

Quo vadis universitas?

Kritische Beiträge zur Idee und Zukunft der Universität

Edited by Hans-Ulrich Rügger

No. 13, February 2010

Publication Ethics

Contributions by
Sabine Kleinert
Irene Knüsel

Introduction by
Nikola Biller-Andorno
and Frank Rühli

ISSN 1662-2502

Contact „Quo vadis universitas?“
Hans-Ulrich Rügger
University of Zurich, Research and Academic Career Development
8001 Zurich, Kuenstlergasse 15

<http://www.fnf.uzh.ch/quovadis.html>

Content

Preface	3
<i>Nikola Biller-Andorno and Frank Rühli</i>	
Introduction	4
<i>Sabine Kleinert</i>	
Research Integrity and Publication Ethics: Common Transgressions and Solutions	6
<i>Irene Knüsel</i>	
View from the Bench-Side	18
Literature	22
Authors	22

Preface

In the world of scientific publication, morals are not completely different than in daily life. There are cases of clear research misconduct; they are serious, but few. If misconduct occurs, it creates a disruption, not only for the scientific community and their institutions, but for all individuals involved. Relationships of personal trust are broken, academic careers may be terminated. Apart from cases of clear misconduct, there are also questionable research practices. This is a huge area, with which all scientists are confronted in their daily work. Perhaps a researcher cannot remember the source of a quotation – could it not just as easily be his own thought? Why shouldn't a scientist do a colleague a favour by adding his name to a paper? It is, however, not only the question as to what the right thing to do is, but also as to what is appropriate in a given situation. As Cicero said, "nothing is more difficult than recognising what is appropriate" (Orator 21,70). There are moral problems and ethical questions in scientific publication – we should talk about them.

Sabine Kleinert gave a talk and shared her experiences as a medical editor with publication ethics issues as well as her general view on research integrity in a panel discussion on 30 September 2009 at the UZH Irchel. We have published a transcript of her talk that reflects the style of spoken language. Irene Knüsel participated in the panel discussion as a young academic and has contributed a summary of her view from the benchside: "Science today is like driving on a busy highway." Frank Rühli and Nikola Biller-Andorno initiated the panel discussion as members of the Ethics Committee UZH.

I am grateful to Janine Hall for the transcription of Sabine Kleinert's talk, to Martina Arioli and Sandra Weiser for editorial help and to Mary Carozza and Alexander Borbély for the corrections.

Zurich, in February 2010, Hans-Ulrich Rügger

Introduction

by Nikola Biller-Andorno and Frank Rühli

Falsified data, manipulated analyses, illegitimate authorship – the issue of scientific misconducts arises time and again in the media. Such scandals can have considerable consequences for a discipline, particularly if the field of research itself is controversial, as was the case with “Hwang-gate”, which occurred at a time of fierce public controversy over the moral legitimacy of human embryonic stem cell research.

Most of the time, however, instances of misconduct tend to be forgotten after a few weeks, perhaps leaving a vague sense of public mistrust in science in general and scientists in particular. Yet, public attention generally moves on to other topics, and researchers get back to business as usual.

There is, however, a danger in this way of handling the phenomenon. For one, it can reasonably be assumed that the scandals hitting the news may be nothing but the tip of the iceberg, leaving many instances of scientific misconduct undiscovered. Despite discussions among scientists (e.g. Paalman 2000, Wilmshurst 1997), no reliable statistics exist on issues of misconduct – neither in Switzerland nor internationally. Yet, an experiment substantiated that two-thirds of intentional major errors in a fake manuscript were not noticed by reviewers (Baxt et al. 1998).

Secondly, we need to turn our attention to those practices that are in the grey zone between proper and improper conduct. Many such grey areas have been clarified in recent years through agreements made for dealing with the issues. Some of these agreements are limited to certain areas – e.g. authorship criteria in medical sciences. Theoretically – at least for issue of publication – proper conduct has been clearly defined by the Vancouver convention. And yet, the convention is often not implemented correctly (Goodman 1994). However, with interdisciplinary projects becoming more and more common, it will become increasingly necessary to discuss and – where it makes sense – to harmonise varying scientific cultures with respect to the issue of proper conduct.

