Academic Ethos, Pathos, and Logos

RESEARCH ETHOS

Nagib Callaos and Bekis Callaos

Simón Bolívar University and
The International Institute of Informatics and Cybernetics (IIIS, www.iiis.org)

ABSTRACT

Elsewhere (N. Callaos and B. Callaos, 2014)[1] we have shown the conceptual necessity and the pragmatic importance of including Ethos, Pathos, and Logos in any systemic methodology for Information Systems Development (including software-based systems) and for the design and implementation of informing processes. This is the first article of a planned series in which we will try to apply what has been shown and concluded in the mentioned article to the specific case of Academic Informing or Academic Information Systems. Research activities include informing processes, which should address the respective Ethos. Our purpose in this article is to address one of the issues involved in this aspect. With this article we are trying to make a step forward according to the recommendations we included in the conclusions of the referred article (N. Callaos and B. Callaos, 2014). To do so, we will briefly abridge previous work, provide some facts via real life examples, give few opinions and ask many questions. Few of these questions will be rhetorical ones while most of them will be oriented to generate reflections regarding the respective issue and potentially some research, intellectual enquiry, or practice based position papers.

GENERAL CONTEXT

It is evident that effective communication is a necessary condition for Academic Informing. This effectiveness has been basically related to academic writing, pedagogical innovations, and educational technologies, mostly in the context of disciplinary logic and rigor. Persuasiveness in academic writing has been admitted for a long time as necessary condition for effective academic communication and informing. That academic writing is, or should be, *persuasive* is not news. Ken Hyland affirms that "It dates back at least as far Aristotle and *it is widely accepted by academics themselves*."[2] This includes scientific communication. An increasing number of articles and books have been published lately regarding the importance of persuasiveness in scientific communications and on the Rhetoric of Science.[3] But, the focus has been, up to the present, on academic writing. Our academic and professional experience show that persuasiveness is, or should be, implicitly or explicitly, an essential characteristic in all academic activities: research, education, and consulting or problem solving, and not just in academic writing. Experience-based reflections show that a more comprehensive and systemic approach is required for enhancing the effectiveness of Academic Informing in its societal and civic contexts. A main purpose of the articles series, mentioned above, is to examine and reflect on a more comprehensive approach to Academic Informing for a higher effectiveness of these activities. This will be attempted from a *pragmatic-teleological* perspective, i.e. oriented by the **ends** of Academic Informing and by the potential **means** that might be used to achieve these ends. We will focus in applying classical means which have effectively been applied in the past but they are not being applied (at least not explicitly) in the last few decades to support academic informing. Consequently, we will examine the relationships between academic activities and persuasive processes or methodologies, focusing mostly on *Academic Ethos, Pathos, and Logos*, as fundamental and necessary characteristics of more persuasive academic informing and, hence, more effective academic activities.

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[1] This article is based on previous articles and on practice-based reflections as well as on Action-Research and Action-Learning in the context Methodological Action-Design.

[2] Italics added

[3] See for example Alan G. Gross, 1996, 2006, and H. W. Simon (Ed.), 1990.

SPECIFIC CONTEXT

A main source of the mentioned series of articles, and of this first one, will be our about 45 years of academic and professional activities. This will be a main input to applying a mostly *Reflexive Methodology,*[4] regarding the issue described above. Part of this practice-based reflections and conclusions was the article mentioned above which mostly represent a product of our professional experience in the context of Information Systems and Informing Processes analysis, design, and implementation.

In the mentioned article, we basically applied what Donald Schön (1983) proposed, in "The "*Reflective Practitioner*" in the context of our professional Practice. Now we will try to apply it in the context of our academic activities. Our aim in this paper is to make a short presentation of the main reflections and conclusions we have had during our academic practice in research-oriented informing activities (including peer review, conferences organization, journal editing, research administration, etc.). Educations and consulting will be addressed in following articles, and mentioned in this article when they have relationships with the main topic here.

INTELLECTUAL AND PRAGMATIC IMPORTANCE OF ACADEMIC ETHOS, PATHOS, AND LOGOS

In this section we will address the intellectual and pragmatic importance of Academic Ethos, Pathos, and Logos. Next articles will go in more details regarding this issue. Let us her provide a very brief discussion on this issue which objective is to provide Intellectual and Pragmatic context for following sections.

We have been for a long time explicitly and frequently emphasizing in our classes, to our students and colleagues, in both Higher Education and Industrial contexts, that the very well known Medieval Trivium is not being adequately applied Higher Education[5], or not applied at all in some Higher Education organizations. We noticed this educational gap while teaching Information Systems (to students in Computer Engineering) and practicing in the area of Information Systems Engineering, for about 35 years simultaneously in both cases. We have discussed at length (including conferences presentations and publications[6]) during these 35 years that *Computing and Software Engineering are __necessary__ conditions for the development of computing-based information systems, tailored to the specific needs and requirements of a specific organization or sub-organization. But, they certainly are not __sufficient__ conditions for the professional effectiveness in developing this kind of information systems*. Computer or software engineers need to adequately communicate with machines, but they also need to have the skills for effective communication with human beings (the users) for adequately eliciting the respective requirements, designing an adequate system, training the users for an effective use of the system, and maintaining the system especially when new requirements emerge as a consequence of the dynamics, uncertainties, and changes in which the organizations are always immersed in. This means that the system analyst/synthesist needs to communicate with both computers via *artificial languages* and the users via *natural languages*. He/She also need to make adequate translation between both languages. Otherwise, there will be a high probability of failure no matter how good he/she is as computer or software engineer or computer scientist. Skills in natural languages and effective communication are what the Medieval Trivium is about. This is why we included a detailed exploration regarding this issue in our detailed work regarding a "Systemic Systems Methodology" (N. Callaos, 1995) which might contain local *systematic* parts but it is a *systemic* one as a hole.

As a communicational process, academic informing effectiveness depends, at least, on the adequacy of the communicational means used; which, in turn, depends on the comprehensiveness of the possible/feasible means, as well as on the potential synergies and emergent properties that might be generated in their simultaneous design and implementation. In order the increase the probability of being comprehensive, it might be advisable to

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[4] See for, example, M. Alvesson and K. Sköldberg, 2001, 2009; and K. Etherington, 2004.
[5] See, for example, Callaos N. and Callaos B., 2014, pp. 21-25; and N. Callaos, 1995, pp. 527-534 for the case of Systems Engineering and Computing Engineering.

[6] In Callaos and Callaos, 2014, we integrated and resumed what we presented in many conferences, written in many publications, and emphasized in many academic and industrial courses.

explore the product of many years of reflection regarding the essence of human communication and the means suggested as necessary for its effectiveness. Our experience shows us that *the classical means are far from being obsoletes, though they require being adapted to the present objectives of academic informing as well as to the new communicational technologies, tools, and methodologies.*

Beside *comprehensiveness*, a systemic approach would require an adequate *contextualization* of what is being examined. Since Academic Informing is an essential part of academic activities, it should be examined from the perspective of its general context of academic activities which include academic thinking, academic behaving, academic caring, academic valuing, etc. besides academic informing. Consequently, we will be referring mostly to academic activities and in some specific situations to academic informing and to the relationships that exist, or should exist, between academic informing and other academic activities.

