Katowice, May 2015

Dean of the Faculty

of Mathematics, Physics and Chemistry

Prof. dr hab. Alicja Ratuszna

Dear Ms. Dean,

I am writing to warn strongly of the over- and abuse of bibliometry in the determination of the funding for the Mathematical Institute. I am writing this letter to lend a clear voice to the concerns I have heard from several members of the Mathematical Institute. I share these concerns, as a future member of the Institute.

I will be striving to build up a new internationally acclaimed working group in the area of Valuation Theory, building on the already existing strength in algebra. This working group is intended to replace the now lost working group at the University of Saskatchewan that was viewed as a world center for Valuation Theory. New scientific initiatives do not only need brains, they also need financial support. While universities in North America offer start up grants to new faculty to develop their initiatives to a point where they are competitive enough to be successful in grant applications, resources at the Mathematical Institute are too low to provide effective ``seed money'' to its young researchers, new faculty, and faculty who are developing new initiatives. This is a result of the distribution algorithm that has been applied.

The ever rising use of bibliometry by administrators is in stark contrast to the following facts that have become well known:

a) Bibliometry was invented to give librarians a tool to determine which journals should be bought; it was not meant to measure the quality of the work of scientists.

b) Impact factors of journals do not represent the quality of the papers published in them, they often depend on other influences. For instance, open access journals (often with mandatory article processing charges) automatically have higher impact factors. Impact factors are vulnerable to fraud, such as groups of researchers systematically citing each other. They can also depend on unscientific influences such as journal mergers.

c) More and more scientists of high reputation are standing up fighting bibliometry. These are by far not only mathematicians. For example, the San Francisco Declaration on Research Assessment (see http://am.ascb.org/dora) was initiated by the American Society for Cell Biology.

d) The points for journals, which are used by the Polish Ministry of Science and Higher Education, among many others, are assembled by a private company which earns a lot of money by this activity, although there would be alternatives. Scientific integrity is not guaranteed.

e) Publishing mathematics is entirely different from publishing in other (in particular, experimental) sciences. It is not rare that mathematical articles unfold their influence only after many years or even decades. But their "impact" is practically only measured after the first two years. However, the "half life" of mathematical papers is in average much longer than that of papers in the experimental sciences. Mathematicians often work with papers that are very old (I recently read parts of the stunning work of Hausdorff, more than 100 years old, and in a way continued it.)

Bibliometry does not take into consideration the amount of pages (in particular, per author) of a paper and the amount of effort and time it took to write it (and nor did you in your recent presentation). Here are the number of pages of some of my papers published in journals worth at least 35 points: 42, 41, 29, 26, 26, 25. Four of them were single-authored. May I respectfully suggest that you multiply the points with the number of pages and divide by the number of authors? The result would make a very interesting comparison between the disciplines of mathematics, physics and chemistry. I guess you would agree that this method is crude; but then, let us also agree that bibliometry itself is crude.

A good mathematical paper often takes years to develop. The fact that it may not be cited very often (in the first years) is not an indicator for low quality. It can mean that initially there may only be a few people who are experts in this area of research. Well, the same was true for the results in number theory centuries ago which are now the cornerstone for public key cryptography, without which our daily life would be very different. It is also true for group theory, which developed as curiosity driven research, but then quite unexpectedly went on to form the indispensable basis of crystallography.

You may think that this form of curiosity driven foundational research is not needed anymore today. But if politicians, administrators and CEOs of companies believe that they know which research has to be done to produce the great innovative developments of tomorrow, then this borders on arrogance and ignorance of the history of science and the nature of scientific research itself. If we know what we have to do to discover something, we have already almost discovered it. In the past, fundamental insights have

not been found by research pushed in certain directions by commercial interests, and they will not be in the future. Loss of foundational research is a loss for society and for future generations, brought about by short-sightedness of politicians and administrators. When the loss will become evident, they cannot be held responsible anymore.

