

“Hints from Hans”: Texts of a column in the ICSA Bulletin

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This document contains the texts of twelve columns which I wrote in the period 2015 - 2021 for the Bulletin of the International Chinese Statistical Association (ICSA). I chose topics which I assumed to be of interest to new researchers and I would be happy if the columns find new readers also outside of the ICSA community when the material is more easily accessible.

I have corrected a few typos and unclear sentences. Moreover, I kept only links to websites which were still accessible in August 2025. Apart from this the texts are the same as when they appeared in the Bulletin. In particular I did not add additional content or references. You can access the original columns at <https://www.icsa.org/publications/icsa-bulletin/icsa-bulletin-archive/>. I thank the editor Chixiang Chen for the permission to put the columns on my webpage. And last, but not least I thank Marcel Wolbers for writing the column number 6 for me and letting me publish it also here.

1 Lemma 1 was false

This is the title of a short story by Helga Bunke, published under her maiden name, Helga Koenigsdorf. She was a mathematical statistician in the former German Democratic Republic who worked in the 1970's on general regression methods. Some of the statistics books to which she contributed were translated into English and published by Wiley in the 1980's. In 1978 she started a second career as a fiction writer with a collection of short stories, among them the one entitled “Lemma 1 was false”. In 1990 she gave up her academic career to be a full-time writer.

The main hero of that short story is Johanna Bock, a young ambitious PhD student in mathematics who attacks a famous long-standing open problem. She submits a thesis where she solves this problem using geometric methods, but unfortunately Lemma 1 on which all her subsequent lemmas and theorems rely is false. Neither her supervisor nor the two other examiners of her thesis notice it and write positive reports, but Johanna discovers it when she prepares for her defense. This is not only embarrassing for the supervisor and the other two examiners, but for the whole institute because they had mentioned the work on this problem prominently in their research proposals. They try to cover this in their yearly report with a vague statement, but Johanna insists that the failure is declared clearly. The story portrays the weak points of the academic system with intrigues, bureaucracies and lack of interest in true science in a satirical way. At the end, Johanna is transferred to an industry position. She does not object, but rather is excited about the chance to see how mathematical methods become useful for society. The story closes with the remark of her supervisor that in such a position “she would finally have a chance to do real damage”.

My aim in summarizing this short story is not to discourage ambitious young statisticians. If you do not dare to make mistakes from time to time, then you presumably do not develop bold new ideas either. However – as I know from my own experience – what seems to be a great

new idea often fails because a small, but crucial detail has been overlooked, like the error in Lemma 1 of Johanna Bock. And once your mind has been set to believe in an argument, you become blind for its weak spot. Hence you need to make big efforts to put into doubt what looks convincing and ask yourself the right questions. For this it is best if you find someone who is listening to your line of deductions with an open and critical attitude. In Johanna Bock's case, the biggest mistake was that her supervisor did not discuss with her about her ideas as they developed. He only listened to a presentation of Johanna in a seminar after she had given him the completed paper, and then read that paper when he had to write his report. In such moments, it is difficult to find mistakes because the speaker or author unconsciously makes the audience follow his/her way of thinking. So if you are supervising students, make sure that you meet them regularly and that you follow how they progress.

You might object that for more applied work in statistics, proofs are not as crucial as in pure mathematics. However, not only Lemma 1 can be false, there can also be a subtle bug in one of your auxiliary functions of your computer code. And it is not always true that a bug in your code makes the algorithm produce obviously wrong output. For example, if you use the true parameter as the starting value in a simulation study (something that you shouldn't do), you can get excellent results if you stop too early because of an error in the evaluation of the stopping criterion.

A second point raised by the story of Lemma 1 is about the formulations you choose when you submit a research proposal or write a report about what you have achieved for a funding agency or the university administration.

Finally, the example of Helga Koenigsdorf shows that even if you have followed a career path in some area for some time, you can change directions if you really think that a different path is the right one for you. So let us hope that Johanna became successful in her industry position by developing an ability to work in a team so that she finally did something useful for society as she had wished.

2 Scientific writing

As a reader of this column, your mother tongue is most presumably Chinese. This gives you a substantial disadvantage when you have to communicate results of your work or to write a research proposal. Besides reasons related to the content of a manuscript, also careless preparation or poor use of English often lead to rejection in the peer review process. This disadvantage is not only compared to native English speakers, but also to those with French, Spanish or German as mother tongue since these languages share many words and grammatical concepts with English.

Writing papers involves many phases. Typically, you start with a brainstorming phase where you collect all your ideas, search for additional literature, look for examples, check your proofs, consider which notation to use, decide about where to submit, etc. In this phase, you need not care about grammar or style – perfectionism is a danger that takes your attention away from the important things. In the next phase, you produce a coherent first draft. Now the logical flow of your arguments is important, and language is essential to help the reader see how the ideas and concepts connect. If this is clear to yourself, writing will become easier. Too much perfectionism is however still a danger that gets you stuck at a minor issue. The third phase is editing where feedback from colleagues or a mentor, self-criticism and perfectionism are important.

Nobody becomes a good writer overnight, you have to get training and help. Most universities have some scientific writing courses, and ASA usually offers such a course at the JSM. Check the program of the JSM in spring if you are interested.

In addition, there are many useful tools available on the web. Here is a partial and subjective list:

oxforddictionaries.com/ An authoritative and comprehensive English-English dictionary online. Includes a thesaurus (collection of synonyms).

www.ozdic.com/ A collocation dictionary of general English. If you are uncertain which preposition a word takes, which adjectives are frequently used with a certain noun, or which noun and verb combinations sound acceptable, this is the page to visit.

<https://hypcol.marutank.net> A search engine for finding example sentences from 811,761 English papers included in arXiv. If you enter a word or phrase it presents instances of its use, sorted by frequency of appearance. Use this to check collocations, especially for scientific terms.

<http://cn.linguee.com/> A useful resource for translating; the site presents translations in context, so you can see who uses which translation of your term and how.

<https://owl.english.purdue.edu/sitemap/> A large resource for general academic writing, including some good advice about punctuation, etc.

<http://www.oxfordlearnersdictionaries.com/wordlist/english/academic/> A list of most used words in academic texts that can help you to extend your vocabulary for scientific writing.