NINE AREAS THAT SHOULD BE ADDRESSED

With regard to a comprehensive study, we suggest that the traditional triad of Ethos (character, integrity, credibility), Pathos (emotion, feelings), and Logos (logic, language) are applicable and/or are being (implicitly or explicitly) applied and/or should be applied in academic activities each of the three main academic activities: research, education, and consulting or real life problem solving. Each one of these three academic activities requires:

A. Convincing by means of the *character, integrity, and credibility* of the academic as author, educator and/or consultant.

B. Persuading colleagues, students, and/or clients by also appealing to *emotions* of both the communicating academic and receiver of the message intended to be communicated.

C. Persuading colleagues, students, and/or clients by the use of *reasoning*, *logical arguments*, and an effective use of the communication languages (technical an natural) being used

This might be framed in the context of a 3x3 matrix, i.e. Ethos, Pathos, and Logos as related to each of the three basic academic activities, i.e. Research, Education, and Consulting or Real Life Problem Solving. With this framework we can relate/integrate the three academic activities and the three persuading means, among each other and between activities and means. Consequently, nine specific areas should be addressed. If we add to these areas the relationships among them and the second level of *Meta-Ethics*, *Meta-Pathos*, and *Meta-Logos*, then we can notice that there are many analytical areas that might be addressed in a comprehensive analysis. This is why we are thinking about a series of articles; which, as a set, might address the most important aspects of this issue.

On the other hand, if we accept that, 1) the three academic activities should be ***integrated*** for the potential generation of synergies and beneficial emergent properties, and 2) the classical triad of Ethos, Pathos, and Logos are related to each other[7] and integrated in human intellectual activities, then it is easy to imagine that all 9 kinds (matrix 3x3) of academic ends/means would be, or should be thought as, integrated in a synergic whole, which synergy would be greater than integrating academic activities according to just one of triadic elements of Ethos, Pathos, and Logos.

Having provided a brief description of the general context, the specific context, and a an initial analysis, which should precede to a necessary integration of the parts produced by the analysis, our purpose in what follows is to focus in one very important (we would say vital) aspect of Research Ethos (i.e. one of the nine fundamental issues presented above); while pointing to the relationships it has (or might have) with the other analytical ingredients mentioned above.

RESEARCH ETHOS

An important, probably a ***necessary condition*** in research activities is to adequately communicate the

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[7] In N. Callaos and B. Callaos, 2014 we have shown that the relationships among Ethos, Pathos, and Logos are actually, or potentially might be, of a cybernetic nature, including potential co-regulative loops (via reciprocal negative feedback and feedforward) and co-amplificatory loops (via reciprocal positive feedback)

results of these activities. Consequently, Ethos, Pathos, and Logos are required for this kind of communication. Even so, an increasing number of research communication are lacking of the respective Ethos, Pathos, or Logos. Many scientific or engineering communications lack even the three of them. Let us present some recent (and less recent) much known examples.

1. The International Weekly Journal of Science *Nature* reported on February 25th, 2014 that "*Publishers withdraw more than 120 gibberish papers.*" Richard Van Noorden[8] (2014) affirmed that "Conference proceedings removed from subscription databases after scientist reveals that they were computer-generated…The publishers Springer and IEEE are removing more than 120 papers from their subscription services after a French researcher discovered that the works were computer-generated nonsense… Ruth Francis, UK head of communications at Springer, says that the company has contacted editors, and is trying to contact authors, about the issues surrounding the articles that are coming down. The relevant conference proceedings were peer reviewed, she confirms — making it more mystifying that the papers were accepted."

Consequently, many questions arise:

- ***Did the Publishers have Scientific Misconduct or Unethical Behavior?*** No, they did not, in our opinion. Publishers like IEEE and Elsevier would not do it because it makes no sense at all. The amount of money involved is extremely negligible as compared with their annual revenue and they would never risk their prestigious image and high credibility level. This is just a pragmatic reasoning. There are many other reasons, especially related to their history and the great service they provided, for a long time, to be credible channels for scientific

communications via publications of papers.

- ***Did the respective Editor-in-Chief have Scientific Misconduct or Unethical Behavior?*** Not necessarily, in our opinion, because for similar reasons, it would make no sense.

- ***The conference Organizers?*** Not necessarily in our opinion, because reputable journals with high scientific prestige and reputable editors also had the same kind of ethical problems, and pragmatic concerns. We will present one example later.

- ***The authors?*** In this specific case, our opinion is an almost a certainty that authors have had unethical behavior and academic or scientific misconduct. But, authors has not always had this kind of misconduct because there have been several intentional hoaxes that have been submitted in order to announce them later. We will see some of these cases below.

- ***The reviewers of these papers? The Peer Reviewing Methodology Applied?*** Very probably in our opinion and according to our experience, this is the case. In a survey of members of the Scientific Research Society **"only 8% agreed that 'peer review work well as it is'."** (Chubin and Hackett, 1990, p. 192) Is the essence of the scientific publications quality assurance highly ineffective? Is the whole academic promotional system based on something that just the 8% think is working? Is it ethical to continue "measuring" the research performance of academic with a toll that just the 8% believe it is effective? ***Is this ethical?*** How many scholars are really concerned about this issue? *Is there any consensus about what the notion of "peer" means?* How many concerned scholars, conference organizers, editors, or publishers are trying to find a solution to this paradoxical problem?

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[8] Richard Van Noorden "has reported for *Nature* in London since 2009, after spending two years as a reporter at *Chemistry World*. He has a master's degree in natural sciences from the University of Cambridge." (*Nature,* doi:10.1038/nature.2014.14763)

While one of the authors of this article was Dean of Research and Development of a university, we had the experience of trying to identify, during two years, a consensual meaning, or definition, of an internal "peer" in the university, and it was not possible. The more we tried to generate a consensus regarding this issue, among the university's professors, the more controversial became what the term means or should mean. Paradoxically, in the same university, external "peer" generated an immediate consensus, i.e. the (***unknown***) peers of a "prestigious" journal, and the level of prestige of the journal was associated with its impact factor. *Isn't paradoxical that there was no way to define a "peer" associated to the professors of the university, but it was "evident" who are peers, as long as they were professors from other universities, who are **unknown** and selected by **unknown** editors.* Later, we found out, after a literature search, that the notions related to these terms have not been sufficiently addressed. We tried to find in the literature short description of the meaning of "peer" and "peer reviewing" in order to elicit from scholars some intellectual feedback, but the attempt was unsuccessful. Consequently, we proceeded to write very short descriptions of the notion of "peer" and "peer reviewing" (Callaos, 2005). Our intention, in keeping these descriptions short, was (and still is) to ask for small amount of time from the reader in order to increase the readership potential and, hence, the probability of generating comments as well as awareness regarding this issue

- ***Is anyone else, implicitly and/or unknowingly, having ethical issues, beside those mentioned above?*** Should some chairs of academic departments consider the Academic Ethos (and probably the Pathos and Logos) related to the fact that only 8% of the members of the Scientific Research Society agreed that 'peer review work well as it

is'? Should they try to identify a consensus among the professors of their departments regarding the meaning of "peer" and/or "peer review"? In such a case should they publish these meanings in order to clarify it to the faculty members of their department? Should they continue delegating the ingredients of their decisions regarding the promotions of their faculties in the hand of unknown reviewers selected by not necessarily well known editors?