Last, and possibly most important, we tend to miss the chance to discuss the underlying reasons for such cases happening in the first place: Are they only caused by extraordinarily greedy, ambitious individuals, or are there structural factors fostering such an unfortunate development? Does the climate of

continuous competition, as well as the focus on grant acquisition and high-impact publication, lead to an environment in which the threshold for scientific misconduct is rather low? Does this create an environment in which it may, in reality, be considered a smart move to not take the rules too seriously? Perhaps we ought to reassess whether our current expectations are realistic and whether a “normal” academic career can legitimately include phases of diminished output, of experimenting with something that turns out to be unsuccessful or even lack innovation. Perhaps we need to question whether negative results really mean “no results” at all. We may find that acknowledging apparent detours, negative results and less productive phases may actually lead to more sustainable careers as well as a richer and possibly better science.

Proper scientific conduct cannot be learned by simply studying guidelines. It needs to be lived in the lab or in the group. Young scholars must learn from their superiors that scientific integrity is not an ideal irrelevant to their practice in real life, but that it belongs to the heart of science. The loss of scientific integrity not only ruins public trust, it also renders scientific progress inefficient, if not impossible.

Research Integrity and Publication Ethics: Common Transgressions and Solutions

by Sabine Kleinert

Introduction

Publication ethics is one of my great passions. I hope at the end of this evening it will become one of yours as well. I want to not only share with you my experience as an editor with publication ethics issues, but also my general view on research integrity. Because I don't think research integrity and publication ethics are two different things, there is definitely an overlap. But what is important is that, as editors, we are often the first people who will see that something is wrong with research and the way it has been conducted. Yes, there are the pure publication ethics issues such as duplicate publications, such as authorship issues. But we also often get confronted with papers where we say: "How did this go through an ethics committee?" "How on earth did patients give consent to these kinds of procedures?" So there is a huge overlap.

The research environment in which I did research – it's now something like fifteen years ago – was different. I think there was less pressure. Now there is pressure to publish – publish or perish. The first question when applying for a position is: "What's your publication list?" There is intense pressure and competition for funding, for career progression. Then there is the issue of conflict of interest. Researchers are increasingly expected to have commercial interest, be it imposed by the university or be it by companies that approach you and enthruse you with lots of money, travels, gifts and so on. There are many guidelines and rules out there by which we are meant to abide. Collaborations are also relevant – research is not done anymore by one or two people in one lab. It is done between departments, between universities, between different cities in countries, and between groups in different countries or continents. There is intense competition, not only between individuals but also between disciplines and between institutions.

So, what I will talk about is not only publication ethics but also a moral and ethical framework of integrity. Then I would like to briefly discuss what I think research misconduct is. How big a problem is it? What are the com-

mon scenarios and what has my experience with misconduct been? What solutions do I see – either on the part of editors, institutions or the researchers themselves. How can we foster integrity? We want to prevent misconduct and I believe the ultimate goal ought to be to foster an environment of integrity.

Moral and ethical framework of integrity

The work environment is a hugely influential factor in terms of integrity. If you work in a lab where your supervisor doesn't take things so seriously, then you learn that this is the accepted way of doing research. If you are in an institution where people get away with gift authorship all the time, you probably won't think there's anything wrong with it. There's also the big issue about how to deal with whistleblowers. It's easy to see things and then to look away.

Lawrence Kohlberg was an American developmental philosopher who researched the stages of moral development in human beings. According to him, development goes from obedience – “How can I avoid detection and punishment?” (the typical infantile stage) – to self-interest as a child – “What is in it for me?” – to conformity due to interpersonal relationships – “What is everyone else doing?” That's the teenage stage. Then it moves on to law and order as a young adult and, ideally, there will be a social contract for the greater good of society. I don't know how many people actually reach the last stage of universal ethical principles.

However, I would like to say that in research integrity, I think, most or all of us are stuck in the stages of the child, teenager, young adult stage, and I think that's worth thinking about. I actually even know examples of people who are still stuck in the infantile stage.

What is research misconduct and how big a problem is it?

How big a problem is it? Can I have a show of hands of people who know of practices they think are not correct? Seven out of thirty people. There you are. People were asked – this was published in *Nature* (Titus et al. 2008) – if they had observed or had direct evidence of research misconduct – and here it's not the vague phrase I've used, but actual fabrication, falsification, plagiarism, serious research misconduct. 8.7% said they had observed it. Okay,

this is a survey and it may not be accurate. But I think, if anything, it is probably an underestimation.

