

ESSAYS ON EDUCATION,  
PHILOSOPHY AND POLITICS

# CONFERO

MANAGING BY MEASURING  
ACADEMIC KNOWLEDGE PRODUCTION UNDER THE RANKS

# Confero

Essays on Education Philosophy and Politics

*Managing by measuring: Academic  
knowledge production under the ranks*

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# Confero: Essays on Education Philosophy & Politics

Volume 1 Number 1 March 2013

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# Managing by measuring: Academic knowledge production under the ranks

*Erik Nylander, Robert Aman,  
Anders Hallqvist, Anna Malmquist  
& Fredrik Sandberg*

It is not virtue which can found a free intellectual order; it is a free intellectual order which can found intellectual virtue.

- Pierre Bourdieu, *The Rules of Art*, 1996

**O**n the 21st of January 2012, mathematician Timothy Gowers wrote a blog post in which he listed a number of problems related to the current system of scientific publishing and, in particular, what he described as the ill-doings of publisher Elsevier.<sup>1</sup> Gowers considered the high prices set on journals by publishing companies, the praxis of ‘blackmailing’ libraries to buy bundles of journals and the way publishing companies tried to block the on-going process towards more open access

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<sup>1</sup> <http://gowers.wordpress.com/2012/01/21/elsevier-my-part-in-its-downfall/>

publishing especially harmful to the research community. Another issue that Gowers' blog post touched upon was the ambiguities inherent in the publishing system. On the one hand, we have the researchers and the ethics of the research community. As we know from Robert Merton,<sup>2</sup> researchers are urged to work in rather disinterested and communistic modes: helping colleagues in peer-review processes, returning favours to editors of journals in their specific field of expertise, i.e. neglecting the 'real' economy. On the other hand, there are publishing companies that follow the logic of profit where the craft of researchers are transformed into corporate revenue. The object of Gowers' irritation, Elsevier, is one of the biggest players in the oligarchic market of scientific journals; a conglomerate that in 2010 had a profit margin of a stunning 36%, earning €724 million out of a total revenue of €2 billion.<sup>3</sup>

In the current state of affairs, researchers work with hardly any costs to the publishing companies because they seek recognition from and among their peers in the academic community. The publishing companies, on their end, leverage on this ethos and are able to push their profit margins to new heights by extorting public resources (funds of salaries as well as libraries). The market idea of the research community, as a whole, can thus be summarized as: *work for free, and then buy the work back expensively*. No wonder few public universities ever make any profit! Gowers concluded that the scientific community needed to organise and 'take a stand'. He wrote:

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<sup>2</sup> Merton, 1973

<sup>3</sup> Source: <http://www.economist.com/node/18744177>. These extraordinary high profit margins did not change much during the years the recent recession. Arnold & Cohen (2012) reports that the earnings of Elsevier have been steady around 33- 36 % in the years between 2008-2010, figures unheard of in many other braches during the financial downturn.

It might seem inexplicable that this situation has been allowed to continue. After all, mathematicians (and other scientists) have been complaining about it for a long time. Why can't we just tell Elsevier that we no longer wish to publish with them? Well, part of the answer is that we *can*.

Gowers' blog post struck a chord with the research community of mathematicians – probably partly due to the strong symbolic position of which they had learned to associate his name – and went viral.<sup>4</sup> Gowers encouraged the scientists to boycott Elsevier for the time being. These measures would be a first step to *bundle back*, and to create more decent conditions for scientific research. After Gowers made his position official, many followed soon after. A webpage was set up where scientists from all over the world signed a protest, promising each other to avoid taking part in the voluntary work that help generate the high profit margins of giant companies, in this particular case Elsevier, while locking the knowledge away, inaccessible for public scrutiny and debate. At the point of writing this, one year after Gowers' initial petition, the number of people who have signed up for this protest has started to slow down. Despite the recent deceleration in sign-up rate, the petition has gathered more than 13,200 researchers and has provoked a necessary debate.

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<sup>4</sup> Timothy Gowers is the Rouse Ball Professor of pure mathematics at the University of Cambridge (UK) and the recipient of many academic prizes and honorariums, such as the 1998 Fields Medal for his contributions to functional analysis.

In this first number of the new journal *Confero* we engage with questions that Gowers raised in his blog post regarding the conditions for academic knowledge production. Before outlining the content of our issue, however, we wish to address this debate from an additional point of departure. Staffan Larsson, an Emeritus Professor in Adult Education in Sweden, has claimed that a virtual ‘*economy of publications and citations*’ (EPC) is emerging.<sup>5</sup> Calculations of publications and citations are used more and more to allocate resources (both financial and merit/prestige), to create incentives through measurements and standardized forms of quality, such as impact-factors, league tables and ISI-rankings.

The emergence of an economy of publications and citations arguably leads to a gradual shift in relationships between colleagues. As quality is reduced to measurable ‘output’, competition between colleagues concentrate on their publications record, rather than on seeking new knowledge or pursuing ground-breaking scholarship.

Furthermore, the emergence of league-tables and ranking-lists foster impact-anxieties among young aspiring researchers who are trying to find a place to publish their articles and, eventually, to obtain research funding, tenure or other symbolically important assets. Whether imaginary or real, the effect of the EPC leads academics to pursue publication strategies based on particular assessments of worth. As the current ISI-ranking systems are clearly dominated by English-speaking countries, the research traditions stemming from these countries possess a privileged position to partake in the economy. For instance, the English-American interpreters of

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<sup>5</sup> Larsson, 2009

Pierre Bourdieu can easily become a much more lucrative group to cite than the original books or the many French scholars who have published their research elsewhere, at safe distance from the current dogma of ‘publish or perish’. The irony here is that researchers working within, say, the French tradition of Bourdieu or Foucault are often better able to grasp the significance of the scientific fields and discursive battles that made Bourdieu into Bourdieu or turned Foucault into Foucault.<sup>6</sup>

In sociological terms, the opposition between Gower and Elsevier could be thought of as an inherent conflict between work and capital. Currently, the channels for communicating research are *not* owned by the ones that produce them. That global publishing companies are in the quest for financial return should hardly come as a surprise. What is at stake here is nothing less than the on-going commodification of research and research results.<sup>7</sup> If this is true then research ideas and methods that are believed to benefit the market of publications and citation are, in the long run, likely to be favoured by the wrong reasons.

In such a climate of scientific publishing, scholars are encouraged to embody more of an entrepreneurial vocational identity that ‘produce’ and ‘sell’ research rather than a curious scholar or team of scholars, that in disinterested modes, seek a better understanding of the world they live in. Having outsourced essential aspects of the valuation of knowledge to blind bureaucratic regimes of quality assurance, it is arguable if scholars can be seen as a profession at all.<sup>8</sup>

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<sup>6</sup> For Bourdieu’s own view on scientific translations see: Bourdieu, 2000

<sup>7</sup> Hasselberg, 2012

<sup>8</sup> Abbott, 1988.

Research runs the risk of becoming transformed into a textual commodity just like any other, where academic journals come to symbolize the privatization of knowledge by a *pay-to-view* logic.<sup>9</sup> Returning to the questions raised in the beginning of this text, it is about time for more scientific communities than the mathematical one to stand up for more autonomous conditions of assessing *quality* and making research results more easily available for the public as well as among scholars. We also need to develop new non-commercialized models of academic publishing.

In this first issue of *Confero* a series of papers target the market of academic publishing and the way the notion of quality is currently fabricated within and outside of this craft. Traditionally, the role of journals is seen as securing the quality of research through professional evaluation and to promote the dissemination of scientific discoveries, argumentation and results. Reading the contributions to this number of *Confero*, we understand that it is more to it than that.

Providing a both personal and political opening to this issue, Professor Ylva Hasselberg describes the current age of ‘economic planning and regulation of science’ and what consequences this has in terms of a conceptual change and redefinition of the notion of quality. Referring to the seminal work of Karl Polanyi,<sup>10</sup> Hasselberg claims that ‘the freeing of

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<sup>9</sup> Recently, opposing the locking up of knowledge in academic journals, Aaron Swartz tried to make articles in JSTOR public by downloading entire archives at the Massachusetts Institute of Technology. Prosecuted and under the threat of a 35-year sentence to prison, Swartz took his own life earlier this year. See Schwartz, 2013

<sup>10</sup> Polanyi, 2001 [1944]

the market mechanism' in fact needs comprehensive regulation and that, in the emerging market of academic knowledge, this regulation is heavily dependent on bibliometrics with its shallow and superficial quality-concept. She invites us into the professional life of a historian, pointing to the salient role of 'non-selective' and 'non-instrumental' reading as a necessary condition for original thoughts to emerge and, consequently, truly novel research findings. Hasselberg raises serious doubts regarding the actual quality of the emerging quality assurance systems, particularly in terms of all non-measurable elements. The on-going initiatives on an 'utilitarian' culture of reading thus risks promoting stupidity: 'If you only read things that are of certain use, thinking about waste, you read too narrowly, and will be more stupid as a result.' Concluding her essay *Drowning by numbers*, Hasselberg asks herself and her readers if reading 'will have to go underground' from here?

In the next essay, Hasselberg's doubts regarding the measurability of quality is further elaborated and discussed by Sven-Erik Liedman. Liedman's essay *Pseudo-quantities: New public management and human judgement* traces the current quality assessment system to the introduction of conjoined management models within academia and, above all, the prevailing effects of New Public Management (NPM) as a dominant ideology. In response to the current obsession of quantifying the unquantifiable, Liedman launches a new concept: *pseudo-quantities*. Contrary to real quantities, that does inform us about 'the number, weight or velocity of something', pseudo-quantities are, according to Liedman, best seen as 'a quality that can more accurately be characterized verbally (either by description or by more expressive means)'. Emerging as a steering-mechanism within the wider tenets of neo-liberal governance, New Public Management (NPM) and the

deployment of pseudo-quantities is not at all limited to the universities. Even though Liedman gives several vivid examples of how pseudo-quantities are launched from inside schools and universities – for instance in the form of league-tables and student grading – the full scope of his argument is even more far-reaching than that. Especially within countries with a big public sector gradually transformed by the dominant NPM-doctrine, pseudo-quantities are possible to identify in a growing number of professional fields as, for instance, within the sphere of medicine and law. Liedman argues that the deployment of blind quality measurements, under the pretext of efficiency, actually *de*-professionalises work. In hospitals, efficiency-rates become more important than the actual symptoms and in schools it becomes more important for the kids to learn how to spell ‘critical thinking’ correctly, than to act and think this way.

Returning to the scientific trade: research ‘output’ is commonly measured and evaluated when researchers apply for funding. This is the point of departure in Professor Diana Hicks’ essay *One size doesn’t fit all* where she provides an overview of differences in scholastic output from various disciplines and countries. Hicks presents an analysis of publication practices in contemporary history, focusing particularly on the differences between natural sciences, social sciences and the humanities. Hicks shows that while natural scientists primarily publish in international journals, social scientists have a more varied publication pattern that encompasses books, national journals and enlightenment literature. Because research outcomes today are based predominantly on citation incidences in international journals, Hicks argues that the social sciences and the humanities are disadvantaged structurally. That national evaluation systems, designed in a one size fits all form, adapt better to the practices of natural scientists than others is not a

new argument. What Hicks' contribution illustrates clearly though, primarily by synthesizing a lot of research carried out in various fields and across geographical locations, is that future evaluation systems that fail to incorporate the variations in forms of publication will risk deteriorating the publishing traditions established within the humanities and the social sciences.

The next piece is this number, *Managing your assets in the publication economy*, is written by the bibliometrician, Ulf Kronman, who has developed a 'survival kit' for researchers in this age of digital scientific reproduction. Kronman provides a highly pedagogical account of how academic publications are used to assess impact and quality in research. By outlining the different steps of what happens to an article *after* publication, Kronman offers proficient insights to a scientific community that is increasingly guided by parameters, ranking lists and impact factors – but have not yet been fully familiarized with the rules of this game. Kronman's text shows *where, how* and *what* is being counted as valuable in the publication economy. Apart from advising individual researchers how to navigate within the system and maximize the exposure and dissemination of articles, Kronman challenges the researchers to come up with more apt ways of evaluating knowledge production. In a frank remark, Kronman summarizes the current state of affairs in following way: *'In the urging need for something to measure, governments and university managements turn to what can be measured, rather than what should be measured, since no one seems to know the answer to the latter question.'*

With the first four articles identifying and problematizing core symptoms of how research and knowledge production is being governed, the final piece of this issue discusses one possible

strategy for rupture or circumvention. Against the backdrop of previous interventions, Walter Mignolo discerns further layers of the fabrication of knowledge and traces the dominance of certain languages as mediums *par excellence* of both thinking and writing to its imperial legacy – English, French, Spanish etcetera. According to Mignolo, this legacy is witnessed in, for instance, the current hierarchies between publishing languages (journals in English tend to be higher ranked than journals in Russian or Thai) as well as in relation to the theories we teach and draw upon (say European philosophers from Aristotle to Foucault; from Plato to Marx). Against this background, Mignolo encourages us to be disobedient by regarding knowledge as geo-politically situated within given contexts, where the power balance that straddles different parts of the world influences whether a certain view of knowledge is ascribed global reach or remains ‘local’ or ‘domestic’. As an example, Mignolo points out how theories produced by Western philosophers – Foucault, Bourdieu, Derrida, to name a few – travel around the globe; contrastively, the presence of Asians, South Americans or Africans tend to be slim to none within the western academia. Challenging the hegemony of Western cultural institutions more broadly – including the universities, publishing companies and the entire knowledge-base produced from within the modern European languages – Mignolo launches another option, a *decolonial* one, where the knowledges of those who have been marginalized and gagged by European macro-narratives are brought to the fore.

### **The journal *Confero*: Essays on Education, Philosophy and Politics**

Having summarised the themes of the inaugural issue of *Confero* above, we want to briefly include a few notes on the

vision of this new scientific platform. This journal came to life as a collective endeavour by a group of Swedish doctoral students whom all, albeit originating from different disciplinary backgrounds, were at unease with the emerging regime of the scientific economy of publications and citations as well as the templates of mass article-production.

*Confero* aims to provide essays in the field of education that do not stay faithful to the hegemonic format of a 'scientific article'. Often very narrow in scope, most scientific journals enforce 'economic' modes of expressions, such as employing an alliance-signalling argot, inserting a fast, limited and recycled review of previous research, and twisting the overarching line of argumentation very modestly. Consequently, *Confero* challenges the mainstream reliance on form and structure to guarantee quality in social scientific writing and provides a new space for essayistic writing in the area of education. For us, high academic quality requires consistency and persuasiveness, rich and thick descriptions and reflexivity. By focusing primarily on essays related to education broadly defined, we hope to receive contributions that are not only stringent and systematic, but also beautiful, esoteric and profound.

The issues and problems related to the emerging economy of publications and citations, that are the theme of this first issue, are scrutinized from different perspectives both in terms of method and theory. Apart from drawing attention to the ongoing transformation of scientific publishing, the more long-term aim of *Confero* is to provide a space for critical inquiries at the crossroads between education, philosophy and politics. Launching this journal, we hope that the pluralism evident in the first issue can signal a broad enough space for scholars to feel welcomed to submit essays to *Confero*. By bringing together

social scientific research that often is kept apart – by that very publication system that this issue has taken as its primary focal-point to scrutinize – we wish to simulate academic debate as well as to challenge the current state of academic affairs.

*Confero* will be a peer-reviewed open access journal, available for free to people engaged in social science research as well as a wider intellectual public. To be accepted for publication, the essay can be written from a wide range of theoretical perspectives and academic traditions. We particularly welcome a broad range of empirical sources used to explore the issue or phenomenon at hand: unconventional sources such as art works, pictures, movies as well as conventional empirical material like interviews, ethnographies or statistics.<sup>11</sup> We hope you will enjoy the collection of papers in this inaugural issue and we look forward to your future contributions – be it as author, reviewer or reader. It is with great excitement that we hereby launch the first issue of *Confero*.

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<sup>11</sup> Although web technologies have made the integration of sounds, images, text, and pictorial animations possible, mainstream scientific publishing has been failing to leverage such possibilities for research communication. *Confero* expects this dominant use of text in research dissemination to shift gradually, and hopes to be at the forefront of this development.

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## Drowning by numbers: On reading, writing and bibliometrics

*Ylva Hasselberg*

**T**he purpose of this text is manifold. The primary purpose is to look into the effects of marketization of academia on the reading habits of academics, which also demands a problematization of reading and its role in the process of creating new knowledge. The second purpose is to discuss and problematize the citation as a sign of intellectual debt. And the third, but not least important, purpose is to write a text that demands the reader to *read* in a manner that is necessary to learn, instead of writing it in a manner that is adapted to promoting “citability”. And so of course, what I would like more than anything to teach the reader is that the only possible way forward, the only method of reproducing real scholarship in a commodified setting, is to *live it yourself*. This way of writing a text is my way of living real scholarship. If this does not agree with you – don’t bother citing me.

## What I do

Let me describe my work to you. I am not at all sure whether you are interested in my work, but I suspect that you can gain some satisfaction through comparing my work process with your own. It is always good to be given a point of reference from which you can reflect on yourself, isn't it?

I'm a historian. There are many ways of being a historian. Still, there are certain values connected to the concept of good work that are particular for a historian, and that historians in general agree on. Historians are usually solidly empirical people. It is not uncommon that they feel an obligation to analyse a particular historical context in its totality. Historians often want to turn over every scrap of paper that relates to their question before they attempt to answer it. They feel a deeper need for realism, which is bound to the issue of facticity. What do I actually know about this historical situation? Have I laid eyes on all the relevant documents that can be used to gain more knowledge of this context? Did I manage to find the relevant literature? Is something missing? Not only very old-fashioned historians, who work without questions that are theoretically anchored, fear this. I do, although I pride myself on working very consciously with questions that are anchored in general societal and existential issues, and do a lot of work with my interpretation. I think this fear resides in scholars from many disciplines. Maybe it is stronger in a historian, for reasons that have to do with our often studying something that has ceased to be and that we did not ourselves experience. What do I actually understand of Sweden in the 1930s? Am I even sure what people had for breakfast?

As information grows in sheer volume, the historian's task grows harder. Being a medievalist poses serious challenges in

terms of not having a lot of information. Most things we cannot know, or we have to use our deductive powers to reason ourselves to a point where the ground seems stable enough to make a (modest) claim. A historian like me, who nowadays studies the 20<sup>th</sup> century, has the opposite problem. There is too much bleeding information! You have to narrow your focus in order to be able to satisfy your ambition to really have penetrated the subject you work with. (It is of course possible to write syntheses or course books, but then you rely on what others before you have written, so let's hope that other historians have done solid empirical work, shall we?) Not only can I, as a historian, not study the general development of party politics in Sweden or "Swedish culture during the early modern period", but it is sometimes even hard to write about one single individual. Say that I would study the Swedish economist Eli F. Heckscher (which I in fact do). Do you know how many texts Heckscher published in his lifetime? 1148.<sup>1</sup> In order to portray Heckscher as a writer, it seems I have to do a lot of reading, doesn't it? Do I have time to read all he has written in the three years an average research project lasts? I would have to read almost a text a day, and he wrote some very substantial works, that man. If I thought that maybe Heckscher is not so important, I would rather study the sociologist Max Weber who has had a much more profound influence on 20<sup>th</sup> century society, my problem would be even greater. So many others before me thought Weber was important, and I have to read what they have written in order to be able to stand on the shoulders of giants, so to speak. If 4000 people before me have written books on some aspect of another of Weber's works, this calls for some afterthought. To be able to reinterpret, I have to absorb previous interpretations, don't I?

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<sup>1</sup> Eli F. Heckscher's bibliografi, 1879-1949

So, it takes a lot of reading to be a historian, or indeed a scholar of any kind. It also takes more and more focus and specialisation, as a result of the increasing number of available texts. Some say historians have become very boring nowadays, partly for this reason. Each does a small piece of the total puzzle. There lies some truth in this accusation. Specialisation tends to diminish breadth and limit the level of generalisation. If I know everything about Heckscher's critique against mercantilism and nothing about the rest of his work, about his immediate context or Sweden in the early 20<sup>th</sup> century, there are lots of connections I can't make. If I as a historian hesitate to draw conclusions regarding anything that is socially or existentially important, because I shun speaking about what I don't think I know enough about, for the sole reason that I cannot claim to have mastered it totally, then it is no wonder if few people find what I read interesting. Contextualisation is essential to problematization. Problematization is essential to analysis. And analysis is essential to generalisation.

