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The race to publish in the age of ever-increasing productivity

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When we walk watching our feet we feel as though we are moving faster. However, in so doing and even if we have a target in mind we may be taking the wrong route. Like broader society our scientific system has been walking faster and faster, or so we imagine. I posit the view that the time has come to look up and consider where our current path is leading us and to consider new routes, as the need for long-term thinking is more pressing than ever. In scientific research we are witnessing an ever faster race to publish since the first publication metrics and the quantitative evaluation of researcher performances were invented. Some of the most obvious drifts of this race have been increasingly discussed since the 1990s (e.g. Gendron, 2008; Hochberg *et al.*, 2009; Jacqué, 2011; Lawrence, 2003, 2007; Mulligan, 2004; Statzner and Resh, 2010; Tainer, 1991; West, 2010) and their causes generally attributed to mechanisms within our scientific systems (e.g. competition, reputation). By contrast, Statzner and Resh (2010) recently suggested that science is being affected by "complex changes such as globalization and the increasing dominance of commercial interest", where quantity of publications as the ultimate performance measure for scientists is having serious consequences not only on the quality of papers, but also on how the whole research process is being conducted (see also Gendron, 2008; Lawrence, 2003, 2007). Indeed, since our scientific system and we scientists are part of a society and political governance where the paradigm of "more is better" prevails, there is no reason to think that science is not affected by the negative drifts of such a productivity race.

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Science as a productive system

One can make straight comparisons between other productive systems in our society, for example, between finance and science. While those in the finance sector are under relentless pressure to increase profits, when scientists seek work or funding they are essentially judged on the number of publications they produce. Why should scientific research be any different from financial ethics? As J. Rohn (2010) recently wrote: "In many professions, one's output is directly proportional to the amount of effort put in. Not so in science." Neither does this seem to be the case in the financial and commercial world. All of us – scientists and commercials alike – look for individual and short-term profitability of our work, since we have contrived a paradigm where numbers are the ultimate Grail. This race has dragged us into a pernicious system where the value of a product, whether financial, industrial or a scientific publication, no longer rests on the effort it entails, but in the strength-of-desire that it provokes: a system where the packaging matters more than the product and stands as a seal of approval and where the solidity and durability of a product give way to the need for constant renewal. In our scientific world these statements can be illustrated by the practical situation mentioned by Statzner and Resh (2010): "formerly, a standard question from colleagues to an author who has had a paper accepted for publication would have been 'what was the topic?' Today, the question is more likely to be 'in which journal?' and 'what is the impact factor of that journal?'". Under these circumstances, negative drifts like redundancy, repackaging of previous ideas (whether proper or not), "salami slicing" (i.e. publishing the smallest acceptable

unit; Hochberg *et al.*, 2009) and even homogenization of science (i.e. supporting of the trendiest science; Tainer, 1991) are all relegated to the background, as they all require reading or asking about the work. Like the current "financiarized" economy, where firms have recourse to the markets and their managers are exclusively judged on their financial output, perverse effects are to be expected (Pestre, 2003) if scientists are evaluated (and auto-evaluated; Gendron, 2008) exclusively on performance metrics.

Inflation in author numbers in a paper or the amount of multi-author papers in a journal or discipline are commonly observed patterns in science (e.g. Cronin, 2001; Greene, 2007; Hudson, 1996; Osborne and Holland, 2009; Slone, 1996; Weltzin *et al.*, 2006), although generally not considered as a drift of the race to publish (but see Lawrence, 2003, 2007). Justifiably, this phenomenon has been related to increasing complexity in scientific research which leads to intensified collaboration, notably through technological development in communicating tools like e-mails and large-file transfer facilities. As M. Greene (2007) wrote, "fewer and fewer people know enough to work and write alone". While this is certainly true for interdisciplinary research linking scientists from distant disciplines (Bennett and Taylor, 2003), it is not so obvious within a given discipline. In that sense, such increase can also be observed in highly specialized journals (for example the mean number of authors per article increased from 1.5 to 3.8 in the *Journal of Fish Biology* from 1970 to 2010), and the number of collaborating centers has proved to be insufficient to explain the inflationary trend of authorship in medical science (Epstein, 1993; Khan *et al.*, 1999). Furthermore, evaluations of research groups and their members are partly based on the amount of collaborative work they have achieved, without any concern as to whether the complexity of the topic actually needed the contribution of any additional collaborator or institution. Unfortunately, the existence of "honorary", "guest", "gift", "all-lab" or "traded" authorship (i.e. treated as a commodity exchange, direct or implied) seems to be common practice in science, obviously raising ethical questions (Cullington, 1998; Flanagin *et al.*, 1998; Louis *et al.*, 2008; Weltzin *et al.*, 2006), but also potentially including other long-term concealed consequences.