So what is research misconduct? There is no black and white. We often talk about serious research misconduct, which is *fabrication, falsification, plagiarism*. But then there are all these other practices which are now commonly termed as *questionable research practices*, like ignoring outliers in your data (data points that you don't like – just forget about them), undeclared conflicts of interest, no data on side effects (I will keep them for a later paper), gift authorship. These are all transgressions in good research practice. If we take a different definition: 'Questionable research practices' is less than responsible conduct, it falls short of good research practice. There are a lot of issues that fall under that umbrella.

Very few people will be in the area which is *responsible conduct of research*, where you do absolutely everything correct. We are talking here about 10–15%. Very few people would also be in the very serious research misconduct area of fabrication, falsification, plagiarism. Estimations have been up to 1%, although I think in reality it's probably slightly higher. But there's a huge, fat middle bit where we talk about questionable research practices, and I think that's an area we have to think much, much more about. There are all these spectacular cases that you've all heard about – Hwang from Korea, stem cell falsification, and so on. Yes, these are spectacular cases. But I think we need to really look at this whole area of questionable research practices.

Common transgressions

As a framework there is integrity in research planning, conduct, analysis, and reporting. And publication ethics falls into the last category. But as editors, as I said before, we see often problems in all of these other areas before.

What, for example, can go wrong in *planning*? Is my research needed? Remember our Kohlberg stages. Is that research I'm doing just for my CV, or is it research that really needs to be done? For example, is there enough animal data to do a first human study? You need to do a lot of research to know what your research question should be and what direction your research should take. If you do a drug trial, are you using an inferior comparator? That's often done deliberately, of course, to make a new drug look better. Is there a detailed protocol for the study you're doing, including ethics

approval, a valid consent procedure, especially if you do research on humans? Is there an analysis plan? All these fall into the area of planning.

Now, *conduct of research*. This is especially the case in randomised trials. Is there an adequate randomisation method? Have I maintained blinding? Is the patient consent procedure actually done as described? If you do more basic science research, how has the data collection been done? What happened to the lab books, case report forms, excel spreadsheets, etc.? These often kind of disappear, or are not to be found, or go up in fire, are eaten by white ants. I've heard all sorts of explanations as to how lab books, excel sheets, computers etc. disappear. Has there been regular oversight during the conduct of research? I'll come back to that because I think that's really important.

Analyses – you now get your data. You don't like some of the data? Get rid of the outliers. What happens to missing data? How to handle that? How many analyses have you done? Have you planned to do three or four analyses or are you just analysing your data to death until you find a positive result because, yes, we all know that positive results are more likely to be published. *Post hoc* analyses – are they clearly defined as *post hoc* analyses? They are different, they are hypotheses-generating and they are not your main findings. At the *Lancet*, what we always do now with randomised, controlled trials, is to ask researchers to submit the protocol with the trial because then we can at least see what was planned. There are some odd cases where I have been sent a protocol that was written in the past tense. Or where the protocol was so different from the study that was submitted that I rang up the author and asked: "Have you sent the wrong protocol?" And he said: "No, no, no, but we found something completely different, so we've just sort of written it that way." It was not written the way it was planned. Changing your endpoints: secondary are positive, primary are negative, just swap them around, makes it sound more exciting, doesn't it? Omitting results, omitting adverse events – there are many, many possibilities, all of which I can assure you I've seen many times.

In *reporting*, one of the forms of misconduct – and this might be slightly controversial – is failure to publish, to not publish at all. You've done your research, you didn't like your results, it was not what you expected – you put it in a drawer. Why is that misconduct? If you have done research on people, or you have used animals or cells, you have a duty to bring it out there into the open. People always come back to me and say: "But you journals, you don't publish this kind of unexciting results". Which is true, I ad-

mit that the *Lancet* probably is guilty of that. We do get after all 8000-9000 submissions per year. We do publish negative data, too, but negative trial data only when it is an important negative. However, there are now so many web-based journals out there – there is even a Journal of Negative Results – where you can actually publish those results. It's better to do so – even if it hasn't got an impact factor of 20-something – than having your study disappearing in a drawer.

