



theFuture ofScience andEthics

Rivista scientifica a cura del Comitato Etico
della Fondazione Umberto Veronesi

Volume 1 numero 2 ■ novembre 2016



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REPUTAZIONE

La reputazione comporta impegni morali e crea aspettative rispetto alle azioni altrui. Non può esservi economia o etica, diritto o politica senza reputazione. La cultura della reputazione, essenziale per la società contemporanea nel suo complesso, si collega per di più al grande tema della fiducia, sentimento morale costitutivo della comunità scientifica e di fondamentale importanza nei rapporti tra questa e i cittadini. Spesso, inoltre, la reputazione dello scienziato dipende o viene influenzata dalla sua immagine pubblica, così come veicolata dai media, con effetti talora fuorvianti e con ricadute potenzialmente distorsive sul sistema della ricerca.

The new markets
of academic reputation:
impact, prestige and
bad incentives
in the market of research¹

*I nuovi mercati della
reputazione accademica:
impatto, prestigio e
cattivi incentivi
nel mercato della ricerca¹*

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ABSTRACT

In this paper, I analyse the recent changes of academic reputation due to the new dynamics of scholarly publishing. I distinguish between two families of incentives that have doubtful consequences on publication practices and on the overall ethics of scientific research: (1) bad incentives that market forces dominating academic research and (2) incentives to act badly, that is, the encouragement of practices that harm the credibility of academic reputation. I conclude that only this second kind of incentives is harming research practices, while the first one is transforming them in ways that should be monitored by researchers in order to adjust their production to these new unintended effects.

ABSTRACT

In questo articolo analizzo i recenti cambiamenti della reputazione accademica dovuti alle nuove dinamiche del pubblicare in ambito accademico. Distinguo tra due famiglie di incentivi che hanno conseguenze dubbie sulle pratiche di pubblicazione e in generale per l'etica della ricerca scientifica: (1) cattivi incentivi dovuti alle forze economiche che dominano la ricerca accademica e (2) incentivi ad agire in modo scorretto, ovvero che danneggiano la credibilità della reputazione accademica. Concludo quindi che solo questo secondo tipo di incentivi danneggia la ricerca nella pratica, mentre il primo sta trasformando la ricerca secondo modalità che dovrebbero essere monitorate dai ricercatori al fine di modificare la loro produzione a tali effetti non voluti.

KEYWORDS

Research evaluation
Valutazione della ricerca

Sociology of knowledge
Sociologia della conoscenza

Academic reputation
Reputazione accademica

THE EPISTEMIC DIMENSION OF REPUTATION

People do not share information. They share evaluated and classified information that creates a “reputational stream” of shared judgements. The epistemological implications of the massive use of shared ratings in networked societies are significant. Relying on other people’s judgements and authority has become a customary epistemological practice in our *informationdense societies*. Each social interaction leaves an informational track, a pattern of reciprocal judgements, an evaluated image of ourselves in the eyes of others. This evaluative dimension of social interaction, this generation of opinions on each other is what I call here *reputation*.

The new social and comparative dimension of social action, so typical of our late modern societies, has an epistemological counterpart: from the point of view of the observer, the reputations of other people, of things and of ideas are the more and more relevant to extract information about them. How do we use these reputations to formulate a judgement? How are these reputations constructed? Are they reliable? Which are the social/economic/cognitive biases that shape their credibility? In this article, I would like to focus on the case of *academic reputation*, how it is constructed nowadays, what is its objective value and how it impacts the production of knowledge.

THE NEW DYNAMICS OF KNOWLEDGE PRODUCTION

The academic world is one of the most exposed to the influences of reputation: prestige, honour, emulation are the currency that is exchanged among researchers, who are usually more motivated by these symbolic rewards than by economic interest. The most fundamental aim of the academy is to design institutions that create and foster evaluation, rankings and reputation in an objective way, the most precious ingredient to brew what the scientific community acknowledges as “Truth”. The system of “peer review”, introduced in 1665 with the first publication of the *Philosophical Transactions of the Royal Society* is still the core of scientific evaluation and publication. In spite of its biases and problems, it is still considered an essential tool of demarcation between science and pseudo-scien-

