: The fact that languages differ drastically in what they can describe and distinguish leads to another question: Do natives to different languages think drastically different? The obvious answer may be: No, why should they? But if we give the question a bit more thought: Isn't language the tool we use to think beyond pure instinctive emotions? And in this way of reasoning: Isn't it just natural that speakers of different languages have to think differently? Does this mean that there are languages that are better suited to think about one task or another?

It is, at least, established that language influences the way we perceive our surroundings, a claim going by the "Whorf hypothesis". For example Gilbert and coworkers were able to find support for the Whorf hypothesis for the right visual field in a conscious search test.[1] This was to be expected, since the left hemisphere of the brain is strongly involved in many language tasks.

So language causes differences in our conscious ways of thought and therewith in our conscious way to view the world, but does it also influence subconscious, so-called preattentive perception? Three years ago a group of Britain-based researchers addressed this question with a study titled "Unconscious effects of language-specific terminology on preattentive color perception".[2]

To investigate these effects the authors chose colors of blue and green in different hues and set a group of native English speakers against a group of native Greek speakers. The Greek language possesses the words to distinguish light blue from dark blue suggested an advantage for the Greek speakers. To test whether advantages in language terminology lead to faster color perception the authors monitored the brainwaves of their participants for the perception of a very fast color change. The surprising result: The Greek participants were indeed significantly more competent in the detection of changing blue hues, which the authors attribute to their extended terminology for blue colors.

So we can say language influences color perception, but what about math? Everybody says that math is a language by itself and therewith universal. In fact I do have a colleague from Bulgaria who studied math in Germany who said just this: It was an easy start for him, since math is math in every language. Does that mean that every language is suited for doing math or does this mean that math is a language–independent way of thinking? A recent study actually claims the latter: By magnetic resonance imaging experiments Mori and coworkers revealed that different brain regions are responsible for language syntax and algebra operations.[3]

Still many questions remain open: Who knows whether the ideal language to think about anything exists? Many will say: Yes, it's math! But you cannot think about anything using math, can you?

Read more:

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Do Women and Men Agree on the Ideal Body for Them and Their Partner?

by Felix Spenkuch

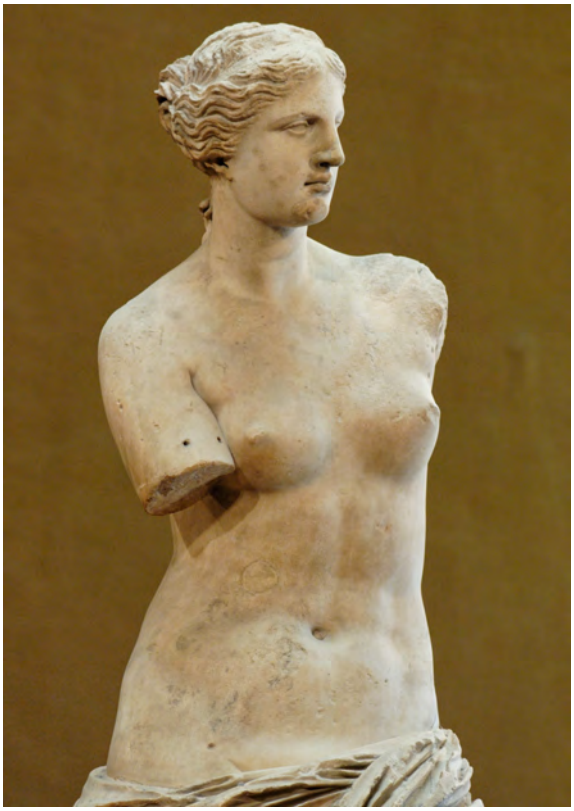
This was the question a group of british neurologists asked themselves.[1] To address this question they asked forty caucasian women and forty caucasian men aged around twenty to create male and female bodies that they thought ideal using a 3D modeling program. To remove any bias that the starting point might generate the participants had to fulfill their task twice: Once starting from a thin and once starting from an overweight body. To account for differences between preference and reality the bodies resulting from these sessions were compared to the actual bodies of the participants. Waist-to-hip ratio (WHR), waist-to-chest ratio (WCR) and body mass index (BMI) were used as indicators.

The result: Yes, men and women have a similar preference concerning ideal female and male bodies. The authors explain this consensus by turning to mate selection theory, which expects women and men to have a "very precise and accurate idea of what the opposite sex finds attractive".[2]

Both sexes preferred a female body that is just not underweight on BMI scale, which is consistent with previous results identifying a low BMI as main indicator of female attractiveness. Still the body shape remains an important criterion, since the study also showed that a slim but curvaceous female body is desired by both sexes.

Concerning the ideal male body both sexes preferred a body with a slender waist and narrow hips but with an extremely broad and muscular chest, creating a V-shaped torso. This puts the bodies at the upper limit of the healthy BMI range (since the BMI does not reflect the fat-to-muscle ratio), which might still be an underestimate, as the authors point out.

The ideal BMI for women was lower than the BMI of 39 out of the 40 female participants, while half of the men were below and the other half above their "ideal" BMI. A surprising difference in what the participants desired for themselves and what the other sex preferred was that women wanted to be larger in bust size than the men preferred them, while the men wanted to be (even) more muscular than the women preferred them. If there would be an influence of the media men should prefer heavier and bustier women instead, since female models in men's magazines are like that.[3]



Venus de Milo. [9]

This leads to the question where this desired overcompensation of an attractive feature comes from. Also still open are the questions whether these body ideals differ with age, nationality and social status. And the most important question of all: How happy are we with striving for a body that is so significantly different than our actual body?

Read more:

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[3] M. J. Tovee, S. M. Mason, J. L. Emery, S. E. McCluskey, E. M. Cohen-Tovee, *Lancet* **1997**, *350*, 1474–1475.

Where Does the Greeting "Moin" Come From?

by David Huesmann

In northern Germany, as well as Luxembourg and South Denmark, people greet each other with "Moin!" (or "Mojn!"). One possible origin comes from the German good morning (guten Morgen –> Morgen –> Moin). However, "Moin" is often not only used in the morning, but in the afternoon and evening as well. Another attempt to explain the origin of "Moin" points at the Low German word "moi" (meaning beautiful, good). The issue is further complicated by the use of "Moin Moin", where the first "Moin" might stand for good and the second "Moin" stand for morning.

Read more:

[1] Küppers' Wörterbuch der deutschen Umgangssprache, Klett, 1997.
[2] http://www.plattmaster.de/moinmoin.htm
[3] http://de.wikipedia.org/wiki/Moin

[9] Downloaded from http://upload.wikimedia.org/wikipedia/commons/2/21/Venus_de_Milo_Louvre_Ma399_n4.jpg

Can Machines Be Taught to See like Humans?

by Thomas Jagau

There are robots that walk like humans and computer algorithms that mimic human behavior. But what about machines that see like humans? That is a project the USC School of Engineering is currently working on.
To develop new algorithms for visual processing, the researchers learn from the human brain and try to transfer two principles to machines: Top-down attention and compositionality. The former refers to the decision tree we automatically engage in when we are looking for something. For example, if you want to find a stapler in an office, you look for it on the desk or on the table, but not at the ceiling. Compositionality refers to our hierarchical way of recognizing objects: You know what a wheel is and you can recognize whether it is on a bike or a car.
Both of these tasks are difficult for a computer. Today's recognition software is mostly task-specific. The current research focuses on outlining a dictionary of basic components and writing algorithms that define ways they can combine to form different objects. The longe-range objective is a smart camera that approaches the cognitive abilities of the human cortex.

Read more:

[1] A. Borji, D. N. Sihite, L. Itti, *Vision Research* **2013**, *91*, 62.

How Fast Should You Run when it is Raining?

by Stephan Köhler

At the time of writing the gray days of autumn seem to be right around the corner and the water pouring from the sky is much too cold to be enjoyable. So most people just want to get through the rain to some dry place. Our instinct tells us to run as fast as we can to avoid getting wet. But is this intuition correct? Do you really get less wet if you run at your maximum speed?
The short answer is no. But as always the long answer is more complicated and while this might not be cutting edge research in physics it presents a useful training problem for undergraduates and has thus been discussed in the literature. So naturally different answers can be found depending on the assumptions the authors made.

Does rain influence the speed with which we walk? [10]

The first assumption one has to make is the shape of the body that is moving in the rain. While it is usually assumed that this does not influence the general answer a recent paper showed that it actually does.[1] Borrowing methods from electrodynamics more complex body shapes could be addressed in this paper and the value of the optimal velocity was compared. That is if an optimal velocity even exists, i.e. it is not the best strategy to run as fast as you can.
Not surprisingly, the answer does not only depend on the shape of the body but also on the direction from which the rain is coming. If the rain is coming straight from the back, for example, an optimal velocity always exists and its value does not even dependent on the body shape. For other direction the answers vary and no general rules seem to exist.
Another restriction that may apply is the assumption of a rigid body motion. While it is convenient to calculate a rigid cylinder floating effortlessly through the rain, humans are not rigid, especially while running. No attempt has so far been made to include this into the models.
So if the rain is not coming straight from the back you might need to do some further calculations to figure out what the best speed is for your body shape and running style. But as a final tip make sure that you do the calculation in a nice and dry place and only then venture out into the rain. If the weather catches you off guard, just take your chances and run to the next cover.