Now let us discuss authorship issues, and we'll talk about that a little more carefully. Who did what is sometimes unclear. There are three Gs: ghost authors, guest authors or gift authors. Duplicate, redundancy, salami publication, that's all part of the spectrum of the same theme. Plagiarism is on the rise. I think it's partly because it's just easy to do; electronically copying and pasting is just very easy. The concept of self plagiarism, where you have already published a review, an article or a paper with very similar research and you actually just lift your whole discussion or your whole introduction or your whole methods section and stick them into the new paper. Is anything wrong with that? Some people say no. I always get very annoyed as an editor when this happens, I have to say.

Misleading reporting is very, very common, we see it all the time. There's a big drug study, a pharmaceutical company funded study: the findings are negative. Then they torture the data with *post hoc* sub-analyses and the bottom-line is: In the important subgroup X, Y, and Z, this drug works brilliantly. Or all side effects are reported in one little sentence somewhere. And we have to dig deep and ask the authors what were they, how many were there? Were they serious, were there minor side effects? This is the most common area, I would say, of publication ethics transgressions.

Falsification and fabrication we see too, but these are rare. What does happen quite often, and that's an interesting area, is figure manipulation. There are two types of figure manipulation. Again, it's easy to do it now with Photoshop and so on. There's the figure manipulation where you just make the picture look prettier, you change the contrast slightly or whatever. Well, that's still manipulation, but it's probably not influencing the data. But there is more sinister figure manipulation, as well, where a wrong band gets taken out and suddenly the results look much, much better than before. Some journals have started to screen for figure manipulation, especially in those papers they are planning to publish. And the figures there are quite staggering once again. They found in one study that about 20% of the figures were

manipulated to a certain degree. Half of those were manipulated in a way which actually changed the results.

Plagiarism

The answer I always get when I confront authors about plagiarism is either “Oh, I didn’t know there’s anything wrong with that” or “I’m reading so much that, just, you know, I don’t know how else to write it, how to write it differently”. I would like to challenge any of you to write a review twice within let’s say a framework of four or five months with exactly the same words. I think it would be a very good achievement if you could do that. So, again, that’s not an excuse I buy because I do think you have to actively copy and paste to get the same review word for word – a case I had very recently. Well, actually not word for word: he changed females to women. Omitted reference is another explanation we get and that usually applies if there is a little sentence which is taken word for word from a previous publication or from somebody else’s publication. And, you know, if it’s really only one sentence, we probably would say, ok that’s possible and we would then issue a correction rather than go down the route of investigating this person for misconduct. Who usually discovers plagiarism? Editors? Unlikely, although we have done so in the past. Reviewers? They do sometimes, it depends. It can happen that it’s actually the person’s review that is plagiarized, the person we sent it to for review – fortunate for us editors, unfortunate for the author. Readers? They will definitely discover it eventually. The most common scenario is that we are contacted afterwards by readers who say: “Did you notice that this was very similar to a paper published three years ago?” Plagiarism, again, but are all authors equally guilty? Not necessarily. It is often the first author who writes most of the paper or, in this case, plagiarized most of the paper. But we do challenge all the authors because we then get into the area of authorship questions. We had an interesting case where almost everyone in ophthalmology in Norway was named as author of a paper that was plagiarized from one of our review papers. The author in question clearly did the deed, but all the other authors who were listed on this paper, about ten or something like that, said they had nothing to do with the plagiarism.

In future I think there will be software available to screen for plagiarism. At the moment, one in development is Crosscheck. It’s expensive, you have to pay per article screened and we would only do that, I think, for papers that we are intending to publish. It’s not yet fully developed because it needs many journals to sign up to it so that all their material can be screened and it

still means that the editor manually checks any manuscript that comes up with a high percentage of overlap. Free systems that are out there are either not good at all or not useful for us editors to screen.

Authorship

I said the three Gs. I'm sure that you all were once in that position on either the receiving or the giving end. The *guest author* says: "I'm the head of the department – I always have to be on the paper." "I give you lab space – I have to be on the paper."

Gift authors, authorship as a present, is very common, we find, in Asian countries. I had this weird case where a Chinese paper was rejected by us and then I got rung up by Fiona Godlee, the British Medical Journal editor, and she said: "Your name is on a Chinese paper. I don't quite know how this came about." So I contacted the author and he said: "I just put you on the paper because you so kindly looked at my paper and rejected it." And the story gets more bizarre. We contacted the author and said: "That's not right. You can't do that. I didn't have any input in this paper other than reading it." And then three weeks later Jeff Drazen from the New England Journal of Medicine emails me and Fiona Godlee, said: "Did you both know that you are on this paper from this Chinese author?" He didn't quite get this message.