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ce: «An article in a reputable journal does not merely represent the opinions of its author; it bears the imprimatur of scientific authenticity, as given to it by the editor and by the referees he may have consulted. The referee is the linchpin about which the whole business of Science is pivoted»². Yet, the way in which scientific research is produced and transmitted nowadays has been dramatically changed in the last decades by a series of techno-societal major transformations, such as:

1. changes in the dynamics of literature-based research caused by the communication revolution;
2. changes in the interconnectedness of the scientific literature, due to the collaborative mode of production of knowledge typical of "Big Science";
3. biases and perturbations in the network of publications created by the introduction of new indexes and other scientometric measures, such as citation indexes and impact factors;
4. new forms of control and accountability introduced by governments and funding on the production of scientific knowledge and, most notably, the introduction of an *audit* culture in the national academic systems (Amman 2013);
5. new business models for the publishing industry and a new "business-like" competitive culture introduced in academia, notoriously described by the "publish or perish" *motto*. 'Publish or perish' is the tenet governing the day-to-day work and careers of university researchers, although the underlying long-run goal should be the production of new knowledge.

All these new dynamics have deeply transformed the scientific life and conditioned in new ways the economics of knowledge production. They have also challenged the received, common-sense view of scientific research. Science today resembles more to an entrepreneurial business than to a contemplative, disinterested activity (Shapin 2008). Scientific research is not governed anymore or, at least, not *only*, by the Mertonian CUDOS norms (Merton 1942/73) of *communalism, universalism, disinterestedness and organized scepticism*. It is a wholly interested activity,

an economic and competitive enterprise that produces and sells knowledge through a multifarious system of incentives. Reputation is one of the incentives.

THE ECONOMY OF REPUTATION IN THE MARKET OF SCIENCE

There are at least three reputational dimensions that the new game of science has taken into account due to the transformations above:

1. an *economy of esteem* (cf. Brennan and Pettit 2004) upon which the mutual respect of scientists is based
2. a *business economy* that underlies the new modes of production and publishing science and
3. a *grandeur economy* (prizes, recognition, national reputation and national and international ranking systems) that underlies the public incentive systems and the new governance of research institutions (and that can be compared with other "*grandeur economies*" such as the prestige of sport for national reputation - numbers of medals at the Olympic games, etc.) and that is well highlighted by the accrued importance of the relative positioning of research institutions in the international rankings and by the proliferation of a rhetoric of excellence and of national agencies of evaluation such as ANVUR (Italy), AERES (France), ANECA (Spain), etc.

These three dimensions emerge from very different histories and motivations, creating today a sort of "cacophony" of incentives that clash profoundly with the vocational activity of the scientist. One of the major tensions is between the spontaneously generated norms of the academic systems and the norms generated by the adoption of the new modes of massive production of knowledge listed above. The new dynamics of knowledge-production have penetrated the highly idiosyncratic system of academic reputation by borrowing features from the market. Yet, market and academy have profound differences and specificities that are deeply entrenched within the two cultures. Most notably, the peculiar nature of scientific copyright – that it is not a *right* of property on a piece of personal expression, but a *reward* for having produced an objective statement

about nature (Biagioli 2013) – and the not exclusively – economic nature of this reward that is primarily distributed through reputational benefits in terms of credit and impact and that can only ancillary used for obtaining an economic reward as research funds or for career upgrades³. The current dynamics of the sector are thus the resultant of two distinct forces: the scientific norms governing researchers' activities, and the incentives of the market for journals (Migheli & Ramello 2013).

The overlap and feedback between these systems has transformed the competition among scientific ideas into a competition among publishers and journals – or bundles of journals, in what is known as the 'Big Deal' (Odlyzko, 2014). At the same time, validation and evaluation systems have shifted from serving scholars and the community to serving the 'market value', of journals relating both to attractiveness of every single title within the market and its ability to capture scholars attention (Migheli & Ramello 2014).