By its very nature, mathematical research does not achieve the same impact factors as research in other sciences (although its impact on the other sciences and on society and culture is without doubt very high). One can see this phenomenon everywhere in the world, at MIT or Harvard or Warsaw University as much as at the University of Katowice. Comparing mathematics with other sciences by impact factors amounts to comparing apples with oranges. Rather, one has to compare one mathematical department with others, apples with apples. And the comparison of the Mathematical Institute in Katowice with others in Poland is quite favorable. This comparison, together with peer review, the method that for centuries has been the best tool for the determination of scientific quality, should inform the distribution of funds. Peer review is not perfect, as nothing in human life is, but bibliometry is much less perfect. For a collection of background information and discussion about bibliometry, see http://math.usask.ca/fvk/biblapc.htm.)

Unfortunately, we recently see a deplorable decay in the peer review procedures of some journals. This happens in particular in journals with article processing charges. A main cause and motor of this decay is the pressure on scientists to produce ever more papers in journals with high impact factor.

Allow me also the following remark. You may find it divisive, but it reflects the truth. The majority of experiments are not reproduced, and in particular not by referees. An astonishing percentage of experiments have even been impossible to reproduce. (See

http://www.theguardian.com/science/2015/feb/18/haruko-obokata-stap-cells-controversy-scientists-lie?CMP=fb_gu.)

In contrast, in a good refereeing process every mathematical theorem is checked by the referee. It is then usually checked again by the researchers who wish to learn from the ideas and tools it uses and who base their own research on them. One should consider this when one thinks of the use of research for society and culture, in particular at a time when scientists are ever more confronted with the loss of credibility due to bad science. More and more first rank scientists are protesting about bad science appearing in the top scientific journals like Nature. However, bad science and fraud appears to be rare in mathematics.

All of these facts show that making bibliometry the basis of the distribution of funds is already highly questionable. But an algorithm that cuts off journal articles published in journals with less than 35 points

constitutes an amazing and unscientific arbitrariness and is a slap in the face of every serious mathematician. You may submit your paper to a journal which has 35 points, and in the following year, when your paper is accepted, the journal has only 30 points. (Yes, the refereeing process can take two or more years in mathematics, even for top journals!) In addition, it has now happened that points allotted to journals have been *changed retroactively* by the ministry. All this means that articles can be shifted quite randomly below and above the 35 points threshold. Is that ambiguity of measure scientific?

Many of the well-respected and important journals in mathematics have less than 35 points. I will give some examples from my own area of research.

1) The best journal to reach a large audience of algebraists is the Journal of Algebra which has 25 points. Recently, two highly celebrated papers on resolution of singularities have been published in this journal. The results are so important that a conference and school has been organized last year at the University of Regensburg, Germany, in order to introduce them to a large group of mathematics and to discuss further research based on them. The Journal of Algebra publishes a broad range of results in algebra, and this automatically lowers its impact factor. But among its articles are several of highest quality, and it is an invaluable source of information for every algebraist. I have myself published several papers in it, also together with my Ph.D. student from the University of Katowice. Just now I got an article accepted for publication in the Journal of Algebra. The referee wrote: "This paper belongs in a strong algebra journal".

2) The journals in logic with a broad readership and a wealth of very important papers since many, many years are the Journal of Symbolic Logic and the Annals of Pure and Applied Logic. Both are rated 20 points. The situation is similar to the Journal of Algebra. Yet, I published one key paper in the Journal of Symbolic Logic which is well known to researchers in my area, and I will be very happy when also my recently submitted paper will be accepted for publication in this journal.

3) One of my most important and most cited papers, and the most programmatic one (76 pages long), was published in a prestigious collection of papers contributed to Zariski, one of the fathers of algebraic geometry. Marginal points, certainly below 35, and yet this paper was definitely one of the main contributions known to many peers that helped me earn the Polish Professor title.

We are all human beings, the development of science is to an important part based on relationships between us, on respect for our teachers and colleagues. And so it will happen that we wish to publish in special issues of journals or in proceedings or books with little or no points, because they are devoted to,

or published by, an individual we wish to honor with our contribution. From the point of dissemination, this does not really matter, as our papers are often freely available through websites, and in particular, on arXive.