Read more:

[1] F. Bocci, *Eur. J. Phys.*, **2012** *33*, 1321–1332.

[10] Downloaded from: http://commons.wikimedia.org/wiki/File:StateLibQld_1_107360_People_crossing_George_Street_in_the_rain,_Brisbane,_1952.jpg

How Many Distinct Life Forms – Species – Does Your Planet Have?

by Stefan Kuhn

According to Robert May, this would be the first question an alien visiting earth would have. Besides the pure curiosity, that such aliens might have, this is quite a relevant question for us humans already here on earth due to the ongoing biodiversity crisis. That is the extinction of more and more species in the course of time. So how many species are there now on earth? What would you guess?

A recent study predicts a diversity of around 8.7 million species globally with an uncertainty of ~1.3 million. Roughly 2.2 million of these estimated distinct life forms are marine. This study also claims, that 86% of the existing species on earth and 91% of the oceanic species have not yet even been described. You will easily realize that the amount of different species on earth is even bigger than this estimate. With the ongoing extinction on the other hand, 18,788 species out of 52,017 so far assessed are threatened, an accurate number is, thus, impossible to give.
These results raise even more questions: What do we actually know about life on earth and how is a species defined in the first place?

Read more:

[1] R. May, *Science* **2004**, *329(2)*, 41–42.
[2] C. Mora, *PLoS Biol.* **2011**, *9(8)*, e1001127.
[3] http://www.sciencedaily.com/releases/2011/08/110823180459.htm
[4] http://www.iucn.org/iyb/about/biodiversity_crisis/

Is English a Scandinavian Language?

by Thomas Jagau

Contrary to popular belief that English descends directly from Old English (a Western Germanic language, which the Angles and Saxons brought with them from Northern Germany and Southern Denmark when they settled in the British Isles), researchers from Oslo now found evidence that English is in reality a Scandinavian language. Obviously, there are many English words that closely resemble their Scandinavian counterparts. But also the fundamental structure of the English language is strikingly similar to that of Norwegian – and not that of Old English or modern German. In fact, the grammar of English and Norwegian is more or less the same.
Researchers from Oslo now believe that Old English died out while the Scandinavian language of the Viking conquerors survived. Like most colonists, the Vikings found no reason to switch to the language of the country they had arrived in. Hence, modern English is just a variant of Norwegian – with parts of the vocabulary borrowed from Old English.

Read more:

[1] http://sciencenordic.com/english-scandinavian-language
[2] http://www.sciencedaily.com/releases/2012/11/121127094111.htm

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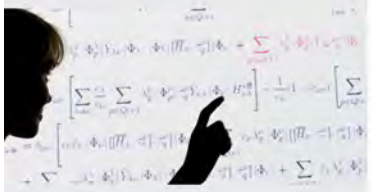

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The main goal of the Graduate School Materials Science in Mainz (MAINZ) is to provide excellent scientific training for PhD students while they conduct high-quality research. Our programme is committed to offering excellent training opportunities in a most flexible and individualized manner to kick-start the future career of our PhD students in academia, industry and beyond. This requires both: excellent training within materials science research (Training through Research) and complementary business, leadership, cultural, and other skills (Training for Life).

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Articles

Smectic LC-Elastomers with NO Shape Change at the Phase Transition

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Liquid crystalline (LC) elastomers are well known for their reversible shape variation at the phase transition from the LC to the isotropic phase. We managed to prepare an oriented smectic monodomain of a crosslinked LC-polysiloxane which showed – contrary to the expectations – NO shape variation at all. This observation is in agreement with mechanical measurements on small LC-elastomer balloons made from the same materials. It is completely unknown why this type of "diluted" LC-polysiloxane (only about 25% of the repeating units are functionalized with mesogens) behaves like this.

1 Introduction

Liquid crystalline elastomers (LC-elastomers) combine LC-phases and the resulting anisotropic properties with the mechanical properties of a soft rubbery solid.[6,11,12,34] As a consequence, the conformation of the polymer chains is influenced by the liquid crystalline director field. This couples the shape of the LC-elastomer to the liquid crystalline order. They are presently finding an increasing interest as actuators.[9,23] Generally, size changes are found at phase transition temperatures, especially at the transition from the ordered liquid crystalline phase (e.g. smectic or nematic) to the disordered isotropic phase.[18] Figure 1 depicts the LC phase behavior of thermotropic calamitic (rod-like) liquid crystalline molecules.

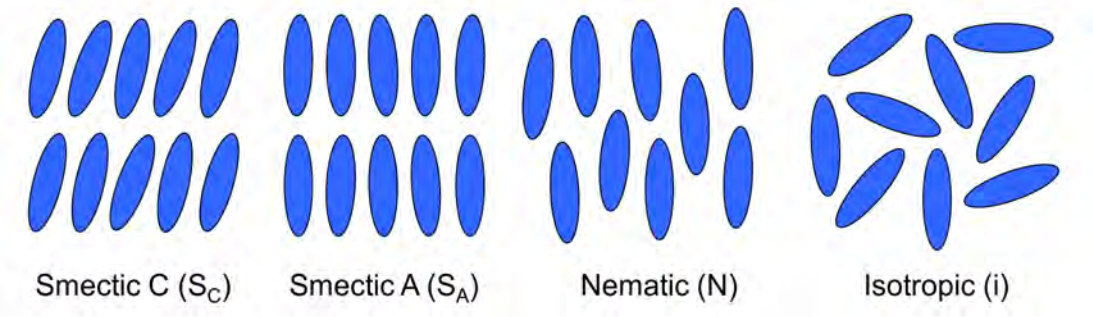

Figure 1: Mesomorphism in thermotropic calamitic mesogens. The molecular order decreases with increasing temperature. All long-range order is lost in the isotropic melt.

To improve the properties of an LC-elastomer, two aspects have to be optimized:

1. The anisotropy of the radius of gyration of the polymer chains in the LC-phase, which is the precondition for the size change at the phase transition, should be as large as possible. In this respect, smectic phases are attractive because they usually possess a larger order parameter compared to nematic phases.[21] Smectic LC-polymers are, however, more difficult to orient uniformly (monodomain) than nematics which is a precondition for a macroscopic shape change at the phase transition.

2. An isothermal shift of the phase transition temperature by an external stimulus is more desirable than a

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temperature variation. In this respect, photochromic dyes, which destabilize the LC-phase during isomerization, are attractive. Azo dyes are especially attractive in this context because they can be used to shift phase transition temperatures reversibly.[10,31,33] Photochromic dyes have been incorporated into LC-elastomers. It had, however, never been possible to prepare/crosslink these LC-elastomers by photocrosslinking, as the dye absorbs most of the photons.

As starting point for this work, we had succeeded in the synthesis of azo containing smectic LC-polymers which could be photocrosslinked – for the first time – into smectic elastomers due to an improved orientation technique.[4] Thereafter, we wanted to study their shape variation at the transition from the smectic A to the isotropic phase.

2 Results and Discussion

A smectic polysiloxane **P1** was synthesized by a hydrosilylation reaction (see Figure 2a). It was modified with azo-mesogens and acrylate terminated mesogens to yield polymers **P1a** and **P1b** (see Tab. 1). The details of their synthesis and characterization is described in Ref. 12. These smectic LC-polymers belong to a series of LC-polysiloxanes, in which only 1 out of 3 repeating units are functionalized with mesogen like groups.[7,8,14,16,22,26] The phase transition temperatures of the polysiloxanes **P1a-b** were characterized by DSC and polarizing microscopy. They are compiled in Tab. 1. All polymers exhibit the phase sequence S_X-$S_C{}^*$-S_A-i.