BAD INCENTIVES VS. INCENTIVES TO ACT BADLY

Here, I would like to introduce a distinction that may be useful in order to better evaluate to what extent these new dynamics have *harmed* the production of research or just *transformed* them into something else. Among the myriads of new incentives to scientific production that these dynamics have created, I would like to distinguish between two categories: *bad incentives and incentives to act badly*. *Bad incentives* are incentives that make researchers produce more for the bad reasons, for example, in order to foster an international market of academic publications that enriches few publishers that control most of the publications⁴ by exploiting a lot of "free" work of researchers (not only papers are not paid for, but the whole process of *peer review*, which is essential for the quality of publications, is realised "graciously" by the community of researchers). Or, they can be incentives that are not bad *per se*, but produce *bad consequences*. On the other hand, incentives to *act badly* are those incentives – mainly coming from the third dimension of reputation, the *grandeur economy* – to act in a sub-optimal way and worsen the quality of research in order to cumulate personal advantage. Although my paper criticises both kinds of in-

centives, I conclude that only the second kind of incentives is radically pernicious for the quality of academic production.

BAD INCENTIVES AND BAD CONSEQUENCES IN KNOWLEDGE PRODUCTION

There are many informal incentive systems in higher education that may end up with bad outcomes. Take for example the selection of vocations. The Australian selection system for tertiary education is one of the highest ranked systems in the world. In order to be able to apply to the best universities, high school students have to reach a certain *score* that is cumulated along their career with a system of points. For some programs, such as medicine and law, only the highest scores are considered. Thus, there are students that end up in a medical school without having any special vocation for medicine or for law.

A similar incentive system, of selection of the "best for the bad" can be at place, especially at the PhD level, in the case of SSH (*Social Science and Humanities*) The best students are encouraged to do obscure things, like toy modeling, to be "hireable" by good departments (the *mindbinding* phenomenon). This creates a pressure for the students in SSH to show some competences in hard sciences. A total lack of mastery of formal techniques is seen as a lack of intelligence, or competence. Thus, the best students in SSH are encouraged by their teachers to engage in mathematical modeling. This creates a leadership of those who have this mastery and use it the more and more to evaluate the new "entries". (Cf. J. Elster 2012)

Another bad incentive in this sense in SSH is the generalised use of English as an international academic language without a clear and shared policy at the local level to give the adequate English linguistic competence to potential researchers. This has created a lot of imbalances in the world of academic publishing and a lot of new weird "jobs" of agencies providing editorial support for the redaction of a paper with bad results and a massive reproduction of bad style in academia (Billig 2013). Although English as a unique academic language can be a good idea, if this does not go with adequate linguistic policies it may turn out into a bad outcome (Van Parjis 2010).

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THE VAGARIES OF IMPACT

One of the best known bad consequence of using scientometric measures for science – that is notorious in scientometrics – is the *Mathieu effect*, or the “rich get richer” effect of any citation network. It is a result first put forward by Robert Merton and then formalised in network theory. Those who are cited more often have better chances to be cited in the future.

Take the *Impact Factor* (IF). This is a measure that contains a lot of biases, but to avoid paranoid attitudes towards it, it is important to know that most biases were just not predicted at the time of the introduction of this system in the Sixties. It is not an *evil* system *per se*: only a system whose consequences were bad on the academia.

When IF was invented by Eugene Garfield in 1955, it was not conceived as an evaluative tool: it was just a classificatory tool in order to navigate with some criteria in the deep sea of scientific scholarly publications, that were just becoming too many. It was a simple bibliometric tool (cf. Garfield 2006). There are three main indicators that are produced by the *Journal Citation Report*, that is a tool delivered each year by the ISI Thomson society, the company which owns today the *Citation Index*:

- Impact factor;
- Immediacy index;
- Cited half-life.

The IF of a journal for the year 2013 is a ratio between all the citations published on that journal in 2011 and 2012 and the number of papers published on that journal in 2011 and 2012. The *immediacy* index measures the immediate citations of the average of papers in a journal by dividing the number citations received by the journal in a year by the number of papers published in the journal that year. The cited half life is the rate of decline of the a paper, that is the number of years needed for the citation rate to diminish of 50%.

