

Taking stock of research evaluation and the conditions for conducting research -- Prof.dr. Sarah de Rijcke

So try it one more time
With feeling darlin', take it from the top

---Kris Kristofferson, *Once More with Feeling*

Distinguished Rector Magnificus, my esteemed colleagues, dear friends and family, valued listeners,

On 30 August 2017 I received a disturbing email from an Italian marine biologist. Ferdinando Boero studies the occurrence of swarms of jellyfish – known as ‘blooms’ – and more specifically the causes of unnaturally high numbers of bloom periods in recent years. He wrote that he was unable to get his findings published in the key journals in his field. This was causing him some concern, for several reasons. Firstly, because this observation of unnatural jellyfish bloom periods is not a trivial finding. It is an important indication that the ecology of seas and oceans is disturbed. But what it means exactly is unknown. Secondly, the jellyfish blooms are having a very negative effect on tourism as a result of the increasing number of jellyfish stings, and the fishing industry is also suffering because jellyfish consume fish eggs and larvae. Finally, the presence of jellyfish impedes the growth of coastal plants. In short, jellyfish play a major role in our ecosystem.

Boero had read one of my articles about the role of performance indicators in life sciences, and this is what triggered his email. My article had confirmed his belief that science is not being properly evaluated today. He referred in his mail to the strict norms in his field, and how they are translated into the national science system in Italy, in which some types of – in his view important – contributions do not count. For example, he wrote that, as an established researcher, he often collaborates on policy reports of considerable societal relevance, but that it would be harmful to his university if he were to include these reports as output in evaluations. This is because these reports are not published in international peer-reviewed journals and have no Journal Impact Factor (a metric that indicates how often a scientific journal is cited):

But I want to make clear that this output is much more valuable than anything I have published in my career to date. However, the current evaluation mechanisms do not assess its true value. I have written manuals for students and given scientific advice to policy makers, but this counts for nothing in the academic world. I also don't add my name unnecessarily to articles written by colleagues and so my publication numbers are decreasing. According to the current evaluation criteria, I am lazier now than I was in the past!

In my work, I specialise in interactions between evaluation and knowledge creation. I therefore see Boero's mail as source material, and a reason to conduct further research.

Evaluations have become a permanent element of the workings of present-day universities. And there are many different reasons for this. One of them is very simple: universities and other knowledge institutions are part of a wider ‘evaluation society’, which completely floods us with rankings and ratings [Dahler]. Since the advent of the internet, this has only increased. We count, measure and rank till we drop: from the restaurants we visit and the hotels we book, to the quality of the educational institutions where we send our children. This phenomenon even penetrates into the public space. Try and notice, next time you visit public toilets. It's now normal practice to see poles with green, orange and red ‘smiley’ buttons to rate the service provided by the staff. ‘How satisfied are you with our services?’ I wonder what happens with the input data. But I can imagine that simply the presence of the poles will discipline the cleaning staff. I am interested in how all this evaluating and rating has actually come about, and in particular how evaluations affect scientific research.

In essence, however, my work is not really about evaluating, counting and measuring in the ‘narrow sense’. Together with my research group, we make a connection between knowledge creation on the one hand, and contemporary forms of management and organisation of science on the other. And this broad perspective is important, because in recent decades there have been major changes in the funding structures [Whitley], and because we witness an increasing formalisation of scientific work. Research is being conducted more and more in externally funded, short-term projects, which researchers have acquired by engaging in competition with others. And these projects are often associated with strict project management and control [Fowler]. Evaluations are

social processes to the core, as they are inextricably linked to funding structures, organisational forms, and political and social systems.

In my chair, I wish to draw together a number of shared concerns from the international debate on the state of the science system. I aim to analyse the conditions for conducting research in relation to developments in the content of the research. And this work is not only about social processes. Equally important are Boero's jellyfish, whose lives may or may not become part of the academic and public debate. What we do is thus not only a sociology of scientists and their institutions; it is also a sociology of the content of science itself [Latour]. We want to understand the situation of the scientist, but also the situation of her object of study.

And this involves three central questions:

1) How do we evaluate science and scholarship?

