Theoretical Computer Science 429 (2012) 222–226

Contents lists available at SciVerse ScienceDirect

Theoretical Computer Science

journal homepage: www.elsevier.com/locate/tcs

A linear ordering of a multi-parameter universe is usually nonsense

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A B S T R A C T

We are talking about the most beautiful woman in the world, about the largest cave in Europe, about the best sportsman of a particular year. We have even started to rank companies according to their "intellectual assets", universities according to their "importance", and scientists according to their "impact". It is time that we protest against this culture of "linearity" in a world that is multi-faceted, where many things cannot be described by one parameter, but only (if at all) by a very long vector of parameters.

In this paper, I investigate this phenomenon to some extent as it applies to (computer) scientists and also indicate a link to the information avalanche we are confronted with.

1. Introduction

We are in an age with the tendency that everything is measured by a single number. This may be alright for simple physical properties like temperature or height, but breaks down for all more complex matters. What does beauty in a person mean? The shape of the nose, the flow of hair, the posture, the smile, . . .? What do I mean if I say this is the largest cave: do I mean the one with largest volume, with largest height, with largest average height, with the longest corridor, . . .? How can I compare in sports a football-player with a golfer, a skier with a butterfly-swimmer, . . .? Is a company better off with a good sales-force, or with better products? And is a sales-force better if it has one outstanding individual and only second rate others, or if it has an all rounded team, . . .? How can I say that a university is better than another one? Do I count the papers published, the Nobel-laureates working (or originating) at that institution, the numbers of companies founded by staff members, the number of students graduated, the positions of students that come from that university, . . .? And how can I judge the impact of a scientist: by publications? If so, do I count one break-through paper more than an excellent textbook that influences thousands? Is extending a well established area more valuable than introducing completely new ideas? Or should I rather count the Ph.D. students supervised, may be taking their "quality" into account or not?

The beauty is that there is (fortunately) no linear ordering of most things (except in some very capitalistic countries where it is the amount of monetary value), and there is certainly no best scientist. I do not only mean that it is difficult to compare the impact of a computer scientist from the impact of a chemist, or to compare the impact of a computer scientist from the one of an archaeologist, or the impact of a theoretician compared to someone working in the applied areas. No, I mean more. Whatever the research area may be, there are so many parameters like background knowledge, imagination, deep thinking, the skill to find examples or counterexamples, the skill to teach research, the way to guide young researchers, etc.: no single parameter can summarize such a variety of properties.

The above list can be extended indefinitely. Lest I am misunderstood: there will always be situations when we have to judge whether one person is more suitable for a particular task or a particular job than another person. The point is, in such cases it will depend on many parameters and also on the point of view of the person(s) judging the candidate.

In this paper, I will concentrate on two issues. In Section 2, on the kind of parameters that are considered today for defining a good researcher. I will try a somewhat superficial analysis and propose one more parameter. Then in Section 3,

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0304-3975/$ – see front matter
doi:10.1016/j.tcs.2011.12.042

I will ask a question on how to find scientific contributions in a world where we are being overwhelmed with information. In particular, I will look into the role of scientific journals and possible new versions thereof.

In Section 4 I will reiterate what I am saying here right now, loud and clear: try to protest whenever you have a chance that some organization above you wants a ranking of institutes, of colleagues, or what have you. We have to make sure that all, including all governmental and EU bodies, understand that a single number can potentially describe the value of some material goods, but cannot be used as measurement for more complex issues.

2. How to measure the quality of a scientist

Let us first quote from [14]: *"How does one measure productivity, quality, and visibility of a scientific researcher? How does one quantify the cumulative impact and relevance of an individual's scientific research output? In our current academic system, many crucial decisions around faculty recruitment, research time, Ph.D. positions, travel money, award of grants, and promotions depend on our answers to these questions. Natural approaches are based on the publication records and the citation records. Relevant parameters are for instance the number of published papers, the number of citations for each paper, the journals where the papers are published, the impact factors of these journals, etc".*

I believe that this paragraph does indeed show how important the study of bibliometrics has become, whether we like it or not. Hence it is worthwhile to look at some attempts to capture the quality of a person as scientist. To be more specific, I concentrate on areas where persons are measured by publications (rather than by products like in art or architecture; or finds, like in archaeology), and draw most of my conclusions form experiences in computer science.