There are of course many biases in non of the system. Some are historical biases: as all classificatory/rating system, history has a value, and these systems are very slow in correcting data about “representative” journals that were collected many years ago. For example, the SSCI, that was introduced to deal especially

with SSH journals, contains among its representative journals *The New York Review of Books*. Of course, we all know that this is a very authoritative journal in the humanities, but it is not a peer reviewed journal so it should not be in the list. (Klein and Chiang 2004). But then, when asked why non-peer reviewed publications such as *The New York Review of Books* and *The Republic* were included in the SSCI and in the AHCI, the reply was that their prestige was obvious: everybody knows that it is very difficult to publish in these journals and that they matter a lot to the community. Of course, this creates a vicious circle: scientific prestige should be *defined* by the bibliometric measures and not presupposed in order to design these measures.

There is also a huge literature that shows strong disciplinary biases that influence the presence in the *Citation Index*. The discipline, the type of article (letter, full papers, reviews) and the number of authors per paper influence the number of citations. Journals of pure disciplinary areas tend to have a higher IF. The more authors per paper the more citations they have. And there is a strong discrepancy among disciplines: 2 average in the social science and 4 average in physics. A letter or a review has a higher immediacy factor and a lower cited half life. (cf. Casati, Origgi, Simon 2011).

Other indexes of impact have been proposed in order to balance the mere *popularity* measure of the IF (number of citations in other papers) with a more weighted conception of *prestige*. For example, the Eigenfactor is a PageRank factor, that is, it weights the sources of citations⁵. From these indexes, it has been extracted a researcher’s impact factor, the H-factor, introduced by George Hirsch, a physicist at UCSD. The index is a measure of the number of highly impactful papers a scientist has published. The larger the number of important papers, the higher the h-index, regardless of where the work was published. It is a way of calculating the impact of your own research independently of the impact of the journal.

A scientist has index H if h of his/her N_p papers (N_p = total number of papers) have at least h citations each, and the other ($N_p - h$) papers have no more than h citations each.

In other words, a scholar with an index of H has published h papers each of which has been cited in other pa-

pers at least h times. Thus, the H-index reflects both the number of publications and the number of citations per publication. The index is designed to improve upon simpler measures such as the total number of citations or publications. The index works properly only for comparing scientists working in the same field; citation conventions differ widely among different fields. Although the H-factor has been proved a fairer measure than other scientometric indexes, its non is own biases:

1. It counts a highly cited paper regardless of why it is being referenced for negative reasons (a paper can become notorious and cited many times for its flaws instead of for its qualities);
2. It does not account for variations in average number of publications and citations in various fields;
3. It ignores the number and position of authors on a paper;
4. It limits authors by the total number of publications, so shorter careers are at a disadvantage⁶.

THE PASSION OF RANKING: THE WORLD'S UNIVERSITIES RANKINGS

Each year, three important rankings of the universities around the world are delivered:

1. The Times Higher Education World Universities Rankings: <https://www.timeshighereducation.com/world-university-rankings>;
2. The Shanghai Jiao Tong Ranking: <http://cwur.org/2016/Shanghai-Jiao-Tong-University.php>;
3. QS World University Ranking <http://www.topuniversities.com/universityrankings/world-university-rankings>.

Although each year these three publications provoke panicking reactions by university administrators and governments, the rankings are surprisingly stable if compared to ranking of companies, with the top 20 universities more or less always at the top at least since 1911⁷. This is due to many structural features of the “market” of higher education

(universities rarely disappear and rarely merge, they do not expand often and thus have a system of incentives to production that is very different from that of companies), but also to some biases of these systems that have been pointed out by various researchers.

Michael Bastedo and Nicholas Bowman⁸ have found *anchoring effects* in the ranking systems. They have looked at the first three years of world university rankings contained in *The Times Higher Education*, looking specifically at the reputational survey, and found that the reputational results that were formed in the first year of the survey—rankings formed in the absence of world rankings—were reinforced in subsequent years. They attribute this reinforcement to the anchoring effect of previous rankings. «Clearly, rankings drive reputation, and not the other way around», write the authors.

