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Publish or perish

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Contents

Publish or perish	3
Introduction	3
Publication is integral part of research work	3
Necessity for CV	5
Authors' list and order	5
Citation index	7
Conferences or journals?	10
Discussion	12

Publish or perish

Introduction

"Publish or perish". Which researcher never heard this fatalistic sentence, often pronounced ironically, but so true with "modern" research?

Publication is an integral part of research. Without publication (or patenting), new inventions and developments are not exploited, are rapidly forgotten, or are reinvented by others. Without publication, Science does not progress. Without publication, research itself has no sense.

But scientific publication has its pernicious effects. Quantity is often the main goal, rather than quality. Researchers write their results too early, too quickly, and are permanently looking for what they could publish, regardless of a true interest. The ultimate goal of research becomes publication, instead of making Science progress for the benefit of everybody. Researchers are often "pushed" to publish too. They are evaluated through their publications (what else could be evaluated?); moreover a presentation is often mandatory to participate to a conference.

A direct consequence of this evolution is the increasing number of publications, making serious bibliographical search more and more difficult. Scientific press may be compared to the internet: you know that the information you are looking for exists, you know that you could find it, but effectively finding it within a reasonable delay is another thing!

To follow this evolution, rankings of publications and of journals have been invented. It is like the TV ratings: it is nice and useful to have it, but using it to take important decisions makes you blind...

This thesis presents some thoughts about the positive and negative aspects of this publication policy. There is of course no universal way of thinking, nor we do pretend to give final conclusions. But there is no harm to think that the situation could be improved in the future...

Publication is integral part of research work

There is no doubt that publication is integral part of research work. By integral part, we mean that a research work is not accomplished until it is published, in a way or another (we include patenting into the possible methods of publication). Many

arguments can be found to prove that publication is not the icing on the cake, but well a necessity in any research work.

Modern research is undoubtedly international, worldwide. What would be international cooperation without the possibility for every researcher to know rapidly, and efficiently, the other teams and researchers working on the same topic? One could argue that publication is not needed to know the other few people working on the same, narrow, research field. Nevertheless, today's research necessitates contacts and close cooperation between complementary teams, probably much more than between specialists of the same topic. Complementary teams are necessary when a research must cover theoretical, experimental and applications aspects, or when the research field is large. We won't enter here the debate whether working with teams having sometimes very different scientific cultures is positive or not, but in any case this often becomes a requirement from the funding sources for research, at least concerning the application aspect. Scientific publication is naturally a wonderful way to make such cooperation feasible, at least to some extent: even if the scientific language is somewhat uniform, scientific papers remain hard to understand by people having strongly different backgrounds. This is probably one of the reasons why review papers and plenary talks to conferences are more and more appreciated, sometimes to the point that researchers are more recognized through this kind of papers than through publications on their own work (see below for a further discussion on that point).

Research is not a competition, but... Whether commercial exploitation of research is looked for (through patenting) or not, publication is always necessary, to some extent. In the first case, publication is necessary to protect the rights of the inventors, but also to attract and convince buyers or potential users of the technology developed. In the second case, publication is necessary to show who developed an idea, when it was first developed, etc. Even if in this last case one cannot speak about "competition" between researchers, it remains that it is more valuable to be the first to publish an idea than the second one... The pride taken by original publications is probably one of the most effective incentives for an efficient work!

Publications are necessary to prove the value of a research team. While looking to the past results of a team seems adequate, for example to decide of the attribution of research funding, an excessive use of this criterion can lead to extreme situations where a research team can only be funded if it already proved (by publications) its skill on the same topic, sometimes on the same work!

But one of the most interesting aspects of publications is that it is a way for a researcher to be evaluated by its peers. It will never be repeated enough that a useful research must be, at the same time, something that is intellectually profitable and useful for others. The advantages of a –serious– review is that both aspects are evaluated, the second one being often underestimated by the authors of a work.

Given these arguments, it becomes obvious that research without publication is nothing else than useless. Publication is an integral part of the research, and a research work should not be considered as successful if it is not accompanied by publications. But, as it will be detailed below, this should not be taken as argument to artificially inflate someone's list of publication: quantity of publications does not mean quality of work!

