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Professional ethics and scholarly communication

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Introduction: a short history of scholarly reading and writing

Science (or, more generally, the procurement, propagation and dissemination of knowledge)[1] has not always been a profession based on the transmission of written materials. On the contrary, when Western science originated in ancient Greece some twenty-five centuries ago, dissemination usually took the form of verbal exchange and personal tuition (Zwart 2001). In the context of schools, scientists would share their (more or less secret) knowledge with a limited number of pupils by means of immediate verbal interaction. In those days, the transmission of knowledge was not a writing profession. The use of texts implied the risk of losing control over one's ideas. Moreover, the use of textual information was not really necessary. The emphasis was on principles and method. The teachings conveyed by teachers to their disciples basically consisted of a limited set of ideas that could be transmitted and elucidated orally, while brief expositions of principles would alternate with sessions of exercise. This applied to disciplines like philosophy and mathematics, and more generally to the scientific practices of investigation that emerged within the ancient schools of Physicists and Pythagoreans.

The situation changed, however, when a new generation of teachers, the Sophists, stepped forward. They initiated a different kind of investigation called 'history' (Zeller 1980). Besides history in the present sense, this new branch of learning included ethnographical, sociological and geographical studies as well. A different kind of knowledge was produced, namely *factual* knowledge. And the wealth of materials collected by generations of 'historians' had to be filed somehow, in order to prevent their loss, and thus the use of texts became important. Sophists took great care, moreover, in developing their rhetorical skills, both as orators and as writers. This was due to the fact that for Sophists knowledge was a commodity to sell and the way it was presented affected in a substantial way its (practical) relevance. Some of these 'stylistics of knowledge' can still be found in the way modern scientists disseminate their results.

The significant difference between philosophy in a strict sense (as the 'unwritten' science of principles) and sophistry (as the art of producing elegant prose) is still noticeable in the case of Plato. While in his written dialogues he entered into competition with the Sophists, notably in terms of literary style and narrative composition, he abstained from the use of textual materials as a teacher in the context of his own school (Wippern 1972). This had to do with the 'propagandistic' character of his dialogues. They were aimed at publicly refuting the Sophists' positions. Indeed,

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the difference between these two scholarly styles is still noticeable today, even if the motivations of modern scholars and ancient philosophers may not always coincide. Prominent scientists still have the habit of practicing two genres. On the one hand, they will write compact articles directed at a limited number of fellow experts. Usually, this part of their output is utterly unreadable and incomprehensible for lay audiences. But every now and then they will have recourse to plain and elegant prose as well, in order to produce pieces of text meant to address much broader audiences. A very well-known contemporary example is physicist Stephen Hawking. His *Brief History of Time* uses a jargon and a style of argumentation that would probably be totally unacceptable if submitted to a scientific publisher, but as an educational text it has substantial merit. It succeeded, for example, in fascinating lay audiences with mind-boggling concepts like evaporating black holes and other exotic objects. Also among philosophers, important examples of this double writing practice can be given. Immanuel Kant, for example, had been a successful writer in his youth, addressing relatively broad audiences, using elegant and quite readable prose during the first part of his career, before switching to the abstract, reader-unfriendly, 'academic' and critical work later in life – the work that *really* made him famous (Vorländer 1977) – although he continued to write newspaper articles (on issues such as 'Enlightenment') as well. Likewise, Leibniz produced a series of essays on popular subjects during his lifetime, written for audiences interested in philosophy and science but lacking systematic training, while in the meanwhile he accumulated the bulk of his scientific output in unpublished notes and manuscripts. We will come back to this interesting phenomenon later on.

What is important is that 'real' scientists and scholars, from the early onset, took a rather ambivalent stance towards writing as such, and to publishing in particular. The knowledge of principles directly appeals to the pupil's own intellectual faculties. It is impersonal and concise. The shift to written discourse, with all the stylistic and rhetorical devices that come into play, makes the attention of these pupils (readers) shift from what they see and think for themselves to 'information', and finally to the 'opinions' of authors – who in the long run will become 'authorities'. In scientific *prose*, we encounter the scholar *as a person*, expounding his personal opinions and trying to persuade his audience to accept his particular point of view, for example by showing off his erudition, his acquaintance with literary sources – thus forcing his readership into a more or less passive and receptive role. For indeed, although initially 'historians' (collectors of opinions and facts) would base their knowledge on personal observation, gradually the *reading* and processing of textual materials became increasingly important. Knowledge claims were increasingly based on textual sources produced by previous generations of researchers. Thus the writing scientist became a *compiler*. The archetypical representative of this branch of knowledge is without doubt Plinius; whose gigantic compilation called *Historia Naturalis* was really a storehouse of factual knowledge. In his works, the emphasis is not on principles, not on 'thinking', not on method, but on the techniques for storage and retrieval of tremendous amounts of information.