In a critical piece published on the *New Yorker*, Malcolm Gladwell stresses the heterogeneity of the basic units that are compared by the rankings: it is like comparing apples with pears, there aren't clear-cut criteria that tell us what are the basic objects we are rankings. Big research institutions like the French CNRS are compared with medium size campuses whose vocation is mainly teaching and not research⁹. Also, there are different temporalities of quality and prestige. Changes in prestige take a lot of time to be registered in the ranking. And this may create other biased perspectives.

Ranking systems and Citation indexes contain, as we have seen, a lot of biases. Incentives to play the game of these systems should consider these biases, try to correct them or mildly “game” the systems. But these systems are not bad per se. They may constitute bad incentives, that is, incentives that have bad outcomes but have not been intentionally designed to produce bad practices. There are other categories of incentive systems that are intrinsically bad, that is, they have been designed in order to lower the standards of scientific outcomes by keeping the rhetoric of excellence and *grandeur*.

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THE AUDIT CULTURE IN THE ACADEMIES AND THE NEW GOVERNANCE OF RESEARCH

From 1994 until 2002, Ron Amman was Chief Executive of the Economic and Social Research Council and then Director General and Permanent Secretary of the Center for Management and Policy Studies in the Cabinet Office. A previous sovietologist, he wrote in 2003 a paper on the kremlinology of the British academic system¹⁰. Once at the head of the ESRC, he realised how useful his education as a sovietologist was: «The careful study of the Soviet central planning which had absorbed my attention for over twenty years, far from being a waste of time, had instead provided me with unique qualifications – not so much in technical expertise but certainly in understanding». As he denounces: «The growing managerial pressures in the public sector in Britain, which caused dismay and incomprehension to many colleagues, were instantaneously recognizable to an old Soviet hand». Amman mapped the administrative levels of the UK higher education system and those of the Soviet machinetool industry to conclude that «within such a planning system the volume of transactions is so huge and the interdependent relationships were so complex that real control was quite impossible». Amman writes that the way the system was permanently gamed is a «huge triumph of microrationality over macrorationality». All actors in the system *had* to succeed in fulfilling their plans, with huge falsifications of the actual activities in order to fulfil the indicators of performance imposed by the system. The origins of the planning culture in Britain and now in many other countries lie at the very opposite political spectrum from those which inspired the Soviet central planning, the latter to be found in Lenin's critique of "economism" and the former in the effort of Mrs. Thatcher's politics since 1979 to "strengthen the market", by reducing the levels of public spending and create more incentives for private initiatives. As for the higher education, the idea was the radical introduction of quasi-markets in the sector, with a clear division between purchasers and *providers*, but of course these are not real markets with real customers buying services with real money. The money is provided by central funding bodies, and those who control the transfer of resources are less competent about the "products" (like the quality of the publications) than those who receive the resources. In order to avoid the risk of un-

scrupulous institution to "game the systems" the audit and controls were introduced with such an artificial system of indicators that the system became even easier to game. Amman calls the "*audit explosion*" a form of "*cultural colonization* intended as a political challenge to professional autonomy". Professionals of higher education system absorbed these new norms with different levels of commitments, thus creating the ideal conditions of the spread of mediocre outcomes by formally accepting a norm without adhering to it substantially. A lot of falsifications in the reports of the work done in projects (given the impossibility of having "objective" measures of productivity given that the productivity criteria were "created" by the performance indicators) was the result, among which, an immense increase of publications, that are at the core of the performance indicator systems, produced with less and less "meaning", and with more and more biases, such as the well documented practice of paper slicing (or *salami slicing*, Souder 2011) that is the tendency to publish the least publishable unit as a paper (Scott-Litcher 2011).

Another interesting analysis of the same attempt to introduce a new governance in the research institutions through an audit culture, this time in France, is provided by the sociologist of economy Lucien Karpik in his paper: *What is the Price of a Scientific Paper?*¹¹ Given the quality uncertainty on the academic publications, he tries to explain the new criteria of quality in these terms:

The broader changes brought about by the new research policy: the transformation of researchers into entrepreneurs; the extension of managerial vocabulary and practices to the research realm; the use of indicators and rankings combined with the apparent disappearance of peer review; new modalities of research resource distribution; a generalized individual competition; the creation of new control organizations; the emergence of upheavals, schisms, and conflicts within the research world; and changes in relationships between the state and science. The new system of scientific evaluation ties all of these elements together. With some variations, it occupies a prominent position in a growing number of countries. It incessantly expands its circle of experts, devotees, cynics, and victims within each country. And it has provoked the development of a vast and diverse literature in statistics, management, economics, sociology,

political science, and law, not to mention ideological and existential thought (Karpik, cit, p. 62-3).