<https://www.msu.edu/~jdowell/135/transw.html> Gives an overview of transition words that connect one idea to the next.

If writing English is difficult for you, avoid the trap of plagiarism. When you find that someone else has explained an idea so much better than you ever could, or when you have spent a lot of time and effort for writing the introduction to your last paper, copying this material into your draft seems most efficient. However, this is not acceptable without the use of quotation marks and appropriate reference. If you just make some trivial modifications to hide that you copied this material, it becomes worse. Since many journals now use plagiarism detection software, the chance to be caught is high.

Another way to improve your writing skills is to read English also in your free time. Popular science books are particularly suitable since they use a style that is similar to academic writing and the authors often have a talent to explain concepts in simple language. Something will stick if you read regularly, and it can also be fun.

If you are despairing about the difficulties to write good English, you can console yourself that it is sufficient to explain rational concepts in a logical way. There is no need to use poetry or humour in a scientific paper. Still, also Asians are able to achieve even such higher levels of mastery of the English language, as illustrated by the following story about Okakura Kakuzō (1862 – 1913), a Japanese art scholar and writer. In 1903 he was visiting Boston together with three Japanese painters, and one day when they walked through the streets of Boston in their traditional clothes, he was asked by an American “What sort of ’nese are you people? Are you Chinese, or Japanese, or Javanese?”. Okakura replied “We are Japanese gentlemen. But what kind of ’key are you? Are you a Yankee, or a donkey, or a monkey?”

3 Peer review

Publications are essential in science. If you have discovered a new statistical method or proved results about the performance of such a method or obtained new insight about a scientific question based on an innovative statistical analysis, you want to publish your findings so that others can make use of it. In order to keep informed about new developments, to find out what is already known or maybe to find a related result that could provide an idea how to solve

your problem, you have to read what has been published. And in all cases of promotions, job applications or decisions about awards, an important factor is what a person has published.

Peer review has the goal to make sure that publications are well-written and contain results that are correct, substantial, relevant and do not duplicate what is already known. Peer review thus has an important gate-keeping function which makes a substantial contribution to the quality of publications. In the review process, editors, associate editors and anonymous referees judge whether the above criteria are satisfied and then make a decision whether a manuscript is accepted for publication or rejected. Often such a decision is conditional, that is the authors have to revise the manuscript taking the comments by the referees into account, usually followed by another round in the review process.

If you are a new researcher with little experience with the peer review process, it might be helpful to read the document “Peer Review: The Nuts and Bolts”, available at <https://senseaboutscience.org/activities/peer-review-the-nuts-and-bolts-2/>. I find this report really informative and can only recommend it.

The goals of peer review to improve the quality of publications and to facilitate progress in research are not controversial, but there is a growing concern that peer review fails to achieve these goals. Almost everybody tells stories about unfair treatment by editors, long delays until first decision and low quality referee reports showing misunderstanding, lack of knowledge of the topic or prejudice. On the other hand, there is also much dissatisfaction about publications of low quality papers in leading journals or unwillingness of authors to take critical comments by reviewers into account adequately, see for instance Vlada (2016) or Sommerville (2016).

The current system, what to do and what not to do:

- If you are asked to review a paper, make a quick, but honest assessment whether your knowledge about the topic, your research interests and time constraints allow you to provide an informative and timely report. Then reply within a week whether you accept or decline the request.
- Once you have accepted to review a paper, you have an obligation to study the manuscript and provide a report without long delays.
- Participating actively in the peer review process is part of your job as an academic. Providing an informative report of a submission can be a greater contribution to science than writing a paper which contains a minor progress on a previous publication of yours.
- Information or results that become available to you by reviewing a submission must be kept confidential until the paper has been published by the authors. If you want to consult with a colleague about a manuscript you have agreed to review you should ask for the consent of the editor or associate editor who handles this manuscript.
- Reviewing papers in case of conflicts of interests is unethical. Conflicts of interests exist if the author is a former student of you, a colleague at the same institution or a close and recent collaborator. If you are in doubt about a potential conflict of interest, declare it before you agree to review.
- Be aware and avoid potential biases.
- “Reject papers, not people” (Steve Stigler).
- Avoid asking for minor changes that are time-consuming for the authors and avoid bringing up new issues when reviewing a revision.
- As an author, submitting a manuscript that is still in the review process of one journal to a second journal is an absolute no-go. The same applies if two manuscripts are not

identical, but have a substantial overlap.

- Only persons who have made substantial contributions to the results of a manuscript may become authors.
- If your manuscript has been rejected, don't send immediately a protest to the editor. Read carefully what the reports say, wait a few days and discuss the case with a colleague if possible. If you still think that the reports show a basic misunderstanding or lack of objectivity from the referees, you can write a polite and concise appeal. In most cases, revising the manuscript in a way that addresses the concerns and the misunderstandings of the referees and then resubmitting to a different journal is usually the better option.

4 Time

Recently I visited an exhibition about Europe in the period 1400-1600. There I saw a drawing of a book wheel, invented by the military engineer Agostino Ramelli (1531 -1610). This invention allows the user to switch easily between several books open at the same time without standing up or risking to drop books to the floor. You can find illustrations and descriptions of this wheel at <https://en.wikipedia.org/wiki/Bookwheel>.

That this idea came up in the 16th century is no accident: The invention of book printing by Gutenberg had lead to a dramatic increase of available information, and Ramelli's book wheel is a technological idea to help his contemporaries to better digest this information. Nowadays we can access an enormous amount of information on the internet without standing up, we can open several documents at the same time on our screen and jump around in documents using links and hypertext. So something far better than the book wheel has become reality. Nonetheless we feel that we need help to digest the flood of information that has become available and separate what is relevant from junk. What has not increased at all during all the technological progress is the available amount of time. The day still has only 24 hours, and even if you work also during the night and 8 days a week, time is not enough for all the things you would like to do.

If you have the feeling that you could use your time more efficiently, there are time management techniques available. The first step for this is to analyse your current use of time by keeping a diary for at least one week where you enter accurately and in detail for which tasks you use your time and when and by whom you have been disturbed. This will help you to find out not only how you split your time between the different tasks, but also how realistic your estimates of the required time for specific tasks are, which time of the day you are most productive, when and for what you have wasted your time, whether you need to create more periods where you can work without disturbance, whether you took enough time for recreation, etc.