Depriving us of such choices that have a longstanding history, just because we need to generate points, is a chilling consequence of the ever growing monetary principles used in the administration of science and higher education. (Well, research on artificial intelligence is advancing, so perhaps we can replace scientists by machines in the near future? But what will mankind live for, then?)

4) My wife and I have recently developed a very promising new direction in the area of fixed point theory. There are essentially three journals in this area: one with 25 points, one with 30 points, and one open access journal with 45 points, with mandatory article processing charges. By reading and refereeing articles for the latter, I know that the quality is not at all better than in the journals with 25 or 30 points. That means that authors simply *buy* the additional 15 points! My wife and I will not publish in this journal, which means that our papers, though developing a whole new theory, will stay below the random 35 points threshold.

The last example makes another big problem obvious. To receive a research grant in Poland, you need articles in highly ranked journals. In several areas, to get articles published in them, you need to pay article processing charges. But in order to be able to pay article processing charges, you need a research grant. (Or you need to be in an institute that gets a lot of funds. In any case, the situation is daunting for mathematicians, but that is not their own fault.)

The pitfalls of bibliometry, different publishing processes, circularity and, to say it clearly, merciless monetary principles also come in through the classification of scientists as "n_zeros", and the consequences. What if Peter Higgs would have been classified "n_zero" as he did not publish for years while he was working on the theory of what we now call the "Higgs-boson"? What if a researcher with a well established publication portfolio goes through a crisis (such as severe illness or divorce)? What if the paper one spent a lot of work on gets rejected by a journal after 18 months? This happens quite often in mathematics, and it happened to me; the same paper got then accepted in an equally highly ranked journal, but only after a new refereeing process of several months.

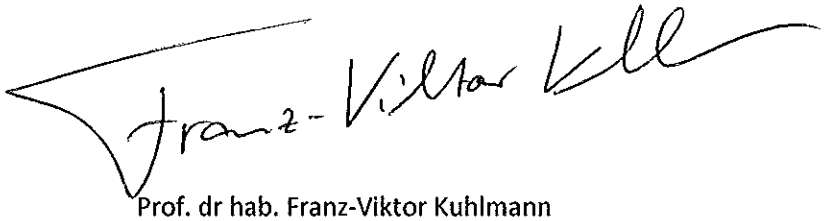
The "n_zero" classification is a crude and degrading attempt at what is often more subtly achieved by differential teaching loads. This is a method that assigns to inactive researchers temporarily a slightly

higher teaching load, but is flexible enough to revert to a lower load in the moment the researcher is active again.

Do we really have to instill fear in scientists and subject them to pressure in order to improve science? Using crude measures in the fight for funding pits departments against departments, colleagues against colleagues, who should rather work together closely and in mutual respect in order to improve science and education, and to oppose nonsensical procedures imposed on them by politicians and administrators. We are all human beings and produce our best results when the environment is peaceful, supportive and collegial. (See the Ant Story: https://www.youtube.com/watch?v=ejw9Coycfcw.)