The new system of research governance of research, especially at the national level, cannot only be considered as *bad incentives*. They are incentives to *act badly*. Given the impossibility of quality control by the audit management, whose members are outsiders of the community of peers typical of researchers, the culture of audit has created a system of incentives in publishing in any peer review journal whatsoever in order to raise your IF (but not your H factor). The rhetoric of publications as the index of productivity of a research grant has created other kinds of incentives to bad practices such as *self citation* (Hyland 2011) and *paper slicing* (what Souder (2011) calls: *salami slicing*, that is, the practice to “slice” a paper in thinner units so that you can end up with more than a publication with basically the same idea). At the level of National and European funding agencies, the very structure of the grant application encourages researchers to commit themselves to false deliverables and false tasks. The relative low rate of success in obtaining a grant (ranging from 10% for the National Agency of Research in France (ANR) and to 1% for very prestigious high tech grant EC schemas such as FET-OPEN) creates incentives to report a lower outcome in order to ask more funding the subsequent years.

THE KAKONOMICS OF ACADEMIC PUBLICATIONS

Based on my previous work on the preference of low outcomes in academia¹², I will call these incentives to act badly: *Kakonomic Incentive Systems*, that is, systems that have incentives to receive mediocre benefits and deliver mediocre products. In my work with the sociologist Diego Gambetta, we realised that people sometimes rationally prefer a mediocre outcome instead of a better one. This is due to particular structural forms of interactions that create incentives to lower outcomes. Rational agents usually prefer to have the best at the lowest cost. They may end up without the most preferred outcome because the cost was too high. But still, they would have preferred to have high for low. Kakonomic exchanges are those where you prefer to exchange at a low quality, that is, to deliver a low outcome and receive a low one. While standard game-theoretical

approaches posit that, whatever people are trading (ideas, services, or goods), each one wants to receive *High-quality* work from others. *Kakonomics* describes cases where people not only have standard preferences to receive a *high-quality* good and deliver a *low-quality* one (the standard sucker's payoff) but they actually *prefer* to deliver a *low-quality* good and receive a *low-quality* one, that is, they connive on a mediocre exchange¹³. The most striking case of this preference for mediocre outcomes is the persistence of publications in peer review journals with a low IF. When asked why do they still publish in mediocre peer reviewed journals, researchers reply with a series of justifications among which the most common are the following: (1) Research papers are the way to inform the other members of our research community about the advancements we are accomplishing in our domain. (2) They are a standard format for communicating science, a format set by robust and ancient social norms within the learned community. The persistence of this format is due to the fact that they are the minimal rhetorical unit in which a scientific contribution can be expressed and evaluated by the community. (3) Publications in peer reviewed journals are the only ones that assure reputation and credit to researchers. (4) Research papers are a sort of “conversation” at slow motion: I write a paper for an academic journal, the paper is evaluated by other philosophers who suggest improvements, it is then disseminated to the academic community in order to prompt new conversations on a topic or launch new topics for discussion.

All these justifications can be challenged (Origgi 2011). Papers are no more the most efficient way to keep in touch with the academic community. Emails, lists, conferences, blogs are a more efficient ways. Papers are not the minimal meaningful unit to communicate a result. The format of the paper varies largely from the 800 words format of Nature to the free style essay more common in the humanities. The publication in peer reviewed journals is *not* the only one that assures reputation and credit to researchers. Studies of the discrepancies of reputation show that a maximum of exposure in citation indexes does not influence the reputation for the quality of a researcher. (Hamermesh and Pfann 2009).