2. On July 13, 2014, in an op-ed of the *Wall Street Journal*, Hank Campbell (2014), founder of Science 2:0 web site, in an article titled "***The Corruption of Peer Review Is Harming Scientific Credibility,***" informed that the reputable SAGE Publications retracted 60 articles implicated in a peer review ring at the *Journal of Vibration and Control*. This peer review ring involved assumed and fabricated identities which were used to manipulate the online SAGE submission and reviewing system. Previously The Guardian reported this news with the title "***Academic journal retracts articles over 'peer review ring' with bogus scholars***." (Jon Swaine, 2014) Steven T. Physics Today reported this fact, on July 11, 2014, with the title "***Peer-review fraud cited in retraction of 60 academic papers.***" Corneliussen (2014), *a media analyst for the American Institute of Physics,* referring on other publications, affirms that "the penalties for scientific fraud are generally insufficient, with too little repayment of misused funding, with too little professional ostracism of offenders, and with resignations forced—and criminal charges filed—too rarely." This means (in our opinion) that ***meta-ethical*** issues have to be considered besides the ethical ones; i.e. peer reviewing methodologies should have to include ways, methods (a systemic methodology?[9]) of enforcing ethical behavior in science, and the Scientific Enterprise should also include stronger and more explicit rules and policies with regards to scientific misconduct and unethical behavior; i.e. it should be more involved and concerned at the ***meta-ethical level***. In a recent comprehensive study DuBois, Anderson, and Chibnall (2013), with the aim of

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[9] See for example (Callaos and Callaos, 2014)

determining the frequency and kinds of wrongdoing at ***leading research institutions***[10] in the United States," concluded with the following terms:

"Wrongdoing in research is relatively common with nearly all research-intensive institutions confronting cases over the past 2 years. Only 13% of respondents indicated that a case involved termination, despite the fact that more than 50% of the cases reported by RIOs [research integrity officers] involved FFP [falsification, fabrication, or plagiarism]. This means that ***most investigators who engage in wrongdoing, even serious wrongdoing, continue to conduct research at their institutions***."[11]

This clearly shows that even leading research institutions are requiring addressing both the meta-ethical and ethical levels in research. Actually, in our opinion, the academic promotional policies are contributing in the generation of unethical activities in both research and education. An academic who is unethical in the publications of his/her research the more unethical might be in his/her activities in education. In this case there are at least two generating causes of academic misconduct: a) a promotional system oriented to research production that frequently undermines the educational activities of the academic, and b) educational misconduct is usually less visible than research publications.

Consequently, it seems evident that the Scientific Enterprise, and specially leading research institutions (especially leading research universities), should urgently and carefully review both the ethical and the meta-ethical issues related to research, education, and consulting. In our opinion, the *Academic Ethos should be examined not in isolation, but along with 1) its relations with the Academic Pathos, i.e. the kind of emotions which generation should be addressed and promoted in order to increase the probability of ethical behavior and 2) its meta-ethical rules, policies, enforcement, and behavior.* To have a promotional system

based essentially (and sometimes exclusively) in research production ***metrics*** (number of publications, citation index, journal impact, etc.) may be making more harm than good. Metrics are ***means***, and as such should never be confused with the ***ends*** or (which is worst) taken as ***ends in themselves***. *The later is one of the powerful sources of corruption, including both the conscious and the unconscious ones.* Should an academic department's chair reduce assessments of the research performance of the professors of his/her departments to an accounting exercise based on metrics produced by other institutions? Should departmental evaluation be reduced to the results of other organizations which decide the reviewing methodology which is usually, in turn, based on the evaluations and comments of <u>***unknown***</u> peer-reviewers chosen according the traditional double-blind reviewing methodologie? Why Ph.D. dissertations have explicitly <u>***known***</u> reviewers (the Ph.D. committee's members) who sign the respective thesis while just anonymous reviewers are who recommend to accept or to refuse the publication of a given article? *Shouldn't peer reviewing methodologies be based on non-anonymous reviewers or on both anonymous (double-blind) and non-anonymous reviewers?* In trying to answer this question we proposed, and have been working with (since 2006) a two-tier reviewing methodology which include both anonymous (double-blind) and non-anonymous reviewers. In our methodology, both reviewing processes should end up recommending the acceptance of a paper in order to generate and editorial decision regarding the acceptance of the article for its publication as a peer-reviewed article. Publication recommendation in each tier is a ***necessary*** condition but not a ***sufficient*** one for acceptance for presentation and/or publication. Recommendations from both tiers are required. We think that with this methodology we are making ***an initial step in addressing the meta-ethical level*** of the reviewing/publication processes. More details (but in a short description) can be found at (Callaos, 2006). More details in a larger article can be found at (Callaos, 2011)

3. On April 11, 2012 Carl Zimmer, in an article in the New Work Time, entitled "*A Sharp Rise in Retractions Prompts Calls for Reform*"

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[10] Italics and emphasis added.

[11] Italics and emphasis added.

addressed the issue of *the exponentially increasing number of retractions in scientific journals in the last 10 years*. Zimmer based his article on an unsettling discovery made by Dr. Fang, who is editor in chief of the journal Infection and Immunity regarding the increasing number of retractions. Simmer reports that Dr. Fang, who is a professor at the University of Washington School of Medicine, affirmed regarding the increasing number of retractions that "[n]o body had noticed the whole thing was rotten … a symptom of a dysfunctional scientific climate." Zimmer reports that Dr. Fang looked, with a fellow editor at the journal, Dr. Arturo Casadevall, "at the rate of retractions in 17 journals from 2001 to 2010 and compared it with the journals' 'impact factor,' a score based on how often their papers are cited by scientists. *The higher a journal's impact factor, the two editors found, the higher its retraction rate*."[12] Consequently, if we were to measure the *quality* of a journal by the number of retractions has had, the journal with high impact (which articles are the most cited) would have lesser quality than those journals with lower impact. Does that make any sense? Should the quality of a journal be measured just with its impact factor? Should the impact factor be defined just as the number of average citations per article? Should there be other accepted definitions or metrics of journals' quality or "impact factor"? *Isn't an ethical issue to answer, or at least to try to answer, this kind of questions?*

4. The most preoccupying aspect of the retraction rate is its explosive increase in the last 10 years. Richard Van Noorden (2011) reports, in an article published by Nature (International Weekly Journal of Science), that "In the past decade, the number of retraction notices has shot up 10-fold [1000%], even as the literature has expanded by only 44%." The exponential growth is shown in the figure included in the Van Noorden's (2011) article, as well as in figure 1a of Brembs et al.'s (2013) article entitled "*Deep impact: unintended consequences of journal rank*." Brembs et al. (2013) also shows (in figure 1D of their article) *the exponential relationships between the retraction index and the impact factor of the*

retracting journal: the more the impact factor, the exponentially more the retracting index. Consequently, among their conclusions, Brembs et al. (2013) conclude that

> "There are thus several converging lines of evidence which indicate that publications in high ranking journals are not only more likely to be fraudulent than articles in lower ranking journals, but also more likely to present discoveries which are less reliable (i.e., are inflated, or cannot subsequently be replicated). Some of the sociological mechanisms behind these correlations have been documented, such as pressure to publish (preferably positive results in high-ranking journals), leading to the potential for *decreased ethical standards.*[13] (Anderson et al., 2007)"

Shi V. Liu (2006) showed that "the percentage of retraction of the above four top journals among all retractions are on the rising trend, from 1.42% in the1980s to 6.96% in the 1990s and to 9.18% in the first 6 years of 2000s" Based on a search in PubMed on May 6, 2006, Liu (2006) listed 47 journals. The top of them according to their respective impact factors (Science, Nature, PNAS, and Cell) had 38, 32, 32, and 13 retractions respectively. All 47 journals had 309 retractions. This means that the 0.085% of the journals (the top four) had the 37.22% of all retractions. This is astonishing! 0.085% of the journals (the ones with the highest impact factors) are generating the 37.22% of the retractions.