Necessity for CV

Everyone's activity is evaluated, one day or another. A convenient way to evaluate a research activity is through publications. A problem arises when the quantity of publications is evaluated more than the quality. Of course, everyone in charge of an evaluation will pretend to evaluate the quality, to sort out between important publications and not so important ones, etc. But the sorting criteria are usually ill defined, and vary from one evaluator to another. Worst, the tendency is to define so-called "objective" criteria, in principle a good idea, but dangerous when used with no care (see for example the citation index below).

Moreover, even when the quality is considered as an important criterion, there is no doubt that a long list of publications, *including* high-quality ones, is more impressive than a short list, even made up only by high-quality contributions. An unavoidable consequence is the increasing number of publications, as detailed in a next section.

Authors' list and order

Facing this problem of "scientific recognition" through publications, a common habit that has been taken since several years consists in co-authoring papers. Depending on the field, it becomes usual to have lists of authors with 5, sometimes 10 or more authors! While of course this is fully justified in the case of true team work, it is not a secret that more and more people add non-justified names in lists of authors, under the pretext that the persons mentioned in the list "contributed" to the paper, their contribution being often very limited... In particular, when a published contribution is used in a new paper, there is no reason to mention (some of) the authors of the published work as authors of the new publication. This seems obvious: nobody will include the name of an unknown scientist in a list of authors, even if the work published by this scientist is used in the publication; rather, a proper reference will be included in the paper, which is the way to do it. Nevertheless, the problem becomes different when the scientist in question is renowned rather than unknown, or when he/she is in the same research team as the authors of the new publication. In the first case, who has never been tempted to ask the permission to a renowned scientist to include his/her name in a list of authors (of course when his/her work is used), the sole goal in view being to

increase the notoriety of the paper? In the second case, the justification is rather "it doesn't cost anything to add a name in the list, it will please somebody, and maybe later the (true) authors could benefit from the same "present" from the beneficiaries...". This practice cannot be justified; more objectivity (or guidelines) would be necessary when authors must choose between adding a name in the list, adding a reference or an acknowledgement.

Another debate is unavoidable when dealing with the question of the list of authors. Even when the list of names is determined, in which order do they have to appear? Here again, usages are very different from one domain to another, but also from one team to another, even in the same institution. Some practices include:

- the first author is the one who made the work, the last one is the supervisor or director, all the other ones contributed in a way or another;
- the supervisor is the first author;
- the supervisor is included in the list of authors even when he/she didn't participated to the work, but his/her name is in the middle of the list;
- the head of the laboratory/department is included as last author (even if he/she didn't contribute to the work);
- the Ph.D. student who made the work is not first author unless he wrote the paper too;
- etc.

Again, there is no harm to define different standards, as soon as everybody is aware of them. The danger comes from misinterpretations: how is it possible to know which scheme is adopted by a specific team? And if it is not possible to know it, are any of the schemes still meaningful?

Some funding institutions seem to adopt a specified scheme. The Belgian FNRS for example asks researchers to split their publications into two lists, one when the researcher is first or last author, and the other with all other papers. The underlying statement seems to be "it is more valuable to be the first author or the last one", probably because it is considered that the first author made the work or the main part of it, and the last one supervised the work and gave the first ideas. Again, this practice is not better or worst than others. But when the goal is to attribute funding, comparison or ranking between researchers and/or projects takes place, at one time or another. In this context, what about the teams having different habits (or different requirements!) concerning the list of authors? What about researchers in teams where the head of the laboratory is systematically the last author?

More generally, any general rule adopted concerning the choice and place of

authors cannot be universal. How to cover situations where several people put the same effort in the work? Where several directors supervised the same work? How to differentiate the specific situation of a Ph.D. student's paper from another one written by experienced scientists without supervisors?

Citation index

Facing this inextricable situation, an attempt has been made since a few years, in order to establish objective measures of the quality of research. Of course, as one may expect, the result of this attempt is not perfect, but at least it is better than nothing.