Table 1: Composition and phase sequences the LC-polymer series **P1-P1b**.

	x	y	z	w	Phase sequence
P1	1	0	0	2.9	S_X 29 °C $S_C{}^*$ 61 °C S_A 89 °C i
P1a	0.8	0.08	0.12	2.9	S_X 36 °C $S_C{}^*$ 67 °C S_A 86 °C i
P1b	0.8	0.14	0.06	2.9	S_X 30 °C $S_C{}^*$ 57 °C S_A 85 °C i

As the *cis*-form of the azobenzene side group destabilizes the LC-phase, it is possible to shift the phase transition temperature by photo-isomerization (Figure 2b).[4,22] This effect could be detected both for uncrosslinked polymers **P1a-b** and for crosslinked samples made from **P1a** and **P1b** at the clearing temperature (S_A-i) by polarizing microscopy. For **P1a** e.g. an isothermal shift of the S_A-i transition of more than 10 °C could be observed.[22]

These photoswitchable polymers could be photocrosslinked into oriented LC-elastomers or "single crystal LC-elastomers (SCLCE)" in the nomenclature of Finkelmann[20] by using the dichroism of the azo-chromophore in perfectly aligned samples to eliminate the absorption by the azo-chromophores parallel to the director.[4] Perfectly homeotropic aligned samples can be obtained by spin-coating and annealing thin films on substrates[15] (film thickness 1-2 μm) because of the smectic layer structure which wants to orient parallel to the substrate. They look completely black if viewed between crossed polarizers which proves the homeotropic orientation. X-ray and TEM measurements prove their smectic phase and their orientation.[26,32] To crosslink polymers **P1a** and **P1b** they were mixed with Lucirin TPO as photoinitiator and irradiated with UV-light (about 360 nm) perpendicular to the surface of the substrate which is parallel to the director and perpendicular to the transition dipole moment of the azo-mesogens.

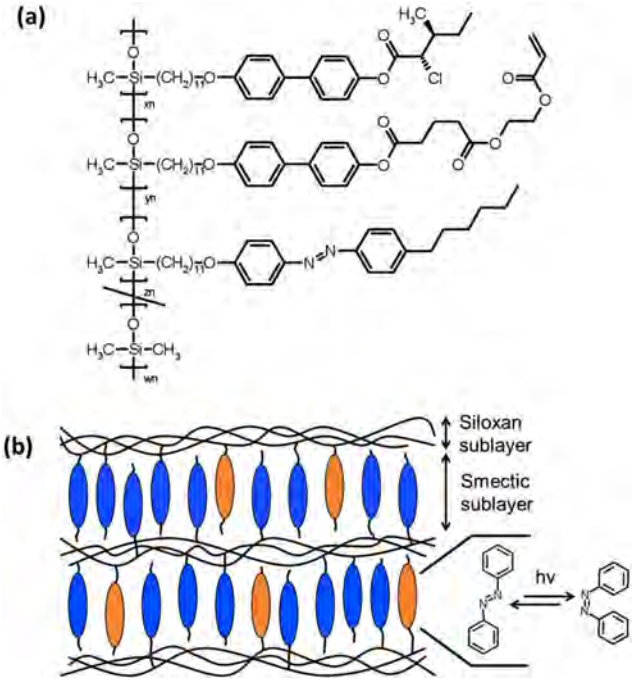

Figure 2: (a) Molecular structure of LC-polymers **P1**-**P1b**. (b) The *trans-cis* isomerization in oriented LC elastomers destabilizes the LC-phase and lowers the phase transition temperature, allowing an isothermal phase transition.

To demonstrate the influence of crosslinking, Fig. 3a shows a POM-image of a patterned partially crosslinked film of polymer **P1a** at room temperature. This polymer had been irradiated through a mask (left side covered, right side exposed) so that only the right part gets crosslinked. Then, it was heated into the isotropic phase and quickly cooled back to the LC-phase. After this process (rapid cooling), the covered part of the film (left side, uncrosslinked) turns birefringent because of the unoriented LC-phase. The crosslinked part (right side) shows, however, still the homeotropic orientation (black) because the network retains the memory of the homeotropic orientation. The irradiated part of the film swells – as expected – in good solvents but does not dissolve. This proves the crosslinking and the fact that an elastomer has been made.

Free-standing thin elastomer films can be prepared with this method by spincasting the LC-polymer onto NaCl pellets. After dissolution of the substrate with water, one obtains unsoluble and homeotropically oriented LC-elastomers with photo-addressable azo-chromophors (see

Fig. 3b). As expected, photocrosslinking is also possible in free standing films obtained by drawing of the uncrosslinked polymer over a hole.[29]

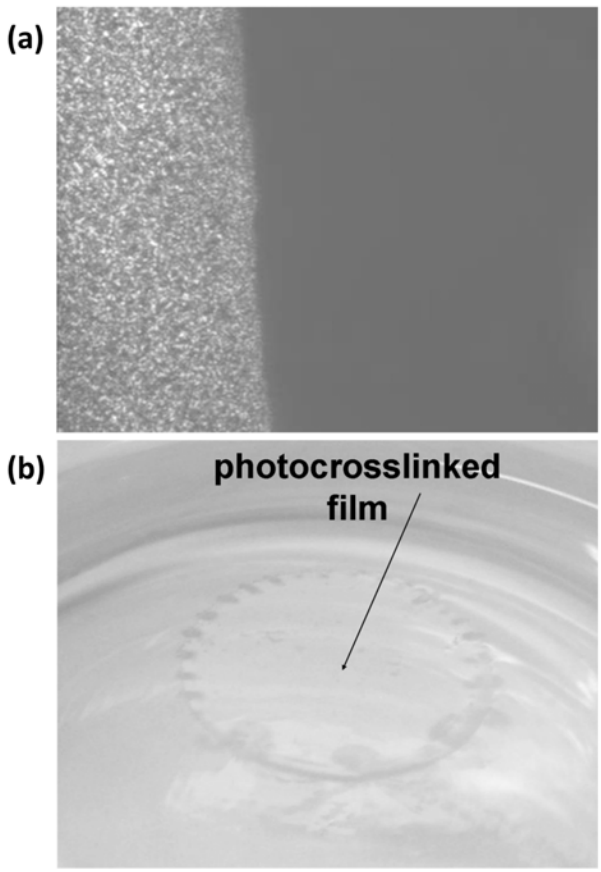

Figure 3: (a) Polarizing microscopy image of a partly crosslinked film (left: uncrosslinked, right: crosslinked) of **P1a** on a substrate; picture taken at room temperature after crosslinking the right side in the S_X-phase, heating to the isotropic phase and quick cooling to room temperature. (b) Free floating photocrosslinked film of **P1b** after dissolution of the substrate. The outer circle of the film is thicker and better visible. Pictures taken from Ref. 12.

As a result, we had obtained perfectly oriented smectic elastomers which are photo switchable because of the azochromophores. This allows an isothermal phase shift. Such monodomains of photoswitchable smectic LC-elastomers should open the possibility to induce shape changes either isothermally by irradiation (loss of the LC-phase due to isomerization) or by temperature changes (heating above the phase transition to the isotropic phase).

In the following, we tried very hard to observe these expected shape changes. At first, by irradiation and then, simply by heating the sample into the isotropic phase (see Tab. 1 for the corresponding phase sequence). To prevent sticking to the substrate, we used freely floating samples on silicone oil. To improve our resolution, we used a microscope to look at the samples (in size similar to Figure 3a, 1 cm diameter). But we never observed any shape change at all. With our microscopical set-up, we are able to observe shape changes of the order of several micrometers. Given the size of the sample (cm), this would correspond to a relative shape change of less than 0.1%. Usually 20 to 100% of length changes are reported.[2,3,24]

The question to solve is then, why did we not observe any reasonable shape change in our system. At first, it should be noted that most of the experiments on LC-elastomers were made with nematic phases and only very few on elastomers with smectic phases. If smectic phases were investigated, the interest focused on the transition from the tilted smectic C to the orthogonal smectic A phase.[17] Thus, there are very few data available for comparison. Usually it is believed that smectics should show a larger shape change as the liquid crystalline order is larger in smectics than in nematics.[21,23,27] For shape variation, we have to consider, however, the anisotropy of the polymer chains in the smectic phase. And no systematic study exists on this topic.

Reports about shape variations during the transition from a smectic phase to the isotropic phase give rather different values. For some LC-main chain elastomers, rather large shape variations are observed[1,5,27] which are larger than that of comparable elastomers at the nematic-isotropic transition; for others (both of the LC-main chain- or LC-side chain type), a rather small shape change is observed which is smaller than that of comparable nematic systems.[3,19] However, it is difficult to relate the order parameter of the mesogens in the LC-phase to the chain anisotropy. This is evident e.g. from the fact that in side chain systems the polymer chain can adopt a prolate, an oblate or an isotropic confirmation (i.e., no preferred orientation at all).[23] Chain anisotropy has been explained for some very special cases but there is no way to predict it in a general way from the molecular structure. For smectics, the following argument can apply, too. If chain extends parallel to director (prolate configuration) in the nematic phase, they might get confined to the smectic layers. In this case, the overall anisotropy gets reduced. In addition, little is known for most of the systems about the quality of the smectic order. I.e., the correlation length of the smectic layers may be long range – as in classical smectics – or rather short range.[13]