Ghost authors, this is also very common. There is a peer review meeting held in Vancouver every four years. The most recent was in September and it was said that about 10% of all papers have so-called ghost authors. These are authors who have written the paper, usually medical writers from a company or a communication agency, and then are mentioned nowhere. We say they need to be at least mentioned in the acknowledgement section. Depending on how much they have done, they might be qualified for authorship. A year ago the *Lancet* started to ask every corresponding author of a paper before submission in our web submission system: "Was there a medical writer involved?" So they have to say actively yes or no. If yes: who is it, what's his or her name, and who funded this medical writer? Since then we have seen this much, much more often than when we didn't ask that upfront but just relied on authors to disclose the information.

You are probably familiar with the *authorship definitions*. Not all journals go by these and the *Lancet* actually doesn't. We have a contributor system by which we say: "You, the authors, it's up to you as a group to decide who

is an author, you all have to agree, and then tell us who did what.” If it says XYZ has given the lab space, then it’s up to everyone to see that this person really isn’t mentioned as an author. The International Committee of Medical Journal Editors (ICMJE) has gone further with the authorship definition recently: Criteria are 1. substantial contribution to conception and design, or acquisition of data, or analysis and interpretation of data; 2. drafting the article or revising it critically for important intellectual content; *and* 3. final approval of the version to be published (1, 2, and 3 must all be met). All persons designated as authors should qualify for authorship and all those who qualify should be listed. Each author should have participated sufficiently for an appropriate portion of the content as part of the attempt to further clarify authorship.

Authorship is now difficult because it’s all team science, it’s cross-disciplinary research. It is complex, we sometimes have twenty, thirty, sometimes forty authors – when you look at physics papers, there are hundreds of authors. There is a pressure to publish, and authorship credit is professional advancement. But I always urge you to decide at the beginning, at the outset of a study, at the planning stage, who should be an author and why and what should their roles be. What we also see infrequently is people wanting to take the glory, wanting to be an author but when something is found to be wrong with the paper actually not standing by it. We had a case last year with a group from Innsbruck where some authors said “I had nothing to do with that” and, actually, it turned out in the investigation that they really had not much to do with it. They wanted to use the fact as a defence, but it is not a defence. *You can’t take the glory without the responsibility.* The journal *Nature* has tried to respond to the problem of authors not wanting to take responsibility when something has gone wrong by making the author in question sign something like (November 2007): “I have ensured that every author in my research group has seen and approved this manuscript. The data that are presented in the figures and tables were reviewed in raw form, the analysis and statistics applied are appropriate and the figures are accurate representations of the data. All journal policies have been adhered to. I have confidence that all of the conclusions presented are based on accurate extrapolations from the data collected for this study and that my colleagues listed as co-authors have contributed and deserve the designation ‘author’.” They wanted the author to actually sign this statement, but there was a lot of disquiet about the suggestion and, as far as I know, they have not enforced it.

Duplicate, redundant, salami publication

Salami publication, this more or less means: slice the thinnest slice you can get away with to make as many papers out of a salami that you possibly can. *Redundant* as well as overlap. You have twenty data points and you have ten more, so you publish your first twenty and then publish a paper with thirty afterwards, and so on. *Duplicates* mean purely trying to republish what was published before. And this is almost always inappropriate. There are exceptions. The exception is if you want to publish something again in a different language. To some extent you can justify some of the salami type of publication if there is a very different readership, for example, when you publish main trial data with clinical outcomes and you publish the immunological outcomes as well, although increasingly journals don't like that anymore. We want to see the whole picture. We want to see primary and secondary outcomes. We want to see quality of life data in the main paper. We want to see the whole picture together. Because people try to publish when there are two primary outcomes, they try to publish two different papers from that and that's absolutely inappropriate. But I believe that a solution is simply to have maximum transparency. Speak to the editor, tell him: "This has already been published and now I want to publish this part." Or talk to people, let them know. Editors may say: "Yes, that's OK. I'll buy that, as long as it's referenced, as long as it is out there, people can make a decision."