## **Reading and the hermeneutic circle**

Reading. Reading is a task that is too little discussed. A PhD education often contains courses on qualitative and quantitative methodology. Some also contain courses on how to write scientific texts. But there are no courses on reading. Yet the art of reading is more fundamental, I would say, to good scholarship than all other things we do. There are many types of reading. A historian has to master a number of reading techniques: from browsing a book in order to gaining a grip on the general argument, to the application of ingenious techniques to find a particular fact that one is sorely in need of. There are even ways of reading that equal "not listening to" somebody, or even "not hearing what was said because you were thinking of

something else” and that must be deemed a total waste of time. One also has to learn to deconstruct other scholars’ arguments through reading, for example to discern which concepts in a historical argument are the analytical tools of the scholar, which are historical categories, and which are simply everyday language, behind the use of which lies no problematization.

One aspect of reading that shapes the choice of texts as well as the approach to them is the motive for reading them. Looking for support for a thesis gives rise to a different type of reading from that done in order to gain a general orientation in a field. The reading process is often done with an eye to how a text can be used in one’s own work. In a very general sense, this is of course always so. Even a very general aim of furthering one’s own education means that the reader believes this is something one will benefit from, in some way or another. But to read in order to become more learned or out of curiosity is still something very different from reading only that which is of immediate use in one’s own text. The latter reading strategy is much more instrumental.

The number of texts available to the student of a particular subject influences the reading strategies. I have already mentioned that more sources and texts give rise to more specialisation, when it comes to choice of research topic. It also leads to a more focused reading strategy. Choices have to be made in order to bring the reading list down to a manageable size. This can be done in many ways, and the methods often relate clearly to the reader’s definition of good science. Empiricists tend to read everything they can find on the empirical category they are studying, say, all about medieval Swedish churches. Another strategy is to base reading on an empirical generalisation or an analytic category. Whatever strategy you apply, a choice has to be made. In my experience,

most people around me combine a number of reading strategies, of which perhaps the most underestimated is the strategy to read what other people recommend you to read, and to prioritise according to how much you trust the recommender. Reading strategies also tend to vary over time. The further along you move in your work, the more instrumental the reading becomes. The most selective reader is a PhD with three months to go before the defence of the dissertation. The more time pressure there is, the more selective the reading becomes.

Reading non-selectively works for me as a kind of revitalizing bath. It often gives rise to parallels in time or space. It allows your mind to wander freely. Non-selective reading gives birth to ideas. There is no telling in advance whether an idea will come out of reading African women's history or a cookbook. I think of this phenomenon in terms of my mind being a bit like a big cupboard. Non-selective reading is like opening drawers in this cupboard that you weren't aware of before or that haven't been open for a long time. In reality, ideas are probably not "new" in the sense that they were not there before. Reading just brings them out, and it also helps you combine things in new ways. When there is resonance, something happens. What you do when you read like this is also that you store things in these boxes that can come in handy later. It is very seldom that something I read doesn't come in handy sooner or later.

A particular type of non-selective reading is essential to the qualitative analyst, as it is a prerequisite for the hermeneutic circle. To me, this type of reading is connected to archive work. Let me take you back to Eli Heckscher in order to explain. A historian's problem, or one of them, is, as I said, time. I was born in 1967, and my first memories are from around 1970. I don't have firsthand knowledge of *anything* that happened before. Historical sources tend to emanate from organisations,

as it is institutionalization and organisation that casts off documents that are preserved. Such documents tend to yield certain types of historical facts, like decisions, rules and registers, but also, if you are lucky, they can be used as a source of thoughts, actions and material conditions in a broader sense. The archive of Eli Heckscher is largely a letter archive.<sup>2</sup> It contains about 150 volumes of letters, all in all, I would guess, maybe 30 000 letters or so. It is possible to approach this material in a number of ways. What I am doing now, as part of the process of writing a biography, is reading all the family letters. This is of course a step in the process of understanding Eli Heckscher. But it is also like bathing in the life of late 19<sup>th</sup> century Jewish bourgeoisie. The important consequence is perhaps not primarily all the facts that are available, but the increasing feeling that I can relate them to a totality, a perceived image of this life and this time as it was seen by the historical actors. I am beginning to see sense in it, to discover patterns and to become submerged in the material. It is like stepping through C. S. Lewis' wardrobe or standing on the frame of the painting, looking into a the picture and seeing it coming alive.<sup>3</sup> The sheer richness of the material does this to you, if you let it.

Interestingly enough, when this happens, when I sit on the train home and feel almost invaded by the life experience of the Heckschers, it doesn't mean that details, individual facts, become blurred. It means – and this is central to the hermeneutic circle – that they become so bright and clear that they almost start to glow, that they are filled with meaning, in

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<sup>2</sup> Eli F. Heckscher's archive, L 67, Royal Library

<sup>3</sup> The metaphor of stepping into a wardrobe and ending up in another world is used by C. S. Lewis in *The Lion, the Witch and the Wardrobe*, 2001 [1950]. The metaphor of looking at a painting and seeing it come alive right in front of your eyes is from *The Voyage of the Dawn Treader*, 2001 [1952], by the same author.

relation to the context. After having read Rosa Heckscher's 1290 letters to her son, I know a lot about Rosa Heckscher. I know her views on a variety of political and social topics, her shopping habits, how she dressed and a few hints of how things were between her and her husband and children. These are things that a biographer would be interested in. But I also know things that are seemingly unimportant or secondary, little details that in some cases, suddenly lead to insight. Ponder what it is to have toothache. Rosa Heckscher suffered from toothache. Knowing what it is to have toothache and how it affects you, I believe it important that she (and a lot of people around her) were actually in pain a lot of the time, because of the damned teeth, until they were pulled out. It is a insignificant thing of course, but I am not sure it was for her. Here's another detail. She always filled the paper. She really economised that way. Other letter writers leave half of the sheet blank. They don't bleeding care that they pay postage for space that they don't use for communication. Rosa does. She doesn't waste space. I have thought a lot about this. How should it be interpreted? Is she mean? Doesn't want to spend pennies? Perhaps. She certainly often comments on postage, claiming her intention not to fatten the Royal Post Office. But there is something else too. Something that is related to morals. There is an inherent moral recommendation in this. A life has to be lived responsibly. Waste of time or resources cannot be accepted. Ever. I recognize a possible similarity between me and Rosa in this instance. Her use of paper makes me think of how it feels to live a life and place all these demands on yourself. Her life becomes even more interesting because it gives me reason to think about my life.

So, all in all, I want to claim that in order to give the rest of the world texts that are filled with insight, and able to communicate this insight to the reader (presuming there is a reader who is

willing to read this text on its own premises), I, as a researcher, have to do a lot of reading. Some of this reading has to be non-instrumental; it has to be undertaken not with a view of corroborating what is already known or finding support for a hypothesis, it has to be exploratory and open-minded. It must also be allowed to take time. Reading takes a lot of time. Reading necessarily includes a lot of waste. I now contradict myself. A moment ago I said that reading is never wasted in the long run. Having pondered Rosa Heckscher's world and seen myself reflected in the mirror, I want to modify and clarify. It is my experience that things you read come in handy sooner or later. But I do not think that it is good to read economically, so to speak. If you only read things that are of certain use, thinking about waste, you read too narrowly, and won't be any more knowledgeable as a result. Reading must be done lavishly and even sometimes irresponsibly, not in the manner of the "spirit of capitalism"<sup>4</sup> but more in the manner of the passionate amateur or the extravagant aristocrat. Reading has to be done without an eye to the aspect of efficiency.

## **Work, time and efficiency**

Let us now turn to the matter of time. "Time and tide wait for no man", it says on a postcard I have on the door to my office. I'm an inveterate time manager. Always keeping track of time, always planning. I don't really like this in myself. I believe it is something that has to be kept within bounds. It has to do with control, and so the problem is to try to control the need for control. Time cannot be controlled, it just happens to you. The

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<sup>4</sup> "Spirit of capitalism" is the concept used by Max Weber and Werner Sombart to analyse rationalism as a consequence of or a prerequisite to capitalism. See Hasselberg 1998, p. 19-28.

management of time should not be allowed to govern what I do, not fully at least. I just said that above, didn't I?

Still, time is interesting. It forms a very concrete limit to things. Researchers stretch it a lot. We are always late. This is something that is seldom explicitly stated as a central aspect of academia, but it is. We are always late. In teaching, we are not late to the extent that we are late in our research. But even there we are late. We come to class on time, but we do not always finish the lecture on time, we finish it when it is finished, that is, when we have said what we wanted to say. When it comes to research, we are quite frankly hopeless. We do not finish our research projects when we said we would. Something got in the way. Either we didn't start them when we should have, because we were finishing something else that should have been finished a long time ago, or we discovered things (books, sources, new hypotheses, new questions that begged to be answered) along the way that took more time than they should have. This also happens when we write texts. Everybody is always late. Deadlines are seldom kept. Researchers are time optimists that act as if time can be bent to fit the internal logic of research, which is an activity that cannot be entirely planned because you go hunting for the unknown. Of course this does not affect the flow of time. But neither is the flow of time allowed to shape exactly what we do. What we do is dependent on the work process and how it evolves. Oili-Helena Ylijoki, who has written an insightful article on academics and time, calls the favourite time of academics, the time that is spent losing track of time, "timeless time".<sup>5</sup>

Still. Time is there, and it certainly waits for no man. There are 24 hours in a day. No more. No less. How should these hours

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<sup>5</sup> Ylijoki and Mäntylä, 2003

be spent? How do we spend them? Personally, I sleep eight hours. I know people who sleep less. This gives them more time for their research. There is an entire academic folklore relating to time. Some people are said to sleep very little. Some write articles in the airport. And this is why they are so productive. (I don't know if it is true. But it sure renders them a tint of heroism.)

Let's say, for the sake of argument, that work takes eight hours a day. How should these eight hours be spent? How *are* they spent? Is a historian's working day in 2012 the same as it was in 1962? There are, that I am aware of, no quantitative studies that answer this question comprehensively. What can be stated is that university faculty spends more time writing research proposals and with certain administrative tasks that are related to auditing.<sup>6</sup> We spend time applying for money and we spend time being evaluated (and evaluating others). The issue of the administrative burden is hotly debated; it is one of those things we discuss during the coffee-break, and it never ceases to interest us. So, a hypothesis could be that there is less time in general for research and teaching. What then do we do with the time that we have at our disposal for research? What should we do with it? If I ask the reader this question, we could have an interesting discussion about it. This discussion would probably reveal both the scientific norms and the work norms of the discussants. Still, one thing we must agree upon. If more time is spent on one thing, there is less time for another. So, if we

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<sup>6</sup> On the general trend of auditing as a tool for handling risk and making decisions, see Power, 1997. On the epistemological foundation for this trend, see Poovey, 1998. See also the article by Sven-Eric Liedman in this issue on "pseudo-quantities". On the issue of the time spent by academics on administration and accessing funds, see for, example, Morris, 2000, Ylijoki and Mäntälä, 2003, Djelec, 2012.

spend more time writing, for example, there is less time for reading.

These days we live in the age of the market; science as a market for knowledge. No, I take that back. Just saying it becomes a contribution to the victory of a great lie. We do not live in the age of the market. We live in the age of economic planning and regulation of science that goes under the “brand name” of market and gains its legitimacy from the market. To illustrate this very complicated claim, I want to compare this historical situation to the one described by Karl Polanyi in *The Great Transformation*. I read this book when I did my thesis and it has influenced me greatly ever since. Polanyi’s thesis is that the freeing of the market mechanism from its embeddedness in the historical and social context of early 19<sup>th</sup> century England actually took a lot of regulation, not a lot of laissez-faire. Deregulation was undertaken to support the market mechanism, which means giving the supply and demand mechanism a chance to work according to its own logic. In that particular historical context it meant that the price of labour should vary according to the relationship between supply and demand, so that, for example, when supply rose, the price (i.e. wages) should decrease. So, in essence, as an alternative to starvation, people were willing to work for what they could get. At the same time, and this is an important part of the argument, whenever the market mechanism needed regulation to support it, regulation followed. For example, it took a lot of regulatory effort to prevent the formation of trade unions. So whenever the market mechanism needed active deregulation it followed, and whenever it needed active regulation it followed. Laissez-faire was never part of the game.<sup>7</sup>

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<sup>7</sup> Polanyi, 1989 [1944]

The situation in science today is very similar. Political agents and organised bodies all over the world are in the process of creating an efficient market for scientific knowledge. Knowledge, seen as the prime form of capital, is a tool for creating wealth and also the prime asset in promoting competitiveness in a global economy.<sup>8</sup> This does however mean that the call for management is on the rise, and not that the call for non-intervention increases. Governments want their national universities to increase their productivity. This means that they want to spend less money per unit produced. They want teachers to teach more students for less money. They want less waste of time and resources. They want researchers to be more productive in terms of publication. Above all they want research that contributes directly to the GNP. Private enterprise wants the same, but private enterprise also wants research that contributes to profit. The target for all these expectations is the institution of the university, and even more so now than 30 years ago, as big business has to some extent outsourced research to the universities, closing down their own R & D departments.<sup>9</sup>

How does one go about creating an efficient market for knowledge? One thing that has to be done is that actors must be encouraged or even forced to act as rational market actors. This means that the suppliers of knowledge must be exposed to demand. How does one expose research to demand? If university faculty is notorious for being late and caring more about the internal demands of the investigation, it means that they are not susceptible to demand. Scholars act more like the Russian peasants in Chayanov's classical study of the peasant

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<sup>8</sup> Andersson, 2010

<sup>9</sup> Mirowski and Sent, 2008, Pestre, 2010, Mirowski, 2011

economy<sup>10</sup>; they work when they need to, not keeping certain hours, they do not especially care for planning or bookkeeping and they think of resources and time as assets that have to be assembled in order to be able to continue their studies to the point when they feel satisfied with what they have accomplished, when it is good enough. The foremost problem is seldom “How do I spend the exact amount of time and resources that I have in order to get a result?”, but rather “Where do I find time and money to do this interesting thing?”.

The problem with the peasant economy from the viewpoint of Stalinist Soviet Union was that the kulaks could not be relied upon to produce food for the market. (It also became Chayanov’s problem when he was shot after a secret trial in 1939, his book having been understood by Stalin as a defence of the kulaks.) The problem with scientists and scholars is that they cannot be relied upon to produce knowledge for the market. In order to make this happen, it is necessary to get at the definition of quality. This is the only way that scientists and scholars can be convinced to act as rational agents in a market of knowledge. They have to be taught to redefine “interesting” or “good enough”.

This is what is going on at the moment and one of the technical devices for making it happen is the bibliometric system<sup>11</sup>. The bibliometric system helps in making, as Philip Mirowski so

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<sup>10</sup> Chayanov, 1966

<sup>11</sup> I have been made aware that my concept of a ‘bibliometric system’ is an idiosyncrasy, and so it has to be defined. I define it thus: the bibliometric system is the system centred around bibliometrics as a technique, and consisting of the main components 1. bibliometrics, 2. publications that are subject to measurement, 3. a market for scientific journals, 4. agencies and organisations that use bibliometric measures for evaluation, and 5. actors who are involved as producers or consumers in this system.

aptly puts it, “the market mechanism a processor of information”.<sup>12</sup> If this happens, quality will be redefined, not so that the question “Is it good?” ceases to be asked, but so that it is *understood as* “Is it in demand?” We are talking here of a conceptual change that is the necessary prerequisite of the successful commodification<sup>13</sup> of science.

## **Bibliometrics and quality**

The basic node of the bibliometric system is the citation. There is a variety of bibliometric measures, and they not only measure citations, but the central measure is the citation.<sup>14</sup> What then is a citation? Every scholar knows this of course, but I think it important here to verbalise this tacit knowledge. A citation is when you mention what someone else has published in your own text. It is the counting of such references that is the fundament of bibliometrics.

The reasons for mentioning someone else’s writings are manifold. I think it is not too bold a guess to conclude that there are a number of different citation cultures, according to discipline but also according to local varieties. The sheer fact that there are varieties in how references are written probably contributes to this. Does the Oxford system lead to the same references as the Harvard system? It does not, according to my experience. The technique limits expression. It is also plausible to assume that the character of the discipline and the problem affects the references. If a discipline is very cumulative, producing a lot of “normal science”, it probably produces more

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<sup>12</sup> Mirowski, 2011

<sup>13</sup> On commodification in schools and in academia, see Ball, 2004, Ball, 2007, Hasselberg, 2012a and 2012b, Ankarloo, 2012.

<sup>14</sup> Elzinga, 2009

references to other research that one directly builds on than a discipline with, let's say, contested opinions. The latter should lead to more references that are negative, i. e. showing distrust rather than trust in previous research. A well-researched area should lead to more references, but perhaps fewer per existing text treating the subject, than an area which is relatively new. To this one could add individual taste. Some people use a lot of references, and others do not. This of course – at least to some extent – has something to do with reading habits. I think it is safe to say that the number of citations a text will get will depend on a number of factors, including how original and “wild” one's scientific claim is. And originality is most often not a good way of gaining social recognition. I think there are at least two possible instinctive reactions to a truly original work: incomprehension or usurpation. Neither results in references.

The crucial question is of course why people cite things. There is not a lot of work on this. Susan Cozzens, who wrote an article on this issue in 1989, claims that the majority of references have a rhetorical character. Their aim is to convince the reader that the author is right, the aim is not recognizing intellectual debt. Persuasion is done largely with the help of references.<sup>15</sup> People cite authorities and group them like armies ready to back themselves up. Negative citations can also have a rhetorical function. Chop someone's head off in order for your own work to seem worth more. A particularly sophisticated form for this is to give another person credit for a detail while totally neglecting the central result of the text. I also believe that many citations fill a social function; they are there to show where you belong and which other scholars you like or feel affiliated with.

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<sup>15</sup> Cozzens, 1989

I have two analytic remarks regarding the value of a citation that I have not seen previously, (and so I do not need to write a reference). One is that the quality of a citation can never be better than the quality of the reading. When people do not understand the full meaning of a text the risk of writing a very silly reference to it increases substantially. So, the mastery of reading is central to the meaning of a citation. Two is that the value of the citation to the person who is cited is dependant on the value of the work in which the citation is located. Getting a lot of praise from people whom I consider to be wrong or bad scholars, I do not rejoice. Why should I? Ergo: being cited in an appreciating way for qualities that your text does not have or as a result of misunderstanding by someone whom you do not hold in high esteem is not a good thing. It is a cause of grief and sorrow.

Bibliometrics is however a quantitative science. This means that such distinctions are not relevant. There are ways of handling certain problems with citations, such as self-citation, but there is no way of taking the intentions or habits behind a citation into account, and even less so the quality and characteristics of the citer. A citation is taken to be a sign of intellectual debt and each citation is assigned the same value. If you are cited people like your work and find it useful. (If you are not cited your work is of lesser value and has not contributed to the greater whole.) This makes it very easy to misunderstand citations in a very particular way. Citations in general can be taken to be the equivalent of demand in a market. Your texts are published and the number of citations they get are a tool of finding out if they are in demand. The more citations, the higher the demand and, according to the model, the higher their market value.

So, if we want to push researchers to produce texts for a market of knowledge, is there any way we can make use of

bibliometrics? Yes, of course. We can tie resource distribution to bibliometrics. 1. We can start by awarding research funding according to merits measured, grossly, with the help of bibliometrics. Or we can skip bibliometric analysis and just crudely state that the more people write and the “better” their publications are in terms of the bibliometric status of the journal, the more they deserve to be funded. 2. We can do the same with tenure, rewarding people who are “well published”. As a consequence of a high market value of your texts, YOUR market value rises. 3. We can award resources directly to the universities in a similar way, encouraging vice chancellors to think a little about how to make their researchers more productive. All these methods are in use in the Swedish university system today. In some disciplines, steps 1 and 2 of this process are more or less taken. 1. Funding has little to do with your ideas; it has to do with your publication record. In medicine and some natural sciences, people write proposals on the basis of studies they have already done. And then they use the funding they get to do something else, which they can use as a basis of the next application. In arts and social science, however, it is still more a matter of looking at proposals, the quality of ideas, and of reading and judging the quality of what people write, but this is changing fast. Furthermore, 2. Giving someone a job is in some disciplines a question of computing the list of publications. In other disciplines, like my own, again, judging quality is a matter of reading and assessing what is being read.