An independent (US) organisation, the *Institute for Scientific Information* (ISI), now maintains a huge database of scientific publications. By publications, it is meant articles published in international journals. This database contains the usual information about an article (title, authors and affiliations, abstract, title and issue of journal, etc.). But it also contains a double list of references for each publication. The first list of references is the usual one, found in any printed journal, and contains all the articles cited by the authors; of course, hyperlinks are added to the references that are themselves contained in the ISI database. But the ingenuity of the ISI database is found in the second list of references associated to each publication: it contains all the references citing the publication (instead of being cited by). This constitutes a wonderful tool for a researcher; it makes it possible to perform "reverse bibliographical search", i.e. to look after all subsequent works that cite a particular publication. The advantage with respect to the first standard list is twofold. Firstly, it makes it possible to search for articles published *after* a known one, and not before. Secondly, it makes it possible to evaluate if a known article has or had impact on future works by others, in other words if it has been cited (and how often) after its publication.

Besides this (very) interesting aspect for bibliographical search, this database may be used straightforward towards another goal: the evaluation of the quality of research. If it is accepted that a scientific work is valuable when (and only when) it is used (cited) by others, then it is easy to estimate this quality by counting the number of articles citing an author's own ones. This lead to what is known as the *Citation Index*.

A further step can be done in counting all articles that cite any reference in a particular journal. This number, relative to the total number of publications in this journal, gives an idea of the impact this journal has on other works; it is called the *Impact Factor*.

The Citation index (of a researcher) and the Impact Factor (of a journal) are two measures that have been defined to be objective. Indeed, looking to the Citation Index of a researcher and to the Impact Factor of the journals where he/she

publishes, gives a much better idea than just counting the items in a list of publication. It is of course very different to publish an article in *Nature* than in the *International Journal of French-Speaking Researchers in Crystallography* (fictional name)! It is also much more easy to compare researchers through numerical indexes than through their CV and list of publications. Nevertheless, as it often happens, a blind use of such so-called objective criteria can lead to disastrous conclusions and decisions. Indeed such indexes also have drawbacks and problems, among which the following.

- The impact of a journal (measured by the Impact Factor) is not equivalent to the difficulty to publish a scientific article in this media. For example, two journals in electronics are compared: Electronics Letters, and IEEE Journal of Solid-State Circuits. Looking to the Impact Factors (1.164 and 1.317 respectively in 1999), one could think that it is more or less "equivalent" to publish in one of the two journals. In the past, the Impact Factor of the first journal is one year below the second one, and another year above. But the Impact Factor does not reflect their different goals. The first one is a "Letter", aimed to rapidly publish new ideas, even if these ideas are not developed, not tested, or even not confirmed yet. The result is that it is quite easy to have an accepted publication in this journal; moreover, the main selection criterion being the "need for a rapid publication", the decision to accept or reject a submission to this journal rarely reflects its scientific value. The second journal however publishes large-scale works, often the result of a long-term project, as a Ph.D. thesis for example. As a consequence, a researcher rarely publishes more than one such article in two or three years, and specialists of the field know that it is a recognition to be published in such journal. Looking to the mean number of citations in the two journals however (leading to the above-mentioned Impact Factor), the situation is different. Among the ideas published in Electronics Letters, some -a few- are good, and will be widely used by others (leading to a high number of citations and thus a high Impact Factor). The articles published in IEEE Transactions on Solid-State Circuits being more on the level of "completed" works, they are comparatively less used -and cited-, leading to a lower Impact Factor. To summarize and caricaturise the situation, one could say that the number of citations per article in the first journal has a very high variance, while the same number is much more "stable" in the second journal. As we know that the average number of citations in any journal is low (around 2, maybe 3), one easily understands that the mean may be higher in the first case, although this does not reflect necessarily the intrinsic quality of the work. Using blindly the Impact Factor could lead, in this particular case, to a situation where researchers try to publish more and more ideas in journals of the first type, without trying to complete their work to the end for publication in the second type!
- Self- and cross-citations are common. They can appear for objective scientific reasons, they can be the result of a deliberate policy of increasing the number references among a group of researchers, or, more frequently, they result from the fact that a researcher cites more easily, but non-deliberately, the

publications of people he/she knows rather than those from strangers. In specific areas of the artificial neural networks research (let's take for example the Cellular Neural Networks...), it is possible to find groups of people (a few tens) working on a specific topic and citing each other in all their publications. This makes their Citation Index more than reasonable, while the interest and the level of their research still remain questionable. Still more dramatically, the quest for a high Citation Index now makes it not unusual to find people making not necessary citations of works from colleagues, hoping the same favour in return...

