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Hirsch index: a case of anti-scientific method. A new scientometric indicator : $1/\tau$

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Abstract

The scientometric index h (Hirsch index) is critically analysed. It is emphasized its anti-scientific character, generated mainly by the fact that the context is not considered. Its capacity of being falsified is also stressed. It is suggested another scientometric indicator, the $1/\tau$ index which seems more robust. The $1/\tau$ index is derived from three empirical models. In general, it is highlighted the fact that such indicators can be extremely inadequate for evaluating the scientific research activity when naively used.

In modern times the number of scientific researchers increased beyond any bounds. In 1900 they were a few hundred all over the world, almost all of them professors. Today they are 3 million. From 1900 till today the world population is 3 times larger, and the number of scientific researchers multiplied by a factor of 10^4. This huge increase could be correlated, to some extent, with the impetus of science and technology. In the last century, the scientific research led, it's true that indirectly, to ever sophisticated constructions and means of transportations, to the power of steam, electricity, electronic communication, nuclear energy, new materials, laser, pharmaceuticals, and many other things without which life would be today virtually impossible or, in any case, unconceivable. Modern society is chiefly technological, is conditioned by technology. The advent of these technological developments have helped us to enhance our knowledge of the natural world and to develop the scientific method. In 1660, the king of England founded Royal Society "for improving the Natural Knowledge", "for the Promoting of Physico-Mathematicall Experimentall Learning"; and the Royal Society's motto was: "Nullius in Verba". After such a technological boom, it is natural to expect a stagnation, a regress, even a decline.

Today, science is seeking its "soul". The quarks have blocked out our empirical scientific method and all the methodology of the theoretical physics. People are asking themselves whether life could be controlled and created in vitro; whether the Earth's limited resources or the instability of our solar system do not render, after all, life purposeless; whether we should keep hoping, or seeking radical changes on human scale, that come from fear, greed, cynicism, cowardice; whether we should not start off the fight for survival in favour of an elite. The generalized sense of deception and discouragement lays bare the human weaknesses. Man has God in his soul but lives his life according to his human nature. Hence, his erratic, contradictory, ambiguous behavior. Nothing can guarantee the stability of this self-contradictory species. As long as blissful ignorance, hope and faith prevailed, science forged ahead. Now, once we reached the "tree of knowledge", a sense of despair and purposelessness pervades us; if this leads us to sufficiently foolish, but not fatal, actions we can still hope in a new beginning.

Meantime, all the scientific researchers on the planet are concerned with trivial matters, with irrelevant stuff and particularly with deceiving the good faith. In an imperfect, unpredictable, uncontrollable world with limited resources, the fight for survival justifies all means. The modern society questions the social utility of the scientific researchers, the efficiency and the quality of their work, even their honesty. And the researchers question the legitimacy of the society. Highly imaginative as they have always been, the researchers have answered these societal pressures and suspicions by contriving the number of scientific publications, the scientific output, the number of citations (the "impact factor") and more recently, various indicators for evaluating their activity, among which the Hirsch index or h-index is the most known. The h-index is the maximum number of publications, which have, each of them, a number of citations greater than or equal to h. The Internet is rife with information concerning the origin of the h index and various debates on the relevance of such scientometric indicators. The h index is automatically calculated by ISI-Web of Knowledge. The inventor of this index is a physicist named Jorge E. Hirsch, an interesting character who opposed all his life a famous theory in physics known as the "Bardeen-Cooper-Schrieffer theory of superconductivity". He had in turn his own "theory". But no one paid attention to it for a long time. Meanwhile, he succeeded in increasing considerably the number of his publications and citations in other subjects. Consequently, in a fit of anger and frustration, he contrived the h index which will have a ringing success precisely because it will prove itself an efficient instrument to "scientifically" falsify research. Hirsch himself has a high h index ($h = 60$) which can be explained, at least by the facileness and degradation of his scientific field, subjects, themes.

Lately, the h index is increasingly adopted as an instrument for evaluating, categorizing, selecting the scientific researchers despite its many highlighted shortcomings concerning its relevance. The reproaches refer to the fact that the number of citations varies considerably according to the scientific field, journals, subjects; according to the number of scientific researchers existing in the field, in the topic, etc.; varies with the nature of publication (full research paper, letter, review, etc.); varies with time (the researcher's age); varies depending on the database that you use; it is contaminated with self-citations, serial publications. Furthermore, the citations are all too often negative, perfunctory, interested, etc, etc. But, perhaps the biggest flaw of this indicator resides in the fact that it includes co-authors, reciprocal citations and can be easily falsified by fraudulent networking researchers. The h index seems to have been invented precisely because it is capable to falsify effortlessly the significance of the scientific research.