Liu (2006) resumed his article, published in *Scientific Ethics* 1(2), pp. 91-93, in the abstract, as follows:

> "Top journals often use the highly exaggerated and even flawed values of the impact factors to boost their circulations among readers and increase their attractions to authors. This commercial strategy apparently worked very well because many scientific administrators have now used the place (journals) of publication as a criterion for evaluating the value of the publication. However, from a historical and objective

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[12] Italics and emphasis added.

[13] Italics and emphasis added.

perspective, top journals' high-profile publications often stand low in comparing with those truly groundbreaking and thus not "trendy" papers in the then "cold" or even ignored fields. More ironically, many such truly great papers were initially rejected by the top journals. In contrast, many "hot" and "trendy" papers published by top journals actually ended up with "spectacular" retractions. Thus, while top journals emphasize their impact factors they should realize that their impacts are double-sided. They should also confess to the world that they are also the world leaders in publishing retractions." (Liu, 2006, p. 91)

Peter A. Lawrence (2008) resumes his paper entitled "*Lost in publication: how measurement harms science*"

"Measurement of scientific productivity is difficult. The measures used (impact factor of the journal, citations to the paper being measured) are *crude*. But these measures are now so universally adopted that they determine most things that matter: tenure or unemployment, a postdoctoral grant or none, success or failure. As a result, *scientists have been forced to downgrade their primary aim from making discoveries to publishing as many papers as possible*—and trying to work them into high impact factor journals. Consequently, scientific behaviour has become distorted and the utility, quality and objectivity of articles have deteriorated. Changes to the way scientists are assessed are urgently needed, and I suggest some here."[14] (Lawrence, 2008, Abstract, p. 9)

The two abstract mentioned above are just examples of an increasing number of articles in which researchers, scholars, and editors are increasingly questioning the validity of the metrics being used, as unique indicators of the quality of academic articles. *Is it ethical to continue using metrics that increase the probability of unethical behavior in scientific research? Is it ethical to use metrics that are distorting scientific behavior?* **Is it ethical to force scientists "*to downgrade their primary**

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aim from making discoveries to publishing as many papers as possible"? Doesn't this distortion represent intellectual and/or academic corruption? Shouldn't we (at least try to) identify other ways of evaluating the quality of scientific publications? Isn't that an ethical, or meta-ethical, requirement? An increasing number of scientists, editors, academic administrators, and science managers (e.g. Brembs et al., 2013; Anderson et al., 2007) are at least trying to find ways of assessing scientific quality where established means and metrics are not being taken as end in itself. More research is required in this area if we are going to at least try to address both the ethical and the meta-ethical levels of scientific or scholarly research.

5. In June 15, 2009 the academics and scientists were disconcerted when they learned about a reputable journal accepting (after reviewing) and publishing an article which content was randomly generated. Nature published the news with the title "***Editor will quit over hoax paper: Computer-generated manuscript accepted for publication in open-access journal***." In this article, Natasha Gilbert (2009) reports that "[t]he fake, computer-generated manuscript was submitted to The Open Information Science Journal [Bentham Science Publishing] by Philip Davis, a graduate student in communication sciences at Cornell University in Ithaca, New York, and Kent Anderson, executive director of international business and product development at The New England Journal of Medicine. They produced the paper using software that generates grammatically correct but nonsensical text, and submitted the manuscript under pseudonyms." Bambang Parmanto, who is an information scientist at the University of Pittsburgh, Pennsylvania, and was the editor-in-chief of The Open Information Science Journal, declared to Nature (according to Gilbert, 2009) "I think this is a breach of policy … I will definitely resign. Normally I see everything that comes through. I don't know why I did not see this. I at least need to see the reviewer's comments." Parmanto claims that the Bentham published the article without his knowledge, and the director of publications at Bentham Science Publishing defended Bentham's peer review process, saying (according to Gilbert, 2009), "a rigorous peer review process takes place for all

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[14] Italics and emphasis added.

articles that are submitted to us for publication. Our standard policy is that at least two positive comments are required from the referees before an article is accepted for publication." In this particular case, "the paper was reviewed by more than one person". In our opinion, this is another example of traditional peer reviewing failure. What is astonishing is that for several decades many editors, authors, and studies concluded that the traditional double-blind peer review's failures are overwhelming[15], but not much has been done 1) to substitute it by other methods for quality assessment of scientific research articles, or at least 2) to improve it via complementing it with other reviewing methods. This is a really perplexing issue. Traditional peer review is abysmally failing and the Scientific Enterprise is still based on it. Traditional peer review *is astonishingly ineffective* and (as we said above) *only 8% of Scientific Research Society's agreed that 'peer review work well as it is',* (Chubin and Hackett, 1990, p. 192). ***It is ineffective and it is perceived as ineffective by scientists but it is still untouched and untouchable by the academic world. Is it an Academic Totem?***

*Is it ethical for academics, scientists, researchers, engineers, professionals, academic administrators, etc. to continue ignoring this perplexing issue? Is it ethical to force the new generations of scientists and academics to accept that their career depends on a clearly failed quality assessment tools for valuing the merit of their research? Is it ethical not to, at least, ask these questions? Is it ethical not to, at least, **try** to solve this paradoxical situation or to ameliorate its effect while a solution is identified?*

6. Even reputable journals with high prestige and high impact factor that charge readers for their content (via subscriptions) may be prone to accepting nonsense and gibberish papers which are randomly computer-generated. Peter

Aldhous (2007), for example, reported in *New Scientist* (owned by the publishing giant Reed-Elsevier) that graduate students at Sharif University in Iran got a randomly computer-generated paper accepted by "*Applied Mathematics and Computation*," which is a journal with a very high reputation published by Elsevier (part of Reed-Elsevier, the publishing giant that owns *New Scientist* in which this news was also reported). Aldhous (2007) reports that "[a]fter the spoof was revealed, the pre-publication version of the paper was removed from Elsevier's *Science Direct* website." The proof-correcting queries sent to the hoaxers by Elsevier can be found at http://pdos.csail.mit.edu/scigen/sharif_query.pdf . The removal of the paper after being published is at www.sciencedirect.com/science/article/pii/S009 6300307003359. Aldhous (2007), also reports that "Melvin Scott, a retired mathematician based in Ocean Isle Beach, North Carolina, who serves as editor-in-chief of *Applied Mathematics and Computation*, says that the paper was accepted by an editor who has since left the journal. "I've revamped the editorial board significantly," he adds."