Counting solves many problems, problems that exist on various levels of the academic system. On the political level, there is a general problem with evaluation of output in the public sector, of which Swedish universities are a part. This is a consequence of changes in how government agencies are controlled (I refuse to write “governance” here, a deeply troubled and highly

ideological concept). Evaluation as a practice has the general characteristic of being based on quantities. And so, output measures have to be constructed, and publications are very suitable for counting, as are citations. Inside the university, we have problems that have to do with distribution of resources between faculties and disciplines. On this point, it has become more common to solve these problems with the help of bibliometrics. And lastly there is a heap of problems on the level of the individual. There is the time-consuming job of reading and assessing the works of job applicants. It can be shortened and time can be saved by resorting to bibliometrics. And a certain sense of fairness and legitimacy can also be created on the individual level, just because counting limits individual judgment. No more do we have to rely on the “right” referees on order to be judged fairly. Mechanistic objectivity, as Theodore Porter calls it, creates a (false) sense of impartiality.<sup>16</sup>

Wait a minute! Isn't there a problem with my argument? It seems that I am saying that assessment of scientific quality is losing out to counting citations. The counter-argument could be, well, actually, it is only a matter of rationalisation. The bibliometric system contains many readers and assessors, and they are the ones working as editors and referees for scientific journals. Judgment has not been abolished, it has just changed place in the system. Yes, that is a good argument, but it sort of leaves the monograph outside the discussion, doesn't it? And, it is only valid given that the quality of reading in the academic system in general is satisfactory, and it is only valid as long as the decision to publish a text is based primarily on scientific judgment. What do we know regarding this? Well, I should say, very little. There are no qualitative studies that I know of

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<sup>16</sup> Porter, 1991, 1995

regarding the core of the bibliometric system: the publication decision.

There are however two potential problems that have to be addressed openly. One is a bundle of consequences connected to the ongoing commodification process. As bibliometrics has become a cornerstone in a segment of the market for scientific publication, it has also become a tool for making a profit in an oligopolistic<sup>17</sup> market. If a journal has an owner that is a business enterprise, this owner wants to make a profit. It is easier to make profit on commodities that are in high demand than on ones that are not. So, if the articles in a journal are in demand, that is good for business. The temptation to publish articles that seem to have a chance of gaining many references has grown stronger. In leading journals in medical science and natural science, the powerful driving force of profit has already led to problems with published results that are not corroborated or that have been falsified. Overall, the temptation to manipulate the bibliometric measures in other ways has grown stronger, both for editors and authors. Some journals now even demand openly that texts they publish have references to texts published in the journal they appear in; all to create more citations and improve their status in the bibliometric system. Listen to these two arguments:

1. --No, I cannot publish this because it is a thoroughly unoriginal study of the city of London. What you say here has been said before.
2. --No, I cannot publish this because it is about Stockholm, and our readers do not want to read

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<sup>17</sup> Oligopolistic means that a few actors dominate the market.

about Stockholm, they want to read about London or Paris.

The first argument is a very strong scientific argument. You have said nothing new, why should I publish this? The second argument concerns demand. It is about satisfying the customer, the reader. It has absolutely nothing to do with scientific quality.

The second problem is however even more grave. And that is the problem of reproducing scientific judgment itself. As long as the individual actors in the academic system are in principle able to tell (according to the standards of their discipline or school) what a good text is, the problems related to commodification are primarily moral problems. But if we are in the process of constructing a perceived market for knowledge which is linked to a political sphere that places a premium on productivity raises, what then will the long-term consequences be? Can it be that the balance between “input” and “output” in the process of producing new knowledge will be altered? Can it be that in order to write more and publish better, there is less and less time for reading? And if we don’t have time to read, how will we be able to educate ourselves enough to a) do original research ourselves and b) discern quality in other people’s texts? And who will read, that is *use*, the ever-increasing flow of texts that is produced?

When I think about these issues, my mind starts its endless activity to try and create a pattern, and then to find words to describe these patterns with. What must eventually result from this seemingly grim scenario, when publication becomes an industry, more or less, is the loss of the *use value* of texts. Not only will reading (and thinking) be defined as activities that *ought*, as input values, to be minimised in relation to writing,

output. (Absurd, I know.) But if we follow this process to its end point, there will be fewer and fewer texts that are written because their author has something to say and they are therefore worth reading. So, the entire enormous cloud of mediocre texts will in the end become decoupled from use in the sense of “read”, “absorb”, “learn”, “gain insight from”. It’ll fly like a gas-filled balloon with no weight to keep it down. The present discussion concerning the “governance of universities” is absurd for many reasons, one of them being just this decoupling of publication from reading. Texts that are not read *and absorbed* totally lack meaning for the scientific community in general. The only value of a text will be its potential for yielding a reference in someone else’s text, with as little time-consuming learning as possible taking place between consumption of it and producing a new text. Reading in the real sense will have to go underground.

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I decided even before I started writing it to call this text *Drowning by numbers*. This was mainly because I like Peter Greenaway’s films and Michael Nyman’s music. When I started out, I quickly realised that the metaphor of drowning was good to think with. Drowning can mean becoming submerged in, absorbed by, taken in by. Drowning in someone else’s eyes is the starting point of falling in love. Drowning in a book happens seldom to me nowadays, but I cherish the moment when it happens. Drowning in the historical sources – that I’ve talked about.

*Drowning by numbers* to me implies ritual as well as quantities. Numerology, and also death, that’s obvious. Ceasing to exist as an intellectual because of a ritualistic drowning in texts that do not give rise to a will to become submerged. Or ceasing to be

because no-one else reads my texts in the way that I want to read other people's texts.

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## Pseudo-quantities, new public management and human judgement

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**I**n this article I will introduce a new concept: *pseudo-quantities*. A pseudo-quantity is a spurious quantity that purports to indicate the dimensions of something but is actually arbitrary and, if taken at face value, misleading. A real quantity informs us about the number, weight or velocity of something and is, in this respect, always more exact than even the most exhaustive verbal description. By contrast, a pseudo-quantity is less nuanced and less exact than a well-informed and conscientious verbal evaluation.

The term pseudo-quantity is new, but the phenomenon is well known to researchers and has become the subject of a rapidly growing stream of literature.<sup>1</sup> The new term would primarily be intended to provide insight into the on-going mechanisms and processes that underlie this phenomenon. Furthermore, this term has a critical potential that is urgently needed at a time

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<sup>1</sup>The literature that addresses the quantification of everything, especially concerning education and science, is overwhelmingly rich. See, e.g., Ball, 2004, Marglin, 2008, Slaughter and Rhoades, 2004 and Hasselberg, 2012

when many individuals erroneously believe that pseudo-quantities can be used to gain both time and perspective.

In the first part of the article, I will present the new concept and its background in the concepts of quality and quantity. The importance of this term is pertinent to the founding of New Public Management (NPM) in the public sector. NPM will be treated in the following section. I will specifically highlight how this process influences education and research. In the conclusion, I summarise my argument and provide insight regarding a possible alternative.

## Qualities, quantities and pseudo-quantities

The distinction between quality and quantity can be traced back to Aristotle. Quality provides an answer to the question “Of what sort?” (*poiós*, lat. *qualis*), and quantity answers the questions of “How many?” or “How much?” (*posós*, lat. *quantis*). According to Aristotle, quantity and quality represent two spheres of reality that could never meet.<sup>2</sup> This opinion remained dominant among philosophers until the pinnacle of the scientific revolution in the 17<sup>th</sup> and 18<sup>th</sup> centuries, when an increasing array of qualitative standards could be successfully quantified.<sup>3</sup> Even more important for the immediate experience of the world, was the massive introduction of quantities in

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<sup>2</sup> The background and history of the concepts of quality and quantity are presented succinctly and in relatively greater detail in Blasche et al., 1989 and Hager et al., 1989. Aristotle presents the concepts in several different writings, most comprehensively in *Metaphysics*, Book V, 13, 1020 a 7-11. See, e.g., Aristotle, 1933, pp. 256-61.

<sup>3</sup> Isaac Newton’s *Opticks*, 1704, represented a real breakthrough, as Newton was able to quantitatively determine the colours in the spectrum as different refractions of light. Thus far, colours had been viewed as proto-typical examples of real qualities that could never be expressed in quantitative terms (cf. Hall, 1993 and Berlinski, 2001).

urbanised life since the late 15<sup>th</sup> century. Double-entry book-keeping was an important innovation, and the development of statistics that increasingly relied on numerical data also paved the way for a spontaneous conception of reality as fundamentally consisting of quantities.<sup>4</sup>

Modern life is imbued with quantities and quantifications. Real quantities provide a more accurate idea of reality than any verbal description. To state that New York City had 8,175,133 inhabitants in 2010 is more exact than to describe how large and crowded the city is. The fact that the speed of light is 299,792,458 m/s in a vacuum reflects the type of precise calculations that neither eloquence nor poetry can ever match.

These successful quantifications have prompted the idea that *everything* in reality can be quantified. Lord Kelvin, the great British physicist, previously remarked as follows: “When you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind.” A slightly revised version of this utterance still appears at the Social Science Research Building in Chicago.<sup>5</sup> The following statement is a more recent example of the same idea: “If you cannot measure it, you cannot manage it.”<sup>6</sup> Here, we are close to the subject of this article. Quantification is an efficient tool that can be used by and for management.

Of course, those who offer such sweeping statements regarding the overall importance of quantifying everything are fully aware that some values in life cannot be measured. It would be unreasonable to attempt to quantify the intensity of a love

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<sup>4</sup> For the massive introduction of quantities in economic and social life, see Poovey, 1998.

<sup>5</sup> Quotations from Kuhn, 1961.

<sup>6</sup> Kaplan and Norton, 1996, pp. 21

relationship. You may claim that Shakespeare (or Dante or another writer) is the greatest writer of all time, but people of normal intelligence know that one cannot quantify his greatness. Economic value can be quantified, but the value of friendship cannot.

To the best of my knowledge, this notion is uncontroversial. The conflict in the actual situation concerns quantification versus well-grounded human judgements concerning extremely complicated areas, such as schools, universities and health care. Pseudo-quantities are most controversial in these fields. The desire for all-inclusive quantification has given rise to a steadily increasing number of bad or spurious quantities, which I will call pseudo-quantities. *A pseudo-quantity is a quality that can more accurately be characterised verbally (either by description or by more expressive means).*<sup>7</sup>

However, it is important to distinguish between two types of pseudo-quantities: simple and composite. The latter type is the most important and the most harmful.

For an idea of simple pseudo-quantities, let us begin with an extremely simple example: a movie review. Movie reviews can be found in newspapers, in magazines and on the Internet. In such a review, a movie is presented, its plot is described and evaluations are given. However, in recent decades, films have also been given a specific mark or grade in the form of one to five stars or other simple symbols.

Such grades represent a typical (but relatively harmless) pseudo-quantity. Ideally, such a review provides a full and rich

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<sup>7</sup> In this definition, I am not taking a position on the controversy between adherents of quantitative versus qualitative methods in social science. For a discussion of this issue see, e.g., Neuman, 2011.

evaluation of a movie. The grade represents an abbreviated form of the full review and may be useful to readers who are hurried; its popularity could then be attributed to the stresses of modern life. First, such grades facilitate quick comparisons with other movies. Second, the stars that have been attributed to the various on-screen movies may assist you in choosing a movie to view.<sup>8</sup>

The grade that is given to a movie is a pseudo-quantity because the quality is better defined by the full evaluation of the movie. The full review articulates the reasons that a particular viewer may view a film positively and why some interested cineaste specifically ought to view this film rather than another film that may be equally excellent.

A composite pseudo-quantity may consist of first-class statistical material. Let us examine one particular example to clarify this notion. In recent decades, university rankings have developed into a flourishing industry influencing students, teachers, politicians and administrators. The two most well known international ranking systems are the *Shanghai system*<sup>9</sup> and the *British World Universities report*, which is organised by

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<sup>8</sup> For example, a classical movie, such as Stanley Kubrick's *Dr. Strangelove*, which is easily purchased or rented as a DVD, is given five stars by most reviewers. This grade means "excellent" or "world-class", but the number does not provide insight into why and how the film is extraordinary. To find answers to these questions, one must consult the review of a well-known movie specialist. On the Internet, you can easily find several of these reviews. Here, I have chosen Christopher Null's review, which provides a much more qualified evaluation of this remarkable movie than the mere designation of five stars, Null, 2004.

<sup>9</sup> The English name of this system is the *Academic Ranking of World Universities* (ARWU), which is organised by the Shanghai Jiao Tong University, ARWU, 2011.

the Times Higher Education Quarterly Supplement. The former system includes hard facts: first, the number of articles that a university staff has published; second, the number of citations in other scientific articles that these articles have received according to the *Citations Indexes*; and third, the number of Nobel Prizes and Field Medals (in mathematics) that have been awarded to faculty members of a university. However, why are these indicators considered valuable? More importantly, the manner in which one factor is evaluated in comparison with another factor is arbitrary. How many articles is a Nobel Prize worth? How many citations?<sup>10</sup> The *prima facie* objectivity gives way to capriciousness.

The *British World Universities Report* bases its rankings on several different indicators but also assigns weight to subjective evaluations. Hence, the reputation of a university is important for its place in the resulting “league table”. This ranking is a good example of a simple pseudo-quality and an extremely risky category.<sup>11</sup> The riskiness seems unfortunate, as the league tables are intended to replace evaluations that are conducted by a committee of experts. According to the spokesmen for the new system, the reason for this intended change is that such committees may obtain results that are biased. This argument presupposes that well-grounded, versatile evaluations are less reliable than a mechanical summation of certain quantities and a pseudo-quantity that may result from a rapid assessment. On

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<sup>10</sup> Furthermore, Field Medals and Nobel Prizes are few (even if the fake prize in economics is included), and it may be partly accidental for a university to receive such a prize.

<sup>11</sup> In the literature in the field, there is a famous example of this risk. Students who had to list the best law schools included Princeton, although Princeton does not have a law school. See: Frank and Cook, 1995

this basis, human judgement is replaced by seemingly exact, but arbitrary numbers.

The ranking of universities is a relatively new phenomenon. However, another type of pseudo-quantity is age-old and likely to survive all of us: school grades. However fine-grained the scheme of grades may be, a good teacher can always more aptly characterise a pupil in his or her own words than by issuing a grade. A grade may be a number, a letter or a short characteristic, such as *laudatur*, but it does not make specific claims regarding a pupil's study results. In short, school grading systems suffer from the same shortcomings as the number of stars that a movie receives.

However, I do not believe that we will ever have modern schools without grades. The reason is simple; we live in a society and a world in which anonymity reigns in most social relations and in which there are more competitors than positions in many careers.<sup>12</sup> Grades constitute a simple method of distinguishing those who may be viewed as superior to others with respect to a certain educational level or position. However, such a process is risky: one cannot be certain that the person with the highest grades will be the best physician, lawyer, or economist. Those with the highest grades may be overly obedient and less original and creative than some of their classmates.

Pseudo-quantities can also indicate other valuable and interesting information. One school may give higher grades than another school, or children from one social stratum may be disfavoured in comparison with those from another stratum. Such results constitute important facts.

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<sup>12</sup> Aas, 2006

Each good quantity in a composite pseudo-quantity may also be of great interest. In this respect, the ranking of universities does not differ from the ranking of, for example, vacuum cleaners in *Consumer Reports* or *Which?* or, in Sweden, *Råd & Rön*. As consumer reports can indicate how well various models can eliminate dust or pet hairs and how noisy such machines are, the number of citations may give you a specific idea of how influential an article is or has been. Furthermore, by studying a *Quotation Index* more closely, you may obtain good information regarding how new specialties in science are formed by studying how a group of scientists (often in different countries) begins to quote the articles of one another and exclude representatives of adjacent specialities.<sup>13</sup>

## New Public Management

Concepts such as quality control, quality assurance and quality assessment have been extremely frequent topics of discussion in recent decades. Even the Japanese *kaizen*, which is normally translated as “improvement”, has found a place in English and other Western languages. These notions were originally used in industrial production,<sup>14</sup> but they are currently spreading to other fields, such as health care, education and research. In the public sphere, these concepts must be viewed as parts of new public management (NPM)<sup>15</sup> that refers to the general policy intended to improve public efficiency through better management and the type of free competition that previously

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<sup>13</sup> See, e.g. Hage and Meeud, 2006, and Ziman, 1987

<sup>14</sup> Juran, 1995

<sup>15</sup> The concept of new public management was coined by Christopher Hood (1991). However, the policy that this concept denotes was formed a decade earlier by politicians such as Margaret Thatcher and Ronald Reagan (cf. Hughes, 2003, pp. 2-4).

prevailed only in the private sphere. Recently, however, it has been posited that even more efficient methods of control – which are typical of digital era governance against the threats of climate change, terrorism and new pandemics – are replacing NPM.<sup>16</sup> Nevertheless, this possible shift has not influenced the central role of quality assessment and control.

NPM was created in response to the former bureaucratic model as described by Max Weber. This system was strictly hierarchical, and its responsibility was to implement the decisions of politicians. Administration was neutral and anonymous and was designed to obey all legitimate masters, irrespective of ideology. By contrast, NPM replaces administration with management. As Owen E. Hughes has remarked, the concept of management is wider than that of administration; the administrator is a servant, whereas the manager has a steering function.<sup>17</sup>

In the 1970s and 1980s, old bureaucracy was criticised for its inefficiency and ineffectiveness, and the state apparatus was viewed as oversized. NPM should be the opposite (i.e., as result-oriented as the private sector).

Of course, NPM can be viewed as an ideology or, perhaps better, as a composite of different closely related ideological tenets. Neoliberalism must be mentioned because it forms the general intellectual (and emotional!) background of NPM. This concept is associated with such names as Milton Friedman,

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<sup>16</sup> Cf. Christensen and Lægveid, 2011, pp. 13. In a textbook, a third model is distinguished as “democratic governance”. However, its contours appear to be blurred, and its influence is questionable. One of its characteristics, transparency, also appears to be typical of NPM, Pasquier and Villeneuve, 2012, pp. 4-11

<sup>17</sup> Hughes, 2003, pp. 6

Gary S. Becker and other prominent members of the Chicago School of Economics. Politicians such as Margaret Thatcher, Ronald Reagan and their many supporters and successors throughout the world have formulated its main theses in a more praxis-oriented manner. International institutions, such as the International Monetary Fund (IMF), the World Bank Group (WBG) and the Organisation for Economic Co-operation and Development (OECD), were also important. The background was a period of stagflation (i.e., a combination of economic stagnation or even recession combined with inflation; this phenomenon is impossible according to the Keynesian doctrine that had dominated until then). However, equally important was an increasing conviction that the then-existing social welfare system hampered initiative and even diligence among ordinary people. All types of collectivism were banned, and the hard-working, endlessly creative entrepreneur was hailed as a paragon.

However, other currents of ideas that are less encompassing and more pragmatic than neo-liberalism also form NPM. First, managerialism must be mentioned. Central to managerialism is the idea that there is no substantial difference between the private and public sectors in that efficiency and effectiveness are equally important to both sectors. The work of a manager is instrumental in serving well-defined goals, but his or her freedom in choosing the optimal means of fulfilling his or her tasks must be encouraged. A good manager may use broad, unconventional methods to reach his ends. Humans are self-oriented (if not openly selfish) creatures who are heavily influenced by different types of financial rewards. Managers are

typical in this respect. *homo oeconomicus* reigns in managerialism as in neoliberalism.<sup>18</sup>

Even more specific economic trends, such as Transaction Cost Economics (TCE), whose fundamental assumptions pertain to man's fundamental economic rationality and selfishness, are mentioned as important.<sup>19</sup> These trends do not change the overall view of the intellectual background of NPM.

However, it has been maintained that the shift from old-style bureaucracy to the new way of managing the public sector is greater in theory than in practice.<sup>20</sup> To respond to this assertion, I will object that the real changes in many spheres have been radical and profound. In the school system and in universities, a completely new climate is currently dominant compared with the situation only decades ago.

## Critique of pseudo-quantities

The enormous success of pseudo-quantities in a society in which NPM is dominant in the public sector indicates that human judgement is being disregarded and replaced with inexact numbers. To sharpen the critique of pseudo-quantities, I must remind the reader of some fundamental concepts in statistics. Categorical variables are of two types: nominal and ordinal.