Should I want to be presumptuous, I would make an academic digression in Bayesian statistics. I confine myself to remind you that, about 1750, father Bayes pointed out that probabilities are always conditional, so that a naïve, contextless statistics can lead us to aberrant, irrelevant conclusions. There are various domains, themes, subjects in nowadays scientific research. But many of them are facile, as, for example, numerical calculations or numerical data analysis where the results are uncontrollable. Others are degraded, *e.g.* elementary particle physics, astrophysics, but many researchers - who are undoubtedly and inevitably professionally very poor - rush into them for reasons of snobbery or intellectual presumptuousness. Other fields, such as nanosciences, constitute an inexplicable attraction for the popular masses of scientific researchers; most probably, such an attraction can also be explained by the facileness of the domain, by the impossibility to control the results or simply as an irrational fashion.

Such contextual elements must be taken into consideration when trying to compare and contrast scientific researchers according to the h index. Thus, the h index can be, at most, a bibliometric indicator, not a scientometric one.

For instance. Speaking of high temperature superconductivity, Anderson asserted with innocence and candour: "The consensus is that there is absolutely no consensus on the theory of high- Tc

supraconductivity"; Schrieffer, more reserved, was trying to be lucid: "More generally, the first impressions one gets of the theoretical developments on high- Tc (supraconductivity) over the past four years is that theorists do not know what is really going on" (both quotes are from Physics Today 54, June 1991). Now, that ignorance is huge, it is only natural. But why must it be displayed with such presumptuousness in scientific publications? On the other hand, why should we seek a consensus in science? Since when is science performed by voting? When Einstein learned about the pamphlet "Hundert Autoren gegen Einstein" he would have said: "Why 100 authors? If I were wrong, then one could have been enough". There is a huge number of scientific papers on high temperature superconductivity to which Anderson, a Nobel prize winner, personally fully contributed. Scrieffer is also a Nobel recipient but he has $h = 5$ in contrast to Andersons's $h = 70$. There are so-called research managers, or research leaders, who publish enormously. Too few of their publications have some relevance. By using their influential position they garner a vast number of citations and manufacture for themselves a huge h index. Their fans follow suit as well. All these betray vanity, exhibitionism - of intellectual nature, of course - the display of an intelligence, undoubtedly superior, which runs idle. To realize the motivation, the significance of such a phenomenon we must be more clear-sighted than these "prolifics". Are our research managers, many of them fanatical supporters of the h index, really so smart as to see how misleading this index can be in such cases?

The editors of the journal Nature comment recently on two manuscripts, published last year, belonging to the same field of research. One of them immediately got about 200 hundred citations, while the other one contented itself with approximately 13. The surprise came when the later was considered by a council of international experts as the most important result in the field in the last decade. This is a typical situation. Good papers pass unnoticed, because the mass of scientific "publicists" cannot use them to develop their career, so these publications are ignored. In exchange, bad scientific papers are acclaimed, as they generate numerous other papers of the same kind. Since it is unconditionally based on citations, the h index does not reflect but this inflation of imposture. Scientific journals with many citations (and a high impact factor) do nothing else except coarsening science, accommodating it to the taste of popular masses. Typically, the much cited authors are not capable to coherently utter a few words about their results, to present these results by words of mouth; they communicate through gestures like deaf-mutes; they are not capable to hold a seminar, they hide as if ashamed with their results. They might know why. Let me tell you a legend, not a true story. The legend tells that I would have once a foreign visitor inquiring about the scientific paper of a friend of mine who was the proud possessor of a big h index, actually a huge one. The friend was reluctant to discuss his paper. The legend goes that at my insistences I would have arranged for a seminar to be held. It was impossible for the author to give the audience a clear-cut explanation on the meaning of its paper. He asked the help of a younger collaborator who was even more stumbled over his words than his master. Nothing came out of this duet, everything was incomprehensible. In the face of the disaster, my colleague threw the ace: "without further ado, I'll better show you the manuscript". So, he pulled out before us a few nicely, neatly written pages which he kept praising: "Look what beautiful graphics, what beautiful text, how smoothly it flows in the page!". To cap it all he also pointed out: "Pay attention to the final where we've added this sentence to save the situation!". The legend goes that I would have burst out laughing more than fully, while my visitor remained dumbfounded. I emphasize that this is a legend, not a true story.