For many years the "publish or perish" tendency (at least in North American universities) implied that as young professor you either have to turn out 2–3 papers minimum a year or you will have no chance to get tenure or to be promoted. Over the years this measure of counting the number of papers has had two effects: first, papers are often published when only an epsilon is new in them (rather than the typical publication on which a scientist who would work for years and then present a masterpiece); second, administrators started to realize that counting publications does not provide a good measurement for two reasons: it encourages the incremental type of publication just mentioned, and it does not differentiate whether the publication appeared in a well established journal or in the proceedings of some second rate conference.

Looking for a better measurement rather than counting publications the tendency was to count citations. It seems rather convincing that a paper quoted often is more important than one with fewer quotations. Indeed, citation counts are still used today as one measure of judging the level of scientific performance. A short history of citation indices can be found in an essay in [12], but the summary in the English Wikipedia is probably just as useful:

"The Science Citation Index (SCI) is a citation index originally produced by the Institute for Scientific Information (ISI) and created by Eugene Garfield in 1960, which is now owned by Thomson Reuters. The larger version (Science Citation Index Expanded) covers more than 6,500 notable and significant journals, across 150 disciplines, from 1900 to the present. These are alternately described as the world's leading journals of science and technology, because of a rigorous selection process. The index is made available online through the web of Science database, a part of the web of Knowledge collection of databases. (There are also CD and printed editions, covering a smaller number of journals). This database allows a researcher to identify which later articles have cited any particular earlier article, or cited the articles of any particular author, or determine which articles have been cited most frequently. Thomson Reuters also markets several subsets of this database, termed "Specialty Citation Indexes", such as the Neuroscience Citation Index and the Chemistry Citation Index".

There are quite a few other citation sources such as SCOPUS, or the Social Science Citation Index (SSCI), etc. that are not explicitly mentioned in above list.

It is important to note that initially citation indices were not developed to measure the quality of researchers [11], but to help find out if a paper was quoted later-on by others, indicating that new developments had occurred. I mention in passing that to simplify this finding out about new developments led to the idea of "Links into the Future" as introduced in the Journal of Universals Computer Science, www.jucs.org, see e.g. [10].

Returning to our main topic, the number of citations an author receives for publications has become one major factor in determining the quality of a researcher. One typical site used for citation counts (particular in computer science) is [3]. Entering e.g. G. Rozenberg finds 421 documents, and each document shows how often it was cited. By going to "Most cited" and choosing "all" inputting G. Rozenberg shows him at position 1621 (Oct. 10, 2011) i.e. ranked very much better than the author of this paper H. Maurer at position 6429.

Well, I always knew that Rozenberg was 5 times or more ☺ better than myself, and this is what this particular and indeed valuable measure shows. A possible severe drawback of SCI is that only a limited number of journals (and no other publications) are considered to have "enough high quality to be used in citation counts", nor does it show contributions dating a very long way back.

A more liberal system is [5]: inputting here G. Rozenberg yields a huge number of hits, the first one listed (his "Handbook of graph grammars and computing by graph transformation") showing that over 1200 other papers have cited it at the time when I checked it, October 2011).

The introduction of measuring performance has also had a strange effect on how authors are mentioned on papers. Suppose three authors A, B and C are writing three papers together. As long as the number of publication counted it made sense to list all three A, B, and C as author of each paper. Once citations started to count it was better if one paper has only author A (but he quotes as many relevant papers of B and C as possible), one paper has only author B (but he quotes as many

relevant papers of A and C as possible), and analogous for C: the result will be that the citation count of all three will increase. If all three are mentioned of authors of the three papers their citation count would not increase, since self citations are not counted (actually, they are partially counted in Google Scholar). Even more ridiculous situations may arise: if you write a paper x, maybe you should send a note to all your good friends to quote x in forthcoming publications, so that your citation count goes up.

Above examples just show that once the type of measurement is known, it can be easily manipulated. I trust that most serious researchers do not use such tricks, but some might!

As revealing citation counts are (with all the caveats mentioned) they have a number of other drawbacks. First, once more the question arises whether a source is "valuable" enough that it counts for citing? Second, someone who has written one or more often cited excellent survey papers or textbooks but no real new research contribution may end up with a very high citation count, but may not be a top researcher. To take this into account numerous measures have been proposed that give a ranking based not on citations, but on the number of papers with reasonable many citations.