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<sup>18</sup> For a discussion of neoliberalism and managerialism and their importance to NPM, see Bostron, 2011, pp. 17-30. Bostron traces the roots of managerialism back to F. W. Taylor and his scientific management. This seems exaggerated as Taylor's ideas are more typical for industrial production than for the work within state bureaucracies, the latter marked by rather well-defined rules and duties. (cf. Hughes, 2003, p. 1f and *passim*).

<sup>19</sup> Bostron, 2011, pp. 27

<sup>20</sup> Hughes, 2003. pp. 5

Nominal variables have no natural ordering, such as religious affiliations (e.g., Christian, Muslim, Buddhist) or modes of transportation (e.g., bus, subway, private car). In contrast, ordinal variables are ordered, but the distance between them is unknown. A conservative person is considered more to the right than a liberal person, but a socialist is to the left of both. However, it is nonsensical to seek to determine the exact distance among the three categories. Finally, some interval variables have numerical distances between any two values, such as blood pressure levels or annual income.<sup>21</sup>

Evidently, school grades are tuned in to interval variables as soon as different grades are given a distinct numerical value (i.e., when a *laudatur* is determined as being exactly three times as good as an *approbatur* or an *A* as being a certain number of times better than an *C* or an *E*). Of course, it is nonsensical to claim that a *laudatur* student knows exactly three times as much as his *approbatur* classmate. The values that are given to different grades constitute a type of tacit agreement regarding how grades should be handled (e.g., at applications of different types).

This system may appear to be unproblematic. Everyone is informed about the system, and teachers and students must adjust to it. A scrupulous teacher may still experience difficulties. He or she is significantly better informed about the students and their actual knowledge, their potential and their shortcomings than the grades could ever indicate. In addition to grades, teachers possess a wealth of information regarding their students.

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<sup>21</sup> Agresti, 2002, pp. 1

It may be interesting to compare this problem with the investigations of the spontaneous attitudes of the general public that are conducted by opinion institutes, firms specialising in market research studies and other entities. Here, most people who answer such questions are not more informed than they can express with their simple answers. If some person who is unknown to me inquires about my political sympathies, my ideas regarding public transportation in my home city, or my opinion of some trademark, then my response is sufficient to form a minor but substantial part of a trustworthy investigation of what people generally think about politics, public transportation, or a certain trademark.

Of course, all nuances are lost in such investigations. Expert answers are not worth more than those of ignorant people. A professional assessment of a certain phenomenon is not pursued; rather, general public opinion is sought.

Evidently, if I am asked to give a number between 1 and 10 as an answer to the question of how much I trust the public transportation system in my home city, then my answer must form a pseudo-quantity. However, to the investigators, my arbitrary answer poses no problem because I am only one of some 1000 people who were asked the same question. A city that has ordered such an investigation obtains an idea of the degree of satisfaction of the average citizen with respect to the transportation system. The opinion of experts is not sought; rather, such investigations seek to obtain information regarding general attitudes.

The pseudo-quantities that currently play such a crucial role in parts of the public sector differ from the outcomes of public polls. Here, it is the experts (i.e., the professionals), who must formulate pseudo-quantities concerning central parts of their

field of expertise. In other cases, their own work is assessed with the assistance of pseudo-quantities. The tyranny of arbitrary evaluations can be observed everywhere: in health care, in police service, in the school system, in universities and in other arenas. In various fields, complicated professional judgements are now expressed in terms of pseudo-quantities.

What is the reason for this rapidly increasing need for numbers whose collection and analysis involve an increasing number of jobs for both professionals and public managers? Several arguments are given, such as transparency, economic efficiency, and objectivity in the sense of impartiality. These arguments are the standard arguments that are given for NPM in general, and pseudo-quantities are a natural part of NPM. More adequately expressed, the ambition of those who support NPM is to render social reality quantifiable. In this effort, these individuals support Lord Kelvin's conviction that real knowledge must be expressed in numbers. However, Lord Kelvin had no conception of such unreliable numbers as pseudo-quantities.

The pseudo-quantities that are typical in NPM are unrelated to intellectual standards but more pertinent to social ideals, values and power. Pseudo-quantities primarily flourish in the increasing amount of documents that professionals must complete. Of course, these documents contain much more information than merely numbers, but numbers play a substantial part as the most easily comparable element of all of the information in these documents.

Thus, numbers meet one of the main values in NPM, namely transparency. Every part of the public sector must be transparent to outside parties (and to the powers above). This transparency can serve as (and sometimes actually is) a democratic value: when a man or woman in the street obtains

access to information that is important to him or her as an individual, a professional or a member of a democratic society. However, this transparency is frequently also a managerial interest and, indirectly, a political interest. A public manager, whose real influence is more substantial than that of an old-fashioned administrator, is eager to monitor and control what is occurring in the public sector and especially to verify that the quality of its activities meets standards. This quality is expressed in quantitative terms, primarily in pseudo-quantities.

Most politicians who are unconscious adherents of the ideals of NPM also utilise such results and often refer to them. Thus, pseudo-quantities have a role in the process of de-professionalisation.<sup>22</sup> A professional is a person who has received specialised education in his or her field of expertise and is therefore relatively free to make decisions concerning matters that exclusively belong to the same field of expertise. A teacher is the only person who knows how to teach his or her own students and evaluate their learning performance. Of course, experience, cooperation with colleagues and other factors are always important, but in his or her own field, a teacher is always more trustworthy than outsiders, such as politicians, journalists, or even the students themselves and their parents. The outsiders (in the former instance, the democratic institutions) must make decisions concerning all external

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<sup>22</sup> The literature on professions, semi-professions, and professionalisation and, more recently, on de-professionalisation is abundant. The standard reference here is that of Andrew Abbott, 1988. Also important is Abbott, 2001. See also Freidon, 2001 and, concerning the Nordic countries, Aili et al., 2007. The unclear professional status of the librarian is treated in Freeman, 1997, and the counterpart treatment of the teacher can be found in Gore and Morrison, 2011 and Hyland, 1986. An article that is critical of the de-professionalisation thesis is that of Clark, 2005.

circumstances, from the school curricula to economic factors. Nevertheless, the professional still has a unique field of competence.

It has been questioned whether a primary or secondary school teacher, a nurse or librarian has ever had a real professional status. The concept of a semi-profession has sometimes been used to denote those categories that have not attained the real professional autonomy of a doctor or a lawyer.<sup>23</sup>

Irrespective of these questions, in an NPM setting, the professional status of librarians, nurses and teachers has deteriorated, and even the medical profession is threatened. Different mechanical systems of assessment have been designed to evaluate and thus steer one's work as a doctor.<sup>24</sup>

In this process of de-professionalisation, the abundance of pseudo-quantities must be observed. The activities of different types of professionals in public service must be directly supervised and controlled by public managers and indirectly by politicians because it is necessary both to verify that the various tasks are duly fulfilled and to monitor *how* these tasks are completed. Through some cleverly elaborated schemes of measurement, all professional activity will be visible from the outside. At a minimum, this notion is the guiding principle. Through pseudo-quantities, the quality of all public activity will be transparent and will be possible to rank in terms of the service of public managers, politicians, and the general public. Given the untrustworthiness of pseudo-quantities, this endeavour is evidently futile.

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<sup>23</sup> Concerning the professional status of the nurse, see Parkin, 1995.

<sup>24</sup> For a discussion of this issue, see Heath 2008.

## Concluding remarks

Pseudo-quantities are not an invention of recent decades. However, they proliferate in an age in which all types of quality assessments are expected to be expressed in quantitative terms and in which impartiality and illusory exactitude appear to be more reliable than qualified expert judgement.

In the public sector, the flourishing of pseudo-quantities is intimately related to new public management, whose main ideal is to ensure that the public sector is as effective and efficient as a private enterprise. A system of controls is instituted to verify that the highest possible efficiency is achieved. Here, schools, hospitals, courts and other institutions are measured against standards that are as mechanical as those used, for instance, in a car plant. Everything in the public sector must be transparent to outsiders, including politicians, managers, and the general public. This transparency requires a substantial de-professionalisation of professional work and also leads to a loss of status and prestige among professions. Yet, it is also likely to increase the risk that teachers, doctors, scientists and other professionals would concentrate more on fulfilling assessment goals, rather than completing the most important tasks that belong to their professions.

However, the real inadequacy of all assessments that are expressed in pseudo-quantities is equally important. Qualified judgements that are marked by expertise are always more reliable, even if they require more time and more money than an assessment that is based on pseudo-quantities.

Of course, in the last instance, an expert's assessment may be summarised in a simple quantity, similar to the manner in which a teacher's assessment of a pupil is currently summarised

in the form of a grade. In this case, pseudo-quantities may be viewed as a type of shorthand that can never replace a more complete assessment, as in the case of the star or point systems that are used in film reviews.

Needless to say, all public work must be supervised and assessed in the interest of all citizens in their capacity as taxpayers, students, parents, caretakers, etc. However, it is specifically this interest that increases the importance of exposing the illusory precision of pseudo-quantities.

Similarly, higher education and research must be evaluated by real judgements that are marked by expertise, rather than by some mechanical use of numbers of articles, citations, or awards, that do not pertain to creativity, originality or even real scholarship.

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# One size doesn't fit all: On the co-evolution of national evaluation systems and social science publishing

*Diana Hicks*

**I**n recent decades governments have sought greater accountability from those who receive public money. In this environment, universities have faced changing funding regimes with the introduction of national systems of funding conditional on evaluation of research output, or performance based research funding systems.<sup>1</sup> Universities in many countries now face periodic measurement and comparison of their research output. They participate in a single national system used to evaluate research across all types of universities and all fields. Such systems are designed to best suit the most expensive and most powerful universities and fields. Others will need to adapt to better fit the evaluation protocol. In OECD countries, the natural sciences and engineering account for 70-80% of government research spending on higher education.<sup>2</sup> These are the most expensive and powerful fields, thus

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<sup>1</sup> Hicks, 2012

<sup>2</sup> OECD statistics on HERD, 2009

evaluation assumptions and protocols are designed for them. Social sciences must adapt. Since research evaluation rests largely on consideration of publication output – both quantity and impact – it is the form of social science scholarly publication that is evolving in response to the imposition of national research evaluation. At the same time, governments have accepted the argument that a one size fits all research evaluation system is unfair, and research evaluation protocols have been revised to better suit social science and humanities scholarship. Research evaluation and publishing in the social sciences and humanities are co-evolving.

To understand how the imposition of evaluation models favoring the sciences changes social science scholarship, we must understand how social science publishing traditionally differed from the science publishing around which evaluation systems tend to be structured when first introduced. Scientists work with two genres – English language journal articles and patents. Scientists work within disciplinary frameworks and can expect to reach consensus. Related to this, scientists recognize a set of core journals that are high quality and high impact, so a database can offer good coverage of a field by indexing those core journals. Scientists are oriented to the frontier and the latest results, so they reference mostly recent papers. Therefore, papers accumulate citations over a few years at most, so citation analyses can provide a fairly current measure of impact. Publishing English language journal articles in a set of core journals and building quickly on important discoveries - these habits of scientists lend themselves to effective indexing of research output and citations in databases and it is these databases that are used in evaluation.

Traditionally, the form of social science scholarship has differed. Although there is some swift referencing, archival

referencing of much older foundational papers is common.<sup>3</sup> The notion of clear disciplinary boundaries and a core set of journals can be problematic. And, as will be shown in this article, social scientists work with a repertoire of four genres: English language journal articles, books, national journal articles and enlightenment literature. For these reasons social science scholarship has not been well represented in databases. Yet visibility in databases such as Web of Science (WoS) and Scopus is central to being judged productive and worthy of government support in many national performance based research funding systems.

Over time, social scientists became aware of being disadvantaged in their evaluation systems and have lobbied for changes. Therefore some degree of mutual adjustment has taken place. This paper explores the repertoire of four genres that comprise traditional social science as well as evidence that the structure of social science scholarship differs from that of science. This is done in order to better understand the emerging mutual adjustments being made by evaluation systems, databases, publishers and the forms of publication in social science.

## **International Journals**

The first literature of social science that will be discussed is internationally oriented, largely English language<sup>4</sup> peer reviewed journal articles, similar to science. But in social science, these

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<sup>3</sup> Hargens, 2000

<sup>4</sup> Of course, not all English language journals are international. Not even all journals indexed in the Web of Science are international as minor US journals are more likely to be indexed than are minor journals from other countries.

journals comprise a smaller fraction of the literature than in science. Because international journals are highly likely to be indexed in WoS, assessing the coverage of WoS indicates the share of international journals in a nation's output. Butler and Visser examined bibliographies from nine Australian universities in 1997 and 1999.<sup>5</sup> While 90% of chemistry output was covered in the Web of Science database, the database covered only 25% of the output of economics and 17% of the output of policy & politics. Data on this point are also available for Flanders and Norway because both have collected complete bibliographies for their Social Scientists and Humanists (SSH). Ossenbock and colleagues found that in both places, about one-third of SSH publishing is indexed in the Web of Science.<sup>6</sup> Fields that behave more like natural sciences have much higher rates of coverage. More than half of psychology and economics papers are indexed in Web of Science.<sup>7</sup> In contrast, less than 5% of law papers and 19% of theology/religion papers are indexed. Less than one quarter of Flemish history, media studies and sociology papers are indexed. Less than a quarter of Norwegian comparative literature, education, media studies and philosophy papers are indexed. The deficient coverage of SSH literature by the Web of Science makes it a poor basis for evaluation of SSH scholarship. Evaluation systems based on WoS indexed journals will be based on a smaller fraction of research output in the social sciences than in the natural sciences.

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<sup>5</sup> Butler and Visser, 2006

<sup>6</sup> Ossenbock, Engels and Sivertsen, 2012

<sup>7</sup> Eighty-three percent of Flemish and 66% of Norwegian psychology papers are indexed in Web of Science. Fifty-eight percent of Flemish and 72% of Norwegian economics papers are indexed in the Web of Science.

## Books

One of the reasons that that databases index a small share of SSH output is that they do not include books, and books are integral to SSH scholarship. Books have always been important in SSH and insignificant in the scientific literature.<sup>8</sup> So although books are ignored when evaluating science, a social science evaluation that ignored books would miss the large number of citations received by books. Studies have found that within the same area, books are more highly cited than journal articles by ratios ranging from 3:1 up to 6:1.<sup>9</sup>

Perhaps the results of journal-only evaluation correlate with the results of a journal and book based evaluation. Then the less-than-ideal journal based evaluation would be good enough. Unfortunately not, books are not just large, highly-cited journal articles. Four studies investigated the correlation between cites to books and journal articles and showed that such correlations traditionally have been low. Nederhof and colleagues listed the citations per book and journal article for 19 departments; the correlation between the two was 0.32.<sup>10</sup> Hicks and Potter collected a bibliography of 17 authors' output in the field of sociology of scientific knowledge; the correlation between citations per book and journal article was 0.35.<sup>11</sup> Bourke and colleagues compared the rankings of departments using total and journal only citation counts.<sup>12</sup> They concluded: "In the social sciences and humanities, the use of journal citation rates as a surrogate for total publication citation rates is more likely

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<sup>8</sup> Small and Crane, 1979

<sup>9</sup> Clemens, Powell, McIlwaine and Okamoto, 1995, Webster, 1998

<sup>10</sup> Nederhof, Zwaan, DeBruin and Dekker, 1989

<sup>11</sup> Hicks and Potter, 1991

<sup>12</sup> Bourke, Butler and Biglia, 1996

to be misleading than in the sciences.”<sup>13</sup> Finally, Cronin and colleagues constructed a database comprising 30,000 references from 90 books reviewed in top sociology journals and published between 1985 and 1993.<sup>14</sup> Cronin and colleagues compared lists of the 26 authors most cited in the monographs and in the top 24 sociology journals. They found that nine authors featured on both lists. The five authors ranked 22 to 26 on the book list did not even appear among the top 532 authors most cited in the journals.

Low correlations in citation counts combined with differing highly cited author sets suggests that the journal and book literature have developed as different genres. That these genres may overlap but retain a distinct identity was supported by Line.<sup>15</sup> Line constructed a set of 59,000 references: 11,041 from monographs and 47,925 from journals. Line found that about half the time journal articles referenced journal articles and books referenced books. The rest of the references were spread across many different publication types. This suggests that the journal and book literatures have been somewhat self-contained, although obviously interdependent and overlapping.

Why did social science literature develop in two genres? Perhaps because they carry two types of scholarship; journal articles may reflect a more scientific, and books a more humanities approach to scholarship. Clemens and colleagues' study of US sociology helps us understand this.<sup>16</sup> Clemens and colleagues compared book and journal publishing within the context of a long standing debate in sociology. Is sociology professional,

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<sup>13</sup> Bourke et al., 1996, pp. 54

<sup>14</sup> Cronin, Snyder and Atkins, 1997

<sup>15</sup> Line, 1979

<sup>16</sup> Clemens et al., 1995

technical, cumulative, and convergent as one would gather from its journal literature or is it a diversified, intellectually open endeavor as found in the books? Examining the two types of publishing sheds light on the themes of scientific integrity versus intellectual vitality that underpin the debate.

Clemens and colleagues' evidence supported the notion that book and journal publishing form different genres.<sup>17</sup> They argued that entry into article publishing is competitive and so more egalitarian than entry into book publishing, which relies more heavily on patronage, recommendations and reputation. They found that book authors were more likely to be trained and located at elite private universities than were journal article authors. Article authors were more junior than book authors. Articles were more likely to be based upon quantitative evidence and books on qualitative evidence (although books based on quantitative evidence were the most cited of all). Clemens concluded:

... books and articles play different roles. Books are high-stakes endeavors that, when successful, are effective in enrolling allies from neighboring fields. In contrast, articles discipline the troops, generating a common currency of evaluation, be it in comprehensive exams or tenure decisions. To the extent that we care about scholarly reputation, both our discipline's and our own, neither genre should be ignored.<sup>18</sup>

Clemens and colleagues' analysis painted a picture of a heterogeneous field of scholarship with distinct journal and

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<sup>17</sup> Clemens et al., 1995

<sup>18</sup> Clemens et al., 1995, pp. 484

book traditions.<sup>19</sup> Journals represent a more scientific type of research and books a more humanities type of scholarship. Because books are more transdisciplinary, very highly cited and often produced by different people than journal articles, journal article evaluation will differ from studies that are more inclusive. Each genre contributes differently to the efforts of social science scholarship to develop a full understanding of society. There is no reason to discourage book publishing and the type of scholarship it represents.

### **National Scholarly Journals**

The third genre of social science is national. Scientific research transcends national borders, but social sciences are more embedded in their social context because society is their concern. Social science research agendas are influenced by national trends and by policy concerns of national governments. Theoretical concepts are subtle and expressed in national languages. They can often be fully appreciated only in the original language. Some disagree; Moed and colleagues have argued that:

...genuine scholarly research in any area leads to results relevant outside the home country. [Though] this may be less true for more applied or practical research. Therefore [at least some] outcomes of genuine scholarly research, even those primarily related to national aspects, deserve to be communicated — in an appropriate form — to scholars in other countries as well.<sup>20</sup>

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<sup>19</sup> Clemens et al., 1995

<sup>20</sup> Moed, Nederhof and Luwel, 2002, pp. 513

Optimists studying social science literature in the late 1980's found that in the international literature indexed in the SSCI:

With the exception of a minority of topics related to political science, to social issues, and to a lesser extent physical health and geographical location, the large majority of the topics seem to reflect a transnational substantive interest. In addition, the [US and European countries] studied here share many social and political issues. Of course, this may not be true for other countries, and in particular non-Western countries. The present data suggest that the research front on many topics in the social and behavioral sciences is international in the late 1980s . . . Of course, this does not preclude that publications on national issues or national aspects of issues appear in journals or books that address primarily a national audience.<sup>21</sup>

It is the final point, publications addressing a national audience, to which I now turn.

To examine the existence and nature of national scholarly literatures, I will compare national and international journals. By national journals I mean those that primarily publish articles in a language other than English, and whose authors and readers largely work in one country. International journals are largely English language journals either those that were originally American or British but are now targeted by authors from many countries or more recently founded English language European journals.