Today, the scientific researchers organize themselves in groups; these groups form networks; the makeup of the groups fluctuates. The groups belonging to the same network give one another positive reports for their papers to be published, they cite one another's work; the migratory, itinerant researchers cite their previous papers manufactured with ex-groups so these papers are

no longer considered as self-citations for the initial groups. Much worse, the publications produced by such organizations, by these "friendly lucrative associations", these "co-ops of production" have no scientific control whatsoever, so that the scientific literature is littered with a pile of trashy papers manufactured by impostors.

The typical structure of a present scientific collaboration consists of: an initiator - a kind of boss, of guru – who ostensibly has an idea, an initiative; an organizer, who gleans from the Internet an aberrant mathematics and some sort of crooked equations; another who puts a few students to carry out the experience, to gather data; a computer man who produces some calculations, graphics, diagrams. The calculations are meaningless but the computer man does not assume any responsibility as he does not know neither the equations nor the results of the experience; the experience is actually failed, but the students do not assume any responsibility as they do not have access neither to "theory" nor to data processing; the data as well as the theory are fabricated by the organizers to fit the master's idea - sometimes they do all these without being aware; they do it like Phytia of Delphi, in a state of semi-unconsciousness ; the master can control none of these elements because he has no idea about them and this is not his "business" either. The fabricator - picks from the graphics spread out before him by the computer man and from the data presented by the experimental researchers, those that are most pleasant to his eyes. If not pleased, he expresses his wish and the students, the computer man, the subordinates immediately fulfil his wish, "fix" his dissatisfaction in no time. If any of the students dares to voice doubts he is immediately put to the corner: science is difficult, the student is, of course, ignorant, therefore he must trust his master and must not lost sight of his graduation or doctoral thesis the master is to give him in the end. This is the current fabrication process of a scientific research product. A favourable report from a friend is sufficient for all this literature to be published. Big international co-operations ("co-ops") facilitate such frauds, there is big money for survival at stake, the fabrication of these literary products takes little time, the h index is accordingly, substantially increased. Once the h index is considered as a yardstick, it promotes the fraud and the impostors to the detriment of the few, honest researchers who could still exist for some time. The h index is a serious threat for the scientific research. I have never seen in the activity reports of the scientific researchers, of the research institutions, any failed, unsolved subject, theme or experience. There is no fiasco, there are only successes – and these ones more and more ringing. This is a fully anti-scientific standpoint.

For this reason, I try to suggest here a new scientometric indicator which I call the $1/\tau$ index. Not that this index would be more relevant than others but to demonstrate that there are other methods of evaluation whose results are very different from the h index. As the $1/\tau$ index results from some simple and natural hypotheses about the research activity, this index seems, however, more reliable than others and less falsifiable. The $1/\tau$ index, as all other attempts to scientometrically evaluate the scientific performance, does not reflect neither the scientific quality of the research nor its importance for science. These can be assessed only by knowledgeable people, the experts, leaving aside the fact that they are historically constituted (in 1780 the chief justice of England decided: "in matters of science, the reasoning of men of science can only be answered by men of science"). As concerns social utility, though the scientific research can contribute, in a rather indirect way, to the enhancement of culture, civilization, training - especially through learning and education, its possible technological contributions – though amply documented by history - are virtually impossible to estimate, notably on short-term. The $1/\tau$ index can reflect just the researcher's activity level, the volume of his work, his seriousness, honesty, creativity, the constancy of his activity. Setting it against the h index can show us how easily falsifiable can be the later. Therefore, the $1/\tau$ index can provide a suitable instrument for detecting the impostors in scientific research.