What kinds of ideas about research quality, the societal relevance of research, good governance, the importance of certain forms of collaboration, and careers can be seen in formal evaluation systems and informal evaluation criteria? How do stakeholders interpret terms such as excellent and relevant? How are these expressed in policy?

2) How do evaluations interact with science itself?

How do evaluations interact with scientific research and the many different policy and funding instruments that make use of evaluations? We study these interactions 'in action', and we do it specifically where researchers and other stakeholders make decisions about what direction to take with the content of the research and how to organise the work. In the lab, on a ship at sea, but also with humanities scholars in their archives.

3) Is this really how we want to evaluate science?

I will give you the 'spoiler' right now: my answer to this last question is currently 'no'. There are clearly some inherent flaws in the system, that we need to work on and that a few people are fortunately already working on [Benedictus]. And constant adjustments are needed in any case, because the world – including the research world – never stands still. The academic sector is becoming ever more international. Researchers are working increasingly with larger-scale infrastructure. And more people, both within and outside the walls of the universities, are contributing their ideas about what topics are relevant for research (examples here are the input for the National Research Agenda and the role of patient organizations in biomedical research). And the expectations exceed the problem-solving capacity of science, for instance when it comes to finding solutions for major societal problems (healthy aging; ethics and artificial intelligence; climate degradation, etc.). Which is why we need to understand clearly how evaluations can (and do) influence the behaviour and choices of researchers and universities. What evaluations are doing now, and what they did in the past. And what we really *want evaluations to do*.

Thinking with indicators

Recent events do not give me hope, but they do give me purpose

---Kathleen Fitzpatrick, *The Generous University*

Quantitative performance measurement is often seen as the appropriate road to take in the assessment of research quality. The question that I started to ask myself in 2012, and have been asking since then, is whether all this counting and measuring and evaluating actually fulfils those expectations in science and scholarship. We now know the answer, of course, but back then very little empirical research had been conducted on it. It was also before the debate on this topic erupted in the Netherlands. I wanted to know whether the prevailing – usually quantitative – indicators were reliable means for encouraging good research, for ensuring the quality of knowledge production, and for appointing and promoting the right researchers. This is certainly not guaranteed. Far from it: in fact, an important conclusion from my research group's projects in the past years is that the dominant thinking in terms of market economics in evaluations reduces the question about the value of knowledge to how efficient and productive it is. How much have researchers published in a specific period, what was the Journal Impact Factor of the journals in which they published, how many times has their work been cited, how much money have they acquired in recent years?

This type of thinking has at least two consequences. The first relates to the well-being of the researchers in the system [Weijden], and more in general to the space for what you could call ‘social’ elements. Think of the consequences of increasing competition, decreasing collegiality, less commitment to the community. Since 2012 we have been doing fieldwork in many different disciplines. By now, we have conducted hundreds of interviews, carried out many months of observations, analysed mountains of documents. If we just take a look at the medical sciences, we can see that the pressure in many places there is so high that ambitious young researchers think they cannot go on holiday because otherwise their work will perhaps be ‘scooped’ by a group in another country. Then we see that group leaders and heads of department warn each other about the health risks that they run by chronically working very hard, year after year, on articles for journals with a high impact factor:

I have told you many times that you must also take care of your health. I should know; I’ve had three bypasses. There really is more to life than an article in Nature.

The second consequence of a system based on the principles of market economics relates to the content of the work. And this is a very important point. We are observing a serious risk of a decline in the diversity of the subject matter. We are observing that research is designed and adjusted in a way that ensures a good score. We see that people are choosing research questions not just on the basis of interest or importance, but also to improve their chances of getting a job. This is what we call ‘thinking with indicators’ [Müller]. And this thinking is problematic for science as a whole, because we can see that other criteria for scientific quality (such as originality, scientific progress in the long term, societal relevance) are disappearing into the background, or even becoming completely ‘unthinkable’.

All of us must really ask ourselves what this will mean for science and scholarship in the longer term, as a critical part of society. And whether it might not result in universities becoming unattractive workplaces for people who want to contribute more to the world.