Conflict of interest, role of sponsor

This is a big area again. Often people say: "I don't have a conflict." I hear all sorts of funny stories about conflicts of interest. People honestly write to us and say: "I'm funded by pharmaceutical companies A, B, C, D, E and F, so I'm funded by everyone, so I don't have a conflict of interest. "I don't have a perceived conflict of interest." . Perceived by whom? The author obviously wants to just gloss over something. So again, the *conflict of interest* – easy, just be one hundred percent transparent about it, say everything you do, say everything, declare all the money you get, everyone who sponsors you. It's not up to you to decide whether this is a conflict or not. The word conflict is slightly difficult and some journals say: "We call it competing interest." Or maybe there are better words to come in the future. However, I believe authors who represent a certain point of view must explain their position very clearly. The same is needed with the *role of sponsors*. What did the sponsors do? Did they analyse the data? Obviously that needs

to be known because there is a difference between the ways I look at it and reviewers look at it. We say that of course all authors should have access to the data as a prerequisite. Another journal went slightly further and said they want to have independent statistical analyses of all company sponsored trials. Since they introduced this rule about three years ago, their trial publications have plummeted. It is difficult to say companies' statisticians can't do their job as well as an independent statistician. And often company trials are run better than independent trials. So I don't think you can say that just because there is a pharmaceutical company involved that something fishy is going on. But I think *transparency is the key*, again.

Solutions

The best way to handle misconduct is via the *institutions*. *It's good to have a clearly defined person as the first point of contact*. It's terrible when I have a doubt and I don't even know whom to contact at the institution or at the university. In the USA, every institution has a Research Integrity Officer. *Due process is really important*. Have a process in place by which any allegations are investigated. The investigation should be fair and ideally speedy. In my experience, serious allegation investigations take up to a year, the quicker ones perhaps six months but I have never seen anything under six months. And ideally it should be an independent investigation by someone who is outside the university or at least a chair who is outside the university. I think *the results need to be made publicly available*. I had enormous troubles last year with an Austrian case in which the Austrian agency said: "Oh, we can't publish that for legal reasons." So I have all these documents, but they couldn't be published for legal reasons. It makes it very difficult then to justify retracting a paper. Then: *work with and inform all relevant stakeholders*, journals - there might be more than one journal involved - and funders. And I think one of the areas that is often not handled well is the protection of whistleblowers. *Have a clear pathway for whistleblowers*, for what they can do if they've seen something that shouldn't happen. Afterwards *there should be appropriate sanctions and consequences*. That might be obvious, but very often it isn't the case. But we should go further and, once the case has happened, *there should be lessons for continuing culture change within an institution*. Ideally a country should also have national bodies to oversee or perhaps help in conducting independent investigations. The Scandinavian countries are very good at having these national bodies.

Editors - how to handle misconduct? I think the first role is for us not to just reject the paper when it's only submitted. When it's published we have a

duty to do something with the paper because we actually have to correct the published record. But when the paper is just submitted and it looks a bit odd, the common way to deal with it is to just reject it. What often happens then, is that it is published somewhere else. It gets published in a journal that doesn't have as many editors as the *Lancet* does and where there isn't as much time to follow up on these cases. Again, it should be our duty that authors should be challenged first and, if it's serious and I am not satisfied with their reaction, my duty as an editor is to go to the institution in question. I cannot investigate, I don't know what's going on in that research enterprise. That needs to be done by the institution. So, we are instigating the investigation and then acting on the findings, including retraction if appropriate for a published paper. There is always a question about banning authors but that's very problematic. How do we do that? Which of the authors and for how long? We actually don't do that, but we all have a very long institutional memory that's not written down anywhere. In addition, there is the Committee on Publication Ethics (COPE) where we meet every three months and discuss difficult cases anonymously and give each other advice on what to do.

How to foster integrity?

I believe it is really important to foster integrity. Not only react to bad cases but also try to prevent them from happening in the first place. There should be clarity in *reporting*, which makes it a lot easier. Also, authors are aware that, when they give all this information, it will make it a lot easier for me to see if something is wrong. There should be promotion of honesty and transparency. I think I've used that word many times already. *Transparency is key*. In terms of protocols, ethics approval, trial registration, statements. Some journals have gone down the route of screening, which has the advantage that you wouldn't publish something that has been plagiarized or figures that have been manipulated. It has the added advantage that it acts as a deterrent. Because if an author knows that a particular journal is screening, it would be stupid to manipulate figures. We can write editorials and commentaries on the issue – and we do that.