Let's assume that at a moment t a researcher (institution, organization etc.) has N number of publications. It is quite plausible that in the next time interval dt this number shall increase with dN, proportionally to the time t. Let's divide the time t into small boxes of the same duration τ ($\tau \ll t$). The next box τ is identical with all previous boxes and in it each of the previous boxes finds itself again, so that the probability for a paper to appear in the next ensuing box is t/τ times higher than in the case that this box would be independent, not connected with all the past boxes (would not have a history). Let's keep in mind the two abovementioned hypotheses, the same duration τ for all the boxes and their history. From all these it results $dN/dt = t/\tau^2$. Accordingly, the number of publications in the time t is: $N = (t/\tau)^2/2$. In the time interval T we shall have a total number of publications $N = (T/\tau)^2/2$. The $1/\tau$ index is given by the equation

$$\frac{1}{\tau} = \frac{\sqrt{2N}}{T} \ . \tag{1}$$

This law is empirically verified for most cases, in the sense that $1/\tau$ calculated according to this formula gives us a classification consistent, within good reasonable limits, with our opinion. This law has the status of an empirical model.

Let's notice that this index $1/\tau$ includes time T (*e.g.* the age) and gives a limited weight to the number of publications N (which naturally increases in time). Therefore, this index is advantageous for younger researchers and, to a certain extent, limits the natural (and sometimes the routine) prolific activity of senior researchers. If we consider only the number of papers per author, the index $1/\tau$ - which has anyway a limited effect (because of the square root) - cannot be falsified but through an anomalous increase of this number of papers. Such an increase can occur in facile, degraded scientific domains, subjects, themes or, simply, N can be increased through bad papers, many of which rife with experimental measures, for example, or with uncontrollable, even outright wrong numerical calculations. In such situations the opinion of a reliable expert is absolutely necessary. I would notice, however, that an impostor is not satisfied cheating only one time in one place, but, by virtue of this weakness called thievishness, he cheats anywhere and anytime. Most probably, he places himself so that his number of citations to be disproportionately high against his number of published papers; usually, he does this by picking a facile or degraded field of research or by diligently activating in a "comradeship" network. What's the good of numerous citations in such a field, as long as they come from not knowledgeable authors? That is why, a comparison based on scientometric indicators must be made on "homogeneous statistical assemblies" as did, as a matter of fact, the old analysts of experimental data, of the good old scientific method, who knew this too well and long time ago. Let's notice however that by eliminating all these anomalous increases of the number of papers N, we see that as concerns the number of citations, the $1/\tau$ index seems less falsifiable than the h index, at least for the fact that citations are negotiable and reciprocal, whereas the publications (per author) seems more difficult to negotiate.

A quadratic law of the type $h = \sqrt{C}/2$, where C is the number of citations, seems well documented by the database analysis for the h index; on one hand, this might not be accidental because an analysis similar to the one which leads to the equation (1) is also applicable to the number of citations and on the other hand, it allows a comparison between the h index and T/τ ratio. Of course, if it is about one author, then the number of publications N in equation (1) must be considered per author; the same would be desirable for citations (regrettably, databases do not do this). As the value of the $1/\tau$ index given by equation (1) is typically less than unity (with t measured in years), we can, more conveniently, multiply it by, let's say, 100. Let's also notice the arbitrariness of the h index: instead of a citation number greater than or equal to h, we can

consider, as we please, a number of 2, 3 etc. times higher or lower than the number of publications or we can give free rein to other various and sundry fancy choices.

If we apply the same equation (1) to citations we have $C = N$, viz. the number of citations is equal to the number of publications. This thing seems quite reasonable, as a publication answers, in principle, to a problem and the record of this answer means a citation. But since N is the total number of publications it results an appreciable number for citations. Rigorously speaking, once the problem solved there is no reason for this publication to be cited unless other inappropriate reasons are considered. No matter how paradoxical it may seem, multiple citations reflect, inter alia, the fact that the publication in question is wrong, unfinished, imperfect or that the field, the subject are sufficiently facile and degraded (the current term is "hot") to attract many researchers or, generally, they reflect a falsifiable context. After the invention of Feynman diagrams, a huge number of publications emerged, making extensive use of these diagrams with completely irrelevant results. Feynman "brought the calculus to masses". Obviously it was a facile subject and Feynman contributed, perhaps unintentionally, to the degradation of the field (by the way Feynman has $h = 9$). After the discovery of Bardeen-Cooper-Schrieffer theory, a huge number of researchers made their guess on this matter. The snobbery was conspicuous. Superconductivity made no step forward with this theory which rather answered a specific need of theoretical physicists concerning completeness and consistency. The index $h = \sqrt{C}/2$ compared to $T/\tau = 2N$ implies $C = 8N$, which is sufficient to see the inappropriateness of this index. Moreover, if the ratio C/N is much higher than 8, we have a peculiar case of "contextualization", of falsification.

