

Peer Reviewing: Weaknesses and Proposed Solutions

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Abstract

Weaknesses of grants' and journals' peer reviewing or refereeing have been frequently reported by empirical studies as well as by editorial experience. 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) "If peer review was a drug it would never be allowed onto the market," affirmed Drummond Rennie, deputy editor of the *Journal Of the American Medical Association* and "intellectual father of the international congresses of peer review that have been held every four years since 1989." (Cited by Richard Smith, 2010, p. 1) If peer review was a drug, it "would not get onto the market because we have no convincing evidence of its benefits but a lot of evidence of its flaws." (Richard Smith, 2010, p. 1) Richard Smith (2006, p. 116) also affirmed that regarding peer review there is "more evidence of harm than benefit...[and] Studies so far have shown that it is slow, expensive, ineffective, something of a lottery, prone to bias and abuse, and hopeless at spotting errors and fraud." Consequently, several potential improvements and alternatives have been proposed.

In this paper we will 1) describe the weaknesses of peer reviewing or refereeing, by means of the enormous amount of the studies referring to these weaknesses; 2) identify the objectives of peer reviewing/refereeing in the context of scholar/scientific publishing; and 3) describe some solutions that have been proposed; 4) propose an integral model and an integrative methodology where the partial solutions found in the literature are parts of a more comprehensive systemic whole; and 5) to suggest a first step that might be done in the context of action/research in order to try to lessen the weaknesses frequently reported—or at least some of them. The first three parts will be intensely based on citations drawn from the literature on the subject. The first part of this essay will comprehend about 50% of its extension. In future versions, we will try to extend the second part of this working paper.

Introduction

David Kaplan (2005), a highly cited author, stated briefly the problem at hand saying that "Despite its importance as the ultimate gatekeeper of scientific publication and funding, peer review is known to engender bias, incompetence, excessive expense, ineffectiveness, and corruption. A surfeit of publications has documented the deficiencies of this system... Yet so far, in spite of the teeth gnashing, nothing is being chewed... Investigation of the peer-review system has failed to provide validation for its use... In one study, previously published articles were altered to disguise their origin and

resubmitted to the journals that had originally published the manuscripts...Most of these altered papers were not recognized and were rejected on supposed "scientific grounds." Other investigators found that agreement among reviewers about whether specific manuscripts should be published was no greater than would be expected by chance alone." (p. 10). Smith (2003), from the British Medical Journal, affirmed that "Currently peer review is thought to be slow, expensive, profligate of academic time, highly subjective, prone to bias, easily abused, poor at detecting gross defects, and almost useless for detecting fraud. One cannot predict the future but at present there do not seem to be serious alternatives to peer review." (p. 329).

These weaknesses are intensified in peer reviewing processes in the context of conferences' organization, because of their typical time constraints and because of their characteristic informality. Walker and Hurt (1990) affirm that "it is not practical to require the same kind of refereeing of conference papers as for journal articles because of time constraints, and the process would modify, if not eliminate, one of the major desirable characteristics of such meetings: informal exchange of ideas and preliminary findings of new research as well as speculative and even nonconventional presentation of information, both intended to promote innovation and creativity...even for conferences sponsored by societies that have high standards for their journal publications, there is no assurance high standards are also applied to the publications of all conference contributions" (Walker and Hurt, 1990, p. 97). Then, journals' peer reviewing weaknesses are certainly amplified in conferences because of their nature, functions and essential objectives. When conferences are multi-disciplinary events then journals' peer reviewing weaknesses are more amplified because the possible reviewing standards could differ in different disciplines. These weaknesses are even more amplified for those conferences where the purpose of the respective Organizing Committee is to bring together academics and practitioners from public and private corporations.

Let us now briefly describe the most frequently reported weaknesses of "peer reviewing" or "peer refereeing" in the literature, in order to suggest actions that might be taken in order to try to lessen these frequently reported weaknesses.