There is another difference between the LC-elastomers studied here and the "usual" systems and this is based on the molecular structure. Most of the work done on LC-elastomers has been made with polysiloxane-elastomers, in which each dimethysiloxane repeating unit carries mesogens (so called LC-homopolysiloxanes). We work here with systems, in which only some repeating units carry mesogens while the other repeating units consist of unsubstituted dimethysiloxane units[14] (so called "diluted" LC-copolysiloxanes = diluted with dimethysiloxane units). These "diluted" LC-copolysiloxanes show a nanophase separation into mesogen rich and polymer rich sublayers, as proven by X-ray scattering[14,25] and as visualized by TEM measurements on LC-colloids.[32] Based on these facts, the polysiloxane chains in our elastomer have to be restricted – in the smectic phase – to the polysiloxane sublayer and adopt a 2D conformation. Thus, it should be highly

anisotropic and the loss of this anisotropy should lead to an especially large shape variation at the phase transition to the isotropic phase. However, no shape variation is observed at all! On the other side, the classical siloxane elastomers of Finkelmann belong nearly exclusively to the homopolymer type. For them, the shape change is well established.[21,24] So, obviously homo- and copolymers show very different mechanical properties. However, a different response to mechanical deformation has also been clearly identified for the "diluted copolymers" by work on thin films (free standing films or balloons). Small balloons made from smectic polysiloxanes have been used extensively to characterize their elastic property in detail.[28,30] They showed also no sign of a change of the sample dimensions at the transition from the smectic to the isotropic phase. In fact, the smectic elastomers from "diluted" LC-copolysiloxanes are rather soft solids, in which even the thickness of the smectic layers can be changed by stretching.[29]

After all these considerations, it remains still completely unclear why we are not able to observe any shape variations for the LC-elastomers made from "diluted copolysiloxanes" with smectic phases. Taking all available information together, the shape change at the transition to the isotropic melt might be small but some shape variation should be observable. Especially since the polymer main chain is restricted within the smectic phase to a 2D-like smectic sublayer.[14,25,32]

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Is it Possible to Measure Scientific Performance with the h-Index or with Another Variant from the Hirsch Index Zoo?

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The h-index proposed by Hirsch only 8 years ago is already frequently used to measure scientific performance. Nevertheless, several open questions are unsolved, e.g. what does the h-index really measure? Are there better variants available? How reliable is the determination of the h-index? Does it have predictive power?

3 Introduction

In recent years the attempts to measure scientific performance have been felt as a growing pressure on many scientists, exercised by administration, politics and even the general public. This is somewhat understandable because administrators, politicians and more general the citizens want to know whether the tax money is spent in a reasonable way. But it remains unclear not only what is reasonable, but also how to measure scientific performance. Various measures are in use, from counting PhD students (which has recently been strongly criticized due to plagiarism scandals) to counting allocated or spent third-party funding or counting publications and citations. It is not the purpose of the present paper to discuss or even weight the respective different measures. Rather, I will concentrate on the bibliometric issues, related to the h-index proposed by Hirsch[1] as a measure for the scientific performance in terms of citations. It is defined as the (largest) number h of a scientist's publications which have received at least h citations. Thus, it appears to be easy to determine it and therefore, the h-index has become famous among administrators but is also considered infamous by many scientists, even if they have obtained a relatively high index value.

The validity and the advantages and disadvantages of the h-index have been discussed ever since its introduction, and a plethora of variants has been introduced.[2] Nowadays, it is difficult to find a letter in the alphabet which has not yet been proposed at least once as a new bibliometric index in that context.[3] Many of the suggestions are driven by personal taste, some of them might also have been created due to the desire to improve the indicator value of the proposing author in comparison with competing colleagues. It is impossible to review all suggestions, even the reasonable proposals are too numerous. In the following, I will discuss only some of these bibliometric indicators what of course means a subjective choice according to my personal taste.

Let me add a caveat: I am not a scientometric expert but a physicist who has chosen this topic as a hobby horse (hobby deer) several years ago. In these years, I analyzed the citation records of several physicists. For the present paper, I have investigated the data of Prof. Dr. Kurt Binder from the Johannes Gutenberg-University Mainz, whom I got to know as an excellent scientist during my first professorship in the Institute of Physical Chemistry in Mainz three decades ago. He still is a very active researcher, publishing frequently and his papers have received and are still receiving very many citations. In the year 2001, he was distinguished by Thomson Reuters' Institute of Scientific Information (ISI) as one of the most cited authors in the world. There are only very few other German physicists with so many citations. Therefore, dear reader, while you may be impressed by the numbers below you should not be disappointed if your personal citation record is significantly lower.

4 The h-index and its determination

Probably the most simple way of measuring scientific performance in bibliometric terms is counting the number of publications. After this indicator came into use in particular in the US decades ago, some scientists adjusted their publication outcome to it. This led to the tactics of salami slicing, i.e. the apportionment of the obtained results into LPUs (least publishable units). As a countermeasure, people started counting the citations, what was made possible but cumbersome by the then regularly printed Science Citation Index.

Counting citations to help with economic decisions is nothing new. Already in 1927, the citation frequencies of 28 leading chemistry and physics journals in the previous 54(!) years were investigated[4] with the aim of determining for which journals the subscription should be continued or cancelled. Similarly, nowadays some people believe that citation records can be used for determining, whether to allocate research funds or whether to hire scientists or not.

The advance of large bibliometric databases has simplified the evaluation of citation statistics also for individual researchers significantly. For the present paper, I have downloaded K. Binder's citation record from the Web of Science (WoS) provided by Thomson Reuters (formerly ISI) on September 18, 2013. Then, 993 entries were found, but a careful check yielded only 884 publications that were written by the investigated person. This is the well known precision problem: up to now there is no reliable way of

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determining the citation record of the publication set of a person with high accuracy automatically. In some cases, like F. Wilczek, it is easy. In other cases, like M. L. Cohen, it is more difficult to eliminate homographs, i.e. to exclude papers which have not been written by the physicist Marvin L. Cohen.[5] In the other direction, for Pierre-Gilles deGennes one has to combine the WoS search results for deGennes PG, deGennes P, de Gennes PG, Gennes PG, and Gennes PGD. Similar problems occur for scientists whose names changed due to marriage. And the transliteration from other alphabets has changed over the years, which makes it sometimes nearly impossible to find all papers of Russian, Japanese, or Chinese scientists. For an accurate result, it is therefore indispensible to compare the papers in the downloaded citation record with the publication list provided by the author. Fortunately, such lists can usually be found in the WWW. In conclusion, the h-index value, which is automatically calculated in the WoS, is not reliable. One should also be aware that many conference proceedings are not included in the data base. Moreover, books have only recently started to be taken into account.

Sorting the papers according to the number c of citations allows an easy determination of the h-index, see Fig. 1: h is given by the largest rank r for which $c(r) \geq r$. In the present case, one gets $h = 95$. Graphically this means that one has to search for the largest r for which the data point in Fig. 1 lies on or above the diagonal $c(x) = x$. In order to avoid the unequality, it is often helpful to generalize the definition of h to the rational variant $\widetilde{h}$: if one uses a linear interpolation of the citation frequency $c(x)$ between r and $r+1$, then $\widetilde{h}$ can be determined from the equality $\widetilde{h} = c(\widetilde{h})$. Graphically, this means the intersection of the interpolating lines in Fig. 1 with the diagonal. The original h-index is obtained by rounding the rational version to the next lower integer value. In the present case, one gets $\widetilde{h} = 95.0 = h$.

5 Advantages and disadvantages

One advantage of the h-index was already stated by Hirsch in the original publication,[1] namely that it combines the dimension of quantity as expressed by the number of publications with the dimension of quality, assuming that the number of citations reflects the quality of a manuscript. This is certainly not obvious because sometimes faulty papers attract a considerable number of citations. It is an open question whether it is worthwhile to try and eliminate such incorrect publications. On the other hand, review articles are likely to be frequently cited, although they usually do not present new research results. It is another open question whether they should be included in the h-index or not.

The mentioned advantage, however, has been criticized from a methodological point of view because such a mixture of different dimensions into one indicator is questionable in principle. Moreover, only on first sight the mixture appears to be unique because the definition of h does not depend explicitly on any parameter. In fact, one can introduce a prefactor q and demand that h_q publications have obtained at least $q \cdot h_q$ citations each.[6] This arbitrariness allows one to define a generalized index h_q, or rather an infinite number of indices which are more or rather less useful. In particular, $q = 10$ has been suggested as a reasonable choice for highly cited researchers because then the results are much smaller so that the precision problem would be reduced.[7] In Fig. 1 the respective broken line yields $h_{10} = 24$. Already for more moderate values of q, the ranking of scientists can change considerably in comparison with the original h-index.[8] This underlines the problem that small differences in the index values should not be utilized for distinguishing the researchers. It would be an overinterpretation if differences of a few index points were taken as an indication that one scientist is better than the other.