Real and fictive capital in science

This inflation in author numbers has diluted the meaning of the word "author" (Cronin, 2001; Weltzin *et al.*, 2006), since authorship involves accountability for the contents of the publication. Since a published paper is the tangible evidence of an intellectual effort of one or more authors, this effort is also being diluted in return. In the same way, this dilution is exposed in authors able to publish up to 100 papers a year, i.e. a paper every ~4 days (searched on ISI Web of Knowledge for physical sciences, year 2010). Three possibilities may account for this phenomenon: either they are geniuses, or their work relies mostly on redundancy, or relies partly on some sort of "fictive capital" (or a combination thereof). A "fictive capital" (Marx, 1894) designates the creation of capital that does not stand on any real economic value or richness. This means the possibility to make financial or commercial profits from products or natural resources that do not exist yet or have not yet been extracted, as for instance selling fish that has not yet been fished or selling junk bonds that may never be paid back. In our globalized economy great upheavals, such as the global financial crisis that started in 2008, arise when large numbers of economic agents want to convert the fictive capital they have accumulated into the corresponding real value (or exchange value). In the same way, problems may arise when a scientific funding agency controls the grounds of an application (i.e. the published papers of the applying team). Elaborating scientific applications for funding based on papers that have not yet been written is strikingly similar to a financial "fictive capital", and is the most extreme and desperate consequence of the ongoing race to publish I have ever read about (Fröhlingsdorf and Verbeet, 2009). Fortunately (or not), this does not sign the end of our economic or scientific systems, it just points out that they have not been able to keep their promises.

The artificial inflation of the number of authors signing a paper could be viewed as a less extreme, but more fixed and long-term, case of accumulation of fictive capital. As stated by S.F. Quan (2008), "publication in peer-reviewed journals is the 'coin' of the academic realm". Scientists who publish extensively get quicker promotion within their institutions, along with greater national and international recognition and better credentials for research grant applications. However, if an author has contributed little or nothing to the intellectual effort necessary to build part of the coins (i.e. the publications) on which his/her career and reputation are based, these ultimate profits are established, at least partly, on what we could call a "scientific fictive capital" or a "fictive intellectual capital".

The race to publish is not only increasing the accumulation of fictive capital in science but is also proportionally reducing the "true" scientific capital or value (i.e. knowledge). Indeed, homogenization and promotion of the "trendiest" science (Tainer, 1991) is reducing the range of topics a scientist would risk to work on. For instance, despite a growing pool of funds for biodiversity programs and databases (e.g. GBIF, EOL, OBIS, etc.), a "non-hot" and undervalued topic like Taxonomy is on the way out along with the knowledge on which all biodiversity studies are based (McClain, 2011).