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<sup>21</sup> Nederhof and Van Wijk, 1997, pp. 271

Bibliometric evidence suggested that traditionally both producers and consumers of social science were nationally oriented. Gläser established the continuing existence of differentiated national communities in social sciences, even in an English speaking country, Australia.<sup>22</sup> Kyvik, studying the writing habits of Norwegian scientists and social scientists in the early 1980's, found that compared to the scientists fewer social scientists published in a foreign language and more published in Norwegian.<sup>23</sup> Taking authors' citation patterns as an indication of their reading habits, Yitzhaki found that authors over-cite material in their own language.<sup>24</sup> American and British authors cited English language material 99% of the time, although English language sociology probably accounted for 70% of the world literature. German and French authors cited material in their own language more than 60% of the time although such material accounted for less than 10% of literature in the field. In a sense then, each national literature is a genre.

In addition, national literature overlaps to a limited extent with literature indexed in the databases. This was strikingly illustrated by an analysis comparing a unique resource, a Polish sociological citation index (PSCI) with the Social Science Citation Index (SSCI) coverage of Polish sociology. Using a list of Polish sociologists and counting their citations in the Polish index and the SSCI, Webster found that of the top 10 most cited journals in the Polish index only the three foreign ones were indexed in the SSCI.<sup>25</sup> The top 20 most cited documents by Polish sociologists in each index contained none in common. All

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<sup>22</sup> Gläser, 2004

<sup>23</sup> Kyvik, 1988, pp. 165

<sup>24</sup> Yitzhaki, 1998

<sup>25</sup> Webster, 1998

but one of the SSCI cited documents were in English; all the PSCI cited documents were in Polish. The most cited sociologist on the Polish list (with 253 citations) was ranked 41st in the SSCI (with 19 citations). The most cited sociologist on the SSCI list (with 254 citations) was ranked 20th on the PSCI list (with 41 citations). Two studies were done using the Polish sociology citation index. The first covered pre-transition Polish sociology, 1980 to 1988, the second covered pre and post transition sociology. Pre-transition, the SSCI missed 90% of Polish sociologists; post transition, it missed 30%.<sup>26</sup>

Webster's analysis illustrated the bibliometric consequences of the limited overlap between national and Web of Science literatures. Bibliometric indicators based on foreign literature painted one picture of Polish sociology, and the Polish sociology index another. Webster summarizes this point well, concluding that the Social Science Citation Index (SSCI) indicates the presence and the impact of Polish sociology on the international arena, focusing on areas of research done in Poland which are of interest to the international community and the 'best' Polish sociologists and Polish sociological works; but the Social Science Citation Index "does not allow for an in-depth analysis of the local dimensions of the discipline".<sup>27</sup>

The Polish work suggested that the ascendancy of an international social science placed small-country social scientists in the position of applying others' frameworks to their societies. Polish sociologists were recognized internationally mostly when their society presented picturesque episodes that become fashionable topics in big countries. National communities could develop method and theory, but big-country social scientists

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<sup>26</sup> Winclawska, 1996

<sup>27</sup> Webster, 1998, pp. 31

remain impervious. Polish sociologists highly cited handbooks in general sociology by Polish authors, works on the social structure of Polish society, and works on interesting theoretical or methodological issues. Works highly cited in the SSCI included six that dealt with theoretical issues, each was at least 20 years old, others dealt with social unrest in Poland in the early 1980s and the fall of Communism in Eastern Europe. Webster concluded that: “the international sociological community does not notice Polish attempts to tackle universal issues in sociology; it is primarily interested in ‘fashionable’ topics and fads associated with the ‘velvet revolution’ and systemic transformation”.<sup>28</sup>

My own work in progress pursues this line of investigation, taking advantage of a Spanish citation index (INRECS) to compare Spanish authored sociology papers highly cited in WoS with those highly cited in the Spanish index. As in the Polish study, we find that the most cited authors differ. We find that the top 25 most cited authors in WoS are not among the most cited in INRECS. Of the top 25 most cited authors in INRECS, three are among the most cited in WoS; each ranked lower than 155. The most cited topics also differ. Five of the top ten most cited Spanish sociology papers in WoS are about tourism because the journal *Annals of Tourism Research* is classified in sociology (as well as in hospitality and tourism). There are no tourism studies among the most cited 100 papers in any social science field in INRECS. Apart from tourism, the topics of the most cited papers in WoS are: social indicators, language and society, religion, health and community research. The topics most cited in INRECS are poverty, social welfare and social policy; family research, organization and political sociology. Again we see the pattern that foreigners are interested in

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<sup>28</sup> Webster, 1998, pp. 23-24

Spanish research for particular, colorful reasons, especially the tourism industry, whereas the topics of interest to the domestic audience are closer to the core of sociology.

Small country social scientists can be internationally recognized, but perhaps have fewer possible strategies for doing so than US or UK social scientists. Imposing an evaluation system that privileges international citations will force scholars to choose topics that interest foreign academics. Over time this poses the danger of forcing non-English language scholars out of the disciplinary core and into a fringe of colorful topics in the hopes of attracting the international attention so valued by their governments.

## Enlightenment Literature

The fourth genre in the repertoire of social scientists is intellectual or enlightenment writing. This is found in periodicals whose goal is knowledge transfer or “enlightenment” of non-specialists. For example, the Nobel prize winning Princeton economist Paul Krugman exerts influence through his *New York Times* column. Burnhill and Tubby-Hille found that in the UK “projects in education [were] reaching practitioners through the *Times Education Supplement*, with researchers in sociology, social administration, and socio-legal studies publishing in such periodicals as *New Society* and *Nursing Times*”.<sup>29</sup> Kyvik found that in Norway one-half of social scientists published contributions to public debate.<sup>30</sup> In contrast, one-quarter to one-third of scientists contributed to public debate.

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<sup>29</sup> Burnhill and Tubby-Hille, 1994, pp. 142

<sup>30</sup> Kyvik, 2003

Burnhill and Tubby-Hille investigated this issue in some depth.<sup>31</sup> They constructed a publications database from grant holders' reports to a granting agency, supplemented by a survey. They classified journals as peer-reviewed using two directories that identify peer-reviewed serials, or the judgment of at least two authors. Assigning non peer reviewed journals to the enlightenment category suggests that psychologists, statisticians and geographers did not publish much in non-scholarly literature. Other fields did. Even economics, normally quite scientific in its publication patterns, exhibited a healthy percentage of articles in non-scholarly venues. Linguistics, education and sociology led in share of enlightenment publications.

Nederhof and Zwaan have also looked quite closely at this issue.<sup>32</sup> They surveyed Dutch and foreign scholars asking them about the scholarliness of a number of journals in which Dutch social scientists published. They found that journals considered scholarly in university annual reports were not always considered so by experts. The share of non-scholarly journals ranged from 11% in experimental psychology to 25% in public administration. If departmental output were recounted, including only articles in journals judged scholarly, in the best case one experimental psychology department would have lost only 1% of its output, and in the worst case one public administration department would have lost 61% of its output.

The Nederhof and Zwaan study opened up the issue of distinguishing enlightenment from scholarly literature.<sup>33</sup> That enlightenment and national scholarly literatures are not usually

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<sup>31</sup> Burnhill and Tubby-Hille, 1994

<sup>32</sup> Nederhof and Zwaan, 1991

<sup>33</sup> Nederhof and Zwaan, 1991

distinguished may contribute to the devaluation of the later. The classic problem with the national literature is the lower level of critique and peer review applied, leading to a reputation for lower quality. If enlightenment literature was acknowledged as such and reported in a separate category from national scholarly literature, we might find that the national scholarly literature is not as problematic as its reputation suggests. True scholarly journals need to be distinguished from enlightenment literature so that the quality of the former and the outreach function of the later can both be appreciated and valued. Studies have found that separation of enlightenment literature from scholarly literature is laborious because people disagree on where the boundary lies. However, when scholarly and enlightenment literature are carefully distinguished, database coverage rates for scholarly literature rise substantially.<sup>34</sup>

Enlightenment literature moves knowledge into application, performing a function for social scientists analogous to patenting for scientists. But patent systems are indexed, contain citation structures enabling evaluation, and have gained respect as a valued output worthy of evaluation. In contrast, enlightenment literature being also national literature, is less well indexed, tends not to be cited and is often viewed as low quality scholarship. The result is that enlightenment literature is not valued as an output of scholarly work interacting with application.

## Discussion

In social science there are four distinct genres: international journal articles, books, national scholarly journal articles and enlightenment publications. International journal articles are

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<sup>34</sup> Burnhill and Tubby-Hille, 1994, Schoepflin, 1990

indexed in databases and have been the currency of evaluation around the world. This is not wrong; using journal articles to communicate research results to an international audience is important. However, there is more to scholarly work in social science. Books can have a very high impact. National scholarly literature represents a body of knowledge specific to a society, developed in a local context and of particular relevance to people who share that context. Enlightenment literature represents knowledge reaching out to application. The authors and topics associated with the four genres overlap somewhat, but not completely. So the results of international journal bibliometrics will not be the same as the results of an evaluation which included all four genres.

National research output evaluation systems privilege the international journal literature. An early system, the Australian Composite Index simply counted papers indexed in the Web of Science (WoS). The Flemish government introduced performance-based funding in 2003 based on counts of WoS indexed papers. Such international journal-based evaluation models will work for scientific fields but will be partial and misleading when applied to social sciences. Social scientists and humanists, well aware of the limitations, have objected to WoS only systems. As a result, we are now seeing adaptation by all parties: the databases, the evaluation models, publishers, and the social scientists themselves.

Because Thomson Reuters and Elsevier are in competition for government contracts to supply data to national evaluation systems, they are sensitive to the SSH coverage problem. In 2009 Web of Science and Scopus added a large number of social science and humanities journals, increasing the size of the social science list in WoS by 22% and in Scopus by 39%.

Evaluation systems have adapted as well. Today systems go beyond the Web of Science to count a wider range of journal literature. For example, in 2008 construction of a comprehensive database of Flemish university social science output began. Australia has expanded beyond WoS as well. However, expansion does not completely solve the problem because national journal articles are positioned within these expanded literature counts as an inferior version of international journal articles. This is done by assigning them less weight in the count. It is this methodological detail that threatens to create a strong incentive to move away from the core of a discipline into colorful topics favored by foreigners, a danger revealed above through the Polish and Spanish sociology studies. The four genres perspective would suggest national literature should be seen as a different dimension of publishing, and not necessarily as an inferior version of English language publishing.

Social scientists have adjusted as well.<sup>35</sup> Between 2000 and 2009, publication by Flemish social scientists in journals indexed in WoS almost tripled. Growth came both from increased publication in journals indexed throughout the period – i.e. social scientists changing their publication habits – and from more journals being indexed in WoS – i.e. journal publishers seeking to meet the criteria for inclusion in WoS and WoS expanding its coverage.<sup>36</sup> Trends in coverage and publishing in indexed journals suggest a mutual adjustment between social science scholarship and systems implicated in evaluation.

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<sup>35</sup> Kyvik, 2003

<sup>36</sup> The number of indexed journals that included Flemish authored publications grew from 133 to 858 over the period; see Engels Ossenblok and Spruyt, 2012.

Book publishing seems not to decline even after the introduction of a national research evaluation system.<sup>37</sup> Seemingly books will not be discounted, and evaluation systems adjust to their presence. Significant in this regard is that Thomson-Reuters, publisher of the Web of Science, has introduced a book index. This product promises to make citation counts of books available in evaluation processes. Although this seems to suggest that no adjustment on the part of scholars will be necessary, the construction of the index mirrors that of the journal index in that large, English language publishers will be better represented than small, non-English language publishers.

This happened even in the Flemish system. The first version of the Flemish social science and humanities database included only 17% of the submitted records with an ISBN. The excluded 83% were books produced by unapproved publishing houses. The top 11 of these unapproved publishers by frequency of records were local publishers accounting for 45% of the excluded book material. In this case, publishers are adjusting. Faced with the prospect of their academic book lists not being considered scholarly enough to be included in the Flemish university evaluation system, Flemish publishers have launched the “Guaranteed Peer Reviewed Content” label. By making peer review explicit and traceable, the publishers aim to make their content eligible for the evaluation system under the governing regulation that defines scholarly outputs as having been subject to peer review.<sup>38</sup>

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<sup>37</sup> Engels et al., 2012

<sup>38</sup> There are three other criteria in the regulation: be publicly accessible, have an ISBN or ISSN, contribute to the development or application of new insights. Engels et al., 2012, Verleysen and Engels, 2012

Even if more book publishers are included in the Flemish system, the scholarly value of their books could still be devalued. This would happen if differential weights were applied to locally and internationally published books. The weights used in the Flemish system are being renegotiated, which leaves open the possibility of higher valuations for books written in English and published with an international publisher than for those written in Dutch and published locally.

There is no evidence regarding the fate of enlightenment publishing in systems with national research evaluation. The enlightenment literature only enters into evaluation systems, or studies of evaluation systems, as contamination to be eliminated before a sound analysis can begin. Therefore, adaptations affecting enlightenment publishing can only be speculated upon. We do know that performance-based university research funding systems neglect application of research, although research application is a long-standing concern of governments.<sup>39</sup> For social scientists, application is associated with being involved in the public debate and publishing enlightenment literature. It seems safe to guess that like the national literature, enlightenment publishing is in decline in countries with performance-based funding systems. Over the long run, this may serve to reduce the impact of social science research on society and the dissemination of new knowledge to decision makers.

## Conclusion

Law and Urry argue that "the social sciences have always been embedded in, produced by, and productive of the social".<sup>40</sup>

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<sup>39</sup> Hicks, 2012

<sup>40</sup> Law and Urry, 2004, pp. 392

Reflecting on their insight in relation to the shifts in the four genres brought about by evaluation systems suggests some disturbing possibilities. First, enlightenment literature is central to social scientists engaging in relationships with their societies, but it looks set to wither, potentially cutting off scholars from application of their ideas. The same applies to discussion among scholars of issues particular to their societies whether in books or national language journal articles. Governments explicitly devalue such discussion in their evaluation systems, discouraging scholars from engaging local issues. Finally, the push into “international”, i.e. English language literature, risks forcing scholars to adopt the perspective of American academics, who dominate such literature. In this case, those thinking about the future of a society will be thinking in American terms. One partial escape from this fate is offered by the launch of English language journals produced by European scholars. This softens the impact of the international push, while perhaps leading to a desirable European scale convergence in thinking about the future of society. To avoid social scientists retreating to an internationally approved ivory tower of scholarship, performance based evaluation systems need to be designed to value each of the four literatures of social science. If this does not happen, unintended consequences seem likely to damage societies over the long term.

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## Managing your assets in the publication economy

*Ulf Kronman*

**T**he issue this article aims to address is the fact that publications may nowadays be used to assess impact and quality of research in ways academics may not be fully aware of. During recent years, scholarly publications have gained in importance, not primarily as the traditional vehicle for the dissemination of new scientific findings, but as a foundation for assessing the production and impact of organizations, research groups and individual researchers. This means that publications as artefacts *per se* are starting to play a new important role in the scientific community and that researchers need to be aware of how publication and citation counts are being used to assess their research and the outreach, impact and reputation of their mother organization. University rankings, for instance, often have some parameters based on the publishing of the ranked institution. This article is thus not about scientific *writing* as such; it focuses on what happens to your publication *after* the publishing has taken place and on aspects to take into account while planning the publishing of your article, report or book.

The need to assess research seems to be ever growing and in the urgent need for some hard numbers the evaluators turn to

counting publications and citations. For academics, his or her research is thus not always assessed by peers that understand what is written, but by people that do not have the time to read the publications or even would understand the content if they had the time to read. Instead, these evaluators have to resort to using metrics from impact *proxies*, as the renouance of the journals you publish in, or counting citations from people that have read your publications and hopefully understood what was said in them, and then cited your work.

There are a lot of nitty-gritty details involved in the production of bibliometrics, and this article will describe some of them. By outlining these aspects of bibliometrics you can consider how they might affect the metric outcome of your publications when producing and managing them in the future. Before continuing with the publication management advices, a short disclaimer: Even if metrics and statistical aspects of publications are gaining importance for assessment and funding, it is still the *quality* of the research behind the publications and the *dissemination* of research findings to peers and general public that should be the primary goal for your publishing. But, on the other hand, there is no contradiction between doing high quality research and establishing a good communication with fellow peers, and to consider some means for making the research results more visible and influential, utilizing some of the considerations pointed out in this article.

A few words about the disposition of the article: firstly, I will focus on the situations where counts and impact of publications matter. Secondly, I will report on how publications are being measured in bibliometric studies and how this affects their impact in the results of the studies. Thirdly, I will suggest some methods for improving the results in bibliometric studies based

on the details mentioned in the previous part. Lastly, I will round of the article with a discussion on why we see this economy of publications emerging.

## **Publications as measures of production and impact**

As mentioned in the introduction, publication measures are increasingly being used as tools in the race for funding in a world of tightening competition for shares of constrained budgets. For you as a researcher this means that you have to keep a good record of your publishing and see to that all your publications are being visible and attributed to you in the various assessments based on publications. In this section I will outline a number of situations where your publication record may play an important role for you and your organization.

### *Publication lists for CV's and web pages: exhibiting excellence*

The most important tool for exhibiting your scholarly impact as an individual researcher is of course the publication list that is a part of your *curriculum vitae* (CV). Many researchers keep their publication list as word-processing documents, in local EndNote databases or on static or dynamic CV web pages. To keep a local list of publication records in a file and on a local website are both labour-intensive and a bit old-fashion in these network-based times. If your organization runs a publication database – often called a *publication repository* – your chances are good that both the publication listing for your CV and for your personal web page can be generated dynamically from this publication database. Your tasks are to keep the database updated with your publication records, and preferably also upload the full text of the publications when possible. Contact the local support for your publication database – usually

situated at the university library – for information on how to enter records and get listings from the database.

### *Research evaluations and publication based funding schemes*

Research evaluations seem to be a prevailing trend among universities since the turn of the millennium. Every larger university seems to do one evaluation every third or fourth year, and usually publication statistics – *bibliometrics* – play an important role in these assessments. When publication statistics is gathered from commercial bibliographic databases it is very important that the publications can be attributed to you and your organization.

Bibliometrics is also playing a role of increasing importance in performance-based university and department funding all over the world. Among the Nordic countries Norway was first out with a model which allocates funding based on publication counts, Sweden was second with a citation-based model and now Denmark and Finland are introducing publication-based models of the same type as in Norway. When governmental bodies and research funders are starting to use bibliometrics for funding allocation, these kinds of measures are often mimicked at the organizational level by the local university managements. This means that your publication record may well play a role in the funding allocation to your department or research group. The same rules as for publication based research evaluations apply here – it is important to have the attribution of publications in good order.

### *University ranking lists*

Since the turn of the millennium, worldwide university rankings have become increasingly important and they are growing in number for each year. International students use the rankings when they choose among universities, universities use them to evaluate potential cooperation partners and they are used as a foundation for benchmarking and marketing. Politicians, decision makers and the industry also use the rankings to evaluate higher education institutions for policymaking and allocation of funding. Assessment of university research output in the terms of publications often constitutes an important part of the indicator set used to calculate the rankings.

The three most prestigious international rankings are the Shanghai Jiao Tong "The Academic Ranking of World Universities" (ARWU), the Times Higher Education "The World University Rankings" (THE/WUR) and QS "World University Rankings".

The ARWU-list is published yearly by the Institute of Higher Education at Shanghai Jiao Tong University. It was first produced in the year 2003 as part of a plan to create "world-class universities" in China. The methodology is relatively open, well documented and non-subjective. Universities are judged by unusual achievements, e.g. Nobel prizes and Fields Medals over a very long period. Large and old universities are favoured. Biomedical and physical sciences are given more weight than engineering, social sciences and humanities. The ARWU-list is fairly good at ranking the 50-100 most prestigious universities in the world. Outside this scope, it is of limited value. This is acknowledged by the ARWU and therefore universities below rank 100 are grouped together in chunks of 50 and 100.

The most important measures in the THE/WUR ranking are the citation measures and the international reputation surveys. Together they account for two thirds of the total score. The QS list has been published in 2004-2009 by the journal Times Higher Education in cooperation with Quacquarelli Symonds Ltd (QS). Starting with 2010, QS is solely responsible for the ranking. Fifty per cent of the score is based on surveys, the rest on quantitative data.