5 Working as an applied statistician: Hints from Marcel

Nobody can be an expert in everything, and it is important to profit from the experience and knowledge of others. Since my whole career has been in academia, my experience of working in an applied environment is limited. Therefore I have asked my former student Marcel Wolbers to write about the challenges and joys of working as an applied statistician. After completing his PhD in 2002, Marcel worked as a biostatistician at Roche Pharmaceuticals, the University Hospital of Basel and for almost eight years as Head of Biostatistics at the Oxford University Clinical Research Unit in Vietnam. Currently he is a Principal Statistical Scientist in the Methods, Collaboration and Outreach group at Roche Basel. Here is his contribution to my column:

Wikipedia provides a list of 22 fields of applications of statistics from "A" for actuarial science and astrostatistics, "B" for business analytics and biostatistics, to "S" for statistical signal

processing and statistical thermodynamics while not mentioning other areas, such as “Z” like zoology, where statistics is also widely used. After completing a university education with an MSc or PhD in statistics, the majority of graduates work as applied statisticians. It is a privilege of our profession that we can choose the field of application that we are most passionate about. For me, this field is biostatistics, as I have always had an interest in medical research and had contemplated studying medicine instead of mathematics and statistics before entering university.

Learning Each application area has its own toolset of preferred statistical methods that need to be learned. In biostatistics, longitudinal and survival models as well as special designs such as randomized trials and case control studies are frequently used. After my PhD, my expertise in these methods was very limited, but to collaborate with epidemiologists and medical doctors, I had to become an expert in these areas fast. I found that a strong foundation in statistical theory is really helpful to pick up new methods quickly. This foundation also allows to tailor statistical methods to the problem at hand which is often needed, but usually beyond the reach of your scientific collaborators. It should also be appreciated that applied problems come with their own sets of practical challenges, even if the theory is well understood. I have experienced this myself and subsequently saw it in my students when they first tackled an applied problem and learned that their theoretical knowledge was not enough. Finally, some substantive understanding of the applied research field is necessary, requiring continuous learning and interests outside of pure statistics.

Collaboration. Methodological papers in statistics are usually written by a single researcher or a small research team only. In contrast, in applied research, the statistician becomes a member of a much larger project team with members from a variety of backgrounds. In medical research, collaborators may include physicians, laboratory scientists, study managers, and data managers. Communication skills and the ability to explain statistical principles to non-statisticians and occasionally to enforce adherence to these principles are key in collaborative research projects. To remain humble and patient in difficult situations, it helps me to occasionally remind myself that while scientific collaborators may not be experts in statistics, almost all of them are experts in their own fields. Collaboration also means that the statistician is not in full control of the project and its timelines. One of my favorite projects during my time in Vietnam was a large randomized trial to investigate treatments against a deadly opportunistic infection affecting patients with HIV. We started to plan this trial almost immediately after my arrival in Vietnam but I performed the final analysis of the trial only after more than seven years, a few months before I left Vietnam again. Major milestones during the project were the successful award of a research grant to finance the trial, the completion of a four year recruitment period to randomize patients into the study, several safety interim analyses, the database lock and unblinding of the study after all patients had completed follow-up and the data had been cleaned and, finally, the statistical analysis and publication of the results. The statistician’s workload during such long-term projects is very variable over time and as there are often a dozen or more of such projects running in parallel, this requires some skills in planning ahead and project management.

Ethics of applied work. Collaborative projects will usually have a much larger budget and more direct consequences than methodological research. For example, the results of a clinical study can determine the future management and treatment of patients with a particular disease. This implies a high ethical bar that research studies must be appropriately designed, well-conducted to collect credible and high-quality data, correctly and reproducibly analyzed, and that the conclusions and limitations of the study are adequately communicated. Statisticians clearly play a major role in all of these tasks. Unfortunately, not all applied research satisfies these necessary high standards as documented in blogs such as the recommended “Bad science” blog by Ben Goldacre <http://www.badscience.net/> and a blog tracking publications which subsequently had to be retracted <http://retractionwatch.com>.

Applications as seeds for statistical theory. Applications have played an important role in the advancement of statistics throughout its history. For example, the current boom in high-dimensional statistics is largely driven by the rapid advances in genomics and related life science disciplines. On a much smaller scale, I have also found in my own work that applied research projects frequently raise problems for which established statistical methods either need to be adapted or do not exist at all. As an example, my own methodological research in competing risks (a branch of survival analysis) started when a clinical epidemiologist with whom I shared my office wanted to build a prognostic model involving competing risks based on a cardiovascular cohort study. However, when we discussed and investigated the problem, we found out that we had to develop some statistical methodology first before tackling the applied problem. I have always found the route from applications to theory very motivating because it was clear that if a solution to the methodological problem could be found, it would also help to solve an applied problem. This also implies that applied and theoretical statistics is not a dichotomy, but rather a continuous spectrum which allows all of us statisticians to find a place within that spectrum which fits our talents and ambitions best.

6 Writing/supervising a PhD thesis

Recently there was a case of alleged bullying of PhD students by a professor of the physics department here at ETH Zurich. It was discussed not only in local media, but also in Science and other places. The allegations said that the professor asked "superhuman commitment" from the students, e.g. being always reachable on weekends and participating in evening meetings that could go until midnight, and that she criticized students in a personal and inappropriate way, e.g. speaking endlessly about their weaknesses and failures. But there were also voices who said that the behavior of the professor was due to an extraordinary commitment for the students, wanting to make them succeed with the thesis and maximize their career chances in a field characterized by strong pressure and competition.

I do not intend to make here a judgement about this specific case, but rather give some general comments and advice about the relation between PhD students and their supervisor. Every question can be looked at from the viewpoint of the student or from the viewpoint of the supervisor, and I try to give answers for both.

What should a supervisor teach to PhD students? What can a PhD student learn from the supervisor?

In a nutshell, it is about how to do research. This is very different from teaching basic knowledge about a field in the classroom: It is more about asking the right questions and about how to get new ideas when the obvious approaches fail. This is hard to describe in writing. The old book "How to solve it" (Pólya, 1945) is in my view still one of the best attempts to do this, but a good cooperation between supervisor and student can achieve much more.

What are the most important qualities of a PhD student? What are the most important qualities of a PhD supervisor?