Thus, whereas initially science relied on thinking (i.e., the contemplation of basic principles), the emphasis gradually shifted to reading, processing and interpreting texts. Scholars became professional readers and commentators rather than thinkers. Or, to put it more adequately, thinking and reading became intimately interconnected. Reading became the professional scholarly activity *par excellence*.

During the Renaissance, this association or even identification of scientific scholarship with reading was vehemently reinforced[2], but during the subsequent

Scientific Revolution, both activities were disconnected once again. The new scientific professional ideal as it emerged during the early modern period demanded that scientists, rather than relying on textual sources, should themselves become active contributors to the production of factual knowledge. But these facts had to be produced in a *methodological* manner. An experimental set-up was not simply a device that enabled a researcher to assemble interesting facts, but rather a tool for posing and answering very specific questions. And it was this new scientific method that increasingly distinguished scientific (i.e., reliable) facts from anecdotal (unreliable) information (often borrowed from textual sources). The realization of the unreliability of written materials was actually one of the causes that helped the scientific method to come about; as an example, the case of Copernicus stands out. He made some wrong predictions about planetary motion and realized later that this was due to the fact that he had used data 'collected' by ancient astronomers. These turned out to have been doctored in order to fit their preconceived cosmological and cosmogonical theories. Later generations of astronomers realized the importance of collecting data in a methodological and systematic manner.

The new type of professional scientific activity (systematic observation, using sophisticated equipment, and notably experimentation) entailed the emergence of a new scientific genre: the research paper (as well as the scientific journal: a periodic compilation of research papers). During the initial stage, research reports were extensive and often rather personal. They contained exciting coverage of the vicissitudes of doing research. The focus would be on the 'context of discovery'. Activities and findings were reported in a more or less 'narrative' fashion. Gradually, however, scientific discourse became less personal, less subject-dependent. Scientific articles submitted to journals had to be written and composed in accordance with standard formats. These formats increasingly determined the way in which a scholarly contribution was to be framed and phrased. And not only the composition of an article was formalized, also the terms and sentences available to professional authors were normalized and standardized. The author as a person gradually disappeared from the text. Thus, whereas in ancient Greece the individuality of the author gradually became more important, now we noticed a trend in the opposite direction: from personal to impersonal. The author as a person became less important, because methodology and principles were regarded as more important than personal prestige. The development of a personal style was no longer appreciated. The text became, to a certain extent, anonymous and depersonalized. All written materials adhered to one and the same professional style. The article no longer described the 'context of discovery' of scientific investigations. On the contrary, detours, failures, anecdotes, hesitations, quotations and other discursive frills were increasingly removed from the final publication. The context of justification increasingly determined the structure of the article. Instead of describing the actual course – the 'history' – of the investigation, the published article reflected professional methodological standards. The publication emphasized the methodological *principle*s the author subscribed to. Only those findings and considerations that reflected the basic experimental script (question, hypothesis, procedure, measurements, statistical analysis etc.) were presented. The final result of this development was the scholarly article as we know it today: compact, using standard terms, formulae and phrases, preferring quantitative information (usually presented by means of matrices or tables) to more extended narrative accounts. The research as it is published reflects the methodological norm rather than personal experiences. And this has had an impact on laboratory life as well. The methodological requirement of reproducibility, for example, made real

experiments increasingly successful in meeting the standard protocols delineated by the ways in which published experiments were being described. For indeed, whenever we use standard formats to describe what we do, such formats will tend to 'realize' themselves in the long run. They will influence ('normalize') our practice. The way we describe our experiments has an impact on the way we perform them.

Besides *methodological normativity*, another form of normativity has had a tremendous influence on scientific discourse, namely *ethical normativity*. The moral ideal of the unprejudiced, self-critical, reliable, unimpeachable, accurate, autonomous, responsible researcher is reflected in the self-presentation of the author of a scientific text. The moral principle of fairness, moreover, requires from scholars that they cite or quote the authors whose data or ideas they consciously used. At the same time, they justify the way they perform their professional work by referring to the examples, methods, results and statements of others. Therefore, the act of citing one another, both as an act of fairness and as a way of justifying one's own activities and decisions, is an important element in scholarly writing. But there is a moral import in 'strictly' methodological decisions as well. As standard formats force us to be brief, it is sometimes difficult to decide what precisely should be revealed, and what can safely be left out. How to select relevant information from the profuse archives of laboratory experiments? These questions are not always easy to answer.