However, and as was touched upon in passing above, there are a number of inherent shortcomings with university rankings – a few of the more notable ones being:

- All measured aspects of a university’s activities and duties – education and research – are squeezed into one single measure, while another aspect – societal impact – is even neglected.
- Ranking is a way to make a champions' league and *magnify small differences* in the underlying indicator values. A small indicator discrepancy of 0.01 might be the only difference between two different rank positions in the list.
- When the final composite score is calculated, ranking providers *assign weights to each indicator* in the overall score. This means that the ranking provider’s subjective judgement determines which indicators are more important.

Even though university rankings are crude and one-dimensional, they represent an easy-to-digest form of information to the broad public and they are very influential. This is what the European University Association has to say about the rankings in a recent report: “Despite their many shortcomings, biases and

flaws ‘rankings enjoy a high level of acceptance among stakeholders and the wider public because of their simplicity and consumer-type information’. Thus, university rankings are not going to disappear; indeed, the number of rankings is expected to increase although they will become more specialised.”<sup>1</sup>

## **Bibliometric studies and indicators: what counts where?**

Bibliometrics is simply statistics done on publications, most commonly scholarly publications. A more precise definition might be: “Bibliometrics is the application of statistical methods to publications and is commonly used to assess scientific research through quantitative studies on research publications, primarily articles in peer-reviewed journals.”<sup>2</sup>

The reason for bibliometrics gaining in popularity and importance is the present urge of measurability in research assessment and funding allocation. Review by peers is still the gold standard in research assessment, but has the drawbacks that it usually not presents hard numbers and may also suffer from personal bias in judgements. Furthermore, it is hard work to do a peer-review of research, so statistics on publications and citations are often used as a shortcut for research assessment.

However, bibliometric indicators should rarely be used alone. If interpreted without caution they might be quite misleading. There are a number of reasons why good research may end up with poor bibliometric indicator values even though the

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<sup>1</sup> Ruhvargers, 2011

<sup>2</sup> Karolinska Institutet, 2011

research is of good quality. If the research lab is in a start-up phase, if the research field is very narrow, or the researchers publish their findings in forms and channels not covered by the bibliometric data sources the bibliometric indicators may show poor values, even if the research is of excellent quality.

The best usage of bibliometrics is to *supplement* peer judgement and supply extra statistical information to the reviewing experts, which preferably are knowledgeable of the organization and the research field that is being assessed. If the bibliometric indicators support the expert opinions, the experts can feel a bit more assured in their judgement. If the indicators contradict their opinions, it may be a signal for consideration and rethinking, or at least to try to explain the discrepancy between peer review and bibliometrics. After these initial words on bibliometrics in general I will go into the details on how bibliometric studies are performed.

### *Sources for bibliometrics*

There are only a few data sources that capture enough publication data to be used as viable sources for a bibliometric study. The most important sources for bibliometric data are:

- Thomson Reuters citation indices (approximately the same content as the Thomson Reuters Web of Science)
- Elsevier Scopus
- Google Scholar
- Your organization's own publication database

The most basic forms of bibliometrics, as counting publications and citations, can be done in the online versions of the commercial databases Web of Science (WoS), Scopus and

Google Scholar. The publication database of your own organization can only be used for publication counting, since it is not possible to build a citation matching and counting in a one-organization publication database. To do proper citation matching, a large portion of the world's scientific publication production is needed in the same database. The commercial vendors Thomson Reuters and Elsevier are adding around 1.5-1.8 million publication records per year to their systems, which is believed to be around two thirds of a roughly estimated yearly world-wide production of 2.5 million scholarly publications.

When it comes to more advanced bibliometrics, doing comparisons of citation counts to world-wide averages, even the commercial online services won't do the job, since they don't have any world averages to compare citation counts with. To be able to get world citation averages, you have to licence the data for the whole publication indices and build your own analysing system, usually covering about 20-30 million publication records. This is a procedure that involves large costs, both in licenses from the commercial vendors and in costs for personnel building and maintaining the database system, as well as computer hardware.

When you are about to decide which publications to include in a bibliometric analysis of an organization, you need some sort of identifier that links publications to the organization. Your own organization's publication database usually has the advantage of internal unique ID's for your organization's organizational units and your organization's staff, so publication records may be selected based on those ID's. On the other hand, your publication database does not have any citation counts, so if you want to do citation-based

bibliometrics, you need to get data from one of the commercial vendors.

In the commercial databases there are no unique identifiers for organizations or researchers, so the selection of publication records has to be based on error-prone text string matching of author and organization names. This less desirable method of record selection is the reason for the importance of keeping author and organization names unique and consistent over time. Trying to locate publication records for a department or research group using this text-based method is near to impossible, due to the large variation in naming of the organizational units, and frequent name-changes, mergers and splits of departments. In the section about publishing and promoting your research, further down, I will show the best way to state your and your organization's name for your entry in the author list.

### *Counting fractions of publications*

When doing bibliometric studies on co-authored publications, publication and citation counts are often shared between the contributing parties. This is called *fractionalization* and can be based on either author names or addresses. The easiest and most common method when doing analyses of organizations is to do an address-based fractionalization. This is, for instance, what the Swedish Research Council does when it analyses the output of Swedish research.

The address fractionalization means that if the researchers in your organization have one of four affiliation addresses in a publication, your organization will get attributed one fourth of the publication, regardless of the number of researchers that are affiliated with each of the addresses and regardless of which

amount of work each researcher has put into the publication. The share of addresses is also often used as a weight when doing calculation of citation averages, so that publications where your organization addresses have a larger share will weigh heavier in the average calculation.

The methodology opposite to fractionalization is called *full* or *whole counting*, where each contributing organization or researcher gets full credit for the publication and all its citations. This method can on one hand be considered more “fair” to the researchers and the involved organizations, but has the disadvantage of the sum of the parts being larger than the whole. For instance, when doing full counting, the sum of publications from Swedish organizations will be almost twice the total Swedish publication production. What is counted here is not the number of publications, but rather the number of *authorships* or “affiliations”.

It may also be noted that the new practice of assigning publications to all involved staff in a large research project creates severe adverse effects on bibliometric studies. For instance, some large research cooperatives in particle physics put over 2300 authors and 200-300 affiliations on each publication. If you fractionalize publication and citation counts, almost nothing of this kind of publication will be visible in the assessment of an organization involved. If you on the other hand do full counting, such a publication can make a large difference in bibliometric indicators for each of the mentioned researchers and their respective organizations, even if the researcher may not even be aware that he or she has a part in the publication in question.

### *Research fields and average citations*

Citation-based bibliometric indicators are based on the assumption that a reference (an outbound citation) from a scientific work to a previously published work represents an indication of scientific impact of the cited publication. It is also assumed that the number of (inbound) citations to a publication can act as a proxy to assess the impact of the scientific work of the author or the group that has produced the cited publication. This assumption does not always hold true at the *micro* level, i.e. for a single article, researcher or research group. There may be negative citations, claiming the cited author to be wrong or that the results are disputable and there are also a number of other reasons to cite a publication that can be considered less valid in relation to the assumption stated above. On the other hand, we also know that if we use bibliometrical methods on a large number of publications, like a thousand or more, we usually find a good correlation between citation-based indicators and a peer review of the work of the studied group.<sup>3</sup> This means that the major part of the citations is to be considered as valid in relation to the bibliometric impact assumption. Thus, we can conclude that there is a good reason to believe that high scores in citation-based bibliometric indicators are to be seen as a sign of high-impact research when working at the *macro* level.

Different research fields have different publication and citation cultures. In some fields, as for instance mathematics, the publication frequency is low and reference lists are short. In other fields, as for instance biotechnology, publication frequency is high and reference lists are long. This means that the *citation density* in the field of biotechnology will be much

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<sup>3</sup> Moed, 2005

higher than the citation density in mathematics and that raw citation counts to publications from the two fields should not be compared without any precautions.

In the commercial databases Thomson Reuters Science Citation Index and Elsevier Scopus, the publications are classified into research subject fields. Thomson Reuters uses 250 field categories to classify each journal issue in 1-6 fields, and the classification of the publications is inherited from the classification of the journal issue they were published. When doing more advanced bibliometrics the classification of the journal issues are used to sort the publications into different research fields and compare the assessed publications only to publications within the same research field, due to differences in publication and citations frequencies between the fields. See Figure 1 for a picture of the differences in average citation rates between research fields.

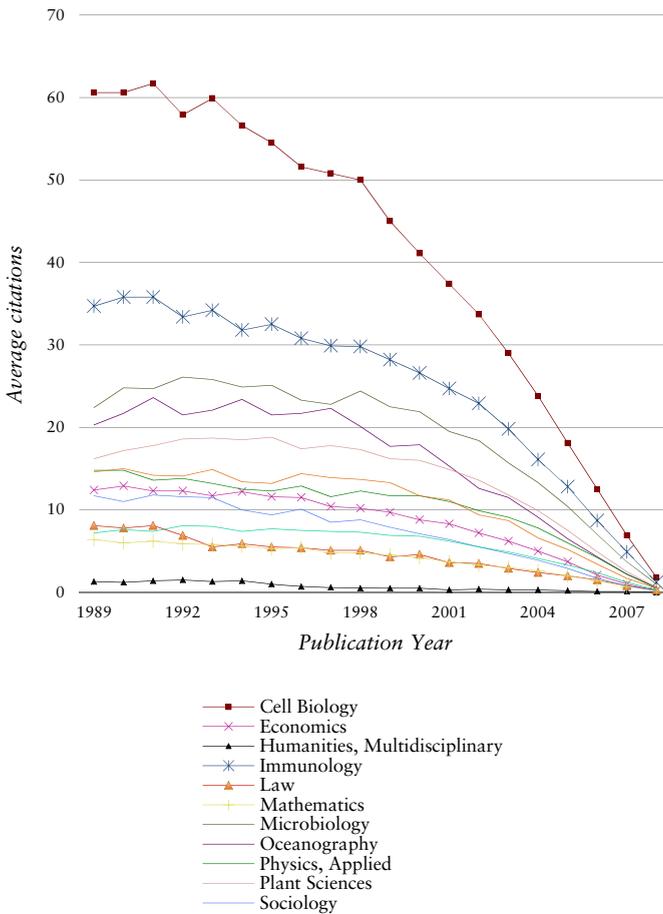


Figure 1. Average citation rate for publications in a number of research fields. Measurements were done in the Swedish Research Council's bibliometric system 2009.<sup>4</sup>

<sup>4</sup> Kronman, Gunnarsson and Karlsson, 2010. An open citation window was used and self-citations were included. A self-citation is when a researcher refers to her/his own previous publications in the reference

The state-of-the-art bibliometric method to handle differences in citation densities between research fields is the *field normalized citation rate* ( $c_i$ ).<sup>5</sup> When calculating the field normalized citation rate, citation counts for publications are compared with the world average citation rate for publications of the same *type* and the same *publication year* and within the same *research field*. Dividing each publication's citation count with the world average citation count for publications of the same type, the same year, within the same research field, results in a normalized value. Using this normalization procedure, the world average within each combination of field, year and publication type will per definition be 1, and a field normalized citation rate value above 1 will indicate that a publication has been cited more than the average in the field.

When publications are measured using the field normalized citation rate it is thus the *journal* in which you publish that decides which field your publication will be compared to. If you publish in a journal classified in a low-cited field as mathematics, humanities or social sciences the citations your publication receives will end up having more weight in the field normalized citation rate indicator than if you publish in a journal that is classified in a field with a higher citation density. There are examples of researchers who are active in multidisciplinary areas that link together computer

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list of an article. Certain data included herein are derived from the Web of Science ® prepared by THOMSON REUTERS ®, Inc. (Thomson®), Philadelphia, Pennsylvania, USA: © Copyright THOMSON REUTERS ® 2010. All rights reserved.

<sup>5</sup> The field normalized citation rate was introduced under the name *The Crown Indicator* by the bibliometric centre CWTS at the Leiden University in the middle of the 90's and refined and documented by the Swedish Research Council and Karolinska Institutet in the 00's.

programming and arts who will publish in journals classified in the arts field. The result of having articles about programming classified in the arts field will often be high field normalized citation indicators due to the relatively high citation rates among computer scientist compared to the low citation rates in the humanities.

## **Managing your assets: publish for maximum visibility and impact**

Now that we know a bit more about bibliometrics and the ways publication records are being used to assess research volume and impact, it is time to take a look at the ways in which you can improve your bibliometric indicators and rankings. First, I will address the importance of choosing the right channel and the right publication type, and then I will address ways to make your publication more visible and influential in bibliometric studies.

### *Where to publish*

The key to research impact, both for you and for your organization, is to make high-quality research and to reach the right audience with your research findings. Choosing the right channel – journal or publisher – for your publication can leverage its impact. Publishing in an international peer-reviewed journal with high impact, covered by the large indexing services, will usually render higher scores in bibliometric studies than publishing in another channel.

The channels with the most prominent outreach and impact in bibliometric studies are international journals covered by the indexing service Thomson Reuters Web of Science (WoS).

Thomson Reuters indexes about 11 500 journals and add 1.6 - 1.7 million publication records to their database each year. The Thomson Reuters' indices are usually the main data source for bibliometric studies and therefore it is of vital importance to publish in a journal that is covered by them. If you have a choice when deciding which journal to publish in, consult the Thomson Reuters Master Journal List<sup>6</sup> to see if you can find an appropriate journal that is indexed.

If you are publishing in a journal, the Thomson *Journal Impact Factor* (JIF) will give you an indication of the average number of citations to articles in the journal. The JIF for a journal is calculated by dividing the number of citations to a journal by the number of articles published in it.<sup>7</sup> The JIF can be seen as a crude measure of how widely spread and how influential a journal is, and is therefore an indication of how much your article may be read and cited when published in the journal. Journal Impact Factors should not be compared between research fields, due to the differences in publication and citation rates between fields mentioned above, but within a field, the JIF can give you an indication of the most influential journals.<sup>8</sup>

### *In what form to publish*

The type of publication you choose for disseminating your findings is of great importance for how the research will be assessed in bibliometric studies. Journal articles will usually give better scores than other types of publications such as conference

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<sup>6</sup> Thomson Reuters, 2012

<sup>7</sup> In practice, the Journal Impact Factor is not a clean quota, since some articles are considered "non-citable" and are removed from the denominator.

<sup>8</sup> The Journal Impact Factor can be found in the Thomson Reuters system Journal Citation Reports.

proceedings, monographs and reports, due to the better coverage of journal articles in the bibliometric data sources. Below, I will outline the most common means of publications and what to take into account in respect to each channel.

Beginning with *journal articles*, and as mentioned above, Thomson Reuters primarily indexes about 11 500 international journals in WoS and Elsevier indexes 18 500 journals in Scopus. The reason for focusing on journals is that the journals are the most influential channels in the most fields, but also because journal articles tend to be easier to capture for indexing than other material due to stable titles with re-occurring issues and regular publishing patterns.

When doing bibliometric studies and counting citations, there is a significant difference between the average number of citations to a regular *original* article and a *review* article.<sup>9</sup> Reviews receive on average 2.5 times the number of citations compared to an original article. This is of course due to the review being easier to digest and covering a broader view of the research field. Reviews get more readers and thus on average more citations. Another finding regarding citation counts is that articles that deal with methodology also tend to gather many citations, since everyone that utilizes the method afterwards will have to refer to the article where it was first presented. So writing reviews and methodology articles could both be considered as acceptable methods to boost citation counts for your research.

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<sup>9</sup> This type of scientific review (“overview”) article should not be confused with “Book reviews”, common within the humanities and the social science, that in average reach little attention.

Another frequent form of publication within the academia is *conference proceedings*. In the databases and indices used for bibliometric studies the publication types “Article” and “Conference Proceeding” are being used and counted in quite different ways. Original research articles published in regular international journals are usually captured and indexed by the databases WoS and Elsevier Scopus. Conference publications, on the other hand, are a bit more problematic to gather and therefore conference proceedings are not covered by the databases to the same extent as regular articles.

If you do research in an area where conference proceedings are the primary vehicle for dissemination information, consider “repackaging” and republishing your material as an article, preferably in a journal indexed by WoS or Scopus. An article in a prestigious journal with a high journal impact factor will also usually make a better impression in the publication list of your CV.

In many research fields, *monographs* and *reports* are the primary vehicles for sharing research findings. When doing bibliometric studies based on the commercial data sources from Thomson Reuters and Elsevier these types of documents will not be counted, since they are not included in the indexes from these vendors. Bibliometric studies can be extended to include monographs and reports by using local data, such as the publication database of your organization, but currently there are no methods to count citations to publications that are not covered by the commercial data suppliers.<sup>10</sup> If you are doing research in a field where monographs and reports are of vital

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<sup>10</sup> Google Scholar supplies citation counts for other publication types than journal articles, but there is presently no method to gather these citation counts for batch computations and field normalization.

importance, the same advice as for conference proceedings apply; try to repackage and republish your findings as an article in a well-renowned journal covered by WoS or Scopus.

### *Choosing language*

Journals with articles written in English is the core of WoS and Scopus, which means that articles in English will always be more influential in bibliometric studies. WoS and Scopus cover some journals in non-English languages but citation counts are usually low on articles in these journals, since the audience for these articles usually is smaller than for an English article.

If you primarily write in a non-English language for a domestic audience, the same repackaging and republishing recommendations as for conference proceedings and monographs apply. For instance, consider if your findings can be targeted at an international audience and republished as an article in an international journal. If you do research in a field where dissemination of results primarily is done via monographs in a national language, incentives for repackaging the result as an English article is of course twofold.

### *Using cooperation to increase visibility*

Cooperation in research is important in many aspects, one of them being the aspect of the “marketing” contact area for the resulting publications. If more researchers are involved in the research and the publication process, the article will be exposed to a broader audience. Studies have shown that there is a correlation between the number of authors and the number of

citations to an article, even if so called self-citations are excluded.<sup>11</sup>

Figure 2. shows that the average number of citations to publications involving two researchers (7.8) is almost twice as much as the citation rate for single-author publications (4.2). The field normalised citation rate, adjusted for differences between research fields, also shows an increase in average citation rate (+20%) when going from one author to two.

A disclaimer may be in place here; not all cooperation is beneficial per se. As seen from the graphs above, the correlation between the number of authors and the citations start to decrease above six authors. If fractional counting is used when counting publications and citations, the correlation between the number of authors and indicator values will decrease. In addition, bringing in other researchers just to enhance the exposure of the finished publication may not be justified during the phases of actual research and writing.

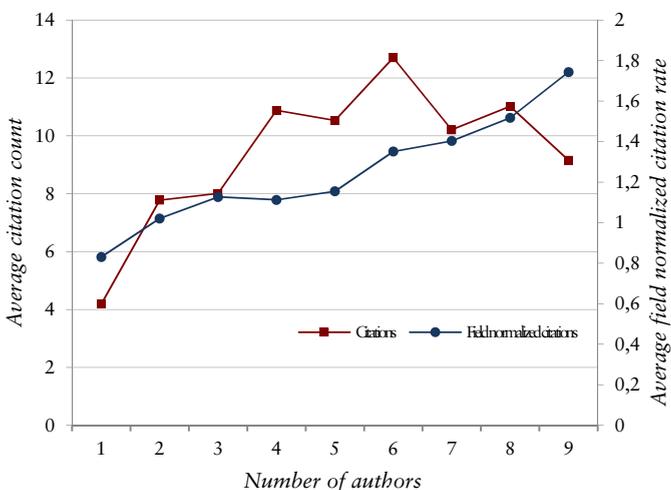


Figure 2. Correlation between the number of authors, the average number of citations and the average field normalized citation rate for publications from KTH Royal Institute of Technology.<sup>12</sup>

Another fair “trick” in the cooperation domain is to cooperate internationally. In bibliometric studies, publications produced as a result of international cooperation are usually seen to give higher citation counts, especially if you cooperate with researchers in countries and regions with high relative citation counts as the United States, United Kingdom or Switzerland. As mentioned before, sometimes international cooperation goes to the extreme as for the particle physicists working at the large hadron collider in CERN, where it is quite common to have around 2300 authors to each article. It is still unclear how bibliometrics should handle this type of publications. If authorship is fractionalized these articles' impact will be reduced to almost nil, if whole counting of authorships is done, they risk to skew the results due to the large and somewhat unfair impact for each researcher and organization involved.