Thus, even if the total number of scientists evolving in a given discipline increases, they will prefer to invest their efforts and funds in resolving the same or similar questions, at best applying a different methodology (Lawrence, 2007; Statzner and Resh, 2010). This tendency is also related to the increasing political guidance of science which privileges marketable disciplines and outcomes (Pestre, 2003) (e.g. neglecting non-profitable "southern" diseases or focusing on short-term profitable technological innovations without any social benefit except economic growth), where even nature and its biodiversity are now considered as economic parameters by the recent and trendy burst of "ecosystem services". Drawing on Y. Gendron (2008) and R. Whitley (2000), the growing "performatization" in science (i.e. reliance on performance measurements and rankings) promotes superficiality and conformity in scientists in restraining intellectual innovativeness and the development of intellectual diversity. In that sense, scientists responding to a survey in the UK felt constrained to "play the game" and recognized that they had modified the direction of their research to fit the criteria of performance (Gendron, 2008). The disciplinary process based on publishing performance has a self-disciplinary feedback because of material consequences in terms of funding, career opportunities and reputation, thus closing the vicious circle of intellectual stagnation within the mirage of an ever-increasing productive system. The ever-expanding scientific production based on performance metrics partly relies on intellectual homogenization and "profits" made by undeserved authorship and has its clear reflection in a society solely based on financial output and economic growth, where products are ever more numerous but ever more similar.

The "value" crisis

The literature that discusses these issues mainly focuses on the development of new guidelines and recommendations (e.g. Anonymous, 1993; Horton, 1998; Osborne and Holland, 2009; Rennie *et al.*, 2000; Weltzin *et al.*, 2006). However, even if "contributorship" policies (i.e. specified contribution of each co-author) or sophisticated measures for published output (Van Noorden, 2010) are necessary, they remind me of the benevolent governmental proposals made before, and mostly after, the current financial crisis to control capital movements, which were either not followed by any concrete measure or by easily circumvented ones. Indeed, how are we to change a system to which we all adhere and which we cover as we are all getting short-term profits from it? Since the profound reasons of this drift in science are societal (i.e. the "more is better" paradigm), attempts to solve the problem will still encounter insurmountable obstacles unless we refocus on the real value of scientific research, which are the fundamental quality and/or societal value of the knowledge and findings produced.

The relevance of the comparison with the financial system stops here, since the amount of "scientific fictive capital" does not necessarily generate concerns about the veracity and reliability of the reported research. In other words, the "coin" is always related to a real value, namely the findings contributed by the published paper and the intellectual effort that it supposes. However, the value of a scientific work, i.e. within publications, applications or careers, either in absolute terms or in relation to the increasing dilution of authorship, is being disconnected from the knowledge produced and from the intellectual effort necessary to produce it. Like a trademark that arouses desire in us, the title of a journal has gained greater value than what is written inside. But scientific papers are meant to be read, not to be counted. Inevitably, to reset the value of knowledge and intellectual effort at the centre of any scientific evaluation or work, the time that each of us is willing to invest in it must necessarily be increased. As an inevitable consequence, and like other social movements trying to focus on quality and sustainability (e.g. "slow food", "slow city" and now also "slow money"; Tasch, 2008), the way research and evaluation processes are being conducted should decelerate toward some sort of "slow science".

Of course this does not mean slowing down the research process while keeping the same direction, but defining new objectives. As an example to be followed in evaluating research grants, the leading German national funding agency (the Deutsche Forschungsgemeinschaft) has recently made an important step toward evaluating quality and innovativeness rather than quantity by restricting the number of publications that support new applications to five references, and two per year for finished projects (Carbone, 2010). Changing grant evaluation criteria toward intrinsic quality (avoiding numbers of publications, impact factors and subsequent metrics) seems a good starting point because it should have repercussions on the other stages of the scientific process, i.e. publications and careers, forcing scientists to modify the direction of their research toward quality and innovation. Evaluating the intellectual/practical value of ideas and research, and comparing them, is difficult and inevitably takes more time since a reviewer will need to gain the required knowledge he/she may not yet have yet. In that sense, any implemented measure at the scientist level toward decelerating and focusing on research quality will therefore require institutional and political decisions that consolidate this shift. For example, by explicitly accounting, in the periodical evaluation of scientists, for the time spent on reviewing papers or projects and journal editing, by fixing the number of publications that individual

scientists and teams can present in those evaluations and by avoiding the possibility of being involved in several projects inevitably cumulating more than 100% working time. Unfortunately, since the reliance on quantitative performance metrics facilitates the political and institutional manageability of intellectuals and instrumentation of science, the current race to publish, as a reflection of the "forward flight" leading all societal issues, is unlikely to stop soon.

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