Some problems involved in scholarly writing today

I started this article with a brief exposition of historical developments because I firmly believe that today, science as a profession (and scholarly authorship as a practice in particular) is once again experiencing a period of intense change. Basic principles and concepts, basic methodological and ethical conceptions are called into question, and this involves moral and conceptual uncertainties. Scientists are confronted with a growing number of normative issues and ethical dilemmas concerning publication, authorship, reliability of information, as well as with questions that used to be of interest to philosophers and social scientists only, such as: What is an author? What is a journal? What is a publication? Although some of these issues have a long history, their gravity has increased of late, due to the growth of scientific data and the acceleration of the process of data dissemination, stimulated by the emergence of new communication and information technologies. Scholars can now, more easily than before, circulate works in various stages of completion. Moreover, the economic conditions for scientific work have changed. There was a time when the bulk of scientific research was conducted by members of the leisure class who had ample time and means at their disposal to pursue their scientific interests. Nowadays, however, because of the transformation of science into a networked activity in which competition for results plays a fundamental role, scientists seem to be forced to worry continuously about funding and to behave as entrepreneurs, acting in a calculating and strategic manner. In this article, some issues of professional ethics will be addressed that have to do with the 'telos' of scientific research: the act of publishing one's research results. I will discuss commercialization and intellectual autonomy; the impact of the informational revolution; the problem of pluralism of ethical styles; and the fairness or unfairness of citation practices.

Commercialization and intellectual autonomy

"The late twentieth century has witnessed a scientific gold rush of astonishing proportions: the headlong and furious haste to commercialize genetic engineering. This enterprise has proceeded so rapidly – with so little outside commentary – that its dimensions and implications are hardly understood at all… Biotechnology research is now carried out in more than two thousand laboratories in America alone. Five hundred corporations spend five billion dollars a year on this technology… The commercialization of molecular biology is the most stunning ethical event in the history of science, and it has happened with astonishing speed. For four hundred years, science has always proceeded as a free and open inquiry into the workings of nature… Scientists have always rebelled against secrecy in research, and have even frowned on the idea of patenting their discoveries, seeing themselves as working for the benefit of mankind. When, in 1953, two young researchers in England, James Watson and Francis Crick, deciphered the structure of DNA, their work was hailed as a triumph of the centuries-old quest to understand the universe in a scientific way. It was confidentially expected that their discovery would be selflessly extended to the greater benefit of mankind. Yet that did not happen. Thirty years later, nearly all of Watson and Crick's scientific colleagues were engaged in another sort of enterprise entirely. Research in molecular biology has become a vast, multibillion-dollar commercial undertaking… Suddenly it seemed as if everyone wanted to become rich…".

The passage quoted above is taken from Michael Crichton's bestseller *Jurassic Park* (1991). Those who are familiar with the history of science will agree that Crichton's account is not very adequate from a purely historical point of view and reflects a widespread and idealized picture of science and scientists. It is not true that for centuries scientists could afford to work in a completely selfless manner until they suddenly awoke from their altruistic slumber a few decades ago, due to the commercialization of biotechnology. Rarely were the conditions of discovery for scientists as optimal as Crichton suggests. Be this as it may, the passage is interesting enough if we read it, not as a historical account, but rather as a kind of sermon in which an ethical ideal is sketched and the contours of a professional ethic for scientists is fleshed out, precisely by presenting the reverse image of such an ideal (that is, by presenting the idealized past as a critical mirror that allows us to reflect on some of the problems involved in current conditions). According to Crichton, commercialization constitutes a threat, while selflessness and altruism are virtues – and he presents this view as something that speaks for itself and should be taken for granted. At present, all of a sudden, the world of science is out of joint. Modern science has lost its moral innocence and splendour.