Weaknesses of Peer Reviewing and Peer Refereeing

Many peer reviewing weaknesses have been reported in an increasing number of empirical studies. Liora Schmelkin resumed these weaknesses, in her Presidential Address to the American Psychological Association, as follows:

- (a) being prone to bias, including reviewer bias, editor bias, various forms of publication bias; (b) unscientific and lacking in evidence for its benefits; (c) having no measurable outcome, and when research is conducted, it is typically on the quality of the review, rather than on the quality of the manuscript; (d) conservative, tending to accept for publication articles that are less controversial and less innovative; (e) slow and expensive; (f) yielding papers that are often grossly flawed; (g) unable to detect fraud; (h) sloppy; (i) subjective; (j) secretive; (k) having many reviewers who are incompetent; (l) having relatively low agreement among reviewers of the same manuscript; (m) having difficulties in handling dissent; (n) unnecessary; (o) leading to potential dishonesty among the reviewers; who sometimes steal ideas from the manuscript they review; (p) stifling scientific communication and hence slowing the advancement of knowledge; (q) subject to various form of

political pressure; (r) incestuous, with a small group of reviewers reviewing each other's work, especially in small narrowly defined specialty areas; and (s) having reviewers who are caustic, nasty, overcritical, arbitrary, self-serving, savage, irresponsible, arrogant, inappropriate.” (Schmelkin, 2003; cited by Shatz, 2004, p. 35)

A shocking list indeed. We found no book, let alone a paper, taking care of all these criticisms adequately. Partial solutions have been proposed to take care of one or few of these criticisms. But, we found no consensus about the acceptance of these proposed partial solutions, let alone a consensual comprehensive solution by which all these weaknesses, or most of them, can be taken care of.

Liora Schmelkin is not alone when making these criticisms. Many studies, and journal editors, concluded that there are significant weaknesses in journal peer reviewing and peer refereeing processes. Consequently, several alternatives have been proposed for journal publishing processes. These weaknesses are certainly amplified in conferences organization. A frequently used alternative in conferences organization is to base the acceptance for presentation in abstracts. Some prestigious conferences would go as far as to accept no more than 50-words abstracts. Other frequently used alternative is to organize focused conferences related to a scientific discipline and to apply a peer reviewing process similar to those used in journals but, perhaps, in more a more informal way. The first alternative is usually used for large conferences and the second one for small focused ones. Is it possible to combine both of them? Are there any other alternatives? Shouldn't other alternatives be explored, especially if we take into account the frequent studies conclusions and expert opinions with regards to the weaknesses of the peer refereeing process?

To try to propose some possible alternatives, or to suggest additional possibilities that could complement the 2-3 tiers reviewing process we are applying in conferences organized by the International Institute of Informatics and Systemics (IIS), let us first give a few examples related to what has been concluded and what has been affirmed with regards to journal's peer review or peer refereeing processes.

We will first present some general statement about peer reviewing weaknesses, and then we will present some conclusions regarding specific weaknesses, in order to suggest some possibilities that may lessen them.

- Horrobin, 1982, referring to the weaknesses of peer reviewing titled his paper: **Peer Review: A Philosophically Faulty Concept which is Proving Disastrous for Science.** (Emphasis added. When Horrobin made this statement, he was editor, in 1982, of Prostaglandins and Medicine, and Medical Hypothesis)
- Magalhães affirms that **“Peer review does not prevent scientific fraud, hardly detects errors, and only modestly improves scientific quality.”** (Magalhães, 2004. emphasis added).
- Among the conclusions Weller (2002) made in her book, after studying more than 200 studies on peer reviewing in more than 300 journals, she affirmed that “Peer

review's outstanding weakness is that of error of judgment, either unintentional or intentional, are sometimes made. **Asking someone to volunteer personal time evaluating the work of another, possibly a competitor, by its very nature invites a host of potential problems**, anywhere from holding a manuscript and not reviewing it to a careless review to fraudulent behavior.” (p. 306; emphasis added)

- Chubin and Hackett (1990) resumed their findings as follows: “Today peer review is besieged on both practical and symbolic grounds. In their complaints, critics point to the operating characteristics of peer review: **low level of consensus among reviewers, inconsistencies of judgment, errors of omission (when a flawed or fraudulent manuscript slips through) and commission (when a competitor's manuscript is blocked or delayed, or its results or arguments are stolen)**, the partisan flavor reviewer comments (which seemingly violates principles of impartiality), and the unsettling influence of authors characteristics on the fate of their manuscripts. These are neither a blueprint for selecting the best science nor an enactment of the values we hope science will honor.” (p. 122; emphasis added). We will briefly examine these characteristics, or weaknesses, of peer review.