How to foster integrity at institutions? I think there should be *guidelines* covering all aspects of research. But many universities have guidelines and when you look at them it looks like there are guidelines all over the place. But what is often lacking are clear consequences. What happens if people don't follow these guidelines? There should be *mandatory education* in responsible conduct of research. I think it's happening more and more at the

student level, but I believe it should also include all researchers, including professors. There should be effective and responsible *mentoring* in terms of role models. That is very important. Remember an environment of integrity is important. It would be great if, somehow, all studies that are in progress or planned, or are done could be *centrally documented* in terms of study protocols. And it would be great to have *central storage of raw data* or lab book data. *Random checks* and audits of ongoing research I think is a very good idea. Again, this may mean that you might find something early on and not some six or ten years after it's published. In addition, it acts as a deterrent. People know that they might be subjected to random checks and might not feel as comfortable to manipulate some of their data. *Clear and transparent policies*, especially in the areas of conflict of interest and intellectual property, I think are very important at an institutional level. I want to leave you now with just one thought. The loss of reputation is a really serious and widely underestimated risk for universities. So I do believe that universities need to make fostering and guarding research integrity part of a discussion on their risk management strategy.

View from the Bench-Side

by Irene Knüsel

Where do we go from here? Is there anything left to say? We have all been confronted with numerous statements, facts and figures on various forms and shades of scientific misconduct, starting with ethically questionable research objectives and methods, unjustified expectations and misinterpretations, on to exclusion, loss, manipulation and even fabrication of scientific data. Some of us were shocked or embarrassed, others still in complete denial, a few perhaps relieved to hear that the majority of scientists actually does follow good laboratory practice and performs not only honest but also excellent scientific work. And, fortunately, there are solutions: hundreds of pages with recommendations and guidelines on teaching and education with respect to handling misconduct in scientific research. Ombudspersons or research integrity officers have been trained and appointed to provide research institutions as well as political authorities with advice on basic questions concerning scientific integrity. Problem solved – we have everything under control, let's go back to the bench!

Problem really solved?

But why, then, does cheating and manipulating data in science still occur? Worse yet, it is not as rare as we all thought and, if we believe the pessimists, there is an upward trend. Quite possibly, we have not completely solved the issue of responsibility, liability or even penalties and sanctions. For punishment, however, we usually opt for the easy solution: find the students who did the experiments and kick them out and, if necessary, withdraw their degrees. That ought to inhibit further misbehaviour as efficiently and thoroughly as our legal precautions prevent criminal behaviour within our society. Or is the situation a bit more complex?

Who does the cheating?

A first step is to identify and understand the type of people who are actually guilty of or prone to dishonest scientific behaviour. It is important to keep in mind that scientists do not belong to a different species of humans but con-

stitute the same variety of people with unique characters, strengths and weaknesses as in any other professional group. There will always be a few individuals with innate criminal energy, prone to any form of misconduct and misbehaviour. This type will certainly not be frightened by a memorandum on scientific integrity; however, as we have learnt from the past, their cheating sooner or later will rise to the surface. On the other end of the bell curve there are individuals with the highest moral and ethical standards, the Gandhis and Dalai Lamas of scientific research who need no guidelines or procedural rules. This leaves the large group between these two extremes, the people who aim for excellence in science, strive for the big breakthrough and are driven by a unique intellectual curiosity necessary to withstand the setbacks and frustrations in scientific research. Of course researchers also strive for recognition and admiration, a trait which to various degrees can be intermingled with pronounced competition for power, influence and money. Again, this is nothing unique to the world of science. This possibly represents an aggravating factor, but is not likely the cause for scientific misconduct. It rather appears as though the scientific environment, in addition to forming new Gandhis and Dalai Lamas, is equally capable of pushing a few individuals into another direction, despite the well-known regulations and legal consequences.

Science as a crowded highway

Quite possibly, scientists behave as irrationally as people do in anonymous crowds and when under pressure. Science today is somewhat like driving on a busy highway. Sitting in your fancy sports car, you might be in a hurry to get to your next meeting, you might be the type who enjoys any form of competition, so you increase your speed, test what your car can do, make it a race. You know very well that the speed limit is 120 kph, and that you might lose your license. But everyone is driving too fast – so it can't be too wrong. The flow of traffic keeps you moving, and, indeed, you would endanger others by driving only 80 kph or stepping on the brakes. And, after all, the speed limit is silly because your driving is so skilful. You tell yourself that you will reduce your speed and drive safely once you have reached your destination. You just need to pass the others right now on this very busy and very crowded highway before it morphs into a traffic jam and someone else wins the race.