Although from a historical point of view Crichton's account is clearly an exaggeration, we at the same time have to confess that it is not *completely* untrue. Until recently, scientific research was conducted in two different, more or less clearly separated realms: namely in a commercial and in an academic environment. These two contexts corresponded with two different styles of doing research, with two different professional and ethical orientations. In a commercial setting, knowledge was produced by companies that tried to convert their knowledge products into patents, while in a scholarly setting knowledge was produced by scientists who respected the intrinsic value of scientific knowledge and were satisfied with having their achievements recorded in scientific journals. The best way to reward them and honour them for their achievements was by citing them. At present, the boundaries between these two different settings tend to blur, even though the magnitude of this phenomenon (as should be expected) varies greatly according to which discipline we

take into account. We will come back to this discipline specificity later. This leads to ethical uncertainties, such as: is a scientist a scholar or rather an entrepreneur?

One of the consequences is that university researchers are becoming increasingly dependent on commercial organizations and companies, not only for funding of their research, but also for tools, equipment, samples, research opportunities and expertise. These companies will often formulate constraints that are at odds, to a greater or lesser extent, with the traditional mores of science. And as a rule, the value conflicts that evolve from this situation will become especially pertinent when the moment has arrived to publish one's research results. This should not come as a surprise, since the act of publishing as such involves already some relevant moral decisions. One of those, as noted in the introduction, is the selection of reliable and relevant results. Does commercialization endanger the intellectual autonomy of scientists in this and other kinds of considerations? In some cases, for example, researchers are only allowed to publish positive results. In other cases, the company wants to have a say in whether the manuscript should be submitted for publication at all or not. Recently, a PhD student was faced with the following situation. She had ordered a sample from a patented product, produced by a private company X, owned by a former university professor and specialized in producing biomaterials for medical purposes. Besides funding part of her research, this company also provided her with research tools (highly specialized software). She eagerly awaited the delivery of the package. Upon arrival of the sample she discovered that a legal document was added to it, indicating that manuscripts have to be submitted to and approved by the company before they can be published or otherwise disseminated through scientific channels. . This document contained the following lines:

"Prior to publication of any work relating to the material, investigator shall furnish X with copies of any such proposed presentation at least thirty days in advance of the submission of same in order to allow X to comment on same. Further, investigator shall acknowledge X's contribution of said material, unless specifically instructed to the contrary by X".

Moreover, it was pointed out in a detailed manner what the legal consequences would be for herself, her department and her university, should she fail to comply with the instructions stipulated in the document. It seems like the intellectual and moral autonomy of the scientist is being seriously threatened.

Scholarly communication in the information age

Problems connected with commercialization are not the only difficulties awaiting those who decide to enter an academic research career. Scientific research, and especially scientific authorship, may entail a number of other quandaries as well. To begin with, our ideas about authorship have evolved under material conditions that were rather different from the ones we are confronted with now. A number of changes may be listed:

- The practice of multiple authorship (i.e. the growing number of authors and co-authors of scientific publications). This increases the opportunity for scientific collaboration, but can also have the unwanted collateral effect of decreasing the sense of moral responsibility of the individual scientists involved for the published results.
- New tools for intellectual exchange (such as electronic preprints made available on the Internet) blur the distinction between (informal) communication and publication.

- Globalization and the dramatic increase of the number of papers submitted for publication.
- The acceleration of knowledge production. Due to this development, the time lag between submission and publication of materials will be increasingly experienced as detrimental to the need of spreading innovative results in a short time. Authors will look for alternative channels to disseminate results, notably to peers.

A number of key concepts are called into question by these and similar developments. For example: what is an author? Or rather: who should count as author? Is only the first author the 'real' author of a scientific article? Should the head of the department be listed as author, for example because he provided the funding as well as the infrastructure needed to carry out the research? And what about the technician who did the laboratory work? Or the colleague who gave advice on statistical issues? Can a clear boundary be drawn between 'authors' and those contributors whose names are mentioned under the heading 'acknowledgements'? Is it morally acceptable that scientists or research groups agree to cite one another mutually in order to increase their number of hits on the citation index? Difficult decisions have to be made and conventions may prove to be rather unstable.