Reliability of Peer Review

Horrobin (1982) affirmed that “about one third of the referee's reports are accurate... About one third are accurate but obsessed on the trivial and recommend revision or rejection on inadequate grounds; and about one third are inaccurate...The referee system as currently constituted is a disaster...The concept of peer review is philosophically faulty at its core...As far as research funding is concerned...**I believe that the review system has such faults that it is beyond rescue.**” (Emphasis added)

Lindsey (1988), “**after reviewing the literature on interjudge reliability in the manuscript reviewing process...concludes that researchers agree that reliability is quite low**” (in Speck, 1993, p.113; emphasis added)

David Lazarus, Editor-in-Chief, in 1982, for the American Physical Society, which publishes The Physical Review, Physical Review Letters and Review of Modern Physics, asserted that “**In only about 10-15% of cases do two referees agree on acceptance or rejection the first time around.**” (Emphasis added)

Michael Mahoney, who conducted several studies on peer reviewing processes, “criticizes the journal publication system, because it is unreliable and prejudicial.” (Speck, p. 127). For example, he said that referees' comments “are so divergent that one wonders whether they [referees] were actually reading the same manuscript.” (Mahoney, 1976; p.90). “To reform the journal publishing systems, Mahoney [1990] recommends eliminating referees or using graduate students as referee.” (Speck, 1993, p. 128) Other authors have made similar conclusions. Speck (1993), for example, referring A) to Michael Gordon (1980) affirms that one of his conclusions is that “Younger referees

across disciplines tend to perform more conscientiously than more experienced referees” (p. 81); B) to William Honig (1882) affirming that “Younger referees tend to do better job than seasoned referees” (p. 96); and C) to McCartney and Leavy (1973) assuring that “Younger reviewers often are more competent than eminent, visible, mature, or experienced scholars.” (p. 123). Weller (2002), referring to the Gordon’s research mentioned above, affirmed that “Through a series of interview, Gordon attempted to establish a profile of a good reviewer...[and] found that, in general, editors thought that younger reviewers, took their job more seriously than older, more eminent reviewers.” (Weller, 2002, p. 154)

Ernst and colleagues sent the same manuscript to 45 experts to review it. Each one of the experts held editorial board appointments with journals that publish articles in areas similar to that of the submitted paper. 20% rated the manuscript as excellent and recommended its acceptance. 12% found the statistics of the manuscripts unacceptable, 10% recommended its rejection and the rest of the experts classified the manuscript as good or fair (Ernst, et. al. 1993) Furthermore, they asked the experts to evaluate the paper against eight measures of quality. Almost every measure received the best and the worst evaluation from the reviewers. Ernst and colleagues concluded that “**the absence of reliability...seems unacceptable for anyone aspiring to publish in peer-reviewed journals**” (p. 296; emphasis added).

If peer reviewing is so unreliable and “philosophically faulty at its core” for journals and research funding, then peer reviewing will be even less reliable in conferences organization. This is why, in our opinion, more conference reviewings are being done on abstracts or extended abstracts, and not on full papers. Some conferences stress the fact that any submission that exceeds the limit of a given number of words will not be considered. This is something that could be considered for future Conferences organized by The International Institute of Informatics and Systemics (IIS)

Weller (2002) summarized 40 studies made on reviewing reliability in 32 journals and concluded that according to all these studies “An average of 44.9 percent of the reviewers agree when they make a rejection recommendation while an average of 22.0 percent agree when they make acceptance recommendation.” (p. 193). This means that “**reviewers are twice as likely to agree on rejection than on acceptance.**” (p. 193; emphasis added). Other authors had similar conclusions. Franz Ingelfinger (1974) former editor of the *New England Journal of Medicine*, affirmed that “outstandingly poor papers... are recognized with reasonable consistency.” (p. 342; from Weller, 2002, p. 193) Weller (2002) also found out that Journals’ editors seek more reviews when they have disagreement among the reviewers. She affirms that “between 30 percent and 40 percent of medical journal editors opted for more review when reviewers disagreed; the rest resolved the disagreement by themselves, sought an input from an associate editor or discussed the next steps at an additional meeting.” (p.196). Wilkes and Kravitz (1995) had similar results after examining editor policies in 221 leading medical journals. They found that “43 percent of responding editors sent manuscripts with opposing recommendations from reviewers out for more reviews.” (Cited in Weller, 2002; p.196).