It is evident, in our opinion, that the publisher did not have an unethical behavior. It is also highly probable that the editor-in-chief did not have unethical behavior either. Very probably it was the editor and/or the reviewers of the paper who behaved unethically. *It is also very probably that the reviewing methodology failed in its scientific quality assessment, especially because it very probably did not include the **meta-ethical dimension**, i.e. 1) a procedure or a method for the identification unethical behavior from the authors, the reviewers and/or the editors, or 2) a methodological ingredient for **enforcing ethical behavior**, or for minimizing the probability of scientific misconduct.*

7. Another example, which shows other aspects of the problem at hand, the acceptance of an article we did for WMSCI 2005. This article was accepted for *presentation* as a ***non-reviewed***[16]

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[15] We reported on many of these conclusions made by editors, authors, and specific studies regarding the ineffectiveness of traditional peer review in Callaos, 2011, *Peer Reviewing: Weaknesses and Proposed Solutions* at
https://www.academia.edu/4437207/Peer_Reviewing_W eaknesses_and_Proposed_Solutions

[16] A copy of the acceptance letter sent to the corresponding author is shown as Appendix B of the document at
http://iiis.org/contents/With_Regards_to_the_bogus_pape

one and its acceptance was based on the CVs of the authors. The acceptance letter clearly said so, and the authors were informed that the paper might be accepted later as a ***reviewed*** one as soon as its reviewing process is finished. The conference's web site said clearly that about 15% of the submitted articles might be accepted as non-reviewed. The related article happened to be a randomly computer-generated one. This news was published in many outlets without informing about the whole truth, i.e. the article was accepted as a non-reviewed one and the conference web site informed up-front that about 15% of the articles will be accepted as non-reviewed. Is it ethical to present part of the truth and to take it completely out of its context? Many well intentioned academics repeated what they read in the web without any confirmation of what they read and what they are saying. Is this academically ethical? The WMSCI 2005 web page (saved in Web.Archive at http://web.archive.org/web/20070209005022/http://www.iiisci.org/sci2005/website/Papers_acceptance.asp) informed very clearly the following:

> Acceptance decisions related to the submitted papers will be based on their respective content review and/or on the respective author's CV. Invited papers will not be reviewed and their acceptance decision will be based on the topic and the respective author's CV.

> If the reviewers selected for reviewing a given paper do not make their respective reviews before the papers acceptance deadline, the selection committee may accept the paper as a non-reviewed paper.

> If a paper does not meet the criteria for inclusion as reviewed paper, the selection committee may invite the author to present it as a non-reviewed paper.

> Each accepted paper (reviewed and non-reviewed) is candidate for being a best paper of its respective session and, consequently, it is candidate for a second reviewing process to be made by the reviewers of the Journal of Systemics,

Cybernetics and Informatics (JSCI), by means of which the best 10%-20% of the papers presented at the conference will be selected and published in the JSCI after doing possible modifications (in content/format) and extensions as to adequate them to a journal publication.

Many academics rushed to judgment before reading this text and continued with a false narrative based on part of the truth and taken completely out of its context. Is that academically ethical? Journalistically it is not ethical and journalists stopped the story after interviewed us and after reading the text above. Shouldn't academics follow journalists ethics when making citizenship journalism via blogs, email lists, etc.?

According to WMSCI 2005 ***published acceptance policy***, the article was accepted for presentation as a ***non-reviewed*** one and because of the previous publications of its authors (the MIT's Ph.D. students). The reasons supporting this acceptance policy have been explained with details elsewhere (Callaos, 2014; pp. 7-10). These reasons are valid in some disciplines and not valid in other disciplines. There are reputable conferences with no peer-review at all. Examples can be found in the meetings of the American Mathematical Society: AMS, The Southeastern International Conference on Combinatorics, Graph Theory, and Computing, etc.
(http://blog.computationalcomplexity.org/2007/11/unrefereed-does-not-equal-bogus.html).
Another example is found in the prestigious, large, and very known INFORMS/IFORS conferences, of the Institute for Operations Research and the Management Sciences (INFORMS) and the International Federation of Operations Research Societies (IFORS), which we attended several times. They announce clearly and explicitly that "***Contributed abstracts are not reviewed and virtually all abstracts are accepted***."[17]

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rs_submitted_to_WMSCI2005_%28Ed.%29_31-5-2014.pdf

[17] see for example
http://meetings2.informs.org/sanfrancisco2014/abstract_contributed_i.html

Different disciplines have different conceptions regarding this issue. Then, _**what should a multidisciplinary conference do with this regard?**_ Being WMSCI a multi-disciplinary conference we tried to apply a multi-modal acceptance policy in which the presentation of reviewed papers are combined the presentation of a small number of non-reviewed ones, but all those that would be published in the journal are or will be reviewed, some of them twice or three times.

ETHICAL ISSUES REGARDING WMSCI 2005 CASE

How many academics read this text above which was explicitly and clearly posted in WMSCI 2005 web site and respective call for papers? How many did so before rushing to judgment? Is it acceptable to judge a conference in a given discipline according the standards of other discipline? Is it ethical to smear a whole conference repeating half truths completely taken out of their context? Should this kind of academics provide education to our kids? What is the difference between this kinds of academics and the scientists who select what data from his/her observations to present and what not to present (or to hide) in order to confirm his/her hypothesis or pre-judgments? Should scientific ethics be followed just in the context of scientific activities while choosing not to follow it when judging activities of other academics? Isn't perplexing that reputable academics, with the very good intention of protecting Science from misbehavior, misbehave when judging other academic activities? Do these scholars have consciousness or awareness about the unethical behavior they are having while their intention is to do the right things of protecting Science from those who abuse it?

As we asked above, should scientists in a given discipline impose their disciplinary standards on academics from other disciplines? If the answer is yes, which discipline should impose its standards on other disciplines? Who are those who are going to make this kind of decisions? Should self-appointed gatekeepers of what they call "good science" impose their criteria by means of smearing who does not agree with them? Is that scientific? Is that ethical? _**Should intellectual intolerance be tolerated in the academic world? Shouldn't**_ _**different academic perspectives be allowed and intellectually honest disagreement be allowed and even promoted and encouraged, especially in the universities and in research centers? Should the intellectual intolerance be considered as unethical behavior en Academia? Is it ethical not to, at least, try to stop or ameliorate any intellectual bullying in the academic world? How many academics are aware about the intellectual intolerance, bigotry, and bullying that is happening (according to an increasing number of academics) in the academic world?**_

THE EVENT OF WMSCI 2005 AS A CASE STUDY

The above mentioned example was input to a "Case Study" that generated about 150 written and published pages. Thank to this case study a new Peer Reviewing Methodology emerged that took into account not just the ethical dimension but also the _**meta-ethical one**_. _This case study was presented at a Workshop sponsored by the USA's National Science Foundation which included Faculty and Ph.D. Students in Business Administration of the University of South Florida._ A short article has been written regarding this case study; which we are including as an appendix of this article. It is a short article with pointers to larger articles with more details regarding the Action-Research project which supported (and still supports) the finding of potential solutions (or improvement of the implemented ones) for this ethical and meta-ethical problem.