On authorship

Is a manager an author? In 1897, the Russian physiologist Ivan Pavlov (1849-1936) published his *Lectures on the work of the main digestive glands.* Because of this influential achievement, he was nominated for the Nobel Prize. There were, however, important reservations. To what extent were Pavlov's works really Pavlov's? At that time, physiological research was migrating from scientific workshops (where experienced researchers were performing small-scale experiments themselves, together with a limited number of co-workers or assistants) to real laboratories that involved division of labour and made it possible for researchers to become managers and to leave the bulk of the laboratory work to their co-workers. The nominee had himself pronounced his *Lectures* "the deed of an entire laboratory" and credited his co-workers by name for conducting the experiments on which it was based. Was his work an original contribution to science, or rather a compilation of a series of experimental dissertations by others (Todes 2002, p. xiii)? This is an early example of a problem that is still with us today. Two years ago Craig Venter et al. (2001) published his famous account on the structure of the human genome. In this article, the names of 285 authors were listed. What are the implications of this development for our ideas on authorship? Traditional concepts no longer seem to reflect the way research is actually done.

What is an author? Michel Foucault (1995) pointed out that in the course of history, scientific authorship has performed a number of different functions. In the medieval era, for example, the name of the author – the name of *Aristotle* for example – was regarded as a guarantee of truth, as a reliability indicator. In modern times, the name of the author came to be used to designate methods, tests, scales, special numbers, diseases, bodily parts, comets, or straits and islands – a phenomenon which is known as eponymy (cf. Merton and Storer 1973). Nowadays, authorship has lost much of its former prestige and is used in a rather technical manner. We use the name of the author for the retrieval of documents and for the evaluation or monitoring (by means of a citation index) of individual scholars, research groups and research institutes. Still, contemporary scientists and scholars are not radically consistent in following this trend. The new, technical function of authorship still has to compete

with other functions. Something of the old prestige of authorship is still left intact; we still value the papers authored by the 'big names' as almost intrinsically better that those written by less famous scientists. This reveals an interesting tension between the trend towards anonymization of the author, versus the aura some authors still seem to have, an aura which allows them to function as 'authorities', and their name as indices for reliability and quality.

And what is a publication? In a laboratory, a stream of data is gradually being processed from raw materials to printable products. But when exactly is it time to stop gathering data and to submit research findings for publication, notably in the case of pressing competition with rival research groups? Due to the time gap between submission and publication of output in scientific journals, scientists will look for alternative, electronic channels of dissemination. The high-energy-physics experiment on CPT invariance conducted by Walter Oelert and his team at CERN, resulting in the discovery of antimatter, may stand as an example here. While publication of their results in *Physics Letters* was delayed due to methodological problems noticed by one of the referees, the news that antimatter was discovered spread via the Internet, forcing CERN to take the unusual step of issuing a press release on a scientific result before the scientific paper had been published (Fraser 2000). Once a paper is published, however, the process of refinement and updating may continue. What version should count as 'final' when scientists are able to update continuously the materials they made electronically available? Indeed, electronic dissemination nowadays allows scientists to enter into an interactive exchange process with their readers. What does 'final' mean when authors are able to replace existing electronic versions by revised ones, or to solicit and incorporate feedback? Electronic publishing, as an alternative to hard-copy formats, allows the reader to play a more active role in the production of scientific output. It is clear that concepts like 'authorship' and 'publication' have to be redefined and their moral implications reconsidered.

The future of the library

Over the centuries, scientists and scholars have produced and accumulated enormous quantities of textual materials. This immediately brings us to the question of the archive: the storage and retrieval of scholarly writing. During the epoch when research and reading were intimately associated, even in the natural sciences, as explained above, the library would constitute the centre of scholarly life. The famous library of Alexandria, together with those libraries as existed in medieval monasteries, still constitutes the basic image, the archetypal idea so to speak of what a library is. An archetype, however, not only entails a basic image, but also a basic scenario, a script. An archetypal library is a collection of documents so overwhelmingly large that we feel discouraged and intimidated by it. The story, however, is likely to end with the library being destroyed by a fire – either in real life (such as the library of Alexandria) or in fiction (such as the libraries in the writings of Eco and Borges for example). And there is an element of grief involved in such an event, of course, but also of relief. The basic attitude of scholars towards libraries (or towards archives in general) is an ambiguous one. On the one hand, our work depends on their existence. On the other hand, we want to be liberated from such a heavy burden: the net result of centuries of intellectual labour. We want to stand on our own feet, rather than on the shoulders of paper 'giants'. This ambivalent attitude parallels and is intimately

connected with the relation already analysed between textual (compilative) and empirical knowledge.