Up or out

To a certain extent, the jammed highway depicts the situation we scientists are currently in: the fight for financial support for research projects and increasing numbers of excellent scientists applying for limited amounts of money. We feel pressure to publish our work in high-impact scientific journals and, ideally, have a major break-through every year that hopefully will facilitate further fund-raising and expand the research team. Competition rages among research groups worldwide. This is all good – as a matter of fact, it is excellent. We honestly need this competition to move science forward, to discover new principles and to understand the laws of nature. Journals ought to accept only the best data because that is what maintains motivation and alertness. What is not necessary, however, is an indiscriminate belief in a causal relationship between the number of publications in prestigious journals and professional qualifications. Of course there is a strong positive correlation, but there are well-known facts like “not-so-great-scientific-content-in-great-journal” and “great-scientific-content-in-not-so-great-journal” that distort this simple qualification ratio. And I believe this is one of the possible reasons why scientists cheat: there is a growing tolerance for speeding when moving from A to B, in getting tenure, the permanent position, or a professorship at a prestigious university. Under enough pressure, some individuals might do anything it takes to achieve what they have always wanted and dreamed of. And, if the situation is either up or out, break-through or break-down, you will speed up your car because others are also driving too fast – or even faster.

Getting the best

Some people may argue that the system regulates itself, that this is exactly the way the best candidates succeed: natural selection and survival of the fittest. The question is, however, the best and fittest at what? Who is screened in this selection process and how is the screening done? The candidates with the fanciest, newest or fastest cars or rather the rudest drivers? Universities need excellent scientists and excellent teachers but also excellent role models; this cannot always be found in one and the same person. A fair appointment and promotion system based on sophisticated qualification requirements is necessary. In addition, support and infrastructure could be provided according to potential and talent. Give the scientists their lab and let them do what they do best – research – and reduce their duties in administration, personnel management and teaching. These duties could be filled

by other qualified scientists who – beside their own research – have interest and ability in teaching, training and supervising students and who can take on management duties. This would at the same time facilitate brainstorming, the exchange of ideas and real teamwork, thus providing a more efficient way of getting from A to B – a type of car-sharing in scientific terms. Such a system would alleviate some of the pressure and many individuals could find ideal alternatives within science and research in academia. And, hopefully, scientists would be hired because someone has actually read – and not just counted – their publications.

Literature

- Baxt, William G. / Waeckerle, Joseph F. / Berlin, Jesse A. / Callahan, Michael L. (1998), Who Reviews the Reviewers? Feasibility of Using a Fictitious Manuscript to Evaluate Peer Reviewer Performance, *Annals of Emergency Medicine* 32, Issue 3, 310-317.
- Goodman, Neville W. (1994), Survey of Fulfilment of Criteria for Authorship in Published Medical Research, *British Medical Journal* 309, No. 6967, 1482.
- Paalman, Mark H. (2000), Scientific Misconduct: The Ultimate Negative Career Move, *The Anatomical Record Part B: The New Anatomist* 261, Issue 6, 219-220.
- Wilmshurst, Peter (1997), The Code of Silence, *The Lancet* 349, Issue 9051, 567-569.
- Kohlberg, Lawrence (1981), *The Philosophy of Moral Development: Moral Stages and the Idea of Justice (Essays on Moral Development, vol. I)*, San Francisco: Harper & Row.
- Titus, Sandra L. / Wells, James A. / Rhoades, Lawrence J. (2008), Repairing Research Integrity, *Nature* 453, Issue 7198, 980-982.

Authors

Sabine Kleinert studied medicine, trained as a paediatrician and specialised in Paediatric Cardiology. She is Senior Executive Editor at the medical journal *The Lancet* and Vice-Chair of the Committee on Publication Ethics.

Irene Knüsel is a neurobiologist working as group leader at the Institute of Pharmacology and Toxicology UZH.

Frank Rühli is head of Applied Anatomy at the Institute of Anatomy UZH and member of the Ethics Committee UZH.

Nikola Biller-Andorno is director of the Institute of Biomedical Ethics UZH and member of the Ethics Committee UZH.