- In the same range of ideas but at a larger scale, some areas of research are "closed", i.e. shared by a sometimes large community of researchers, but without or with few connections to other areas and applications. As we explain in another document, the field of VLSI implementations of conventional neural network models is maybe a good example. In the early 90's, most researchers in the neural networks field were faced to difficulties to simulate their models, because of the high computational load. The solution seemed to be the design of parallel machines, neural network models being particularly adapted to parallel computations. Because of several reasons, the two main ones being the lack of flexibility of these machines and the rapid evolution of the power of conventional machines, VLSI implementations of neural networks are now only interesting in some very specific situations, for example when the compactness and lower power consumption are important arguments, or when signals must be processed near sensors. Nevertheless, researchers still continue to explore the world of parallel architectures for neural networks, many of them without taking care of this evolution. Since the problem itself is technologically challenging and interesting, it is still possible to find conferences and journals open to publications, even when the interest of the topics covered by the articles is highly questionable. Again, this shows the lack of correlation that may exist, in some situations, between the value of a research work and its measure through the Citation Index.
- As pointed out above, there is no doubt that the Citation Index of a "good" researcher is typically low; a typical figure for an active researcher is some tens (of course depending of many factors, including his/her number of years of experience). Only in exceptional circumstances goes the Citation Index beyond 100. Such very low numbers are of course irrelevant for evaluation: it makes no sense to say that a researchers with a Citation Index of 5 is "worse" than another one having 10... Only higher numbers could lead to faithful comparisons, making the Citation Index a long-term index. The problem is that the Citation Index is often used to evaluate researchers with a few years of experience only (Ph.D. or post-doc level)!
- Not the more innovative articles (on a scientific point of view) are published in the best journals. For instance, journals with a high review contents, like the IEEE magazines, are widely cited, as a consequence of the fact that these journals are agreeable to read, open to a large public of scientists, and widely

distributed (sometimes 100 times more than a typical high-level scientific journal). While it is clear that writing an article in most of these journals is restricted to high-level scientists, it is not because of these articles that one proves his/her scientific competence. This leads to the grotesque situation where the most appreciated articles (on the Citation Index point of view) may, in extreme situations, lack of any original contribution from the author...

- The Citation Index does not include conference proceedings, except in special circumstances (for example, conference proceedings published in the Springer-Verlag "Lecture Notes in ..." series). While it will be seen below that conference proceedings have a much lower impact than journals, it makes no doubt that exceptions exist; but they are not covered by the rules of the Citation Index.
- On a more practical point of view, while the use of the Citation Index is now well established, there doesn't exist a conventional procedure to count the number of citations in the database. Electronic versions of the database do not contain old references, sometimes the citations are counted only when the scientist is first author of the publication, old citations must be found in printed versions of the database, which are difficult to obtain and which makes the problem of the first author even more difficult, etc. Even with the electronic version only, it is amazing to see how two persons looking for the Citation Index of the same researcher may obtain different results...

Facing these shortcomings, it must be made clear that the Citation Index can be considered as a tool helping the evaluation, but in no case as a ranking that may be used blindly. Careless use of the Citation Index as evaluation tool may strongly bias any evaluation. And since all this is well known by most researchers, there is a high probability that the "Citation Index engineering" will increase in the future, making it less and less useful...

Conferences or journals?

It will never be repeated enough that choosing the right conference or journal to publish a new result is of primary importance. Who doesn't know high-level researchers whose reputation is not at the level of their expertise, only because they are used to publish in national journals or conferences restricted to some small communities? But prior this important choice is the decision to present the results in a journal *or* at a conference.