In the following we shall analyze a few typical cases gleaned from ISI-Web of Knowledge. The examples are randomly selected and have no connection whatsoever with known or widely-known persons or personalities around us.

The author A seems to be "cleaned" of all imperfections of the h index. In 35 years of activity he has 120 publications (alone) and 200 citations (without the self-citations). The ratio C / N = 1.7. The $1/\tau$ index (multiplied by 100) is 44. The h index is 10.

The author B has h = 30. So, he seems to be 3 times more "important" than the author A. But B has 230 publications which would be almost halved (120) if we exclude one co-author. The remainder must be divided to an average of cca 4 co-authors so that N will amount to 30. The h index should also be divided to the number of co-authors, in which case nothing would remain of it. In 35 years of activity, the author B has $1/\tau = 22$, so, actually, he is 2 times weaker than the author A. Moreover, B has cca 1500 citations, therefore C / N = 75. Most probably, there is something suspect here! An expert eye could easily see that even those 30 publications are bad.

The author C has 250 publications and h = 30. He also seems very "important". But these 250 publications actually amount to cca 25 as he has an average of 10 co-authors. In 32 years of activity he has $1/\tau = 22$. Furthermore, C / N ~ 60, what shows, again, that something is rotten in the state of this Denmark! Most interesting, if we eliminate a co-author, the number of his publications will decrease from 250 to 80 and the h index shrinks accordingly to h = 16!

The author D has 150 publications, but, if we eliminate a co-author, this number falls to 30 publications and consequently, his h index will decrease from h = 15 to h = 3. The co-author in question has 160 publications which will be reduced to 50 if we eliminate the author D. I fail to understand what is happening in such case. These two authors seem capable to do only half of a publication each.

A Nobel prize in elementary particles has T = 40 years, N = 300 publications, C = 20000 citations and h = 60. His $1/\tau$ index is $1/\tau = 60$. The ratio C / N $\simeq$ 70 may reflect the degradation of the field. Most probably for this field, h = 60 is not a very high score. It is very interesting that,

for this author the hypothesis of "historicity" of scientific publications does not seem to apply. He is an atypical author. For him, $N = T/\tau$, so that $1/\tau = 750$ (multiplied by 100) seems more appropriate. Of course, such authors must be compared one to another.

Another Nobel prize, cited above, has h = 70 and 150 publications in 40 years of activity. His $1/\tau$ index is no big deal: $1/\tau = 43$. But he has the ratio C / N = 200, which may show how influent he could be in a scientific field where "there is no consensus". For me it is obvious that these scientists are writing and publishing in a state of trance. They are the sick of our science.

The index $1/\tau$ defined by the equation (1) can be also cumulatively calculated in the case of an on-off activity. Another atypical case is the one in which $dN/dt = (N_0/\tau)\cos^2 t/\tau$ (or an overlapping of such harmonics), where N_0 is a fit parameter. This case corresponds to a variable τ in the equation (1). Accordingly, we have $N \simeq N_0 T/2\tau$ and $1/\tau \simeq 2N/N_0 T$ with results that differ from the one presented above. A famous "publicist" in this category has 350 publications in 35 years, with $N_0 = 15$. He has $1/\tau = 1.3$. The foregoing author A also falls into the same category with $1/\tau = 1.5$. But the famous one has 25000 citations and h = 80, while the modest author A has 200 citations and h = 10. The former is cited because it is famous in a very hot field, the later is decently assessed by some elite minority.

I do hope that all these shall be able to persuade us how misleading a scientometric index as h can be; how much we can mislead ourselves in evaluating the scientific research activity if we do not use "homogeneous statistical assemblies"; how different the results given by the h index in comparison to the results provided by empirical models such as those leading to the $1/\tau$ index can be and to what extent we can favour the impostors in the scientific research if we do not resort to experts. I conclude by quoting from anonymous sources according to which Galois has h = 2 and Einstein has h = 5. How's that? How can we compare ourselves among ourselves and whom are we comparing with? Do we want a cheap popularity in tabloids and "at the doorway of cafés", or an honest work with possible relevant results in science?

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