When a researcher puts a reference to your work in her or his reference list, you get a citation and increased impact in bibliometric studies, but you also get increased visibility, since more researchers get aware of your work by studying the reference list of the referring work. This “advertising” effect can lead to more secondary citations from other publications. You

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<sup>12</sup> Citations are measured in Web of Science July 2011 on publications from KTH year 2005 and field normalized citations are calculated on KTH publications from 2005-2009 in the Karolinska Institutet bibliometric system. Both measures are done with open citation window and self-citations are included. Certain data included herein are derived from the Web of Science ® prepared by THOMSON REUTERS ®, Inc. (Thomson®), Philadelphia, Pennsylvania, USA: © Copyright THOMSON REUTERS ® 2010. All rights reserved.

can actually do the best to advertise your own work by referring to your own previous publications whenever this is appropriate. This is called a *self-citation* and in many bibliometric studies self-citations are removed, since they are not seen to represent impact in the rest of the scientific community. However, studies have shown that publications with more self-citations still get higher citation counts, even if the self-citations are removed,<sup>13</sup> presumably by the advertising effect.

### *Making your publications traceable*

A common problem when doing analyses of publications for researchers or research groups is the lack of unique author identifiers in the commercial bibliometric indices. Due to the lack of unique identifiers for authors and affiliations, bibliometric analyses typically involve error-prone searches based on text string matching. To ensure that your publications are credited to you and to your organization it is therefore crucial that names and addresses are stated appropriately.

The names of the authors to the publications are being entered into the database indices in the way they appear in the journal, often just a family name followed by an initial of the given name. If you have a common name like John Smith or Maria Rodriguez, your name may end up like Smith, J and Rodriguez, M in the indices and there might be a lot of other researchers sharing these names. Therefore, the importance of having a unique and consistent author name should not be underestimated.

If you have a common name that you know you might share with other researchers, especially if they are within the same

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<sup>13</sup> Aksnes, 2006

organization and/or field, consider creating a unique author “artist name” by adding an initial from for instance your middle name, for instance Anders Johan Andersson would become Andersson, A J. If you decide to make up a name like this, try to make the decision as early as possible in your research career and be sure to be consistent about its usage, otherwise you might end up having your publication records split up over several “authors” with slightly different names. This is a common problem, especially for researchers with double family names, which might end up with or without a hyphen between the family names or one of the family names interpreted as a given name. For instance, Jessica Wide Cederkvist might end up as author Wide Cederkvist, J; Wide-Cederkvist, J; or even Cederkvist, J W.

There are several initiatives trying to solve the problem with the lack of identifiers for authors, both among the commercial vendors of databases and vendor-independent “global” solutions. Thomson Reuters have their own initiative ResearcherID.com,<sup>14</sup> where researchers can register and do housekeeping of their publication records in the WoS database. This is recommendable to do, especially if you know that your publication records in Web of Science are going to be used for an assessment of your research. Elsevier Scopus also have their own service for author identification, named SciVerse Author Identifier<sup>15</sup> and Google Scholar is building a Google Scholar Citations service<sup>16</sup> with the same purpose. There is also a vendor-neutral global initiative named ORCID – Open Researcher and Creator ID – that was launched in October 2012.

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<sup>14</sup> ResearcherID, 2012

<sup>15</sup> Elsevier, 2012

<sup>16</sup> Google, 2012

If you change your family name during your research career, it is especially important to make use of the vendors' system for author name unification to keep your publication records together. This is because there are yet no automatic methods other than a unique identifier to detect two different family names as belonging to the same researcher. There are examples of female researchers that keep their maiden family name as a researcher “artist” name after getting married, to keep their publication record together.

As mentioned above, the selection of data material used in bibliometric studies that utilize the commercial data sources is usually based on error-prone text string searches. This means that if you want a publication to be credited to your organization, you need to write your organizational affiliation in a way that is easy to understand by an international audience and can be matched using computer-based methods.

Database vendors and other organizations collecting information about scientific publications usually expect author affiliations to be written according to a pattern going from larger organizational units to smaller, followed by city and country information:

Organization, Faculty, Department, Unit, City, Country

If you choose to write your affiliation using a form that starts with the name of your research lab or centre, it may happen that your main organization won't be identified and attributed, since its name will be buried further down in the address and maybe not detected by the system doing the publication selection. If you are affiliated with an organisation with a non-

English name, also check that you are using the proper English name of your organization, rather than trying to guess.

If you do research in a very large collaborating team, make sure that the main author of the publication at least gets information about the proper English name of your organization and the country information to put in the address list:

Your organization, City, Country

It is interesting to notice the changing role of the address here. In a publication economy, the function of the address is changed from that of a postal address to an organizational affiliation. There are still researchers who believe that it is important to put the street name and the zip code in the address. To what use? Do you expect people to write letters to you, so you need the mailman to find his way? In the publication economy, the main purpose of the address is the identification of the right organization to credit the publication to.

*Making your publications accessible*

The world of scholarly publishing is right now going through a transition where the old paper-based reader-pays subscription model is replaced with a new more internet-savvy producer-pays model. This means that journals are beginning to cover the costs for peer review and publishing with a fee from the publishing researcher or her/his organization or funding agency, or by being a part of a publishing-funding learned organization. When the cost of publishing is moved from the reader to the producer, articles can be published on the Internet free for all to read without any barriers as subscriptions or tolls and that is why this new publishing model has been named *Open Access*.

Another way to make the content of the publications freely available to the public is to do *self-archiving* of articles that have been published in a subscription-based journal. The publishers usually gives authors the right to publish the reviewed and accepted last manuscript before publication in an institutional repository, sometimes after an *embargo period* of six to twelve months or even longer after publication. This is called *post-print* self-archiving. The conditions for self-archiving and the length of the embargo periods for various publishers can be checked at the online service SHERPA/RoMEO.<sup>1718</sup>

There are a number of reasons why you should try to get your publications freely available on the Internet:

- It improves the speed and efficiency of research, and also enables interdisciplinary research.
- Your publication will be more visible in the international search engines and may be found and read by a broader audience.
- Studies show that articles published for free access on the Internet gain more citations.<sup>19</sup>
- You have to publish your findings as Open Access if you have funding from a body that mandates it.

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<sup>17</sup> University of Nottingham, 2012

<sup>18</sup> The conditions presently seem to be in a constant flux, so it is safest to do a final check at the website of the publisher or the contract you signed before publishing.

<sup>19</sup> Eysenbach, 2006, Hitchcock, 2012

- Your organization may have a policy for scientific publishing that mandates you to publish your results in Open Access journals or in the organization repository.

While talking about Open Access publishing, a final word of warning may be in place. In the turmoil of the transition of scholarly publishing, a new breed of non-serious, so-called “predatory” publishers with poor or non-existent peer review are entering the scene. If you get invited to publish in a journal that will charge you for accepting your manuscript, check for signals of non-seriousness as spamming e-mails, an amateurish looking website, a non-existent postal address, poor contact details, etc.<sup>20</sup>

Another way to make your work more influential is to publish the underlying research data for public re-use. There are studies showing a correlation between public research data and the number of citations to the publication(s) based on the data.<sup>21 22</sup> However, it is still unclear whether there is a *causal* connection between the publication of data and the increased number of citations or if the correlation is caused by some other related parameter as the funding or the number of researchers involved in the study. On the other hand, no one has so far shown a negative correlation between published data and the number of citations.

### *Using social media for increased visibility*

In today's digital age, the old saying “publish or perish” can be augmented with a more modern counterpart “get visible or

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<sup>20</sup> Beall, 2012

<sup>21</sup> Dorch, 2012

<sup>22</sup> Sears, 2011

vanish”.<sup>23</sup> Besides making publications and data public, developing a comprehensive online presence can leverage the impact of your research. Using online services as Twitter, Facebook, Google Plus, LinkedIn, Mendeley or Zotero can make your research visible to a larger audience and create a debate around your work.<sup>24</sup> <sup>25</sup> Establishing a blog focusing on your research is even better, especially in combination with the other social media tools.

### *Ensuring findability and preservation*

If you want to reach out with your research results and gain impact, it is important that your publications are searchable in the global search engines on the Internet and also preserved for future reference. This is where the publication database of your organization - the *institutional repository* - can play an important role. Publishing in a *subject repository* can also increase the findability and preservation of your work.

In the publication economy of today, most research organizations run a publication database where information about the publications of its researchers is stored. The primary content of the publication database is not publications as such, but *metadata* records with information about the publications and it is used to market and keep track of the output of the researchers in the organization. These publication records are often used as a basis for bibliometric studies.

Publication databases are often used for the following purposes:

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<sup>23</sup> Science Online, 2012

<sup>24</sup> Mendeley, 2012

<sup>25</sup> Zotero, 2012

- to generate publication lists on web pages for departments, research groups and individual researchers
- to generate publication lists for CV's and project applications
- to visualize and market research results from the organization
- as a source for bibliometric analyses
- to make research output more visible to search engines as Google and Google Scholar

In many cases, records can be imported to the publication database from commercial databases as WoS and Scopus. This is often done by the staff at the university library. However, if you have produced publications as monographs, reports and conference proceedings papers that not are indexed in the commercial databases, you usually have to register them manually yourself or get someone to do it for you.

The publication databases are often extended into institutional *repositories*, which can cater for the *full text* of publications, usually as PDF files, besides the metadata records needed for marketing and bibliometrics. This is where your organization's repository comes in handy for disseminating your publication in full text, doing self-archiving, as previously mentioned in the section about Open Access. If you find that you have the right to do self-archiving of your manuscript, do this to increase its visibility and impact.

Subject based repositories give you an opportunity to increase the effectiveness of your reputation building by giving your research and early visibility and allowing your researcher

community to cooperate more efficiently. The fast dissemination to your peers is crucial when it comes to impact and citations. There are studies that show that manuscripts published in subject repositories as *arXiv* "... yields a citation advantage of a factor five."<sup>26</sup>

If you care about the number of citations to your work, you should try to refer from the pre-print to the finally published article, since several versions of an article in different locations can lead to a phenomenon that is known as *citation fragmentation*. Fragmentation occurs when each variant of your publication captures only a portion of the citations the unified publication would get. The effect of citation fragmentation can often be spotted in Google Scholar, where you may find several incarnations the same publication with different citation counts. In Google Scholar, registering for an account and bringing the variants together to one single record can alleviate this phenomenon. This is not possible to do for the other bibliometric database vendors, since they will only index the journal variant of the publication, and then the citations referring to the pre-prints and any other variants of the publication in repositories will get lost.

### **Discussion: why an emerging publication economy?**

Why have publication records and bibliometrics started to be of such importance that we now even are inclined to call it a publication economy?<sup>27</sup> The underlying reasons can probably be spelled globalization and tightening competition for resources and knowledge around the world, together with a historic development of society. Have we perhaps not only transcended

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<sup>26</sup> Gentil-Beccot and Mele, 2012

<sup>27</sup> Larsson, 2009

the farming era and the industrial era, but also the newly celebrated information era and are now heading into a new *era of knowledge*<sup>28</sup> One in which education and research is the *industry* and the higher education and research institutions are the *factories* that produce this knowledge?

Research once used to be reserved for an academic elite, consisting of a few wealthy aristocrats that could support themselves while getting educated and producing science. At that time, research did not put any large expenses on the society and the scholars could therefore have a large degree of freedom in their research. Today, on the other hand, a large portion of the population goes to university and higher education and research is a major financial undertaking for the society. If you are putting a lot of resources into the production of something, don't you then want to be in control over what you get in return for your invested money? At least this is what industrial managers have been doing for over a century now, running business intelligence systems with statistics on their production.

But what is the output of a knowledge production? Knowledge is a much more esoteric and multi-faceted product than, for instance, cars, refrigerators, computer programs or civil services. What should be measured if we want to assess the results of a knowledge production? In the urging need for something to measure, governments and university managements turn to what *can* be measured, rather than what *should* be measured, since no one seems to know the answer to the latter question. Publications and citations are some of the few measurable results of a knowledge production, and that is why they so frequently are being used to assess the return on

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<sup>28</sup> See: Castells, 2000

investment in research. Governments and university managements seem to be acting a bit like in the old joke about the man who lost his car keys one night and started looking for them, not where he lost them, but beneath the lamppost, because there it was light so he could see.

Using bibliometrics to assess research is both right and wrong at the same time. On one hand, there is a legitimate reason to try to find measures on return of investment in research. On the other hand, many more factors should be taken into account when doing the measuring.

To further complicate the picture for the assessment of science, the output of the scholarly society is not a static product, which can be measured without intervening with the production process. In other words, if we start to measure research in certain ways and allocate funding according to the results, researchers will adapt to this and the measurements will start to be an incitement, driving research in directions towards the measurable. To quote a recent critical article: “Metrics of quantity once were the means to assess the performance of researchers, but now they have become an end in their own right.”<sup>29</sup>

Here, I would like to issue a call for help from scholars in various disciplines. If you know that governments and university managements want to measure and put numbers on the results of your research; which measures should be used to make the right assessments and drive the research in the right direction? You are the ones that should know, and if you don't

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<sup>29</sup> Fischer, Ritchie and Hanspach, 2012

help, your research will only be measured with inferior, one-dimensional tools as bibliometrics.

Looking from this economic perspective, we can see how the societal role of the scholar is changing over time. What used to be an economically independent scholar with freedom to do research driven by curiosity is now a worker in a production machine for the knowledge society. In the short run it is off course good that the society doesn't spend money on research that don't give any apparent benefits in return. But how do we know in the end what benefits are to be gained from which research? If researchers only focus on delivering short-term accountable results and managing their publication assets, what will happen with the long-term basic research that may deliver results that are important in 20-30 years?

There is the school of old academics that claims that researchers should be given funding and then left alone to do their research in peace. It is a bit like the sayings of a famous entrepreneur nearly a century ago: How do you run a successful company? You hire competent and talented people and leave them alone to do their job as they think best fit.

But how do we know which researchers are talented and should be recruited and get this safe long-term financing? And how many of them should be financed? And are we not re-building the old academic ivory tower with an elevated elite of untouchables if we do so?

I would like to end this discussion on the publication economy with a quote from a recent critical letter in the journal *Trends in Ecology and Evolution*:<sup>30</sup>

*The modern mantra of quantity is taking a heavy toll on two prerequisites for generating wisdom: creativity and reflection.*

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# Geopolitics of sensing and knowing: On (de)coloniality, border thinking, and epistemic disobedience

*Walter D. Mignolo*

**D**ecoloniality is, in the first place, a concept whose point of origination was the Third World.<sup>1</sup> Better yet, it emerged at the very moment in which the three world division was collapsing and the celebration of the end of history and a new world order was emerging. The nature of its impact was similar to the impact produced by the introduction of the concept of “biopolitics”, whose point of origination was Europe. Like its European counterpart, “coloniality” moved to the center of international debates in the non-European world as well as in “former Eastern Europe.” While “biopolitics” moved to center stage in “former Western Europe” (cf., the European Union) and the United States, as well as among some intellectual minorities of the non-European followers of ideas that originated in Europe, but who adapt them to local circumstances, “coloniality” offers a needed sense of comfort to

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<sup>1</sup> An earlier version of this article appeared in the journal *Transversal*, Vol. 08, 2009. See: <http://eipcp.net/transversal/0112/mignolo/en>.

mainly people of color in developing countries, migrants and, in general, to a vast quantitative majority whose life experiences, long and short-term memories, languages and categories of thoughts are alienated to life experience, long and short-term memories, languages and categories of thought that brought about the concept of “biopolitics” to account for mechanisms of control and state regulations.<sup>2</sup>

Modernity, postmodernity and altermodernity have their historical grounding in the Enlightenment and the French Revolution. Decoloniality has its historical grounding in the Bandung Conference of 1955, in which 29 countries from Asia and Africa gathered. The main goal of the conference was to find a common ground and vision for the future that was neither capitalism nor communism. That way was “decolonization”. It was not “a third way” à la Giddens, but a delinking from the two major Western macro-narratives. The conference of the Non-Aligned countries followed suit in 1961, and took place in Belgrade. On that occasion, several Latin American countries joined forces with Asian and African countries. Frantz Fanon’s *The Wretched of the Earth* was also published in 1961. Thus, the political and epistemic foundations of decoloniality had been established in fifty-five years. From then until now and from now to the future, it will be decoloniality all the way down – not as a new universal that presents itself as the right one that supersedes all the previous and existing ones, but as an option. By presenting itself as an option, the decolonial opens up a way of thinking that delinks from the chronologies of new epistemes or new paradigms

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<sup>2</sup> For a critique of the shortcomings of Giorgio Agamben’s argument seen from the experiences, memories and sensibilities of colonial histories and decolonial reasoning, see: de Oto and Quintana, 2010

(modern, postmodern, altermodern, Newtonian science, quantum theory, the theory of relativity, etc.). Epistemes and paradigms are not alien to decolonial thinking. They cannot be, but are no longer the point of reference and of epistemic legitimacy. While the Bandung Conference pronounced itself in the political terrain as neither capitalism nor communism but as decolonization, today, thinking decolonially is concerned with global equality and economic justice, but it also asserts that Western democracy and socialism are not the only two models to orient our thinking and our doing. Decolonial arguments promote the communal as another option next to capitalism and communism. In the spirit of Bandung, Aymara intellectual, Simon Yampara, makes clear that Aymaras are neither capitalist nor communist. They promote decolonial thinking and communal doing.<sup>3</sup>

Because decoloniality's point of origination was the Third World, in its diversity of local histories and different times and Western imperial countries that first interfered with those local histories – be it in Tawantinsuyu in the sixteenth century, China in the nineteenth century or Iraq from the beginning of the twentieth (France and England) to the beginning of the twenty-first century (the US) – border thinking is the epistemic singularity of any decolonial project. Why? Because border epistemology is the epistemology of the *anthropoi*, who do not want to submit to *humanitas*, but at the same time cannot avoid it. Decoloniality and border thinking/sensing/doing are then strictly interconnected since decoloniality couldn't be Cartesian or Marxian. In other words, decoloniality's point of origination

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<sup>3</sup> On the decolonial option, as described by Simon Yampara and endorsed by many Aymara and Quechua intellectuals and activists, see Flores Pinto, 2009. See also Mignolo, 2010a

in the Third World connects to “immigrant consciousness” in Western Europe and the US today. “Immigrant consciousness” is located in the routes of dispersion of decolonial and border thinking.

## I

Points of origination and routes of dispersion are key concepts to trace geo-politics of knowing/sensing/believing as well as body-politics of knowing/sensing/understanding. When Frantz Fanon closes his exploration in *Black Skin/White Masks* (1967) with a prayer:

*Oh my body, make of me always a man who questions!*

He expressed, in a single sentence, the basic categories of border epistemology: the biographical sensing of the Black body in the Third World, anchoring a politics of knowledge that is both ingrained in the body and in local histories. That is, thinking geo- and body-politically. Now if the point of origination of border thinking/sensing and doing is the Third World, and its routes of dispersion traveled through migrants from the Third to the First World,<sup>4</sup> then border thinking created the conditions to link border epistemology with immigrant consciousness and, consequently, delink from territorial and imperial epistemology grounded on theological (Renaissance) and egological (Enlightenment) politics of knowledge. As it is well known, theo- and ego-politics of knowledge were grounded in the suppression of sensing and the body, and of its geo-historical

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<sup>4</sup> Les Indigènes de la République, in France, is an outstanding case of border thinking and immigrant consciousness. See: “The Decolonizing Struggle in France. An Interview with Houria Bouteldja”, 2009

location. It was precisely that suppression that made it possible for both theo- and ego-politics of knowledge to claim universality.

Border epistemology goes hand in hand with decoloniality. Why? Because decoloniality focuses on changing the terms of the conversation and not only its content. How does border epistemology work? The most enduring legacy of the Bandung Conference was delinking; delinking from capitalism and communism, that is, from Enlightenment political theory (liberalism and republicanism – Locke, Montesquieu) and political economy (Smith) as well as from its opposition, socialism-communism. Now, once you delink, where do you go? You have to go to the reservoir of the ways of life and modes of thinking that have been disqualified by Christian theology since the Renaissance and which continue expanding through secular philosophy and the sciences, for you cannot find your way out in the reservoir of modernity (Greece, Rome, the Renaissance, the Enlightenment). If you go there, you remain chained to the illusion that there is no other way of thinking, doing and living. Modern/colonial racism, that is, the logic of racialization that emerged in the sixteenth century, has two dimensions (ontological and epistemic) and one single purpose: to rank as inferior all languages beyond Greek and Latin and the six modern European languages from the domain of sustainable knowledge and to maintain the enunciative privilege of the Renaissance and Enlightenment European institutions, men and categories of thought. Languages that were not apt for rational thinking (either theological or secular) were considered languages that revealed the inferiority of the human beings speaking them. What could a person that was not born speaking one of the privileged languages and that was not educated in privileged institutions do? Either he or she accepts

his or her inferiority or makes an effort to demonstrate that he or she was a human being equal to those who placed him or her as second class. That is, two of the choices are to accept the humiliation of being inferior to those who decided that you are inferior or to assimilate. And to assimilate means that you accepted your inferiority and resigned to playing the game that is not yours, but that has been imposed upon you – or the third option is border thinking and border epistemology.