One of the first such measures introduced was the Hirsch-index [7]. A person has Hirsch-index n if the person has n publications with at least n citations, and n is maximal. The probably easiest way to find one's Hirsch-index is to use Harzing's "Publish or Perish" algorithm. Harzing's main research is in evaluating the quality of research [6]. To install her free algorithm (based on Google scholar) to find the Hirsch-index of a researcher go to http://www.harzing.com/pop.htm. I very strongly recommend that you first read the two write-ups "What Publish or Perish is for" and "Metrics" under the URL mentioned before installing and using the algorithm.

If you look up H Maurer you find a Hirsch-index of 44 for me. This is much too high: there are also other computer scientists with surname Maurer and first letter of first name H. If you check for Hermann Maurer you find a Hirsch-index of 26, and this is fairly accurate. (And since I am writing this for you, Bolgani, I have seen that even Hirsch-index wise you are much better than I am!)

Independent of the fact that it is not clear how complete and "clean" the Google Scholar data-base is, there are many other points that have been said against the Hirsch-index:

Suppose two authors A and B have written 6 papers each with citations A: 2, 2, 4, 5, 6, 7 and B: 3, 3, 4, 11, 23, 57. Both have Hirsch-index of 4, yet in most cases we would consider B superior, right? To address this problem efforts have been made to take higher citations into account to some extent, yet to avoid the "survey paper syndrome". This observation prompted e.g. [4] to propose an alternative measure, the g-index, which gives higher weight to more highly cited papers. When a publication record has a g-index of x it means that x is the highest rank such that the top x papers have at least x squared citations. Egghe argues that his measure better reflects the "visibility" of scientists.

Vanclay [13] argues the other way: suppose someone has Hirsch-index of 15, but has another 40 papers with 10 to 14 or more citations, should this not be taken into account? Others claim that more recent papers should be counted higher; that the number of co-authors should have some influence. A still more critical factor may be the size of the community: a paper in a very popular area is much more likely to be cited than a paper in an area only considered by a handful may be because the area is too hard to understand for most? A classical case for me is the great effort by Semirings [9] in their book that has revolutionized formal language and automata theory yet was not understood for more than ten years after publication by more than a few specialists!

Overall, bibliometric measurements and their justifications have created a whole new field of measurements. However, I tend to agree with Woeginger [14] who defends the Hirsch-index. In the paper mentioned he also gives a (mathematically superficial) axiomatic definition of it.

Here are some arguments that further strengthen the case for the Hirsch-index:

Hirsch (2005) demonstrates that the Hirsch-index has high predictive value for theoretical physicists: *"For instance, Nobel prize winners in this area usually have a Hirsch-index between 35 and 39, and over the last 20 years every Nobel prize winner had a Hirsch-index between 22 and 79."*

He also notes one important point: *". . . different research areas have different publishing cultures, the Hirsch-index cannot be used to compare researchers from different fields. For instance, the Hirsch-index of a moderately productive scientist in physics typically equals his number of years of service, whereas the Hirsch-index of a biomedical scientist tends to be substantially higher. However, over the last few years, the Hirsch-index has become widely used and recognized."*

Bornmann [2] shows that Hirsch-index is a good measurement using thousands of candidates at ETH.

I have said very clearly that one cannot measure complex matters with one number. I do not think one can even measure a small subset of persons (how important their publications are considered by their community) by a single number. Yet, taken all caveats made above it is not unreasonable to say that with some caution the Hirsch-index is an important indicator of the scientific quality of researchers, although even it must never be used in isolation.

3. Information, journals and citation counts

We all agree (indeed are already allergic to the statement) that we are being flooded with information. Yet, the fact remains. It is thus valid to ask how to deal with it.

As starting point today both relevant journals and databases will be helpful. Good search engines including Google, Google Scholar, Bing, Wolfram-Alpha and others may be good tools to approach the area in general. Citation indices or "links into

the future" like in [10], and more in detail in [1], that point to further work and show how often results in papers have already been used, can help further.

In this section I want to address two inter-related issues that I believe deserve attention: (a) can we replace citation count by other measures and (b) are there alternatives to classical journals?