A good PhD student is disciplined and understands that success often occurs after many failed attempts. He or she makes meetings with the supervisor effective by preparing summaries of what has been achieved recently and where the problems currently are. He or she is open and understands that writing a thesis is the only way to find out whether he or she is suited for an academic career in research. Fortunately, in statistics a PhD has many other career options.

A good supervisor has a genuine concern for the development of the student and a commitment to make him or her succeed. This takes up a non-negligible amount of time. He or she is aware that each student is different and thus needs different types of support and that not every thesis can be a ground-breaking contribution. Also an average thesis has value.

How much work should a student put in his thesis? How much work can a supervisor expect from a student?

The most important requirement for a successful thesis is motivation and the strength not to give up despite unsuccessful and sometimes frustrating attempts to find a proof for a conjecture or an algorithm that is fast and stable. Research cannot be limited to fixed working hours. A researcher has to give up sometimes other activities that seem more pleasant than making another attempt at to solve an open problem. But sitting endless hours in front of one's notes or the computer can also block new ideas. Jacques Dubochet, one of the winners of the 2017 Nobel prize in chemistry, said in a recent interview he insisted that people in his working group take holidays and that they go for a walk or jogging to boost their creativity.

How to avoid dropouts from a PhD program?

The most important measure against dropouts is a careful handling of the admission process. The first year of a PhD program should provide a solid basis that makes dropouts at a later stage rare. If there is no qualifying exam at the end of the first year, the supervisor should provide a substantial amount of training and interaction during this year. This allows to make an informed judgement whether the student is able to complete a thesis.

A good atmosphere in the working group of the supervisor is an important condition for the students to grow and realize their potential. Joint social activities in the group can help a lot.

What can be done if progress is slower than expected?

As a student avoid panicking. Phases where you have the impression of bumping again and again into the same wall are normal. Try to explain to someone else than your supervisor what the problem is. As a supervisor, think what the student needs most. Grass does not grow faster by pulling, but by providing water, light and nutrients.

What other problems can occur?

The supervisor is in a much more powerful position than the student. Not acceptable are bullying and all kinds of abuse; the use of results obtained by the student without proper credit; and to use a student for tasks that are not related to the thesis nor to obligations from the source of financial support. If a student experiences such a thing, he or she should take the courage to bring this forward to a person of trust. All departments and universities should have appointed such persons.

7 Mathematicians in turbulent times

Recently I attended a seminar entitled “Tales of our Forefathers” by Barry Simon, an analyst and mathematical physicist from CalTech. Slides and videos of this talk can be found on the web. It contains stories and anecdotes about many famous mathematicians. In this column I want to focus on the fate of two mathematicians from Russia, Nikolai Luzin (1883-1950) and Mischa Cotlar (1913-2007). Their lives were affected by political circumstances of the time, and I think their fate should be remembered. In preparing this column, I used in addition to the slides of Barry Simon the book Demidov and Levshin (2016) and the bibliographical material on Cotlar in Sadosky (1990).

Luzin worked on analysis and set theory. A theorem that carries his name says informally that measurable functions are nearly continuous although they can be discontinuous at every point. In the 1920's he was the leader of an extremely productive research school at Moscow State University and the supervisor of a number of PhD students who became later outstanding mathematicians, among them Kolmogorov and Khinchin who made fundamental contributions to probability theory. In 1929 he was elected as full member of the USSR Academy of Sciences

in the Department of Pure Mathematics. In 1936, when the Great Terror of Stalin began, an article appeared in the Pravda, the official newspaper of the communist party, entitled “Enemies wearing a Soviet Mask”. It accused Luzin among other things of “academic dishonesty with a hidden hatred of everything Soviet”, claiming that he “is a scion of the infamous tsarist ‘Moscow Mathematical School’ ”.

As a result of this article, a special commission of the Academy of Sciences was formed to investigate Luzin’s case and to make a recommendation to higher authorities. At the beginning, the deliberations of this committee were strongly hostile against Luzin, but after a surprising turn in the end a rather mild resolution was adapted, recommending to remove Luzin from the chairmanship of the Mathematical Group and the Mathematical Qualifications Committee, but not to expell him from the Academy of Sciences. If Luzin had been expelled from the Academy, the state security organs would have put him on trial and presumably sent him to a forced labor camp with little chance of survival.

During the investigations of the special commission, many of Luzin’s former students, including Kolmogorov, testified against him. Why did they do this? They may have feared to come under attack next, as one of the accusations against Luzin, the publication of his main results abroad, was also true in their case. Another explanation says that Luzin was a difficult person, not leaving his talented students enough room to develop their own research agendas. Thus the accusations were an opportunity to cut Luzin’s influence in the Moscow mathematical community. In the case of Kolmogorov, there is also a theory that he was under pressure by the state authorities because of an alleged homosexual relation with Alexandrov, another student of Luzin who became world famous for his work in topology.

The reasons for the rather mild sanctions against Luzin are also not clear. It could be that the abstract nature of mathematics and the kind of accusations were considered not to be suitable for a show trial. It could also be that the higher authorities considered mathematics along with physics to be important for the state and therefore didn’t want to give too much influence party member who had started the case with the Pravda article since they were not strong mathematicians.

Next I turn to Mischa Cotlar who was four years old at the time of the Russian revolution. Because his father was considered a member of the bourgeois class, he was not allowed to attend school. He learned to play the piano and some mathematics from his father. In 1928 the family emigrated to Uruguay where he had to work as a night-time pianist in a harbor bar to help the family survive. His life was marked by a number of fortunate encounters with people who recognized his talent for mathematics and supported him, but also by less fortunate encounters with bureaucrats who put obstacles in his way because he had no school diploma or degree. In 1935 he moved to Buenos Aires where he started to do reseach in ergodic theory and harmonic analysis. But he was not able to obtain a PhD until 1953, 17 years after the publication of his first research paper, from the University of Chicago. He returned to Argentina although the political situation there was difficult for him. He could not get a position at the University of Buenos Aires because he wasn’t a member of the ruling party. After the military coup in 1966, he went into exile to the US, France and finally Venezuela. Despite his hard life and despite suffering from unfair discriminations, the people who knew him describe him as an exceptionally kind and modest person.