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Publishing in low IF peer review journals is a kakonomic incentive: it is a way of maintaining niches of protected publications, fostering the publishing industry to the detriment of our own research interests. The motivations are others, like that of conforming to the requirements of funding agencies and being advantaged in having subsequent grants while pretending to have respected the high quality standards required that in fact do not exist.

This second class of incentives that I have analysed, that is, incentives to act badly, are much more pernicious for academic research than the first ones, mainly created by the "business turn" in the global research market. These latter create an atmosphere of "false consciousness" in research in which people justify their work on the basis of pretended norms of excellence that are in fact based on a rhetoric of reputation that does not correspond to the actual standards of quality of research. Another example of this collective false consciousness is the encouragement by the agencies of quality control of research (ANVUR, AERES, etc) to include in the *curriculum vitae* if the researchers submitted to national evaluation the grant applications that they have submitted and have not been retained, in order to justify months of work in which all that has been accomplished in terms of research is filling forms with lists of unrealistic tasks and deliverables.

CONCLUSION

Research is a reputation-based market. Paradoxically, an activity that should be based only on the objective quality of the scientific work accomplished, due to the transformations seen above, is becoming more fragile than other markets to the infiltration of rhetorical "bubbles" about quality and excellence that are fostered by the *grandeur economies*, a crucial dimension, as we have seen, of the new economy of academic reputation. The quality of scientific research and the respect for a universally shareable truth should be fundamental ingredients of the ethics of research. Kakonomic systems of incentives to act badly are eroding this ethics to the advantage of pure rhetoric of excellence against its substance.

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NOTES

1. I am indebted to to Giovanni Ramello, who has coedited with me a special issue of *Research Evaluation*, on the new dynamics of academic publishing. An older version of this paper was presented in December 2013 in Bogotá, Colombia, at the international conference organised by Jon Elster on *Hard and Soft Obscurantism in the Humanities and Social Sciences*. I thank the participants to the conference for their insightful comments and suggestions, and two anonymous reviewers of this journal for their comments on a previous version of this paper.

2. J.M. Ziman *Public Knowledge: The Social Dimension of Science*, Cambridge UP, 1966, 148.

3. See Origgi & Ramello (2015) "Introduction to Special Issue on Current Dynamics of Academic Publishing" *Evaluation Review*, 39, 1, 1-16.

4. Combined, the top five most prolific publishers (Elsevier, Springer, Basil-Blackwell, Taylor&Francis, SAGE) account for more than 50% of all papers published in 2013. Disciplines of the social sciences have the highest level of concentration (70% of papers from the top five publishers). Cf. Larivière V, Haustein S, Mongeon P (2015) "The Oligopoly of Academic Publishers in the Digital Era". *PLoS ONE*

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10(6): e0127502. doi:10.1371/journal.pone.0127502.

5. www.eigenfactor.org

6. Bornman, L., Daniel, H.D. (2005) "Does the h-index for ranking for scientists really work?" *Scientometrics*, Volume 65, Issue 3, 391–392.

7. Cf. J. Quiggin (2015) "Rank Delusions", *The Chronicle of Higher Education*, 16 Feb. www.chronicle.com/article/Rank-Delusions/189919

8. Cf. Bowman, Nicholas A. and Michael N. Bastedo. 2011. "Anchoring Effects on World University Rankings: Exploring Biases in Reputation Scores." *Higher Education* 61: 431-44.

9. Cf. M. Gladwell, (2014) "The Order of Things" www.newyorker.com/magazine/2011/02/14/the-order-of-things

10. R. Amman (2003) *A Sovietological View of Modern Britain*, *The Political Quarterly*, vol. 74, n. 4.

11. Cf. L. Karpik "What is the Price of a Scientific Paper?" (2011) in J. Beckert, P. Aspers (ed.) *The Worth of Goods*, Oxford UP, 62-85.

12. D. Gambetta, G. Origgi (2013) "The LL-game: the curious preference for low quality outcomes and its norms", *Politics, Philosophy and Economics*, <http://ppe.sagepub.com/content/early/2012/04/05/1470594X11433740.abstract>

13. Cf. G. Origgi (2012) "Kakonomics" in J. Brockman (ed.) *This will make you smarter*, New York, Doubleday, 320 and ff.



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