It is important, for our purposes in this article, to note that computing writer Stan Kelly-Bootle[18] (2005) commented in _ACM Queue_ that many sentences in the "Rooter" paper [accepted for

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[18] STAN KELLY-BOOTLE "born in Liverpool, England, read pure mathematics at Cambridge in the 1950s before tackling the impurities of computer science on the pioneering EDSAC I. His many books include _The Devil's DP Dictionary_ (McGraw- Hill, 1981) and _Understanding Unix_ (Sybex, 1994). _Software Development Magazine_ has named him as the first recipient of the new annual Stan Kelly-Bootle ElecTech Award for his "lifetime achievements in technology and letters." Neither Nobel nor Turing achieved such prized eponymous recognition. Under his nom de-folk, Stan Kelly, he has enjoyed a parallel career as a singer and songwriter." Copied from
http://queue.acm.org/detail.cfm?id=1080884

presentation at WMSCI 2005, not necessarily for publication] were individually plausible. He thinks that this fact poses a problem for automated detection of this kind of articles and suggested that even human readers might be fooled by the effective use of jargon. He concluded as follows "I suppose the conclusion is that a reliable gibberish filter requires a careful holistic review by ***several peer domain experts***. Each word and each sentence may well prove individually impeccable, although nonsense ***in toto,*** which ***probably rules out for many years to come*** a computerized filter for both human and computer-generated hoaxes." This is an important conclusion for the purpose of this article, because it shows that ***peer reviewing methodologies should include a meta-ethical ingredient.*** Consequently, we thought that a combination of Action-Research, Action-Learning, and Action-Design would probably be an effective approach to incrementally design a peer-reviewing methodology that would include meta-ethical methods of procedures. As a result we think we designed a methodology which is more effective than the known ones. It is perplexing that with all previous failures in peer reviewing we found no explicit attempt in *designing, implementing, and testing* a more effective methodology. We did find many suggestions about how peer-reviewing might be improved. We actually included some of these suggestions in our methodological design, but we did not find any reference to the *implementation and testing* of a more effective peer reviewing methodology.

The events described above that happened after Stan Kelly-Bootle published the above mentioned article show clearly that he was right. Methodologies of quality assurance in Science proved not to be effective even in the approval process of doctorate dissertations. The Bogdanov Affair is an example regarding this issue. In Callaos (2011) we resumed this affair that included an incoherent Ph.D. dissertation as follow:

"Five meaningless papers had been published by four leading journals in physics, and served as basis for the approval of the two Ph.D. Dissertations of the Bogdanov brothers. … John Baez, a physicist and quantum gravity theorist at the University of California at Riverside, moderated a physics discussion group entitled "Physics bitten by reverse Alan

Sokal hoax" brought widespread attention to the Bogdanoff affair. Baez (2004) asserts that "Bogdanovs' theses are gibberish to me - even though I work on topological quantum field theory, and know the meaning of almost all the buzzwords they use. Their journal articles make the problem even clearer…some parts almost seem to make sense, but the more carefully I read them, the less sense they make... and eventually I either start laughing or get a headache… all they write about them is a mishmash of superficially plausible sentences containing the right buzzwords in approximately the right order. There is no logic or cohesion in what they write… Hermann Nicolai, editor of *Classical and Quantum Gravity*, told *Die Zeit* that if the Bogdanovs' paper had reached his desk, he would have immediately sent it back: 'The article is a potpourri of the buzzwords of modern physics that is completely incoherent'." (Baez, 2004). The editors of the journals where the articles were published reacted in different ways. "The editors of Classical and Quantum Gravity repudiated their publication of a Bogdanov paper, saying it 'does not meet the standards expected of articles in this journal'… Dr. Wilczek stressed that the publication of a paper by the Bogdanovs in Annals of Physics had occurred before his tenure and that he had been raising standards. Describing it as a deeply theoretical work, he said that while it was 'not a stellar addition to the physics literature,' it was not at first glance clearly nonsensical**.** 'It's a difficult subject,' he said. 'The paper has a lot of the right buzz words. Referees rely on the good will of the authors.' The paper is essentially impossible to read". (Overbye, 2002). Dean Butler wrote in *Nature* that **"the credibility of the peer-review system and journals in string theory and related areas is taking a battering."** George Johnson wrote an article about the Bogdanov affair in the *New York Times*, concluding that: "As the reverberations from the affair begin to die down, physicists seem to have accepted that the papers are probably just the result of fuzzy thinking, bad writing and **journal referees more comfortable with correcting typos than**

challenging thoughts". In the same article Johnson added that "Dr. Sokal seemed almost disappointed." affirming that "If someone wanted to test a physics journal with an intentional hoax, I'd say, `more power to them'…What's sauce for the goose is sauce for the gander." (Johnson, 2002; emphasis added)."

Baez (2010) affirms that "Jackiw, a professor of physics at MIT, was one of two `rapporteurs' who approved Igor Bogdanoff's thesis. Overbye [2002] writes: Igor's thesis had many things Dr. Jackiw didn't understand, but he found it intriguing. "All these were ideas that could possibly make sense," he said. "It showed some originality and some familiarity with the jargon. That's all I ask."

Ignatios Antoniadis (of the École Polytechnique), who approved Grichka Bogdanov's thesis, reversed his review later. He told *Le Monde,* "I had given a favorable opinion for Grichka's defense, based on a rapid and indulgent reading of the thesis text. Alas, I was completely mistaken. The scientific language was just an appearance behind which hid incompetence and ignorance of even basic physics."[19] Other readers of the thesis claimed that they did not understand everything in it and they supposed that other readers do understand what they do not understand.

It is really perplexing that after the Bogdanov affair no one seemed to care about improving the quality assurance of Ph.D. dissertations and/or of peer reviewing in scientific journal, not even in Physics. Isn't that astonishingly perplexing? Why no one cared about taking the Bogdanov affair as a case study in order to improve the effectiveness of Ph.D. dissertations quality assurance and/or the effectiveness of Peer Reviewing? Is this kind of negligence ethical? Is it ethical just to denounce the Bogdanov Affair and announce the intention of making changes as to avoid similar situations? Is it ethical to just blame to the previous department chair and do nothing else regarding this kind of affair? We are not sure about the answers to these questions and this is why are making them? *Our intention in making these questions is not a rhetorical one. This is why we think that each case like the examples shown above should be taken as*

a case study oriented to continuously improve the effectiveness of peer reviewing methodologies.

SOME CONCLUSIONS

The following are among the conclusions we can make with regards to the content of this paper, which are also based on 1) the experience/knowledge we acquired through the Case Study of the WMSCI 2005 event, 2) the experience/knowledge we gathered through an incremental design and implementation of the peer reviewing methodology mentioned above, and 3) the information we gathered regarding similar events, e.g. the examples mentioned above.

1. One of the most important conclusions is that the most frequent source of the peer reviewing methodologies being used is for cases where scientific *misconduct of authors coincide negligence or misconduct of reviewers* of the respective article. Consequently, *a peer reviewing methodology should have a meta-ethical ingredient* related to both potential sources of misconduct: the authors and the respective reviewers. On the other side, academic departments and deanships as well as universities administrators and authorities should explicitly address the Academic Ethics and Meta-Ethics via caring and enforcing the expected ethical behavior in academic issues. It is our opinion that ethics enforcement should be less soft and more rigorous.

2. *Double-blind reviewing* facilitates and sometimes it might even catalyze the coincidence of author's misbehavior and reviewer's negligence or misbehavior. In double blind reviewing the authors names are not supposed to be published as related to the respective author. So, how would it be possible to include a meta-ethical ingredient with regards to reviewers' possible negligence or misbehavior in the context of this anonymity situation? This is why we added to the traditional double-blind reviewing a second reviewing tier with non-anonymous reviewers. In this sense, David Kaplan was our inspiration through his article "*how to Fix Peer Reviewing*" (Kaplan, 2005)

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[19] Hervé Morin, 2002.

3. As we suggested above, we are convinced that the effectiveness of the Scientific Enterprise might be improved if grant Organizations and the academic promotional procedures relies less on structures based on the traditional peer reviewing methodologies.

4. If academic promotions are going to continue being based on journal publications and journal quality is going to be measured by its impact factor, the respective measure should not be limited to the relative quantity of citations of the respective journal. There are increasing efforts in addressing this issue.