From a philosophical point of view, however, the 'invisible fires' that occasionally destroy complete libraries are much more interesting than the real ones. Every now and then in the academic world, an Exodus occurs. A new generation of scholars 'decides' to leave the library and to opt for a journey through the desert, producing new forms of discourse on their way. Early modern philosophy *as such* clearly displays this basic desire. In those days, another word for 'library' was 'metaphysics' (as in the days of Socrates another word for 'textual knowledge' was 'sophistry'). David Hume (1970) summarized the typically modern desire to do away with the library (that is, with metaphysics) altogether when in *An Inquiry Concerning Human Understanding* he states that, while sparing mathematics and empirical science, one should destroy everything else:

"When we run over libraries, persuaded of these principles, what havoc must we make? If we take in our hand any volume of divinity or school metaphysics, for instance, let us ask, *Does it contain any abstract reasoning concerning quantity or number?* No. *Does it contain any experimental reasoning concerning matter of fact and existence?* No. Commit it then to the flames, for it can contain nothing but sophistry and illusion".

This rather extreme attitude can be understood as a revolt against all kinds of non-empirical weights and burdens that are perceived as standing in the way of true intellectual advancement. On other occasions, libraries are being destroyed (or at least threatened with destruction) unintentionally. During the first half of the Twentieth Century, for example, the German language constituted the international scholarly *lingua franca* (French and English coming second). After World War II, however, scholarly discourse migrated into English. And now, as fluency in German and French is declining, enormous archives, written in German and French, are seriously at risk – unless they are 'saved' by translations into English. What is our responsibility, as scholars and scientists, towards the archives of science? Should we save them by converting them to updated formats?

The emergence of electronic archives adds a new dimension to this dilemma. The pace in which information migrates into new formats is increasing. Indeed, languages such as German or English could be regarded as a 'format', but now we are faced with updating and outdating of *electronic* formats, both in terms of hardware (technology) and in terms of software (applications). Documents will have to migrate continuously to updated formats in order to escape the fate of becoming irretrievable or simply unreadable. Are we responsible for the survival of scholarly information stored in obsolete formats? Can we afford to witness these devastating 'digital fires' without doing anything? Is it something we should deplore or something we may rather experience with a sense of relief? Foucault's idea that the opening up and analysis of libraries is a form of 'archaeology' is now truer than ever – as each library becomes a collection of materials in many different mediums and formats (most of them 'outdated').

Ethical styles in conflict

Authors may not only perform different functions, many of them will also practice different genres. For example, they may write and publish for colleagues (experts), but for broader audiences as well, as was already mentioned above. In the first case, they are likely to produce texts that are short, while a considerable portion of it will

consist of numbers, technical terms and mathematical formulae. In the latter case, they will prefer ordinary, colloquial, readable, perhaps even elegant prose. A good example of a scientific writer who was very successful in both genres, was James Watson, who in 1953 (together with Francis Crick) published the famous article on the structure of DNA. It could only be read and appreciated by a relatively small number of experts. In 1968, however, Watson published his bestseller *The double helix*. In this book a much broader audience is addressed. It typifies science as a fascinating endeavour, and the scientist as a challenging role model, counterbalancing popular images of dull lab work (Van Dijck 1998). Moreover, in this book a new ethical style is introduced and justified. Science is defined in terms of competitiveness, rivalry and ambition, rather than altruism and selflessness. In this respect, the kind of ethics that was conveyed by the quote taken from *Jurassic Park* at the beginning of my paper was already rather old-fashioned. Watson's book "purportedly promoted a new professional ethic which encourages competition and justifies any means to attain a set scientific goal" (Van Dijck 1998, p. 40). Its function was to display an example of a new scientific style, which even involved ruthless predation on other people's work, minimal courtesy to supporting colleagues and peers, defiance of troublesome data and a positive contempt for traditional intellectual concerns. Without a sign of bad conscience, Watson relates for example how he secretly cajoled people into giving him information. He simply changed the rules of the game. Rather than obeying the old rules of courtesy, collegiality and openness (or even intellectual communism, as Robert Merton called it), he promoted new methods such as competition and secrecy. His major victim was Rosalind Franklin, whose decisive contribution to the discovery of the structure of DNA was until recently grossly undervalued. Once again, the conflict over ethical style eventually focused on issues concerning scholarly communication such as: sharing or concealing unpublished results, publishing, citing and acknowledging scholarly work. Thus, the question is not: 'publish or perish', but rather 'publish strategically or perish", at the right moment and in the right kind of journal, for example. Timing and communicative strategies are important. Watson unequivocally advocated the idea of science as a race, an ethic that is still visible in '(the press covering of) the important research initiatives of the present, such as the Human Genome Project. In 2000, for example, Celera's president Craig Venter (already mentioned above) proudly announced that his company had beaten its rival, the public Human Genome Project in a long, closely watched race. Yet, the shift in scientific ethos that Watson advocated and that can indeed be recognized in the competition-driven academic community of today, does not invalidate the traditional goal of science, namely the quest for a better understanding of the world. Competitive motivations can be perfectly valid ways to attain cognitive goals.