The reason why I think it would be nice to replace citation counts by other techniques is simple: time delay. Suppose a good paper is written in year x; if lucky, it will appear in a journal in year $x + 1$; by the time it has been digested, valuable papers have been written based on it, it will be year $x + 3$. Until they appear in print and are incorporated into citation databases it will be $x + 4$. I hope that all agree with me that this is an optimistic scenario. Thus, any measure depending on citation counts will be 5 years or more late in being applicable.

It happens that there is an alternative: to publish papers fast in reviewed electronic journals and (for acceptability) also to offer them in printed form using a pagination identical to the one used in the electronic version. Note that this is possible as shown by J.UCS www.jucs.org (a free access computer journal where both publication and reading is free), yet the printed versions (purchased by good libraries as archive material) are printed, however not on an issue per issue basis, but once a year, all issues collected into one volume. The printed volumes are of course not free.

Now comes the punch line: if the journal is set up like J.UCS in the sense that before downloading a paper the reader has to select it based on the title, and then based on whether the abstract sounds interesting or not, then it can be shown that the number of downloads of the full papers has a very strong correlation with a later citation count! Putting it differently, some six months after your paper has appeared in J.UCS in digital form one can give a fairly good estimate of what the citation count is going to be in some 6 years or 10 years time! This idea has been pursued in a recent Ph.D. thesis [8].

Indeed it is hoped that the idea can be pushed a bit further. It does not seem to be impossible to estimate the number of downloads and hence a later citation count to some degree BEFORE a paper is published, by looking at other parameters. Without wanting to dive further into the thesis most will agree that a paper coming from a recognized leader, a top university and in a hot area is more likely to be downloaded than a paper by an unknown scholar, a little know research organization on a topic whose hay-days are over, or where the title is similar to the title of one hundred papers that have appeared before. Lest I am misunderstood: I do not expect to be able to predict the number of downloads, but I may be able to predict the likelihood of many downloads and hence would move such papers into a priority queue.

I believe in open access electronic (digital) journals. I believe that printed versions thereof will still be valuable for prestige reasons for some time to come. The cost for the printed issue has to be recouped. The cost of the digital journal (running the server and backups, the coordination of the reviewing process, the potential re-formatting or proof-editing, . . .) also requires some nontrivial funds. In case of J.UCS, they come from a consortium of research institutes. In other cases they often come from the authors: i.e. to get an accepted paper published the author has to pay a few hundred Euros. I do not believe in this model: the temptation for the publisher to accept a paper whose quality is in the grey-zone is too big. I hope other models will emerge: one I have not seen discussed is this: every university interested pays 100 Euros a year. A member of such university can publish and read contributions without further charge. Assuming that one thousand universities can be convinced to pay such a minor contribution, the revenue of 100.000 per year will be sufficient to maintain the operation of such a journal.

Maybe it is time that big publishers do not charge universities for subscribing electronically for a bundle of journals, or charge submission fees, but rather offer a bundle of journals where everyone from the institution can publish and read at no cost? I hope the difference is clear: the editors of such a journal will have no pangs of guilt refusing a paper whose merits are doubtful, since it does not make a financial difference to the publisher.

4. Conclusion

Complex situations cannot be described by one parameter, one number. A university ranked 268 by whatever spurious measures will not necessarily be better than a university ranked 302. The same applies to the ranking of scientists (independent of which kind of ranking is used.). Still, there are some measurements that if taken together and with some caution do give a certain qualitative measure as long as individuals and institutions are compared that are area-wise not too far apart. Any other kind of applying a single parameter measurement shows an unfounded belief that all things in the world can be sorted linearly.

Special dedication

This short paper is dedicated to Grzegorz Rozenberg on the occasion of his 70th birthday. I had the privilege and luck to meet Rozenberg more than thirty years ago: I have profited from his unlimited energy, imagination, wittiness, reliability and uncanny superb way of presenting difficult material to any audience. I am proud to be co-author of some joint papers. However, the one thing that is more important than all others is that once Rozenberg has accepted you as friend, you will be his friend for a lifetime. This is a rare phenomenon not so usual in science: one tends to become quite close friends as one works together, but as one drifts apart in location and research area, ties often tend to weaken. Not so with Grzegorz. In a message not so long ago he told me he feels towards friends like a mother towards her children: if he does not hear from them for any length of time he gets worried. Bolgani (allow me call you this intimate way that only a few of us are allowed to use): You are a rare specimen and I am grateful for your friendship. Ad multos annos!

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