How does it work? Suppose that you belong to the category of the *anthropos* – the *anthropos* stands for the concept of the “other” in most contemporary debates about alterity – the “other,” however, doesn’t exist ontologically. It is a discursive invention. Who invented “the other” if not *the same* in the process of constructing *the same*? Such an invention is the outcome of an enunciation. The enunciation doesn’t name an existing entity, but invents it. The enunciation needs an enunciator (agent), an institution (not everyone can invent the *anthropos*), but to impose the *anthropos* as “the other” in the collective imaginary, it is necessary to be in a position of managing the discourse (verbal, visual, audial) by which you name and describe an entity (the *anthropos* or “the other”) and succeed in making believe that it exists. Today, the *anthropos* (“the other”) impinges on the lives of men and women of color, gays and lesbians, people and languages of the non-European/US world from China to the Middle East and from Bolivia to Ghana. I am not saying that Bolivian, Ghanaian, Middle Eastern or Chinese are ontologically inferior, for there is no way to empirically determine such ranking. I am saying that there is a territorial and imperial epistemology that invented and established such categories and rankings. So once you realize that your inferiority is a fiction created to dominate you, and you do not want to either assimilate or accept in

resignation the bad luck of having been born equal to all human beings, but having lost your equality shortly after being born, because of the place you were born, then you delink. Delinking means that you do not accept the options that are available to you. That is the legacy of the Bandung Conference. The participants of the conference opted to delink: neither capitalism nor communism. The option was decolonization. The splendor of the Bandung Conference was precisely in showing that another way is possible. Its limit was to remain within the domain of political and economic delinking. The epistemic question was not raised. However, the conditions for raising the epistemic question were already there. It was raised around 35 years later by Colombian sociologist, Orlando Fals Borda, who has been very much involved in the debates on dependency theory. Dependency theory, in Luso and Hispanic America, as well as in Caribbean reasoning and the quest for decolonization in the Caribbean *New World Thoughts*,<sup>5</sup> emerged in the general atmosphere of the Bandung Conference and the invention of the Third World. Here you have a case in point: the Third World was not invented by the people who inhabit the Third World, but by men and institutions, and languages and categories of thoughts in the First World. Dependency Theory was a response to the fact that the myth of development and modernization was a myth to hide the fact that Third World countries cannot develop and modernize under imperial conditions. Similar arguments were advanced in the same period, by a group of Caribbean economists and sociologists, known as the New World studies group. The guiding line of their research was independent thought and Caribbean freedom. Independent thought requires border

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<sup>5</sup> Meeks and Girvan (eds.), 2010

thinking for the simple reason that it cannot be achieved within the categories of Western thoughts and experiences.

You may object to dependency theorists and the New World studies group having written (the former) in Spanish and Portuguese and (the latter) in English. So how can you delink if you are trapped within the categories of Western modern and imperial languages? You can, for delinking and border thinking occurs wherever the conditions are appropriate and the awareness of coloniality (even if you do not use the word) comes into being. Writing in Spanish, Portuguese and English, dependency theorists and the New World studies group were colonial subjects, that is, subjects dwelling in the local histories and experiences of colonial histories. For Spanish and Portuguese in South America have the same grammar as in Spain or Portugal respectively, but they inhabit different bodies, sensibilities, memories and overall different world-sensing. I use the expression “world-sensing” instead of “world vision,” because the latter, restricted and privileged by Western epistemology, blocked the affects and the realms of the senses beyond the eyes. The bodies that thought independent thoughts and independence from economic dependency, were bodies who wrote in modern/colonial languages. For that reason, they needed to create categories of thought that were not derived from European political theory and economy. They needed to delink and to think within the borders they where inhabiting – not borders of nation-states, but borders of the modern/colonial world, epistemic and ontological borders. The New World group wrote in English, but inhabited the memories of the Middle Passage, of the history of slavery, of runaway slaves and of the plantation economy. That experience was not what nourished Adam Smith’s liberal thinking or Marx’s socialist

thinking – the experience of the plantation and the legacies of slavery nourished border thinking.

We, the *anthropoi*, who dwell and think in the borders with decolonial awareness, are already on the way to delinking, and in order to delink, you need to be epistemically disobedient. You will pay the price, for journals, magazines, disciplines in the social sciences, and humanities as well as the social sciences and professional schools, are territorial. In other words, border thinking is the necessary condition for thinking decolonially. And when we, the *anthropoi*, write in modern, Western imperial languages (Spanish, English, French, German, Portuguese or Italian), we write with our bodies on the border. Our senses have been trained by life to perceive the difference, to sense that we have been made *anthropoi*, that we do not belong or belong partially to the sphere and the eyes that look at us as *anthropoi*, as “others.” Border thinking is, in other words, the thinking of us, the *anthropoi*, who do not aspire to become *humanitas*, because it was the enunciation of the *humanitas* that made us *anthropoi*. We delink from the *humanitas*, we become epistemically disobedient, and think and do decolonially, dwelling and thinking in the borders of local histories confronting global designs.

Decolonial thinking can be done within existing academic structure, but is not a way of thinking that will have enthusiastic support of the administration or accumulate grants and fellowships. It can be done however within the academia through courses, seminars, workshops, mentoring students and working with colleagues who have the same conviction. Their belief, our belief, is that decolonial thinking is an important contribution to democratic futures and harmonious society but that is not exactly the main goals of the academy today. Of

course the rhetoric is there, and you will find it everywhere. But the true goals of institutions of higher education is to compete with other institutions of higher education and move up in the several recognized poles of global ranking, from the Shanghai Jiao Tong to the National Research Council in the US. All these ranking favor “success, innovation and excellence.” Of course you can think of what would these three words would mean in decolonial thinking. Decolonial thinking is certainly innovative, it should always strive for excellence otherwise it will not be convincing. And it should be successful in offering an option to many people who do not find in existing option a way of thinking and being that fits their own experience. But that is not what the institutions of higher education means by these three words. The words are used in the sense of: personal success, innovation and excellence for progress and competition, rather than for the good of the many. In spite of the fact that institutions of higher education function within the rhetoric of modernity, and the rhetoric of modernity has been nourishing a conception of the world that is today showing its failure, border thinking and decolonial conceptions of the world are flourishing in the margin of academic institutions and outside of it: in the wide and open “academy of life” of which “academic institutions of higher education” are a very small part.

Examples can be multiplied. The genealogy of border thinking, of thinking and doing decolonially, is being constructed on several fronts.<sup>6</sup> Let’s recall here, Frantz Fanon’s very well-

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<sup>6</sup> It is not just a question of the Native Americans, as I often hear after my lectures. Around the world, critical intellectuals are aware of the limits of Western archives, from the left and from the right. In the case of China, see Hui, 1991. For an analysis of it, see Yongle, 2010. For the Muslim world, see al-Jabri, 1995. In similar spirit, I wrote Mignolo, 1995. See also the work being done at and by the *Caribbean*

known legacies and reread some of his insights in the context of my argument. I have already mentioned the last line of *Black Skin/White Masks*, a book that precedes the Bandung Conference by three years, but a book that was not alien to the global conditions that prompted Bandung. Perhaps the most radical theoretical concept introduced by Fanon is that of “sociogenesis”. Sociogenesis embodies all: delinking, border thinking and epistemic disobedience; delinking from the phylogenetic and ontogenetic options, the dichotomy of territorial and modern thinking. Sociogenesis (in the sphere of body politics), like the logic of the Bandung Conference (in the sphere of geopolitics), is not a happy marriage between the two, a hybrid concept of sorts, but the opening up to the grammar of decoloniality.<sup>7</sup> How does that grammar work? Remember, sociogenesis is a concept that is not based on the logic of denotation (like phylo- and ontogenesis), but on the logic of being classified, on epistemic and ontological racism: you are inferior ontologically and therefore epistemically; you are inferior epistemically and therefore ontologically.<sup>8</sup> Sociogenesis as a concept emerges at the moment of the awareness that you are a “Negro”, not because of the color of your skin, but because of the modern racial imaginary of the modern colonial world – you have been made a “Negro” by a discourse, whose rules you cannot control, and there is no room for complaint, like Josef K., in Kafka’s *The Process*. Sociogenesis came out of thinking and dwelling in the borders and thinking decolonially, for it

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*Philosophical Association*, <http://www.caribbeanphilosophical-association.org/>. There is no intention here to become post-post and be attentive to the last missive of the European left, but to also move South of the North Atlantic.

<sup>7</sup> Mignolo, 2010b

<sup>8</sup> Maldonado-Torres, 2007

came out from *existentia Africana* as Lewis Gordon<sup>9</sup> would have it, but it could have come out of any other similar experiences of racialized individuals. It is unlikely that sociogenesis could have been a concept that originated in and from the European experience, except from the immigrants' today. And in fact, Fanon was already an immigrant from the Third World in France and it was that experience that brought to light the fact that phylogenesis and ontogenesis could not account for the experience of the colonial and racialized subject. That experience could be rendered in "content" (experience as an object) by existing disciplines (sociology, psychology, history, etc.) that could talk "about" the "Negro" and "describe" his experience, but cannot supplant thinking as a "Negro" (experience constitutive of the subject) at the moment you realize that you have been made a "Negro" by the imperial imaginary of the Western world. Certainly, the image of the Black as inferior human being and descendant of Canaan was already imprinted in the Christian imaginary.<sup>10</sup> But I am talking here about the resemantization of that imaginary in the sixteenth century that occurred with the massive slave trade in the Atlantic world. At that moment, Africans and slavery were one and the same. It was not the case before 1500.

Sociogenesis is sustained in and by border epistemology, not in and by the territorial epistemology that undergird the diversity

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<sup>9</sup> Gordon, 2000

<sup>10</sup> As it is well-known and discussed, Noah cursed the youngest son of Ham, Canaan, for an act of disrespect that Ham committed toward his father. As Canaan was supposedly the ancestor of the African people, the curse provided the justification for the enslavement of them by Western Christians and in the ecclesiastical tradition. See Popes for Slavery (<http://www.romancatholicism.org/popes-slavery.htm>).

of all existing disciplines. Sociogenesis is a concept that allows us to delink precisely from Western thoughts, even if Fanon writes in imperial/colonial French and not in French Creole. By delinking, Fanon engages in epistemic disobedience. There is no other way of knowing, doing and being decolonially than simultaneously engaging in border thinking, delinking and epistemic disobedience. Bandung showed us the way to delink geopolitically from capitalism and communism; Fanon how to delink body-politically, two ways of delinking from the colonial matrix of power and of dwelling in border thinking. Why border thinking here? Because sociogenesis presupposes it and it is understood in relation to and detachment from phylogenesis and ontogenesis. At the same time, if sociogenesis changes terrain, it is no longer responding to the logic, the experience and the needs that prompted the concept of phylogenesis in Darwin and ontogenesis in Freud. Sociogenesis is no longer subsumable in the linear paradigm of Foucault's epistemic breaks.

## II

The question that questions the enunciation (when, why, where, what for) leads us to the knowledge of creation and transformations at the very heart of any decolonial inquiries necessary to imagine and build global futures. Why? Because knowledge creation and transformation always responds to actors' desires and needs as well as to institutional demands. Knowledge as such is always anchored in historical, economic and politically-driven projects. What "coloniality" unveiled is the imperial dimension of Western knowledge that has been built, transformed and disseminated over the past 500 years. "Coloniality of knowledge and of being" is hidden behind the celebration of epistemic breaks and paradigmatic changes.

Epistemic breaks and paradigmatic changes belong and happen within a conception of knowledge that originated in the European Renaissance (that is, in that space and at that time), and reached the heart of Europe (Germany, England and France) through the Enlightenment.

In contrast to decoloniality, the point of origination of concepts such as “modernity” and “postmodernity,” epistemic breaks and paradigmatic changes was Europe and its internal history. These concepts are not universal, not even global. They are regional, and as regional, they have their own value as any other regional configuration and transformation of knowledge. The only difference is that the local histories of European concepts became global designs. That means that concepts, such as the aforementioned, were needed to make sense of actors’ desires and institutional demands. When postmodernity or paradigmatic changes become traveling concepts that follow the routes of dispersion and reach Argentina or Iran, China or Algeria, they do it as part of the expansion of Western civilization. Actors from the periphery noticed that postmodernity doesn’t mean the same in France, Germany or England as in Argentina or China.

But if it is possible to say that postmodernity in France and China are different, it is because we assume that there is something that can be identified as “postmodernity,” whatever that is. At the end of the day, it doesn’t matter what it is, but what the people engaged in the conversation for or against it assume it is. What matters is the enunciation, not so much the enunciated. Once established, a set of complementary concepts saw daylight, such as peripheral, alternative or subaltern modernities, and epistemic breaks and paradigmatic changes applied to local colonial histories. First of all, modernity is not

an ontological unfolding of history but the hegemonic narrative of Western civilization. So, there is no need to be modern. Even better, it is urgent to delink from the dream that if you are not modern, you are out of history. Alternative or subaltern modernities claiming their right to exist, reaffirm the imperialism of Western modernity disguised as universal modernity. Secondly, if modernity is to be accepted as a narrative and not as ontology, one answer is to claim “our modernity,” as Partha Chatterjee does in recasting the past and the role of India in global history. It is imperative to eliminate the concept of the “pre-modern” that serves imperial modernity so well and that speaks with pride instead of the “non-modern,” which implies delinking and border thinking for the non-modern shall be argued in its legitimacy to think and build just and equitable futures beyond the logic of coloniality that is constitutive of the rhetoric of modernity.

Such concepts are the materialization of the point of origination and the routes of dispersion that maintain epistemic dependency. The decolonial response has instead simply been: “it is our modernity,” as Indian political theorist Partha Chatterjee has forcefully and convincingly argued.<sup>11</sup> Once border sensing/thinking emerged, the decolonial option came into being and by coming into being as an option, it revealed that modernity (peripheral or just modernity, subaltern or just modernity, alternative or just modernity) are just other options and not the “natural” unfolding of time. Modernity and postmodernity are options, not ontological moments of universal history, and so are subaltern, alternative or peripheral modernities. All of them are options that deny and attempt to

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<sup>11</sup> Chatterjee, 1997. See also Mignolo, 2009.

prevent the unfolding of border thinking and the decolonial option.

Postmodernity did not follow the same path as modernity. There were not, as far as I know, complementary concepts such as peripheral, alternative or subaltern post-modernities. But the void was quickly filled with the materialization of the concept of “post-colonialism.” Interestingly enough, the point of origination of postcolonialism was England and the United States, that is, it originated in Euro-America and in the English-speaking world rather than in the Third World. However, the actors who introduced it came from the non-European world. It would have indeed been difficult for a British, German or French intellectual to come up with the concept of “postcolonialism”. Not impossible, but of low probability. One of the main reasons is that colonial legacies experienced in the colonies are not part of the life and death of postmodern and poststructuralist theoreticians. By the same token, postmodernity and poststructuralism are not at the heart of intellectuals in India or Sub-Saharan Africa (the second point of reference of postcolonialism). Ashis Nandy’s or Vandana Shiva’s work in India are a manifestation of decolonial thinking rather than postcolonial theory. Paulin J. Hountondji and Kwasi Wiredu in Africa are closer to the legacies of decolonization than to postcolonialism. Aymara Patzi Paco in Bolivia or Lewis Gordon, in Jamaica/US argue in decolonial rather than postcolonial terms. Since the point of origination of postcolonialism was mainly England and the US, and the main actors were Third World intellectuals (as Arif Dirlik would put it), it is easier for European intellectuals to endorse postcolonialism (as it is happening in Germany) than decolonial thinking. As I said before, decolonial thinking is more akin to the skin and the geo-historical locations of migrants from the

Third World, than to the skin of “native Europeans” in the First World. Nothing prevents a white body in Western Europe from sensing how coloniality works in non-European bodies. That understanding would be rational and intellectual, not experiential. Therefore, for a white European body to think decolonially means to give; to give in a parallel way than a body of color formed in colonial histories has to give if that body wants to inhabit postmodern and poststructuralist theories.

### III

Today we can see three scenarios in which global futures will be unfolding:

- Rewesternization and the unfinished project of Western modernity
- Dewesternization and the limits of Western modernity
- Decoloniality and the emergence of the global political society delinking from rewesternization and dewesternization

Rewesternization and dewesternization are struggles in the spheres of the control of authority and of the economy. The first is the project of President Barack Obama, repairing the damages caused in the US and Western leadership by the government of George W. Bush and Dick Cheney. Dewesternization is the politics of economically powerful emerging economies (China, Singapore, Indonesia, Brazil and Turkey, now joined by Japan). Decoloniality is the project that defines and motivates the emergence of a global political society delinking from rewesternization and dewesternization. Albeit the complex, ambiguous, mixed and changing things in

“reality”, it is already possible to distinguish the orientations of the three major projects in which global futures are being built.

Border thinking is the necessary condition for the existence of dewesternizing and decolonial projects. However, the aims of both projects differ quite radically. It is the necessary condition, because to affirm dewesternization implies to think and argue from the exteriority of modern Westernization itself. Exteriority is not an outside of capitalism and of Western civilization, but the outside created in the processes of creating the inside. The inside of Western modernity has been built since the Renaissance upon the double, simultaneous and continuous colonization of space and time. Haitian anthropologist, Michel-Rolph Trouillot, puts it this way:

If modernization has to do with the creation of place – as a relation within a definite space – modernity has to do with the projection of that place – the local – against a spatial background that is theoretically unlimited. Modernity has to do with both the relationship between place and space, and the relation between place and time. In order to prefigure the theoretically unlimited space – as opposed to the space within which management occurs – one needs to relate place to time or to address a unique temporality, the position of the subject located in that place. Modernity has to do with those aspects and moments in the development of world capitalism that require the projection of the individual or

collective subject against both space and time. It has to do with historicity.<sup>12</sup>

Not only have people fallen out of history (in exteriority) in general, but also out of non-modern forms of government and of economic organization. “Non-modern” consists of the Incas in Tawantinsuyu, China in the Ming Dynasty and the Mao Revolution, Africa in general, Russia and Japan, just to name a few. Non-modern states and economies (like China and Brazil) are not only growing economically, but also confronting the directives they received in the past from Western institutions. To do so, Marxism doesn’t provide the tools to think in exteriority. Marxism is a modern European invention that emerged to confront, in Europe itself, both Christian theology and liberal economy (that is, capitalism). Marxism in the colonies and in the non-modern world in general is limited, for it remains within the colonial matrix of power that creates exteriorities in space and time (barbarians, primitives and the underdeveloped). For the same reason, Marxism is of limited help to migrants in Europe and the US from the non-European world. To think in exteriority demands border epistemology. Now, border epistemology serves both the purposes of dewesternization and decoloniality – but dewesternization stops short of decoloniality.

Border thinking leading to the decolonial option is becoming a way of being, thinking and doing of the global political society. The global political society defines itself in its processes of thinking and doing decolonially. Its actors and institutions connect the political society in the non-European/US world with

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<sup>12</sup> Trouillot, 2002, pp. 849

migrants from the non-European/US world to “former Western Europe” (e.g., the European Union) and the US. The global political society transforms the organization and regulations established by political authorities (Western monarchies and secular bourgeois states), economic practices and political economy (e.g., capitalism) and the civil society necessary for the existence of the state and the economy.

The worldwide emerging political society, including the struggles of migrants who reject assimilation and promote decolonization,<sup>13</sup> carries on the legacies of the Bandung Conference. If during the Cold War, decolonization was neither communist nor capitalist, at the beginning of the twenty-first

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<sup>13</sup> “Les Indigènes de la République,” See note 4 above.

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# Confero: Essays on Education, Philosophy and Politics

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