Is there a (technical) solution to every (philosophical) problem?

Instead of simply being overwhelmed by the complexities and perplexities of contemporary scholarly discourse, professionals, notably journal editors, have tried to formulate policies that might allow us to deal with the issues at hand. A new – interdisciplinary – field has emerged: publication ethics. The professional scholars involved try to formulate principles of conduct regarding issues such as sponsorship (commercialization), authorship (notably: multiple authorship) and accountability. A group of editors of medical journals met informally in Vancouver, British Columbia, in 1978 to establish guidelines for the format of manuscripts submitted to their journals. The group became known as the *Vancouver Group*. It expanded and evolved into the *International Committee of Medical Journal Editors* (2001). Among the

issues to be considered they listed for example the problem of redundant or duplicate publications. Their guideline stipulates that this practice is admissible only if the editors of both journals have approved it and the paper for secondary publication is intended for a different set of readers.

The focus, however, is on authorship, perhaps the most sensitive and complicated topic in contemporary research ethics. According to their guidelines, all persons designated as authors should qualify for authorship, and all those who qualify should be listed. Each author should have participated sufficiently in the work to take public responsibility for appropriate portions of the content. Authorship credit should be based on substantial contributions to conception and design, or acquisition of data, and should involve final approval of the version to be published. Acquisition of funding, the collection of data or general supervision of the research group does not justify authorship. Authors should provide a description of what each contributed. All others who contributed to the work should be named in acknowledgments. Members of research groups involved in multicentre trials should either meet the criteria as stipulated, or simply be mentioned in the acknowledgments. The order of authorship should be a joint decision of the co-authors.

This has certainly been an important initiative, but I would say that its main importance lies in *listing* and *defining* the problems and issues involved, rather than in solving them. For example, it may in practice be quite difficult to tell exactly what a 'substantial contribution' means and where the difference lies between 'supervising' a research group and 'contributing' to the conception and design of the research group's research. The exact meaning of these terms is bound to be context-dependent. The way these criteria are used is likely to differ from discipline to discipline, or even from research group to research group. Indeed, for several years now, I have been teaching publication ethics to groups of PhD students coming from different disciplines. These groups constitute samples of researchers that are active within one particular university setting, the University of Nijmegen, The Netherlands (KUN). But although most, if not all, of the research groups represented in this sample will tend to agree with the principles as formulated by the Vancouver Group, the range of different interpretations and conventions as described by the PhD students enrolled in my course is simply astonishing. To a certain extent, these differences will reflect power relationships and generational conflicts within particular research areas, and in that sense they may be highly dependent on the personalities and practices of individual scientists involved. But as a rule, these differences tend to reflect more fundamental (and therefore more interesting) differences in the ways these branches of research are actually organized. Indeed, in this area as well as in others, moral conventions and interpretations are often technology-induced. Areas of research using complicated and expensive equipment, for example, and relying on broad collaborative networks and team-based consortia with members in many different roles, will often have a less restricted understanding of what authorship means in comparison to other disciplines (which means that they will be less hesitant in listing someone as 'author'). In disciplines where the dependency on technology is relatively limited (such as in philosophy), however, one-author articles are far from outdated, on the contrary: they still constitute the 'normal' case.

The Vancouver guidelines will certainly help us to clarify some of the problems involved in authorship. Establishing and disseminating good professional practices in this area is important. Yet, scholarly publishing as such has dynamics of its own, and the uncertainties involved in scholarly publishing tend be reproduced, rather than 'solved', by the terms and principles stipulated by these and similar guidelines. It

would be highly naive to claim that we know what an author 'is' simply by stating that he or she should have contributed 'substantially'. The Vancouver guidelines allow for a considerable amount of variation. The difference, for example, between 'earned authorship' and 'honorary authorship' is a gradual or scalar difference (Macrina 1995, p. 77). Different disciplines and fields of research will develop their own conventions within the general Vancouver framework. And there are even broader, cultural differences involved in a concept like authorship. Whereas the 'continental' idea of authorship tends to focus on *intellectual* authorship, for example, in the Anglo-Saxon realm the focus is rather on authorship as a financial and economical category (copyright).