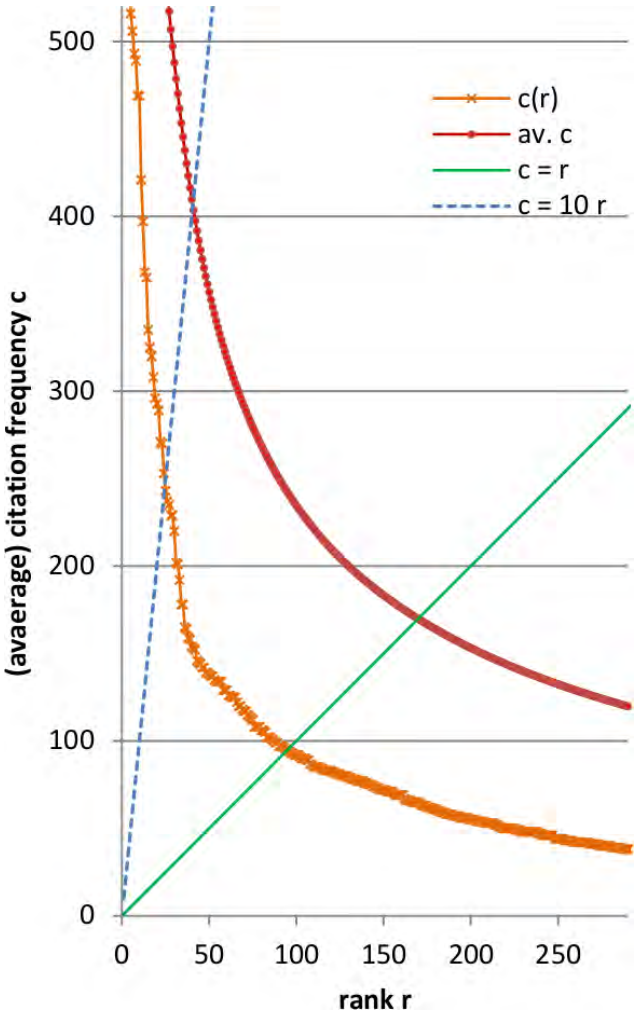

Figure 1: Citation record of Kurt Binder. The papers are ranked according to the number of citations (×). Also given is the average number of citations (•) up to rank r. The straight solid lines reflects the diagonal $c(r) = r$, the broken line indicates $c(r) = 10r$.

At first glance already, Fig. 1 shows that the citation curve is very skewed. This is usual and makes any use of average citation numbers questionable. Such averages have also

been proposed for alternative indicators. The advantage is that they counterbalance the above mentioned salami slicing tactics. However, in my view, it is unfair to punish a productive author, even if some publications have had little impact. In any case, for pushing one's h-index the apportionment of the results to several papers is probably not helpful because possible citations are likely to be also likewise distributed, leading to lower citation frequencies for the smaller units.
The skewness of the citation record can be quantified: in the present case, the 5% most cited papers have attracted 40% of all the 42,531 citations. Excluding the highly cited review articles, the 5% most cited papers still have more than 34% of all the 35,457 citations. The distribution, thus, does not quite reach Pareto's principle, the 80-20 rule but comes close to that: not 80% but 69% of all citations are concentrated on the top 20% of papers. Consequently, one should not consider average values, because the use of averages for strongly skewed distributions violates most-basic scientific standards.
This is by the way a severe criticism which should be applied also against the impact factor which for a given year is calculated as the average number of citations within that year to the papers published in the two previous years. It was originally created to judge the quality of a journal in terms of citations. But as the citation distributions of journals are also strongly skewed, the impact factor is not a good measure.[9] And it should certainly never be used to judge the quality of a single paper, because there is a low correlation between the impact factor and the number of citations to an individual paper in a journal. As an example I note that according to Thomson Reuters, the impact factor in the year 2011 for Physical Review Letters was 7.37, but less than one third of the relevant papers (i.e. from 2009 and 2010) contributed 8 or more citations. The remaining two thirds had less citations and therefore effectively decreased the impact factor of the journal. It would have been better for Physical Review Letters if these papers had not been published in this journal.

6 Further variants of the h-index

One criticism against the h-index is based on the fact that additional citations to the papers in the h core, i.e. the set of h-defining papers, do not have any effect. Many people consider this to be unfair. This shortcoming was remidied in the g-index, defined originally as the largest number g of papers that together received g^2 or more citations.[10] I wonder whether the g-index has not become more popular, only because this definition appeared to be too difficult, when the sum had to be compared with a parabola. In fact, it is equivalent to the demand that the average number $\bar{c}$ of citations to the g most cited papers is larger or equal to g.[11] In this form it looks much more similar to the definition of the h-index. The respective average values are included in Fig. 1, yielding $g = 169$. Due to the averaging, the g data show a much smoother behavior than the h values in Fig. 1.
But in principle, the above reservations against averages apply. However, in this situation there is a way out: the average is only a mathematical formality with the aim of enhancing the index value in a reasonable way by taking into account the excess citations to the core papers,[10] i.e. the $c(r) - h$ citations of the r-th paper. E.g. in Fig. 1 this means $c(1) - h = 3148 - 95 = 3053$ citations to the first paper which are not relevant for the h-index but become relevant for the g-index.
Again, a linear interpolation $c(x)$ as above allows one to define an index $\widetilde{g} = \bar{c}(\widetilde{g})$ which in this case is a real number. Now, every additional citation to the papers in the core causes an albeit small increase of the index. I consider this to be an attractive feature.
Like the h-index, the definition of the g-index is not as unique as it looks. Similarly to the above, one can utilize a prefactor q and thus get an infinite number of generalized indices g_q.[6] Specifically, one gets $g_{10} = 40$ in Fig. 1.
There is another straightforward way of generalizing the g-index. Without explicitly mentioning it in the discussion above, the average was meant to be the arithmetic mean. But there are other means like the harmonic or the geometric mean. In general, one can use an exponent p to define the Hölder means also known as power means

$$\bar{c}_p(r) = \left(\frac{1}{r} \sum_{r'=1}^{r} \Big(c(r') \Big)^p \right)^{\frac{1}{p}}$$

and utilize these means in the same way as the average above.[12] For $p = 1$, one obtains the usual g-index. For $p = 0$ and $p = -1$, the geometric and the harmonic averages yield two generalized indices which have been labeled t and f previously.[13] Surprisingly, as far as I know, the quadratic average for $p = 2$ has not yet been exploited in the present context. In the limit $p \to -\infty$, one obtains the usual h-index. The other limit $p \to \infty$ yields the citation frequency $c(1)$ of the most cited paper which some people also consider a useful quality indicator. In summary, by varying the exponent p it is possible to give more or less preference to highly cited papers.
Other variants of the h-index are based on arithmetically averaged citation frequencies for different core sizes. Sometimes, the median is utilized. Further variants are based on the square root of the summed number of citations for different core sizes. More complicated definitions have been proposed, leading to rather exotic indices which are unlikely to be utilized because the calculation is too cumbersome for practical purposes. Several variants are discussed in short reviews but shall not be given undue credit here.[2,3]

7 Modifying the database

Up to now, the mentioned variants of the h-index have all been based on the original citation data. However, there are good reasons to modify these data. One problem concerns self-citations. Obviously, self-citations do not reflect the impact of a publication. In the WoS, total citation counts without self-citations can be obtained but here, only the direct self-citations, i.e. citations by the investigated scientist

to his/her papers are taken into account. However, sometimes there is a co-author who is much more enthusiastically self-citing and of course these indirect self-citations should also be excluded from the citation record.

In his original publication,[1] Hirsch contended that self-citations would not have a big influence on the index. This conjecture is not true. I have shown that the exclusion of self-citations from the citation record can strongly change the ranking of scientists according to the so-called sharpened index h_s in comparison with the ranking according to h.[14] Unfortunately, as the exclusion of direct (let alone indirect) self-citations is at present not automatically done for each paper separately, I am afraid that this consideration will not be applied in many cases.

Another modification concerns the number of co-authors. It is an open question how multi-author papers can be treated in a fair way.[15] Usually, the contributions of the individual authors to a paper are not quantified. Nevertheless, suggestions have been made to give more weight to the first and/or last author of an author list for each publication. However, this is not very practical because there exist different traditions in different fields how the co-authors are arranged in the list. In conclusion, it remains an open question how to treat this problem. An imperfect way is to share the impact equally among the authors. One respective possibility would be to fractionalize the citation counts and attribute $c(r)/a$ citations to each of the a authors. But for the present purpose, this means that the papers have to be rearranged according to the fractionalized citation counts. This is not only impractical but it also appears unreasonable that highly cited papers with many authors are likely to drop out the core. A better way is to fractionalize the paper count, i.e. to attribute only a share of $1/a$ of the paper to each author. I have labeled the respectively modified index as h_m and shown that this modification can also have a strong effect on the ranking.[5]

Of course, one can combine the modification for multi-author papers with the index sharpened for self-citations and obtain the index h_{ms}.[16] Likewise, a modified sharpened index g_{ms} can be defined.

Another open question is how it is possible to compare the indices of scientists working in different fields. It is well known that there are different citation cultures, e.g. in mathematics and in engineering. Therefore a field normalization is required.[17] But even in one field like physics, it is doubtful whether the indices can be compared in a meaningful way.[18] For example, in mathematical physics the number of citations is usually considerably smaller than in biophysics. Therefore, a comparison without subfield normalization could be very unfair for mathematical physicists. But then, multidisciplinary papers become a problem because it is unclear which normalization should be applied.