Another important conclusion that I draw from our work on ‘thinking with indicators’ is that simply banning indicators in formal evaluation procedures is completely inadequate. On 18 April, the funding bodies ZonMw (Netherlands Organisation for Health Research and Development), NWO (Netherlands Organisation for Scientific Research) and KNAW (Royal Netherlands Academy of Arts and Sciences) signed the San Francisco Declaration on Research Assessment (DORA). DORA is a global initiative to make the evaluation of research and researchers less reliant on indicators such as publications and citations, and more on other criteria. By signing it, these organisations are sending a very important message about the need to improve research evaluation, and it is excellent news that they themselves are also starting to work on this. My hope is, however, that we will all realise that more needs to happen if we really want to see a fundamental effect in the workplace, because the use of indicators has become very closely interwoven with the primary knowledge production process.

Take the Journal Impact Factor (JIF): one of the most well-known indicators. Over the last twenty-plus years, the JIF has gradually been included in institutional regulations, in peer review procedures, in specific disciplinary norms and values about research quality, and in the actual behaviour of researchers. This indicator has helped to determine who now works at universities, which journals are important, and who does or doesn’t receive grants.

Indicators like the JIF have played a part in shaping all these practices. To such an extent, in fact, that it probably now makes little difference whether they are present or absent in formal procedures. Thinking with indicators has become inextricably linked in many labs and offices with reflecting on what the next research question will be. Any plea to reject the JIF needs to be combined with tackling this underlying reality. I don’t know whether all the signatories of DORA are ready for this much more complicated task: a quite fundamental restructuring of the way in which scientific knowledge is created. What we actually want evaluations to do is totally unclear. And also what we then actually want from our universities. There is a great need for a clear substantive vision. And this is my point: it is precisely this underlying structure and culture that we must understand and tackle much better. Merely signing the DORA will not suffice.

A dive into the deep

So, what have we provoked?

---Donna Haraway, *Tentacular Thinking*

Let me now just take a step back. Where do we stand at the moment?

New research policy is urging science to work on societally relevant topics in a team setting and in an open, interdisciplinary and transdisciplinary way. Yet this policy has to be implemented, as I just argued, in a globalised, production-oriented research world, and in a highly bureaucratic evaluation practice. A significant change of direction has taken place in the European Union's research policy during the last ten years [Wilsdon]. A striking feature of this new policy is the extent to which it formulates principles for the entire European research landscape: from open science and open data to adjusting R&D in line with societal priorities. The EU is tackling societal priorities via funding that targets 'global challenges'. I already briefly mentioned these, with examples including climate degradation and smart technology.

This movement towards mission-oriented research is certainly not universally endorsed. Critics are afraid, for example, that it will lead to the further reduction of financial and infrastructural support for fundamental research. So while the EU is going all out to promote interdisciplinary and transdisciplinary research, open science and societal relevance, at the same time this is causing friction with other 'evaluative principles' [Stark] and other criteria for scientific quality (such as excellence and international competitiveness).

I am very interested in carefully analysing this friction. I want to understand how particular norms and values about conducting good research take shape. Do all researchers now have to both be excellent and provide immediately applicable knowledge? Is that even possible? What is the definition of a good contribution? When does a researcher or discipline make a contribution? Who or what determines this? Who benefits from it? *Cui bono?* [Star]

The email from the Italian marine biologist Ferdinando Boero, with which I began this lecture, ultimately resulted in a major new project on evaluating ocean science. I think this project is important for my institute, the Centre for Science and Technology Studies (CWTS), because it can help to inspire methodological innovation. I will come back to this point in a moment. In addition, the main question of the project overlaps with what I see as a crucial question in my chair for the coming years: how are evaluations and scientific insights interrelated?

Why ocean science? Because, apart from a few exceptions [Vermeulen], very little research on this discipline has so far been done in my field. Another reason is that ocean science is under tremendous pressure – like many other disciplines – not only to be excellent and relevant to industry, but also to be relevant to the survival of the world. Ocean scientists need to do fundamental research; to study what the effects of pollution, overfishing and ocean warming are; to participate in awareness-raising activities; and to work on the industrial production of their technological innovations. Can they do all this in a system that focuses on research and efficiency? Can they do all this in a system where it is becoming increasingly difficult on the international scale to obtain funding for high-risk research, and to ask more complex questions [Laudel]?

I am going to delve into this discipline over the next five years, and investigate what role is played there by evaluations. I will do this together with my colleagues, and also together with Ferdinando Boero and five other leaders of important European marine institutes, all of whom have generously agreed to assist with the project.