There is no doubt that, in average, journals have a much higher impact than conferences. The reason is simple: journals are widely distributed and available for long after their publication (usually they are published and distributed by press professionals), while the number of copies of conference proceedings usually does not exceed significantly the number of attendees; proceedings are usually

published by scientific organisations (universities, research centres, national funds, etc.).

On the point of view of a publication impact, the message is clear: the impact of conference proceedings is null. Maybe there are a few exceptions in some domains, but all the exceptions together probably do not reach 1% of the existing conferences... When a scientist wants to publish a new idea or development, he/she *must* find a journal, and preferably the *appropriate* journal, for its paper; presenting new ideas at a conference will *not* give the desired impact to the research.

Why do conferences then exist? The answer is simple: because they serve another goal. Conferences are important because they make meetings and discussions between scientists working on the same subject possible. More and more, they also make possible the meeting of scientists working on different topics; without these meetings, large-scale multidisciplinary projects would probably be more difficult to set up. Conferences are also important to *discuss* new results with specialists; even the modern communication facilities will never replace the easiness of oral discussions. Conferences are important for young scientists to make them possible to appreciate their own place in an international scientific context. Conferences are important because listening to other topics may give new ideas for his/her own research.

But another view of conferences is the fact that more and more universities or departments condition the participation of a researcher to a conference, to the presentation of a communication. Reasons of this policy vary from financial aspects, to the quest for more and more publications... Because of this, researchers are tempted to write more publications, even if they have to split their work into small parts in that purpose. The consequences are easy to imagine: lower quality for many presentations, thus lower interest, making the impact of conferences still lower, etc. In most scientific fields, conferences are so depreciated now that communications are sometimes no more listed in CVs... The fact that the Citation Index does not include the conference proceedings is another proof of this evolution.

Concerning journals, the problem is not easy either. Depending on the fields, some journals only accept the result of long-term works, while others accept short ideas or developments. We already developed this argument in a previous section, taking as example the difference between two journals in the electronics domain. But the consequence on the evaluation of researchers is far from being negligible. Let us take another example in the field of analog microelectronics. The design and fabrication of analog systems (close from applications) on one hand and analog circuits (basic building cells being used in systems) on the other hand are very different research works, though covered by the same journals. But the work necessary to develop a typical analog system is of the order of magnitude of a Ph.D. thesis, while a researcher working on basic cells typically designs several blocks during the same period. The consequence is that the first researcher will

probably publish one article in a high-level journal at the end of his/her Ph.D. thesis, while the second one will publish several articles in the same journal during the same period. And this has nothing to do with the quality of work of the two researchers! Again, problems come from the interpretation that could be made of their respective list of publications: a specialist of microelectronics is well aware of this problem, and will take it into account in evaluations, but a non-specialist will easily draw wrong conclusions...

If it is still necessary, another example may be taken from journal special issues. Special issues are common now to gather a large number of articles dealing with the same topic, in a single issue of a journal. The scientific advantages of special issues are obvious. But the problem comes from how special issues are set up. Usually, a *guest editor* will try to solicit as many contributions as possible on the topic. If he/she succeeds in gathering many contributions, then the selection of the papers published in the special issue will be hard, sometimes more than usual for this journal. If the guest editor only succeeds in attracting a few contributions, then the selection will be easier than usual. In other words, the same balance between the number of pages available in a journal and the number of submissions will exist than when a journal is considered as a whole, but this time at a lower scale. Depending of the success of a topic, the quality of special issues may strongly vary, even in a single journal. Of course, such variations are not covered by the Impact Factor of a journal...

Of course, nobody would argue that the situation described here for conferences and journals is general. Exceptions exist. In the microelectronics field, a presentation to the ISSCC conference is more valuable than most journal papers. In the neural networks domain, the NIPS conference plays the same role. But, conversely, the difference of impact between conferences and journals must not be underestimated: one does not have to think that, in average, a journal paper and two or three conference papers have the same impact!