5. Academic departments should make their own definitions of what is a peer and what peer reviewing methodologies will be acceptable for the discipline of the department.

6. Standards of some disciplines should not be imposed on other disciplines, because this might corrupt the nature of the discipline on which the other standards are being imposed.

7. More intellectual efforts should be done in creating awareness with regards to differentiating and not confusing the *ends* with the *means*, and not taking the means as ends in themselves; which certainly is ineffective with regards to the real ends and it might corrupt the nature of the means. Publication is a means, impact factors is a measure (among many other possible ones) of one of the properties of a mean; it is not and should not be an end in itself.

8. *There is an increasing necessity and urgency in addressing both the ethical and the meta-ethical dimensions of any research activities, not just as a moral issue but also as a **pragmatic** one*.

9. To use systemic (not necessarily systematic) peer reviewing methodologies which are adaptable (to different disciplines, for example) and might perfect themselves in the context of an evolutionary process based on an adequate *integration of Action-Research, Action-Learning, and Action-Design*, in the context of a meta-methodological incremental planning and evolutionary methodological re-design and meta-design.

10. This conclusion is based on our interpretation (or informed opinion, or judgment) regarding some ways which were taken by some academics (and graduate students) to deal with the problems that emerged from academics who misbehaved, or from the intrinsic failures and weaknesses of the traditional peer revising methodology which mostly is being used. In our opinion more attention should be paid to Intellectual Intolerance and to the increasing academic cyber-bullying and cyber-inquisition being practiced by some academic vigilantes who are self-nominated prosecutors, juries, and judges on the name of what they consider "Good Science". Some of these people are well intentioned scholars but they are not aware that they are forming part of lynching mobs and that they are being mislead by people with vested interests or promoting autocratic (and consequently anti-academic) Intellectual Inquisitors. We understand that this is the result of speech freedom and academic autonomy. We also understand that tenured professors should be able to speak their mind; which is very important in honest scientific disagreement and academic freedom. But, is it right to use this freedom to smear prestigious organization like IEEE, ACM, ASME, SIAM, Springer Verlag, etc.?[20] Is this ethical? What academic criteria are being followed when smearing all conferences of these organizations that have been providing adequate support for academic and professional activities for so long time? Should the deficiencies of peer reviewing be used to smear and defame so many academic and professional organizations? Is that ethical? For how long we should have an intentional blindness not identifying the inherent of peer reviewing and continue blaming its failures on the organization using them? Isn't an ethical obligation to identify the right problem and to try to fix it? We are not talking here just about anonymous bloggers, but also about academics and librarians that have earned the respect of some of their colleagues. Did these scholars and librarians thought about the harm they are doing to the same scientific processes and academic activities they are supposed to be protecting, with very good intentions in some of them. Did

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[20] See for example http://fakeconferences.blogspot.com/. 20,100 results are showed when entering "IEEE bogus conference" in Google. 5,890 results when entering "ieee fake conference".

they think about the ethical issues of their behaving? Are they unintentionally misbehaving? Did they think about the new kind of inquisition in which they are being acting, simultaneously, as prosecutors, judges, juries, and executioners by means of web pages that they create, in which they lump together many organizations and refer to them as predators? In the hypothetical case that all what they are listing are predatory journals or organizations[21], aren't they meta-predators, masked with vigilantes of scientific and academic activities? Are they solving the real problem? Are they contributing to the solution of the right problem? Can we blame journals editors and conference organizers for the misbehavior of reviewers and/or authors? Can we blame them for the constant failures of the traditional peer reviewing methodology? Can we blame the driver for consequences of an accident because he/she was required to drive a malfunctioning car? Who is to blame? The driver? The car manufacturer? The boss who required the driver to drive this car? What would be the ethical and practical answer? Is an ostrich strategy an ethical and practical one? Should we address the real problem which is a very complex one instead of doing simple tasks that, far from solving the problem, might create more problems and potentially hurt innocent people by smearing their character, integrity, and honesty? Is this ethical? Is this fair? Is this practical? Is this congruent with the main purpose of Academy which is to always seek the truth?

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[21] See for example at **http://scholarlyoa.com/publishers** a list of what have been named as "Potential, possible, or probable predatory scholarly open-access publishers." This list is being taken into account in processes of academics and librarians promotions. We are not sure if some of listed publishers were contacted before or after listing them. But, we are certain that some editors of journals and organizations included in this list were not asked about their peer reviewing processes. Wikipedia consensually and collectively identified ten criteria to identify predatory publishers are in complete disagreement with Beall's list criteria. Which one should be used? Some of the publishers listed in Beall's list are 100% not "predatory publishers" because none of these criteria apply to them. Is it not an ethical obligation to identify a consensually and collectively standardized set of criteria before listing publishers as predators? Is it academically acceptable to take the criteria dictated by one or a group of persons as the *de facto* standard for the identification of predatory publishers? Is it academically ethical not to seek the truth and to impose the criteria of one or few persons on the labeling of journals as predators? Is it adequate to use this kind of individual lists in decisions oriented to academic promotions? Furthermore, the criteria followed to define this list automatically exclude any academic innovation and/or entrepreneurship. We were informed about the good intention of the librarian who produced this list, and we do not have any doubt about it. But, is this really the way to deal with unethical behavior of some publishers? Is it ethical to smear so many journals and organizations just because they do not follow the criteria of a well intentioned librarian? How many academics were hurt in their careers just because they published in some journal listed in the list? Should departments' chairs and deans use this list in their decisions regarding the promotion of academics? Is that fair? Is that ethical? These are not rhetorical questions, but questions that have been made with the purpose to trigger reflections on this kind of issues.

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Improving Peer-Reviewing:
A Case Study Triggered by the Acceptance of a Bogus Paper

Nagib CALLAOS

International Institute of Informatics and Systemics (IIIS, www.iiis.org)

PURPOSE

The objectives of this very short paper is 1) to briefly describe the sequence of the search/research activities that were triggered by the acceptance of a fake paper submitted to WMSCI 2005 and 2) to present the different reports that were generated by means of a) literature search regarding this kind of problem, b) the published potential solutions, and c) the implemented solution, which was identified by a methodological research fundamentally based on *action-research*, *action-design*, and *action learning*. At least 3000 hours (of senior academics, conference organizers, and journal editors) have been invested in this case study.

In this short paper, we will make a very short description with links to other detailed and larger papers which are being generated as a consequence of this case study and the tentative solutions that has been implemented, which in turn might provide input for more case studies regarding this important issue of improving peer reviewing processes.

MAIN EVENTS

The respective main events and search/research activities have been, up to the present, the following:

1. Randomly generated papers were submitted to WMSCI 2005. Some of them were identified as such by their respective reviewers and were rejected. No reviews were received for one of them and then according to the published policy of the Organizing Committee, the paper was accepted as a ***non-reviewed*** one, because of the **CVs** of its respective authors (three MIT's PhD students). They were told that the paper will be included in the proceedings (with an explicit note) as a *non-reviewed paper*, but if the Organizing Committee received reviews recommending the acceptance of the paper then its status would change to a peer-reviewed one. A more detailed description, where facts were separated from reasoned opinions and judgments, can be found at www.iiis.org/wmsci2005-facts-and-reasoned-judgements (15 pages)

2. All hell broke loose after the email acceptance was sent. Reuter distributed the news as "a computer generated paper was accepted for presentation at a computer science conference." BBC, CNN, Boston Globe, etc. published the news. Half truths and blatant smearing and lies, as well as personal attacks invaded the blogosphere related to Computer Science.