Reading the stories of these two mathematicians, I realized how fortunate I was in my life, having had the privilege of a good education, never experiencing hunger or fear of violence and being able to follow my interests in research without any outside pressure. But I also asked myself: Would I have managed not to become bitter if I had to overcome similar obstacles as Cotlar? What would I have done if I would have been forced by authorities to testify against a colleague in an inquiry based on unjust accusations? Would I have been able not to get influenced by

earlier personal conflicts with this colleague? And finally there are the bigger questions, e.g. What can be done to prevent governments from using intimidation and violence against citizens with different ideas and beliefs? How can we support rational, fair and peaceful approaches to solve conflicts? Cotlar was active in the Movement for Social Responsibility of Scientists which he presumably saw as an answer to such questions.

8 Impact and Citations

Research can be a frustrating and lonely enterprise when all your attempts to prove a conjecture lead nowhere, or when you receive reports by referees who didn't understand the new idea of your paper, or when nobody among your colleagues shows any interest in what you are trying to do. So why do you not give up? One reason is extrinsic: Successful research is an essential condition to obtain a position, a promotion or a salary rise. But there is also the pure intrinsic motivation, "the pleasure of finding things out" which is the title of a book by Richard Feynman (1999). A third reason is presumably that we would like to make an impact on science and – more ambitiously – on society. This third motivation interacts with the two others: Success of research is usually judged by its impact, and the pleasure of finding things out is much higher if what you have found is also relevant for others.

The number of citations of a researcher is an indicator for her or his impact that is better than the number of publications. The main reason for using citations in a scientific work is to distinguish between your own work and that of others, thereby respecting intellectual property. If you cite someone else's work, you acknowledge that this work has had an impact on your research, because it has been useful or even essential for you to arrive at your own result or conclusion. However, the number of citations as an impact measure has its shortcomings too. For instance, the cited work is often not the origin of the idea or result that was used, but it is more easily accessible or understandable than the original source. This means that someone with highly original ideas that are difficult to understand may not get as many citations as he or she deserves. This is similar to "Stigler's Law of eponymy" (due Robert K. Merton and maybe others) which states that no scientific discovery is named after the person who discovered it. Often a citation also serves to convince the reader of the importance of the topic considered. In that case, the cited author is typically someone well known with a high reputation, but not necessarily the one who has made the most essential contribution. In other cases the cited work contains a somewhat different approach to the same problem and the reason for citing it is that it allows the author to show the superiority of the new method. Finally one should not forget that some authors find ways to manipulate citation statistics to their advantage, e.g. by citing earlier work of their own or of their network even though it has little relevance for the paper on hand, or by requiring citations of their own work when refereeing someone else's manuscript.

In order to really understand what a citation means one has to look at the paper where this citation has been made. The quality of research of a scientist or a whole department should therefore not be evaluated on the basis of citation statistics alone. Reducing citation statistics to a single index is even worse. A serious evaluation of someone's research requires that one looks at some of his or her papers. I think it is therefore a good idea to list your five best or most important papers (which need not be your most cited ones) on your cv.

For the same reason, you should not base your intrinsic motivation for research too much on your own citation statistics. Progress in research is not only achieved by singular contributions of a few geniuses, but also by a complex web of interactions between many different contributions. This has been noticed already in the 18th century by Georg Christoph Lichtenberg (1742-1799), a German physicist and satirist. Today he is best known for his aphorisms, concise and witty philosophical statements. He wrote once "If I hadn't written this book, then in 1000 years from today between 6 and 7 pm people in many German towns would speak about entirely different

things than those they will actually be speaking about.” Surely, he didn’t expect that people in a 1000 years in Germany would still be reading and discussing this one book, but rather that everything that is published continues to have some unpredictable indirect effect in the future, even if it is completely forgotten in a short time – a bit like the famous butterfly in Brazil of Ed Lorenz that sets off a tornado in the US by flapping his wings.

Moreover, you can also make an impact by other things than your publications. The guidance and feedback you give to your students, the questions you ask after a seminar talk, the suggestions you give in a referee report all can have a positive effect on the scientific progress. Obviously, by the same means you can also have a negative impact by missing to see the value of an important, but badly described idea. To give an example of a positive impact I received in my own career, my most cited paper that introduced the block bootstrap was triggered by a question from Colin Mallows at Bell Labs. I had told him about my earlier work on defining an influence function for time series data. He then remarked that the influence function has a close relation to the jackknife and that I therefore should look at the jackknife for time series.

I hope that you experience in your career not only the pleasure of finding things out, but also the pleasure of sharing your ideas with others. It will have a positive impact even though it is not always measurable.

A more detailed discussion about the use and misuse of citation statistics can be found in Adler et al. (2009).

9 Travel

There are many reasons for an academic to travel: Large and small conferences, seminars, short courses and the like are ideal places to present your recent and ongoing work to an interested audience, to learn early about the work of others and about exciting new developments, to give and to obtain feedback, to start new collaborations and to extend your personal network. At a later stage in their career, many people travel also for administrative purposes, e.g. to participate in evaluation, planning and hiring committees.

During my career, I have traveled to many places, both work-related and personal. Fortunately, ETH Zurich allowed me to combine these two types of trips which enriched me not only in my research, but also in my interests in culture and nature. Usually I have limited the number of intercontinental trips to one or two per year and trips within Europe to less than five per year. I profited a lot, not only from my own travel, but also from people coming to visit ETH Zurich. I hope that others also have profited by my contributions, especially when I taught a short course somewhere.

Ideally, everybody gains from face-to-face contacts made possible by traveling: Learning about new developments by attending well prepared talks and having the opportunity to ask questions is usually more efficient than looking at the slides, a video or a preprint. Personal interactions are much more enjoyable and informative than email exchanges or Skype calls because of lack of time pressure and the presence of non-verbal communication. However, there are also non-negligible material and immaterial costs from travel. Transport and accomodation can be costly and funds to cover them are not always available. This is especially an issue for scientists working outside of Europe and North America where most of the conferences take place. However, many societies and some conferences offer support; IMS has for instance travel awards for students and young researchers. A second type of cost is stress due to preparation of lectures or a poster, making arrangements for teaching obligations at home during the absence, jetlag, delayed flights or stomach problems. Then there are the personal costs if you have a partner or children, and finally travel is a significant source of greenhouse gas emissions. A study estimates that business trips account for more than half of ETH Zurich’s green house gas emissions. There is even a small,

but growing group of scientists who do not fly at all or fly less, see <https://noflyclimatesci.org/>. A document of ETH Zurich entitled “Stay grounded, keep connected” states as its goal “to reconcile world-class research and teaching with more sustainable travel behaviour without compromising the career chances of young scientists.”