Another difficulty occurs for large collaborations which are typical in high energy physics. If there, the paper counts are fractionalized, they are also marginalized which would be unfair. However, is it fair to take a paper with 1,000 authors fully into account 1,000 times?

8 The predictive power of the h-index

The h-index has been shown to have predictive power in the sense that there is a high correlation between the values after 12 years and after 24 years of the career of researchers.[19] This raises the question whether the h-index can be used profitably in academic appointment processes or for the allocation of research resources. However, I have shown that the evolution of the h-index with time is usually dominated for a long time by citations to previous publications rather than by new scientific achievements.[20] This is visualized in Fig. 2 where the time evolution of Binder's h-index is compared with the fictitious evolutions obtained under the assumption that he had stopped publishing in the selected years s. For example, for $s = 1988$ the index would have increased like h until 1993 and even in 2001 it would have been smaller only by 5 index points, less than 7%. If he had stopped publishing in 1997, there would have been no change compared to the actual h-index in the next 8 years and a change of no more than 2 index points until 2011, that is after 14 years! These observations should not been misinterpreted: The inertness of the h-index cannot be taken as an indication that recent publications had no impact. But it becomes more and more difficult for additional publications to contribute to the h-index when the index values are already high.

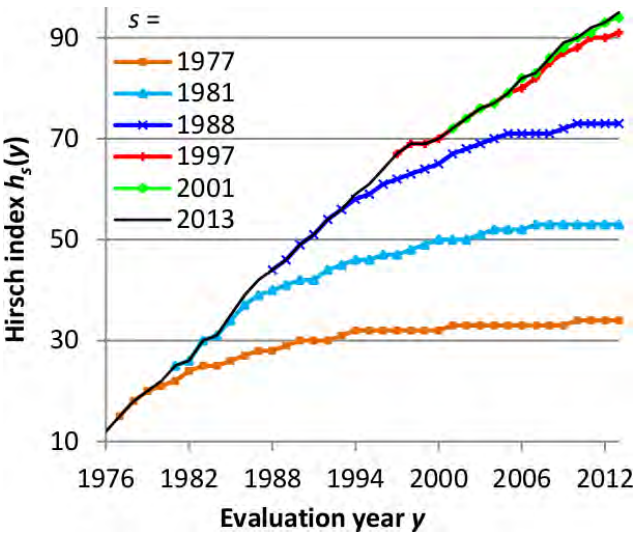

Figure 2: Time evolution of the h-index for Kurt Binder's publications (top black line). Additionally the evolution is displayed for selected years s (see legend) taking only publications up to the year s into account.

In conclusion, the h-index is a good predictor of itself due to its inertia, but it cannot predict future scientific performance. If a researcher goes to sleep in the year s, for example after getting tenure, the h-index is likely to increase anyway. Likewise, the past evolution of the h-index does not automatically mean that a candidate has performed well

in recent years. The index would have increased more or less as it did, even if the candidate had gone to sleep several years ago. On the other hand, the h-index evolution does reflect the impact of the past achievements, although not so much of the very recent research. But of course, this earlier performance is also an aspect which one may want to consider in appointment processes or for allocation of research resources.

In a more general investigation of 10 citation-frequency based indicators,[21] the current annual number of citations was found to be the best predictor of future citations, but not surprisingly, none of the indicators was able to predict citations to future work well.

9 Discussion and conclusions

The h-index has become a popular measure for the scientific impact of a researcher's publications. Whether you or I like it or not, it is here to stay, not only but also because of its simplicity. But it remains an open question whether quality can be measured in this way. However, it is certainly better than just counting publications or citations. Nevertheless, it cannot replace peer review. And in scientometrics, it was recognized already in 1981 that "uncertainties make the concerted use of citation analysis and peer evaluation inevitable".[22] But of course, peer review means reading papers which is time consuming and requires thinking. And already one of the founding fathers of scientometrics warned that "citation analysis is not a shortcut to be used as a replacement for thinking".[23]

In any case, one should always be aware that it is impossible to judge the performance of a researcher by a single indicator. Even for the purpose of measuring the citation impact, more than one number would be better than just the h-index.

But if one really wants to condense the citation record into one indicator, there are some variants of the h-index which are more meaningful, while most versions are too complicated or exotic and will not have much impact. In my admittedly subjective view, the modified sharpened index g_{ms} would be the best variant.

Due to the many possibilities to select one of the index variants, any ranking based on a single indicator should be considered with reservation. Small differences in the index values should be interpreted with caution. One can easily find examples of prominent scientists with low index values. Therefore, it is reasonable to adhere to the principle of antidiagnostics, namely that "in scientometrics, numerical indicators can reliably suggest only eminence but never worthlessness".[24]

As usual, any indicator will lead to creative adjustment processes. Therefore one should be aware that the h-index can also be subject to possible manipulation: I have seen citation records which are surprisingly flat around the actual value of the h-index what can be achieved by the strategy of selfciting the respective papers with the aim of pushing them into the h core. A more clever because not so obvious way is enhancing the respective citation counts by a citation cartel. Indeed, I have already been asked by a colleague to cite specific papers for this purpose, because a minimum value of the h-index was demanded by the administration for the promotion to professorship in that country.

10 Prospect

Given the shortcomings of the h-index which can only be weakened but not remedied by the variants, the question arises whether there might be better alternatives to evaluate the scientific performance of an individual researcher in terms of citations. One possibility is the comparison of the specific citation record with reference sets which are accessable for example via the InCites database which is also provided by Thomson Reuters' ISI.[25-27] For this purpose, reference sets for different fields (or possibly subfields) and different publication years are utilized in order to determine a position of a publication within the reference distribution. For this aim, the papers in the reference distribution are sorted according to their citation frequency and median, quartile, decile or other percentiles are determined. Then age- and field-normalized impact scores can be calculated for each publication of an individual scientist by determining the respective percentile of the reference set to which the publication belongs. Such impact scores avoid most problems associated with the h-index and its variants, because they enable cross-field comparisons, avoid age-dependent discrimination of younger scientists, solve problems with the skewed citation distributions, and also make manipulations much more difficult.

As mentioned, for such an evaluation, a comparison of each publication record with the reference set has to be performed. This is of course much more tedious than the simple determination of the h-index. But given the importance of such evaluations with respect to allocating grant money or selecting candidates for an open position, simplicity and easy access to the data base should not play a decisive role. Of course, additional costs will occur for the determination of the reference distributions. But, compared to the costs of a miscast professorship or misplaced grants, the access to InCites is not so expensive that this means an impregnable hurdle.

On the other hand, the relatively large effort which is necessary for comparing the citation frequency of each publication might be the greater hurdle which is even increased by the necessity of selecting the appropriate reference set (or, rather, sets) especially for scientists which have worked in different (sub)fields or in cross-disciplinary research. Therefore, while the described evaluation in terms of normalized impact scores is certainly a much better way than the h-index comparisons, it appears doubtful whether these proceedings will be performed in many cases.

11 Summary

My answer to the question in the title of this paper is: In principle yes, but already the Greek philosopher Plato had

realized that "a good decision is based on knowledge and not on numbers." What more is there to say?

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Views on Life, the Universe, and Everything

What is the Proper Study of Mankind: Man or Books?

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Science supposedly seeks true knowledge or, simply, truth. Much has been written regarding the scientific method. But what about the sources of science? In the history of science, both the textual and the empirical have been favorites. This article poses the question which is the proper, superior or even supreme source for the study of mankind. An integrative solution is proposed: poetic science.