3. Our huge surprise was that, even after the above mentioned events, we received reviews recommending the acceptance of the gibberish paper. This event couldn't be more astonishing and disconcerting to us. ***Was something wrong (unethical) with some of our reviewers? Was something wrong with our reviewing methodology? How could we have a more effective reviewing methodology?***

4. Point 3 triggered a search process for more information and the more information we gathered the more certain we were that we needed a reviewing methodology different to the traditional and most used one. Parallel to the literature search (not research), we organized conversational sessions and focus groups in the context of the 2006, 2007, and 2008 conferences. Interested attendees of these events were asked the questions that our search was producing. Results of these conversational sessions were included as appendixes of the document posted at http://www.iiis.org/nagib-callaos/peer-review/ (pages 76-107).

5. Results of the processes described in point 4 triggered action-research processes which produced action-design and action-learning processes, in the context of an incrementally-evolutionary methodology to identify the ways of improving traditional double-blind peer reviewing methods.

CONCLUSIONS OF THE SEARCH/RESEARCH

1. The most essential conclusions were as follows

   a. A *high level of agreement among reputable journals' editors* regarding the low effectiveness, weaknesses, and high frequency of failure in peer-review methods. Combining these opinions, perceptions, and facts with the huge amount of time spent (invested?) in peer reviewing, it is easy to conclude that we are facing an important problem that require some solutions. It is estimated that 15.000.000 of yearly hours of work are used in peer reviewing processes (more than what the USA invested in the whole Genome Project); about one billion dollars each year while (according to a survey of members of the Scientific Research Society) "only 8% agreed that 'peer review work well as it is'." So, is peer-reviewing cost-effective? Details regarding the high level of agreement regarding the low level of effectiveness of peer review can be found in pages 1-20 of the report posted at http://www.iiis.org/nagib-callaos/peer-review/

   b. No agreement regarding a standard peer-reviewing methodology.

   c. Lack of agreement regarding the meaning of "Peer" and "Peer-Review." More details at http://www.iiis.org/nagib-callaos/meaning-of-peer-review and at http://peerreviewing.wordpress.com/2012/05/19/meanings-of-peer-and-peer-review/

   d. Lack of agreement about what a conference is and what are, or should be, conferences' objectives. In one extreme, some conferences have peer reviewing standards similar to journals in the respective discipline. In the other extreme, there are reputable conferences with no peer-review at all. Examples are the meetings of the American Mathematical Society: AMS, The Southeastern International Conference on Combinatorics, Graph Theory, and Computing, etc. (http://blog.computationalcomplexity.org/2007/11/unrefereed-does-not-equal-bogus.html). Different disciplines have different conceptions regarding this issue. Then, ***what should a multidisciplinary conference do with this regard?***

   e. Lack of explicitly written information regarding what a conference's proceedings is and what it should contain.

   f. Disagreement among different disciplines with regards to their conceptions of what "conferences" are for and what is, or should be, the functions of their respective proceedings. Consequently, ***what should a multidisciplinary conference do regarding this issue?***

   g. A more adequate reviewing methodology was needed, especially for multi-disciplinary conferences organized for inter-disciplinary communication.

POTENTIAL SOLUTIONS

With the above mentioned results of our search, we tried to design and implement a *Reviewing Methodology for a multi-disciplinary conference* and to explicitly publish what we understand by each of the concepts, objectives, functions, and notions where no explicit standards or implicit agreement exist. The meta-methodological process we have been (and we are still following) following is based on a combination of *action-research, action-design, and action-learning in the context of an evolutionary, incremental, and cybernetic process.*

Up to the present we obtained the following results:

1. We identified the objectives of peer-reviewing: pages 20-35 of the report posted at http://www.iiis.org/nagib-callaos/peer-review/

2. We identified the meaning of Peer-Review, or what we understand by it, and published in the IIIS's conferences web sites and at www.academia.edu/4437203/Meaning_of_Peer_Review

3. We proposed possible solution in pages 35-39 of the document mentioned in point 1. This solution has already been implemented with a reasonable level of effectiveness and success.

4. We proposed A Systemic Model of Scholarly and Professional Publishing and the architecture of its respective supporting information system in pages 39-61 of the document mentioned in point 1. (also at https://www.academia.edu/4437267/Systemic-Cybernetic_model_for_reviewing_and_publishing).
We implemented about the 80% of what has been proposed but because of financial lack of support the proposed system has not yet been completely developed.

5. We proposed and we are working with a three-tier reviewing methodology:

   a. Traditional double-blind with a minimum of 3 reviewers and with an average of about 4 actual reviews as reported in the forewords of the respective proceedings.
   b. Non-anonymous, non-blind with a maximum of three reviewers.

c. Peer-to-peer reviewing (the reasoning supporting this kind of review is presented in pages 61-67 of the above mentioned document.

More details regarding this methodology can be found in "A Multi-Methodological Reviewing Process for Multi-Disciplinary Conferences" that is being posted at all conferences sites, e.g. http://www.iiis2014.org/wmsci/Website/MMRPfMDC.asp?vc=1 A short description of a basic two-tier methodology has been posted at http://iiis.org/peer-reviewing.asp

6. We posted in all conferences sites what are, for us, the objectives of conferences and the functions of the respective proceedings. What we posted was the results of many conversational sessions and focus groups with attendees of our conferences. http://www.iiis2014.org/wmsci/Website/FunctionsofConferencesProceedings.asp?vc=1

7. We have been successfully using a newly designed two-tier methodology for Peer Reviewing in which we combine *traditional double-blind* peer reviewing as a **_necessary_** condition, but not as a **_sufficient_** one. A *non-blind* peer reviewing is also required in the methodology we are using since 2006. A short description of this methodology can be found at page http://www.iiisci.org/Journal/SCI/Methodology.pdf

We posted in the web as many documents as we could in order to continue with the collective efforts of the IIIS's members and its conferences' attendees in contributing for a continuing improvement of the effectiveness in peer reviewing and in adapting the objectives of the conferences and the functions of its respective proceedings to the users of our conferences, who are their actual attendees. Continuing with this process is the essence of the meta-methodological process we are following which combines action-research, action-design, and action-learning in the context of an evolutionary, incremental, and cybernetic process, by means of collective contributions to this process.

A Significantly Indicative Event Happened After the Presentation Was Made at the Workshop (which was resumed above)

The peer-reviewing methodology, briefly described above and in the linked references seems, to have been quite **_effective_** especially if we take into account that "The publishers Springer and IEEE are removing more than 120 papers from their subscription services after a French researcher discovered that the works were computer-generated nonsense."

(http://www.nature.com/news/publishers-withdraw-more-than-120-gibberish-papers-1.14763?WT.mc_id=TWT_NatureNews). Since 2006, all fake papers we received were identified by our two-tier methodology which is described with more details at http://www.iiisci.org/journal/sci/Methodology.pdf and http://www.iiis.org/acceptance-policy.asp. Even we cannot prove that our methodology is more effective (but less efficient because it requires more persons-hours in peer reviewing and acceptance processes), we have several reasons and indicators to believe that it is definitely more effective. One of this indicators is the recent news regarding prestigious publishers trying to remove about 120 fake papers from their publications, while no case has been presented up to the present with our two-tier methodology.