So what should you do? An obvious advice is to carefully and consciously select which events to attend. As a young researcher try to assess objectively what you will gain from attending a conference or a workshop and what the costs due to stress are. As commitments and registrations for conferences are usually made several months in advance, one tends to be overly optimistic concerning the latter. So talk about your plans with your partner early and also ask a senior colleague about the advantages and disadvantages of the conferences you consider. As a senior researcher you often find that you are getting many invitations to conferences and seminars. In that case you cannot avoid to occasionally decline, but in this decision you should also think about giving something back to younger people out of gratitude for what you obtained earlier in your career.

If traveling is difficult for you because of family, a solution can be a longer research visit of several weeks or a whole sabbatical where the family can join you. This will allow you not only to obtain new stimuli and ideas for research, but also to experience a different culture and way of life.

The next important thing is to make the most out of any trip: Carefully look at the schedule of talks at a conference and attend other sessions than the one where you are talking. Be driven by curiosity and occasionally listen also to a talk on a subject you are not working on. Don’t hesitate to ask questions after the talks or during the breaks. Attend also the social events as they are an opportunity to meet new people. Often there is a possibility to combine two visits, e.g. a young researchers meeting that takes place before or after the main meeting or a satellite meeting to a large conference.

For more views on the topic of travel and online conferences see Speed (2013) or Reshef et al. (2020).

10 Careers and personal relationships

John W. Tukey (1915 - 2000), one of the leading figures in statistics, was married to Elizabeth Rapp Tukey. In Section 14.1 of his memorial article (Brillinger, 2002) we read “She was absolutely basic to John’s life from when they met and until her death One of Elizabeth’s remarks tells part of the story behind JWT’s contributions: *As the wife of another dedicated workaholic I understand the selfless love and devotion, accommodation and deprivation required to ‘keep them on the road’.*” How important she was to John Tukey shows also in his eulogy on Elizabeth’s death where he said “One is so much less than two”.

In an interview published by a Swiss newspaper in 2020, Michael Hengartner, a molecular biologist and president of ETH board (the governing body of ETH Zurich, EPF Lausanne and 4 research institutes), answered the question: “You raised six children and had a career at the same time. How did you manage this ?” as follows: “With little sleep, very little. There are days where I drink seven cups of coffee until noon. ... Seriously: the biggest credit for this goes to my wife. If someone deserves admiration, then it is her.”

These two examples illustrate that behind many male scientists there is a woman who gives him emotional support and takes a large share of household care and responsibility of raising children. It would be interesting to hear also the story from the wives in these two examples, but I only have the following pieces of information. According to Brillinger (2002), Elizabeth Tukey was “Personnel Director at Educational Testing Service in Princeton when she and John met”. Later, “she collected 18th and 19th century American furniture ... and was First Chair of

the Princeton Township Historic Preservation Commission. She was ill for several years at the end. John cooked for her and dutifully nursed her until he became too tired at which point a care provider was hired.” Denise Hengartner, the wife of Michael Hengartner, is also a biologist, working at the University of Zurich. She said in another interview “Michael is much involved with the homework of the children. And he takes care of the garden. For us it is right.”

I have no reason to doubt that in many cases such an arrangement is right for both and allows both to have a fulfilling life. John Tukey’s words work also the other way round: Two is so much more than one. But on reading about these two examples, I asked myself: How many female scientists have a partner who provides emotional and practical support to them? Can two individuals with equally high ambitions in a competitive academic or business environment have a relation where both take equal shares of obligations and responsibilities and both sometimes forgo an opportunity for the benefit of the other? Is it true that “Professionally ambitious women really only have two options when it comes to their personal partners — a super-supportive partner or no partner at all”, as Avivah Wittenberg-Cox wrote in a Harvard Business Review article in 2017?

The answers to such question are complex. Much has to do with stereotypes about the character and the role of women and men in society. These stereotypes say that men are outward looking, preferring competition over cooperation and compromises whereas women care more about harmony in their environments and thus are more eager to give in when conflicts arise. But of course such stereotypes are harmful for both men and women as they limit the possibilities for individuals to fully develop their own potential and abilities, trying to fulfill external expectations instead. Changing such stereotypes is a challenging process as they are often unconscious and have been formed during childhood.

The statement above by Wittenberg-Cox might lead women to abandon their plans for an academic career as they judge the chances to find a super-supportive partner to be low. As an older man I don’t know what advice I could give in this case except to express and discuss mutual professional goals and the support expected from the partner at an early stage of a relation. An especially difficult issue is whether to have children and - in case of a positive answer - how to share the responsibilities for raising them.

Men in a dual career relation have to meet the challenging task to remain open to compromises that affect their career when external pressures arise. Often there is a large gap how you see your share of obligations and your support given to your partner and how your partner sees it. Listening attentively without immediately defending yourself is important. An example of how men can take unfair advantage of opportunities is a study I read about some years ago in the New York Times: There the effect of a gender-neutral policy extending the tenure period in case of a child birth for both parents was investigated, and it was found that many males used this to boost their publication list whereas no such rise was visible among the females in the study.

Men who are tenured and have the privilege to live in a fulfilling relationship can (and should) also contribute to making more women succeed in an academic career. Examples are to advance more flexible and family-friendly working arrangements, to create a welcoming environment in their group and in the department or to support women at all stages in the career, either directly by supervising PhD students and postdocs or when evaluating research projects or writing letters for candidates under consideration for tenure. This does not mean to apply different criteria depending on the gender, but to avoid being influenced by unconscious biases against women. Such biases are more common than one might expect. For instance, Moss-Racusin et al. (2012) report an experiment showing that staff in a science faculty rated male applicants for a laboratory manager role as more competent than equally qualified female candidates.