Professional ethics and the virtue of self-denial

The finishing line of scientists regarding their work as a kind of race is the time of publication in an acknowledged, international, peer-reviewed journal. What do scientists hope to achieve by publishing their work? The answer is that they strive for recognition. The best way to acknowledge the achievements of scientific authors is by citing them. The ultimate reward, for scientists, is perhaps to occur (as often as possible) in the citation index. "I was cited, therefore I exist". To cite the work of those whose concepts, methods and ideas we used is not merely a form of courtesy, but rather a form of 'fairness'. But can science really be fair? Can we, for example, be fair to our competitors? Robert Merton, the famous sociologist of science, was emphasizing the lack of fairness in citation practices when he described what he referred to as the Matthew effect in science:

"For unto everyone that hath shall be given, and he shall have in abundance; but from him that hath not shall be taken away even what he hath" (Mt 13:12).

Most articles published will be cited just a few times, and then they will be forgotten completely. Most authors will be read and cited by only a limited number of readers. Some articles, however, will be cited more often and the number of citations may even reach a critical limit. Beyond that limit, the number of citations is bound to increase dramatically. They will receive hundreds or even thousands of citations. Colleagues will continue to cite them for twenty or thirty years, until the paradigm the author helped to create becomes extinct. An author may publish an article that really makes his name, although the time and effort spent on writing it may not significantly exceed the amount of time and effort spent on publications that are treated less respectfully. Eponymy (mentioned above) is one of the causes of this phenomenon, but also priority ('publish timely') and visibility ('publish strategically'). But even eponymy is a rather technical function of authorship. It is simply a convenient way to refer to a test, an illness or a bodily part. Most authors will remain relatively anonymous and even the identity of authors that are cited quite often will disappear behind their name. Hardly anyone who refers to the Stroop effect, for example, will know anything about the individual bearing the surname Stroop. Thus, as a rule, the desire for recognition will remain unsatisfied. Is there something we can do about this? Should we try to make science fairer?

I think it is a mistake to regard science as an endeavour that has to be tailored and constrained by means of ethical rules and norms external to science as such. Rather, science should be looked upon as an intrinsically moral phenomenon. Not only because scientists often want to contribute to the progress of mankind and the quality of human life, but also because doing science and being educated as a scientist constitutes a kind of *Bildung* in itself. According to Friedrich Nietzsche, science is a

powerful tool developed by man to educate and discipline himself. Scientific training is basically a training in self-control. Virtues involved in practicing a science, such as unprejudiced open-mindedness, patience, precision and reliability, are moral values. Science fosters an attitude of self-criticism and perseverance. According to Nietzsche, a true scientist is someone who is really willing to adopt a self-critical, unprejudiced stance, someone who will not allow himself to be dominated by stereotypical views, narcissism and prejudice, but is always willing to put his theories to the test and to enter into an open debate with others, someone who is susceptible to criticism. According to Nietzsche, the true scientist has but one desire: not to deceive, neither himself nor others. That is, science is an intrinsically moral phenomenon. And perhaps the most important scientific virtue of all is self-denial. *"Was liegt an mir!"* It is not me that counts! According to Nietzsche, this phrase more or less sums up the ethic of being 'in science' (Nietzsche 1980, § 547). And a similar phrase occurs in the work of Michel Foucault: "Qu'importe qui parle?" Science is, first and foremost, simply discourse: a conversation or intellectual exchange that already existed before I decided to participate in it and that will continue to exist when I am no longer present or able to contribute to it. *On parle… Man spricht…* Most, if not all, of the words, terms, phrases and arguments a scientific author uses, have been invented by others. In most cases, scientific authorship comes very close to anonymity. There is a certain moral quality in the stoical acceptance of this fact.

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[1] A word of caution is due in order to avoid confusion: the word 'science' with its modern connotations may not seem fully appropriate when dealing with the origins of Western philosophy. Since, however, that endeavour can confidently be seen as the beginning of a process of rational inquiry that has led to our modern concept of science, I have chosen to stress the continuity between ancient and modern 'science'

[2] An illustrious exception can be found in Leonardo da Vinci. Although he is regarded as a typical 'homo universalis', and he was acquainted with the whole corpus of the knowledge of his time, he also was an observer and made considerable original contributions in the empirical sense. We can see him as a transition figure.