Science is the poetry of reality[1]

1 Introduction

As so often, the fundamental question makes its appearance only after more applied questions have already been addressed. This looks like putting the cart before the horse. Apparently, in "normal science" this is not only very well possible, but it even seems to be required.
The most fundamental question is about the starting point of science[2] or, if you will, the source of the study of mankind.[3]

2 Question

In his An Essay on Man Alexander Pope stated: "The proper study of Mankind is Man."[4] Almost two hundred years later, Aldous Huxley teasingly rephrased Pope's pronouncement: "The proper study of mankind is books."[5]
The question that we will have to confront, thus, reads: What is the Proper Study of Mankind? Man or Books? In other words: Should the student of mankind observe or read?
This question does not ask which science or academic discipline is the via regia to truth about mankind. It is rather about the more elementary problem: Which source of science is the royal road to truth?
Is it empirical – concerning observation – evidence that we should trust most? Or should we rather trust textual – concerning reading – evidence? In other words, is empirical or textual evidence worthier of our credence?
The idea that textual evidence should be regarded as superior to empirical evidence may to contemporary scientists appear a little odd and outdated. Even if it seems outdated, it is not manifestly crazy. In fact, textual evidence was regarded superior during the greater part of history.[6] The Bible, the Church Fathers and the ancients, e.g., Plato and Aristotle, Hippocrates and Galen, were considered primary sources of truth. Only relatively recently, only gradually, and only locally, confined to certain provinces of human endeavor, empirical evidence has taken precedence over textual evidence.
Nowadays, as a matter of course, observation holds priority over reading. Therefore, we must ask whether there is a good reason for the contemporary favorite status of the empirical? Let us now then systematically weigh the arguments for the empirical and for the textual, for Man and for Books.[7]

3 The Case for the Empirical

For clarity's sake, while running the risk of appearing flippant, I will adopt once more a simplified position.
A famous and often misunderstood classical maxim maintains: "Art imitates Nature" or "Art imitates Life".[8] This is called the mimetic viewpoint. In response, Oscar Wilde proposed the reverse idea: "Life imitates Art".[9] Wilde's anti-mimetic stance, is taken a stage further in the popular saying "Life is stranger than Fiction" or "Truth is stranger than Fiction". This counter-intuitive idea becomes the more credible when we realize that "Truth is stranger than Fiction [...] because Fiction is obliged to stick to possibili-

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ties; Truth isn't."[10] Put slightly differently: "Truth must of necessity be stranger than fiction, [...] for fiction is the creation of the human mind, and therefore is congenial to it."[11] In short: "Human imagination is immensely poorer than reality."[12] One instance will, in the spirit of naïve verificationism, suffice to confirm the rule that Life is stranger than Fiction.

In March 2012 a baby rabbit was born without ears. This is rare. When the bunny was being filmed by a news team, a cameraman accidentally stepped on the unfortunate animal. The accident was fatal. The bunny didn't suffer, said the distraught director of the zoo in Limbach-Oberfrohna, Saxony, who had hoped to turn this rare rabbit into a media star.[13] Possibly, the bunny had not heard the cameraman coming. Is this "bitter irony" as one German newspaper put it?[14] I think it is even more than that; it is the perfect illustration of the idea that "Life is stranger than Fiction". Even though we do not bat an eyelid when we read that Gregor Samsa had transformed overnight into a big uncanny bug if an author of fiction had included the improbable incident of the "Rabbit Without Ears" in a novel or a short story, we would consider it an attempt to overstretch our credulity. And so this case forcefully suggests that we should be wary of Life. In other words, we should be skeptical of observation and the empirical.

Figure 1: The "Bunny Without Ears" before the tragic mishap.

4 The Case for the Textual

Not everyone will agree that "The world was made in order to result in a beautiful book." However, books, those children of the brain, can actually be both beautiful and true. Many will agree that a book, beautiful or otherwise, can make the world, in the sense that we may see the world radically differently after reading that book. Books are ideally the condensed experience and wisdom of writers. Or as one author put it "[...] personal experience comes often at a high price and it is always late; it is therefore useful to profit by the experience of others. It is in books that one finds this knowledge."[15] If the empirical has the fatal shortcoming that it is often stranger than fiction, the textual has none of this drawback. Instead, reading offers a safe and fast track to knowledge and wisdom, while enabling us to avoid negative experiences.[16] Hence, if we agree that we may prefer books to life that we may favor the textual over the empirical, then the next question arises. What kind of books should we use to arrive at the truth? Should we read Non-Fiction or would we profit more by reading Fiction?

5 The Case for Non-Fiction

Non-Fiction is the form of any narrative, account, or other communicative work whose assertions and descriptions are understood to be factual, as Wikipedia succinctly if somewhat priggishly puts it.

In exactly this spirit, Mr. Drystubble stated:

> I am not accustomed to write novels or works of that kind. [...] Not only that I never wrote anything that resembled a novel, but I even do not like to read such things because I am a man of business. For many years I have asked myself what is the use of such works, and I am astonished at the impudence with which many a poet or novelist dares to tell you stories which never happened and often never could have happened at all. [...] Therefore, I take good care not to write any novels, nor to advance any false statements.[17]

If we substitute "man of business" by "scientist", we get the idea what Ben Goldacre must have meant when he tweeted:

> "just never read any story books (or 'novels'. whatever the technical term is)".[18]

If he reads at all, Drystubble only reads one type of book: Non-Fiction. Drystubble is, in a sense, a homo unius libri. Needless to say that one should beware of such a man.

Francis Bacon summarized the case for Nature and implicitly for Non-Fiction as follows:

> Books must follow sciences, and not sciences books.[19]

This declaration on behalf of the empirical seems more a dogma than a reasoned argument.

Still, some even want to go one step further: not only must books follow sciences, but sciences must furthermore change people, change society, change the world:

> Research that produces nothing but books will not suffice.[20]

It is clear that with this last step, we find ourselves on the brink of an altogether different genre: Horror. All science is quite useless. And if it is not, it should be called technics or if it concerns society, politics. Regrettably, this insight has been all but lost in contemporary universities.

6 The Case for Fiction

Fiction is the form of any narrative or informative work that deals, in part or in whole, with information or events that are not factual, but rather, imaginary – that is, invented by the author. Thus wrote Wikipedia. Fiction is therefore essentially the opposite of Non-Fiction.[21]

Why should fiction be the more reliable source for the study of mankind? Let us hear four voices that argued this point.

Confined to the realm of depth-psychological insights, J. M. Coetzee claimed:

> Artists have told us as much about our inner life as psychologists ever have.[22]

Even further than that went Noam Chomsky:

> It is quite possible – overwhelmingly probable, one might guess – that we will always learn more about human life and human personality from novels than from scientific psychology.[23]

And Doris Lessing asserted confidently:

> There is no doubt that fiction makes a better job of the truth.[24]

Finally, Julian Barnes recently wrote:

> Novels tell us the most truth about life: what it is, how we live it, what it might be for, how we enjoy and value it, how it goes wrong, and how we lose it. Novels speak to and from the mind, the heart, the eye, the genitals, the skin; the conscious and the subconscious. What it is to be an individual, what it means to be part of a society. What it means to be alone. [...] The best fiction rarely provides answers; but it does formulate the questions exceptionally well.[25]

Coetzee, Chomsky, Lessing, and Barnes express here a thought that has a long and respectable history. If not the very first to state it, then at least as one of the most authoritative voices, Aristotle asserted:

> Poetry [...] is a more philosophical and a higher thing than history: for poetry tends to express the universal, history the particular.[26]

In other words, fiction is truer than non-fiction – "truer because of its power to condense and represent the multifarious in the typical."[27]

Within fiction, "realism [is] a corruption of reality". However, "fantastic realism", as Fyodor M. Dostoyevsky called it, improves on "realism" and may even be superior to "reality".

This is an opinion repeated and amplified by many writers and artists:

> Art is not a study of positive reality, it is the seeking for ideal truth.[28]

And with that last step, art and literature become the embodiment of the ideal: art and literature become idealistic. Is that a good thing? Isn't that overdoing it a bit?

7 Impasse

The empirical or the textual? Man or Books? There are good arguments for studying Man, and there are equally good arguments for studying Books. In truth, much may be said on both sides of this question.[29] So, what if the study of books is nothing but the study of men?[30] What, however, if you are deep vers'd in books, and shallow in yourself?[31] What if a multitude of books distracts the mind?[32] And what if it is really true that it is more necessary to study men than books?[33] Thus, we find ourselves lost in the middle of a dark forest and the straight path is nowhere to be found. Where have we departed from the right way? What Is To Be Done?

8 Resolution

When by now, we are utterly confused, we may begin to grasp that the dichotomy of Man and Books is pointless and even false. Still, we may not easily see the solution to our quandary. What we now need is a coup of cunning and guile. As so often, the easiest solution is there for the taking. Let's go back to the original problem. Alexander Pope wrote:

> The proper study of Mankind is Man.[3]

A sensible thing and in itself the perfect illustration of the value of books for the study of man because Pope had found this wisdom in another man's book:

> The true science and study of man is man.[34]

The author of this sentence, the sixteenth century French theologian and philosopher Pierre Charron, in his turn had borrowed it from yet other authors, classical and contemporary.[35]

In general, the interest of science lies in the art of making science. What comes first art or science? If science tries to make order out of perceived chaos, then art should make chaos out of assumed order. Perhaps science is indeed the poetry of reality? Or is poetry the science of reality? All these matters are now even more open than before. So, one last time, let us consider what the question is: Books or Man? The answer can only be an unscrupulously practical one: He does wisest who takes most and

best of both.[36] Therefore, I propose, a methodological triangulation: study both fictional books and empirical man and take the most and the best of both. For this pragmatic-opportunistic solution, I then propose for lack of a better term: poetic science.[37]

9 Afterthought

In the course of dealing with this question, we turned over many books together. I hope that you will not hold it against me that I multiplied words without knowledge. Of course, I quoted others only in order the better to express myself: One must never miss an opportunity of quoting things by others which are always more interesting than those one thinks up oneself. This exposition was made to show that scientists can benefit greatly from reading novels, poems and plays. In truth, the reading scientist will notice that wherever he goes, he will find that a poet has been there before him. And, I confess, it was also to show that it takes many old texts to make a new one.