What are we going to do? In a nutshell, we are going to study the past, present and future of evaluating ocean science. We are going to conduct quantitative analyses to gain a better understanding of how the field has developed over the past decades. Which topics were valued more than others. Which research lines became 'hot topics' and which did not. And who became global players, and who disappeared into the periphery. A number of PhD candidates are also going to do observational research. What is currently important in ocean science? How is the work organised in different European marine institutes, in laboratories, on ships, in the water, in team meetings? And finally, we are also going to look at prioritisation in research policy at the European and national level. What will the criteria for evaluating ocean science be in the near future?

I think this project is an excellent opportunity for the CWTS – and perhaps even for science studies in general – because one of its important ambitions is to work towards a combination of ethnography and scientometrics. This approach fits in well with recent calls for computational and scientometric models to be integrated within Science & Technology Studies (STS). Conversely, scientometrics can learn a great deal from relevant STS concepts [Cambrosio]. None of this will be easy, but that's actually a good thing. Easy is just uninteresting. It will not be easy because although scientometrics and STS 'share a common origin', they have grown apart over the last few decades and now they have each developed their own journals, conferences, skills, standards and norms [Wyatt]. In Leiden we are going to further explore this combination. We are going to combine methods from STS with computational methods, and we will not do this in stages and separately from each other, but rather iteratively and simultaneously [Varga]. We will incorporate the results of computational analyses in the ethnographic work. And vice versa, we will develop computational methods from relevant STS concepts. I am completely biased about

this, of course, but it seems to me that the CWTS – given that the institute is currently in such excellent condition – is the ideal centre for engaging in this experiment.

Options, options

It matters what stories make worlds, what worlds make stories.

---Donna Haraway, *Staying with the Trouble*

Finally, I would like to give some attention to the relevance and applicability of my research.

What does all our work on how science interacts with evaluation actually yield? My thoughts about this are the following. Firstly, our work gives an insight into how research ambitions and research policy turn out in practice. It produces knowledge about the role played by evaluation in the everyday work of researchers and other stakeholders. Secondly, our work may also be beneficial to the researchers that we study. For example, it helps them to employ policy terms – such as excellence and relevance – in a more responsible way, and not to just adopt them indiscriminately. And they may benefit from reflecting with us on their work, on how their local context partly determines what is meant by responsible research, what values concerning quality are embedded in their community, and also what questions and societal problems are now truly relevant and appropriate for the skills and interest of their researchers.

And for this kind of work the conceptual and methodological tools of the social sciences and humanities are absolutely critical. In my view, the social sciences and humanities should not primarily focus on becoming ‘harder’ in order to be taken more seriously. They must, above all, become more engaged in order to be even more meaningful. But the question is whether our own evaluation practices in the social sciences and humanities offer space for this.

I also think that the “regulatory sciences” [Jasanoff] in particular – under which I include STS, evaluation studies and scientometrics – have an obligation to seek out this engagement. Scientometrics, for example, is from the outset not a neutral party, but is deeply involved in producing indicators aimed at serving as a policy instrument [Wouters1]. This means that the field has an extra responsibility to “offer options for future orientation. Options that are speculative, but contain the promise that the abstractions in which the complexity of the world is captured can perhaps be less violent in the future. This is what drives my work.” [Schinkel].

You sometimes hear people complain: can we not just stop with all these assessments? I am not a supporter of this position. It is naïve, because of the considerable interests involved in science (political, financial, ecological). We need structures in order to make the right strategic decisions about content, money, power, reputation. And evaluations are well suited for this purpose. But it is highly irresponsible to outsource most of these decisions to bureaucratic procedures and insensitive indicators. We should make the room for deliberation more visible, and offer options and alternatives. In this context, at the CWTS we are going to continue with developing new methods for research evaluation [Holtrop] [De Rijcke]. We need to see evaluations more as an “exercise in collective future making”, and not remain stuck in attempting to obtain the highest possible score on a set of indicators that are more or less relevant for the work [Wouters2]. And we must be more imaginative in getting people involved with strategic decisions in evaluations, from climate activists to intellectuals who challenge existing paradigms. In the long term, we cannot afford to do anything else. The world is on fire.