Discussion

The situation described in the above paragraphs leads to a dramatic increase of the number of publications. Besides the decreasing general scientific level, this has another consequence: in some domains (for example artificial neural networks), it is virtually impossible to be aware of all new publications. First because some are difficult to find (conference proceedings), and secondly because it would simply require a full-time job just to read or have a look to all new articles... As a consequence, it becomes more and more frequent to find duplicate works, reinventions and redevelopments, which does not help to make science progress!

A straightforward consequence of the quest for publication is that some researchers tend to choose their day-to-day work in function of the possibility for publication, rather than according to more scientific criteria. For the same reason, it

becomes hardly admitted to work on topics where the risk is high. Although risk is often a synonym of real research, most scientists do not want anymore to "take the risk" of working during long periods on topics that could result in no or few publications. Still another consequence is that researchers try to publish reports rather than articles: they are pushed to write publications at regular intervals, rather than when it is worth to do it...

One could argue that this situation is not too dramatic, as high-quality publications are quite easily distinguished from others, even if some time is needed. This would be true if the abundance of low-quality papers would have no influence on high-quality ones. But it is not true. The need for many publications pushes researchers to write a LPU paper, i.e. a paper with the Least Publishable Unit. Of course, once a first paper is written, the next one refers to the first one, etc., resulting in a set of low-quality publications rather than a single memorable one.

Criticism is easy. But how switch to another, better, system? As today's system is based on informal rules, it is of course impossible to change it radically. But any attempt should be considered to improve the situation. Here are a few suggestions, guided by an experience in the field of artificial neural networks.

- Published papers should fit defined criteria; these criteria should be published in the instructions for authors of journals and conferences. A possible criterion is that readers should be able to repeat related experiences; sufficient information should be given in the paper in that purpose. Web addresses containing information impossible to add in a paper (databases for simulation, etc.) should be provided. But conditions of simulation, detailed description of the error criteria, of the algorithm, etc., should be given in the text itself, exactly as detailed experimental settings are necessary in any experimental paper. In particular situations where this is not possible (confidentiality, too complex process, etc.), it is suggested that papers should only be published if the reader can learn an experience from the paper, that he/she could apply to his/her own case. Relating that a MLP applied to a confidential application has been successful is not a publishable result...
- Bad experiences should be publishable, with the same consideration as good ones. Readers can learn much from a failed experience, if reasons for failure are well established and explained. Relating failures is not accepted at all in the scientific literature, although much time could be gained by many researchers if the same mistakes would not be repeated again and again. This is maybe a particularity of the neural networks field, one of the reasons being the fact that it is young and that many researchers still learn neural networks on the spot.
- As most conferences are not recognized as long-term references, it should be commonly accepted that the same results may be published to conferences *and* in a journal paper; duplicate results in more than one journal paper should, on the contrary, be forbidden.

- Conference proceedings should be made widely available at long-term. One possibility is to develop large servers containing Web-based proceedings of conferences. As incomes related to proceedings are negligible in the balance sheets of a conference, one can be surprised that this practise is not more widespread.
- Common rules should be established concerning the list of authors in publications. Standards should define when to put someone's name in the list or not. Authors' lists could also be divided in groups, clearly establishing the level of involvement of the persons. For example, authors' lists in simulation papers could be split into the following groups: those who had the original idea, those who programmed and tested it, those who supervised the work, etc. Such splitting would of course be extremely difficult, and still open to subjectivity. But a single list without commonly accepted rules about inclusion and order is certainly a worst-case solution...
- Copyrights issues about paper should be made clear, at an international level. It is suggested that solutions should be found to allow authors to publish their own papers on Web sites, even after publication of the paper in a journal. The current policy of several editors is that the last-minus-one version of a paper may be made available on Internet, but not the very last one (published in the journal). Researchers could be tempted to add a few commas between these two versions...
- Information on the review process should be systematically published in journals and conference proceedings. In particular, the number of reviewers for a paper, the percentage of submissions accepted for publication, the list of reviewers (of course for the whole journal or conference), etc. are decisive arguments when one tries to appreciate the quality of a journal or proceedings.
- But the most important is probably: do not base a judgement about someone only on his/her list of publications and Citation Index. Publication engineering has become more than words; let's give research a chance to escape from this blight!