References and Notes

[1] Dawkins, R. "Slaves to Superstition", Part 1 of The Enemies of Reason, Channel 4, 13 August 2007, timecode 00:38:16. Couldn't we, in the same vein as Marx turned Proudhon's title around, equally say: "Poetry is the science of reality"? Are there any more meaningful permutations?.

[2] The English noun "science" is understood here as the broader German word "Wissenschaft".

[3] Let us understand "man", "mankind", "nature", "life", and "world" as interchangeable. At first sight, this may appear methodologically reckless. Perhaps it is a trifle reckless but it has "the virtue of dramatizing the issue". Moreover, it turns out to be justified when we consider that all these words stand for "sense experiences other than reading".

[4] Pope, A. "An Essay on Man" (1733–34), Epistle II, line 1. In the first edition it still reads "The only science of Mankind is Man.".

[5] Huxley, A. *Crome Yellow*. London: Chatto & Windus, 1949 [1921], p. 304.

[6] Of course, sometimes the world itself was regarded as a text to be deciphered and interpreted: "The Book of Nature". This was not done primarily as a homage to Nature but rather as a homage to the Book, i.e. the Bible. Also every particular natural object could be understood as a text or book: "Das Gräslein ist ein Buch, suchst du es aufzuschliessen / Du kannst die Schöpfung draus und alle Weisheit wissen." Czepko von Reigersfeld, D. "Sexcenta Monodisticha Sapientum" (III, 10), in *Sämtliche Werke*, vol. I, part 2. Berlin / New York: Walter de Gruyter, 1989 [1655], p. 591.

[7] Whatever the outcome of our evaluation, for practical reasons, when searching for the truth, we always either read texts or observe nature. When we read texts we may study nature indirectly as, or so we prefer to think, texts are written by authors who base their texts on empirical observation. On the other hand, everybody must rely on the spoken or written word. In practice, the written word, especially as a scientific publication, is deemed authority: a tenacious illusion and sometimes a very foolish one (*Nullius in verba*).

[8] Aristotle, *Meteorologica* IV. 3 and *Physica* II. 2; Seneca, *Epistulae Morales ad Lucilium* LXV. 3; Marcus Aurelius, *Meditations* XI. 9.

[9] Or actually: "Life imitates Art far more than Art imitates Life" (Wilde, O. "The Decay of Lying – An Observation [1891]" in *The Collected Works of Oscar Wilde*, (ed. Robert Ross), Vol. 8, London: Routledge, 1993, p. 33). Wilde's wisdom may – in true form – have been derived from another writer's book. «J'ai lu plus de trente romans, j'ai vu plus de vingt pièce de théâtre, [...]; et, croyez-moi, la vie ressemble plus souvent à un roman qu'un roman ne ressemble à la vie.» George Sand, *Métella* [1834], in *Œuvres de George Sand – Mauprat-Métella*, Paris: Hetzel, 1852, p. 369.

[10] Twain, M. "Following the Equator", in *Following the Equator and Anti-imperialist Essays*. New York/Oxford: Oxford University Press, 1996 [1897], p. 156 (*Pudd'nhead Wilson's New Calendar*).

[11] Chesterton, G. K. *The Club of Queer Trades*. Harmondsworth: Penguin, 1984 [1905], p. 66.

[12] Pavese, C. *Il mestiere di vivere*: 1935–1950. Torino: Einaudi, 2000 [1952], p. 127 (25 ottobre 1938): "La fantasia umana è immensamente piú povera della realtà.".

[13] http://www.spiegel.de/international/zeitgeist/0,1518,821389,00.html The hope of turning this bunny into a media star was justified as it was a true "Keinohrhase" or "Rabbit Without Ears", which was the title of a recent popular German film: http://en.wikipedia.org/wiki/Keinohrhasen.

[14] http://www.sueddeutsche.de/panorama/kameramann-zertritt-kaninchen-kurzlebiger-keinohrhase-1.1311135.

[15] Choderlos de Laclos, P. A. F. "Des femmes et de leur éducation", III (Troisième essai), in *Œuvres complètes*, Paris: Gallimard (Bibliothèque de la Pléiade), 1979, p. 434: «[...] l'expérience personnelle est souvent chère et toujours tardive; il est donc utile de profiter de celle des auteurs. C'est dans les livres que celle-là se trouve.».

[16] "People say that life is the thing, but I prefer reading." Smith, L. P. "Myself", in *Afterthoughts*. London: Constable, 1931, p. 71.

[17] Multatuli [Eduard Douwes Dekker], *Max Havelaar*. Edinburgh: Edmonston & Douglas, 1868 [1860], pp. 1–2.

[18] Goldacre, B. Tweet, 27 October 2010: http://twitter.com/#!/bengoldacre/statuses/28888167582.

[19] Bacon, F. "A Proposition etc: Touching the Compiling and Amendment of the Laws of England [1657]", in *The Works of Francis Bacon* (eds. J. Spedding, R. L. Ellis, D. D. Heath), Vol. 13, Cambridge: Cambridge University Press, 2011, p. 67. Compare: "From the earliest days of the experimental pioneers, man's stipulation that psychology be adequate to science outweighed his commitment that it be adequate to man." (Koch, S. 'Epilogue', in *Psychology, A Study of a Science*, Vol. 3, New York: McGraw-Hill, 1959, p. 784).

[20] Lewin, K. "Action Research and Minority Problems", in *Resolving Social Conflicts*; Selected Papers on Group Dynamics, (ed. G.W. Lewin), New York: Harper & Row, 1948, p. 203. See also: "The philosophers have hitherto only interpreted the world in various ways; the point is to change it." Marx, K. "Theses on Feuerbach" [1845], Thesis 11, *Marx/Engels Collected Works*, Vol. 5, Moscow: Progress Publishers, 1975, p. 5. Needless to say that it always ends in tears and necessarily always must end in tears.

[21] I am cutting another corner. For the opposite view are also good arguments: "[...] this puts in question the distinction between fiction and nonfiction [...]." Coetzee, J. M. *Stranger Shores – Literary Essays* 1986–1999. New York: Viking, 2001, p. 145.

[22] Coetzee, J. M. *Stranger Shores – Literary Essays* 1986-1999.

New York: Viking, 2001, p. 37.
[23] Chomsky, N. *Language and Problems of Knowledge: The Managua Lectures*. Cambridge (Mass.): MIT Press, 1988, p. 159.
[24] Lessing, D. *Under My Skin: Volume One of My Autobiography*. London: Flamingo, 1995 [1994], p. 314.
[25] Barnes, J. *Through the Window - Seventeen Essays (and one Short Story)*. London: Vintage Books, 2012, Preface.
[26] Aristotle, *Poetica* IX.
[27] Coetzee, J. M. *Inner Workings – Literary Essays 2000-2005*. New York: Viking, 2008 [2007], p. 232.
[28] Sand, G. *La Mare au Diable.* Bruxelles: Meline, Cans et Compagnie, 1846, p. 9: «L'art n'est pas une étude de la réalité positive; c'est une recherche de la vérité idéale, [...]».
[29] Fielding, H. *The Covent Garden Tragedy*. London: Routledge, 1997 [1732], Vol. 9, p. 182 (Act I, Scene viii).
[30] Sand, G. *Valvèdre* [1861], in *Œuvres de George Sand – Valvèdre*, Paris: Jules Claye, 1864, p. 11: «L'étude des lettres, qui n'est autre que l'étude des hommes, [...]».
[31] Milton, J. *Paradise Regained*, Line 322.
[32] Seneca, *Epistolae Morales ad Lucilium*, II. 3: "Distringit librorum multitudo".
[33] de la Rochefoucauld, F. "Maximes posthumes (No. 550)", in *Œuvres complètes*, Paris: Gallimard (Bibliothèque de la Pléiade), 1964, p. 481.
[34] Charron, P. *De la sagesse*, Book 1, Chapter 1 (Bordeaux, 1601): «[...] la vraye science et le vray estude de l'homme c'est l'homme».
[35] Lessing, G. E. "Nachträgliche Anmerkung zum Art. Charron, Th. XIV, S. 141" in *Gotthold Ephraim Lessing's Sämtliche Schriften*, Vol. 15, Berlin: Göschen, 1826, pp. 295–296.
[36] Fielding, H. *The Covent Garden Tragedy*. London: Routledge, 1997 [1732], Vol. 9, p. 183 (Act I, Scene viii): "he does wisest who takes most of both.".
[37] Poetic in the sense of A. *adj*. 6. in the OED (second edition, vol. XI, 1989, p. 1119). This is in acknowledgment of Richard Dawkins, whose words I used as motto, and Ada Lovelace, whose term – poetical science – I shortened one syllable. Also it is, of course, a term that automatically reminds us of its opposite: Bruno Latour's prosaic science.

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