Acknowledgements

So try it one more time
With feeling darlin', take it from the top

---Kris Kristofferson, *Once More with Feeling*

In conclusion, may I take this opportunity to reflect for a moment on the fact that – if I’m correct – I am, in 2019, the 208th female full professor ever to have been appointed at Leiden University. It is now ninety years since the first female full professor was appointed at Leiden, and the percentage of female full professors, at just one in five, is unfortunately still shockingly low. A few weeks ago I received a very sweet email from my aunt Nel. Unfortunately, she couldn’t be here today. Nel wrote that she saw my inaugural lecture as a “special event in the history of the De Rijcke descendants: the first to hold a PhD, the first to be a professor, and of course to do this as a woman!” The mail filled me with pride, but also with sadness, because she herself did not have the same opportunities in the past as I have been given. She naturally also highlighted the real problem: a shortage of

women in the highest academic positions. It is sometimes said that it is just a matter of time before the same number of women as men are appointed. That would be wonderful. But I have to honestly say that I always feel quite uncomfortable about thinking in terms of gender differences. I can't wait until we, as a society, can stretch even further the acquired patterns associated with them. We haven't been able to achieve this yet at the universities, where my experience is that both men and women struggle with obstinate implicit expectations and norms [Thornton].

Sometimes these expectations can actually become explicit. I am thinking, for example, of the suggestion that someone made a few years ago after I'd given a presentation. The advice was that I would do better to laugh less during a presentation. I should learn not to laugh so much if I was aiming to have a professional career in academia. Really...? A hundred years ago, the first female full professor in the Netherlands, Johanna Westerdijk, proved that intelligence and a sense of humour go very well together. In Patricia Faasse's excellent biography of Westerdijk, she depicts her as a woman who took her place in the academic world, working extremely hard and with a "bellowing laugh, love of parties, drinking and dancing, and aversion to embroidery and meaningless conventions" [Faasse]. Good for her! I hope I'm living up to her example.

I would like to thank the Executive Board and the Faculty for appointing me as a full professor and for the confidence they have shown in me. I think it has helped me to have had very good mentors who embraced the Johanna Westerdijk in me. For this, I give so much thanks to my PhD supervisors and co-supervisor Douwe Draaisma, Trudy Dehue and Anne Beaulieu. Douwe: with you – and also with Maarten – I could read and write in Groningen, and thanks to you, with more and more feeling. Trudy: it is not an exaggeration to say that I am standing here today because of your work and challenging teaching. Anne: you inspired several important steps in my career, and in Amsterdam you showed me how generosity works in academia. You were also the one who advised me to talk to Paul Wouters about possibilities in Leiden after my postdoctoral research. And I certainly found out! Paul: you are unique. Thank you for your courage, laughter, vision and support when I needed it.

I would also like to thank all the other colleagues with whom I work in the CWTS and elsewhere. I'm sorry that I can't name you all individually, but I would like to mention Thed van Leeuwen for your confidence in the new direction that I introduced. And without my fellow deputy directors Ed Noijons and Ludo Waltman, and also Mark Neijssel, my life would be much more complicated; and the same applies for the diligence of the other members of our board. I also thank Suze van der Luijt, Anne Marie van Noord and Petra van der Weel for their important support.

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My current and former research group members deserve a separate mention. I enjoy your energy, trust, and willingness to explore insights and methods beyond your own disciplinary boundaries. That is certainly hard work. I would very much like to dedicate this inaugural lecture to you: Rinze Benedictus, André Brasil, Guus Dix, Thomas Franssen, Björn Hammarfelt, Tjitske Holtrop, Wolfgang Kaltenbrunner, Thed van Leeuwen, Alex Rushforth, Clifford Tatum, and Jochem Zuijderwijk. Thomas, Wolfgang: your input has made this text much clearer.

I will close with a word of thanks to my family and friends. It is so very special to me that I can share this moment with both of my parents in good health, with my sister Mirjam, my dear family-in-law, and my dear friends, including Mariëlle who is following this from Sydney. And of course with Vincent. You have fully accepted me as I am for sixteen years. I am inspired by your calmness, intelligence and creativity. And by how you love our Serein. Without you, I might perhaps still have been standing here, but *with* you it is so much better.

I have spoken.

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