11 Timely or trustworthy ?

The Covid-19 pandemic has dominated our private and professional life during the last year, and I begin this column with a related story. On January 27 2020, Camilla Rothe examined the first patient in Germany who was tested positive for Covid-19. The only way he could have been infected was through a colleague from China who had come to Munich for a business meeting. This colleague didn't feel sick during her stay in Germany, except for a little fatigue that could be attributed to jet lag, but she was tested positive after her return to China. At that time it was believed that only patients with strong symptoms could spread Covid-19, as in the case of the Sars virus in 2002. Camilla Rothe and her boss realized the importance of their observation. Three days later they sent a short report to The New England Journal of Medicine that was published online immediately (Rothe et. al., 2020). Since the report contradicted the generally recognized opinion of most experts, it became a major political issue. Her observation was declared to be flawed and health officials in many countries continued to state that spreading without symptoms was extremely rare and thus irrelevant. It took at least two months until the danger of symptomless transmission was widely accepted and measures like recommending to wear masks and avoiding crowds were adopted. It is not clear how much the spread of the epidemic would have changed if the message of Camilla Rothe had been heard earlier.

However scrutiny and scepticism are essential parts of the scientific process. This is exemplified in my next story. In September 2011, a group of physicists announced in a preprint that they had observed neutrinos traveling faster than the speed of light, thus violating special relativity theory. The reported p-value was $2 \cdot 10^{-9}$. The experiment had taken place six months earlier and during this period, the group had checked the details of the experiment and had found no instrumental error. Once published, the result caused a stir not only in the physics community, but also in the mass media. Among physicists scepticism was prevalent as the theory of special relativity had been confirmed in several experiments before and had thus a much stronger basis than the belief that only patients with clear symptoms could spread the Covid-19 virus. The story ended in spring 2012 with the announcement that two equipment errors had been found, and after correcting for these the speed of neutrinos was no longer higher than the speed of light. See https://en.wikipedia.org/wiki/2011_OPERA_faster-than-light_neutrino_anomaly for more details.

These two stories exemplify a dilemma of science: On the one hand, if an observation or an experiment has potentially serious consequences for human beings or shakes a widely believed dogma, it is important that this result is shared in a timely fashion at least with the scientific community, and possibly also with the general public. On the other hand, if scientists contradict each other continuously, their explanations change frequently and their predicted catastrophic outcomes repeatedly do not happen, the trust in science is undermined. In the current pandemic this is a problem when politicians ask for advice which measures are most efficient to slow down the spread of the virus and least harmful for the economy.

As it is too early to draw conclusions about the value of the many scientific contributions for the handling of the pandemic (except the development of tests and vaccines), let me finish with the story of the discovery of the ozone hole as it is maybe less familiar to younger readers. The ozone layer in the stratosphere protects the surface of the earth from ultraviolet radiation which is the main cause of skin cancer in humans. In the seventies a decrease in the ozone layer was observed, and as the main cause the release of chlorofluorocarbons (CFC) was identified. CFC's are used in refrigerators, air conditioners, foams and as aerosol propellants. This led to a ban of CFC in aerosol sprays in a few countries, but observations towards the end of the seventies showed the ozone decrease to be much smaller than what had been predicted. Thus the early warnings of scientists were considered as exaggerated and the decisions to reduce CFC emissions as premature. But then in 1985 appeared an article in Nature showing a "large seasonal disappearance of ozone ... over the antarctic", the ozone hole, that had started already in 1976.

This hole had been observed by a single ground station, and the long time between discovery and publication was due to delayed data processing and to an extremely cautious leader of the project who wanted to exclude instrumental and other errors. He was also concerned that NASA who had a satellite to monitor the ozone layer in orbit had not discovered the hole. A popular story says that this was due to an automatic rejection of these values as outliers by NASA, but it seems that the values were flagged, but not rejected because nobody took the effort to look at the huge amount of data stored on magnetic tapes. The ozone hole was confirmed by NASA in 1986, and the ensuing political pressure led in 1987 to the signing of the Montreal protocol banning the use of CFC by 43 countries. See Maureen (2004) for more details. In this example, the early scientific warnings about an environmental problem turned out to be justified although the early models made poor predictions and the true extent of the problem was discovered late because of insufficient measuring efforts and an extremely cautious scientist.

I hope these stories help to make you aware of some of the issues in the communication of scientific evidence and uncertainty. For more thoughts and advice I recommend Blastland (2020).

12 Teaching

We all started as pupils and students and then at some point switched to the role of a teacher, either as teaching assistant or as professor. So you know in principle both sides of the coin and can build on your own experience: Avoid mistakes that some of your teachers made and follow the example of the good teachers you encountered. However, all human beings are different. A teacher who was ideal for you maybe failed to connect with other students, and you cannot emulate easily someone else's teaching style, ignoring your personal strengths and weaknesses.

In this column I want to draw your attention to some of the issues in teaching, discussing why, what and how to teach and how to assess the achievements of your teaching efforts. In addition to my own experience it is based on chapters from Halmos (1985) and on Blum (2020).

The obvious answer of why to teach is that in academics most jobs require you to teach. Beyond that, teaching is a chance to pass on your insight, experience, intuition and enthusiasm about the subject to younger people. However, not everybody is a born teacher, and teaching requires in addition to knowing the subject well also other abilities, e.g. to see that something which is obvious to you can be a stumbling block for others. In order to become a good teacher you have to develop an intuition for different ways of thinking and to anticipate difficulties and misunderstandings of your students. This requires time and effort, but as in research you shouldn't give up when you don't succeed immediately.

I had some experiences with students that seemed to expect their teacher to be an entertainer or an animal tamer rather than a scholar. If this happens to you, meet with a delegation of students, listen to their complaints without immediately defending yourself and then decide what you can change without giving up your convictions about what you think they need to learn. Another option is to ask an experienced colleague whom you trust to sit in your class and give you feedback.

The answer to what to teach is more complex, as it includes not only knowledge and skills, but also attitudes. Knowledge means things like formal definitions, rules and methods, theorems and proofs; examples of skills are communication or the use of statistical software, and the category of attitudes contains for instance motivation, curiosity, perseverance, creativity, work ethics or critical thinking. The art of teaching is to transmit not only knowledge and skills, but to develop at the same time also such attitudes.

Turning to the question of how to teach, lecturing is the most common and easiest way, but it has also been called sarcastically the "best way to get information from teacher's notebook to student's notebook without touching the student's mind" (George Leonard, 1968). You have to

find ways to touch the mind of your students, to get them involved beyond passive listening, to encourage them to ask questions and to challenge them with problems at the right level of difficulty.