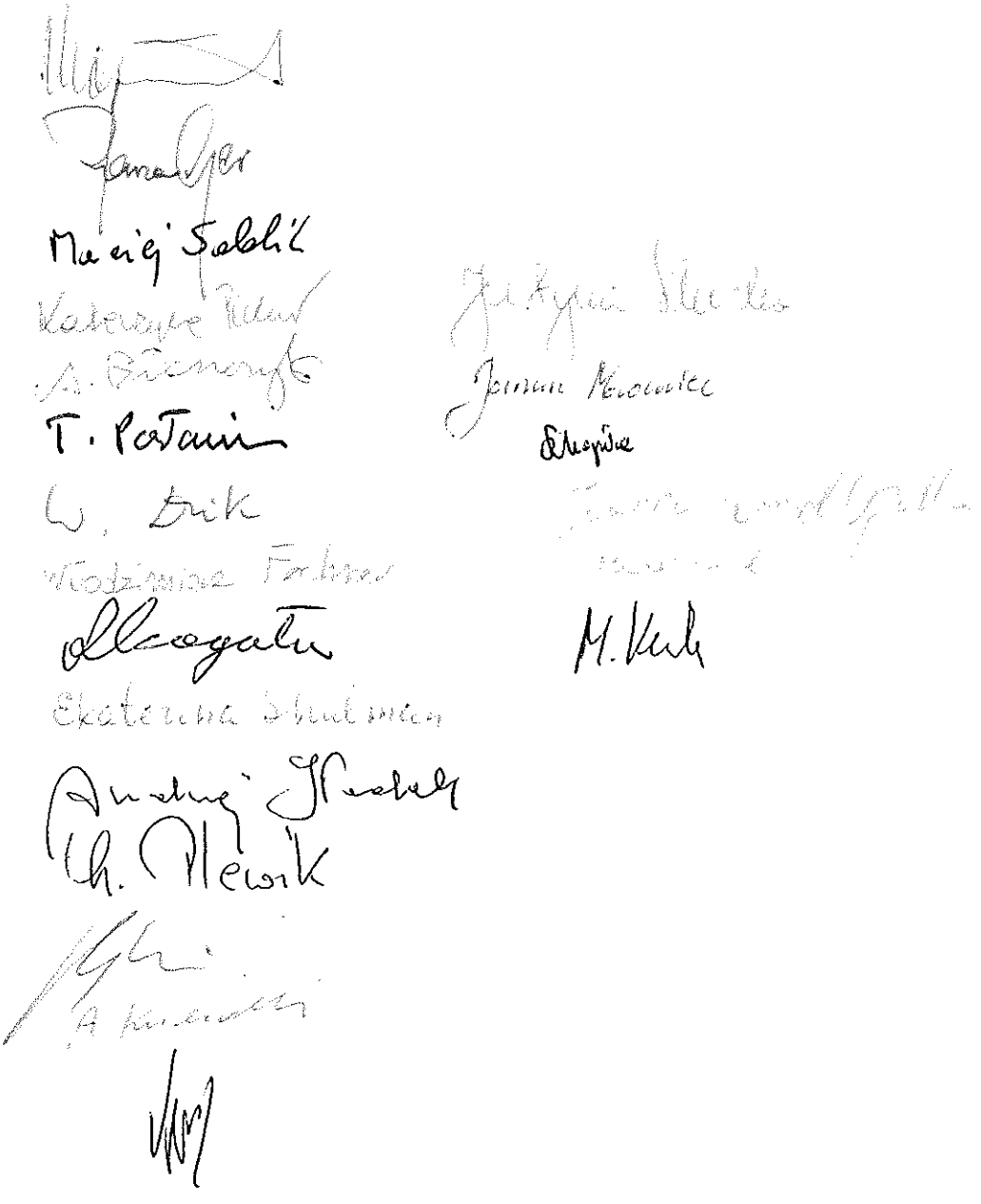
As a mathematician with a long research experience, I refuse to be forced to be a points- or money-making machine. I refuse to partition my results into small pieces in order to produce a larger number of papers. I refuse to lose valid time in the dissemination of my results by hunting for admission in the highest ranked journals. I reserve the right to choose for my papers the journals which are optimal in reaching the audience that is best for my results. I am presently writing a paper together with one author of the above mentioned articles in the Journal of Algebra. If we see it fit, we will submit this paper also to the Journal of Algebra, regardless of the amount of points the Polish Ministry of Science and Higher Education assigns to it.

Further, I refuse to pay article processing charges and will not publish in open access journals with a deficient refereeing process. I have turned down an offer to become the main editor of a new open access journal because it will introduce article processing charges after the first two years. I will continue to speak up against the bad influence of bibliometry and commercial pressures on the quality of scientific research and academic freedom, because I firmly believe that in the long run, these tendencies will cost society dearly.



Prof. dr hab. Franz-Viktor Kuhlmann

Co-signed by:



Cc:

1. Prof. dr hab. Andrzej Kowalczyk, Vice-Rector for Research and Collaboration with Industry
2. Prof. dr hab. Robert Hasterok, chair of the Committee for Research, Artistic Creation and Collaboration with Industry
3. Prof. dr hab. Andrzej Ślebarski, Director of the Institute of Physics
4. Dr hab. , prof. UŚ, Michał Daszykowski, Director of the Institute of Chemistry