In discussions about teaching methods, often the quote "I hear, I forget; I see, I remember; I do, I understand" is mentioned which is attributed to Confucius, although this is apparently not correct according to <https://english.stackexchange.com/questions/226886/origin-of-i-hear-and-i-forget-i-see-and-i-remember-i-do-and-i-understand>. That visual communication is usually more effective than verbal is also reflected in the statement "A picture is worth a thousand words", but I believe John Tukey once said something like "100 words are needed to fully understand the meaning of a picture". And E. Dijkstra, a pioneer of computer science, said "A picture may be worth a thousand words, a formula is worth a thousand pictures" although formulae need maybe even more words than pictures to be understandable. To me it makes no sense to create a hierarchy between pictures, words and formulae, or between oral and written communication. They should all be used in teaching because what is best depends also a lot on the student's personality and the level of understanding. And "I do" should be interpreted as active involvement and reflection instead of passive reception which is possible also while listening and looking.

Exams and grades are the most common way to assess what the students have learned which is relevant not only for the student, but also for the teacher whose aim is to make his teaching more effective. In the foreword to Bloom (2020), Alfie Kohn writes "... research showing that grading has three predictable effects – less interest in learning, a preference for easier tasks, and shallower learning". As an example of these undesirable effects let me mention the story of one of my colleagues who required that students can not only state, but also prove the main theorems in the oral exam. As proofs are often long, he was satisfied when a student described the first steps of it correctly. But this had the effect that students learned only the first steps of the proofs by heart!

Alternatives to grades range from descriptive labels over individual narrative judgements and feedback to letting the students propose or even decide their grade. If you are sceptical if such alternatives work in a conventional system, take a look at Blum (2020).

Much more could be said about teaching than can fit in a single column. As I have emphasized here the importance of active involvement and critical reflection, I end with a few questions instead of a summary: Who was the best teacher you had and what made him or her stand out? With which points in this column do you disagree? Can you imagine yourself teaching a course where students decide themselves which grade they should get?

13 A dispute in science – and a goodbye

As traveling abroad was complicated last summer, I went hiking for a week in a mountain area very close to Zurich where I live. During that trip I visited the Unesco natural world heritage site Sardona area, <https://unesco-sardona.ch/en/homepage>. There phenomena associated with the formation of mountain ranges and plate tectonics are visible also for lay persons. The most striking phenomenon there is the so-called "Glarus thrust", sharp straight lines separating two different types of rock over several kilometers. More than 200 years ago the attention of scientists was first drawn to these rock lines when they realised that the rock above the line is about 200 million years older than the one below. This contradicted the then current theory that younger rock layers are always above older ones. To explain this finding two theories were developed: The "thrust" theory postulated that the older rock was pushed from below over the younger rock. However, forces which could achieve such a gigantic move were unknown. So the geologist who had come up with this postulate concluded "Nobody would believe me, they would take me for a fool". So the "folding" theory postulating two rock folds which tilted and

enclosed a layer of younger rock became dominant. Albert Heim, a leading geologist in the second half of the 19th century strongly publicized the “folding” theory, ridiculing scientists who criticized it. But after two decades Heim acknowledged in 1903 that he had been mistaken and that the “thrust” theory was correct. The force causing this thrust is the collision between the African and European plate. I don’t know how established the idea of such a collision was at the beginning of the last century: Alfred Wegener published his theory of continental drift in 1912 and it was not fully accepted until the 1960s.

There are several reasons why I tell this story in my column. First I find it fascinating how science can unfold the history of the earth which involves time scales and processes beyond ordinary human conception. Although it does not seem that statistics has played a role in this dispute, there are many connections between statistics and earth science. Let me mention only two examples: Harold Jeffreys (1891 - 1981), known for Jeffreys’ prior, was not only a Bayesian statistician, but in the first place a geophysicist. As a side remark, he refused to accept the continental drift theory until his death, but he is nevertheless considered to have been “a great scientist who has left a lasting memorial of achievements in the study of the structure and dynamics of this planet” (Hudson and Smith, 1993). Geoffrey Watson (1921-1998), known for the Durbin-Watson statistic and the Nadaraya-Watson kernel estimator, did fundamental work on directional statistics which he applied among other things to support Wegener’s theory of continental drift. More information about Watson can be found in Beran and Fisher (1998).

The second reason is that the story nicely illustrates how errors in science arise and how long it can take until they are corrected. Disputes among scientists unfortunately are not always carried out in a fair and objective manner. Apparently, Albert Heim had a dominant personality which must have made it difficult for him to acknowledge that he was wrong. But to his credit he did it fully and without excuses, supporting afterwards the correct explanation wholeheartedly. In statistics, there are also disputes and controversies which cannot be resolved by simply checking a mathematical proof. Among them I consider currently: Which concepts should be used to complement or replace p-values?, Which methods are best suited for causal inference and what are their limits?, or What is the best explanation of the apparent success of deep neural networks? I hope some light will be shed on these questions in the coming years and people will be willing to reconsider their convictions when this happens.

With this piece I say goodbye to the readers of the ICSA Bulletin. I have written twelve times on various topics myself and my former student Marcel Wolber wrote one on applied statistics. I often gave historical information because I like history since I was a child (I even considered studying archeology instead of mathematics) and because I believe that history can give a human touch to our abstract subject. My first column was a fictitious story (“Lemma 1 was false”) about a mistake in a PhD thesis overlooked by the supervisor and the other examiners. So my last column complements the first one, pointing out that not only PhD students make mistakes, but also full professors with a big reputation. Moreover, there is an indirect connection of the story here with my earlier piece on gender (“Careers and personal relationships”): Albert Heim’s wife, Marie Heim-Vögtlin, was the first woman in Switzerland to become a medical doctor. She had to fight to be admitted to university and to be allowed to practice medicine in Zurich. At that time, as a married woman she needed by law also the permission from her husband to work. Fortunately he granted it.

The title of this column was “Hints from Hans”. I hope this title or my writings didn’t give you the impression that I am a wise old man who knows everything and whose views and opinions are always correct. I would be happy if you enjoyed some of my stories and if I could nudge you to think about some issue you didn’t pay close attention to before. I wish you a successful and fulfilling career in statistics.

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