But, to send the manuscript to more reviewers does not necessarily solve the disagreement problem being faced by the journal editor. The high level of disagreement that Ernst and colleagues found was based on a study where a manuscript was sent to 45 experts. As we remarked above, they concluded that **“the absence of reliability...seems unacceptable for anyone aspiring to publish in peer-reviewed journals”** (Ernst, et. al. 1993; p. 296, emphasis added). Furthermore, in conference reviewing processes, the inherent time restrictions make it unfeasible to send the manuscript to more reviewers when the respective reviewers disagree. Consequently, if we take these two facts into account, there will be two basic papers acceptance policies left for the selection of papers to be presented in a conference:

1. To accept just those papers where the reviewers have agreed on such an acceptance.
2. To refuse, or not to accept, those papers where the reviewers have agreed on such refusal.

In the first case the conference will have a very low acceptance rate and there will be a higher probability of not accepting very good papers. In this case there will be no warranty of improving the quality average of the papers accepted for presentation. Let us briefly explain this statement.

Campanario (1995) affirms that eight authors won the Nobel Prize after their prize winning ideas were initially rejected by reviewers and editors. He also found out that about 11 percent of the most cited articles were first refused, and that the three most cited articles, of a set of 205, were initially rejected and eventually accepted by another journal editor (Campanario, 1996, p. 302). Rejection of innovative ideas is one of the weaknesses of peer reviewing that has frequently been reported. An increasing number of authors perceive this kind of reviewer bias. In a survey made by the National Cancer Institute where “active, resilient, generally successful scientist researchers” were interviewed, just 17.7 percent of them disagreed with the statement “reviewers are reluctant to support unorthodox or high-risk research”. 60.8 percent of them agreed, and 21.4 percent were neutral. Federal agencies tried to counterbalance the reviewers’ bias against new ideas by means of providing grants with no reviewing support. Chubin and Hackett (1990) affirm that an example of this kind of “strategy is the recent [1990] decision by NSF [National Science Foundation] allowing each program to set aside up to 5 percent of its budget for one-time grants of no more than \$50,000 to be awarded, without external review, in support of risky, innovative proposals” (p. 201). This is one of the reasons why, in WMSCI Conferences, up to 2005, we have been accepting non-reviewed papers, and taking the intrinsic risks of this kind of paper acceptances. Deception was a risk that was not perceived at the moment of examining the risks of this kind of acceptance policy. We will give some more details later regarding the impact of deception in the reviewing process.

So, it is evident that acceptance policies based on the agreement of reviewers will increase the probability of refusing good papers. The larger the level of agreement sought among reviewers in order to accept a paper, the higher the probability of refusing a very

good paper; although it is also true that the larger the level of agreement among the reviewers the lower the probability of accepting a low quality paper. So, **it is a matter of a trade off: to increase the certainty of refusing poor papers has the cost of increasing the probability of refusing good papers.** This trade off will depend on the journal or the conference quality objectives: whether they are related to refuse low quality papers with the cost of taking the risk of refusing good papers, or to increase the probability of the **quality average**. In the first case, the selection criteria would be oriented to the acceptance agreement among the reviewers, and in the second case it may be better related to the agreement among the reviewers who are recommending not accepting the paper. WMSCI Conferences have been mostly based on agreements among reviewers recommending refusal, or non-acceptance. Papers with disagreement among the reviewers have usually been accepted based mostly on a majority rule. This policy may be improved by the two tiers reviewing we will briefly describe in this paper.

Furthermore, there is no study that can relate low acceptance rates, or high refusal rates, with high quality. Moravcsik (1982) asserts that “the rejection rate in the best physics journals is more like 20-30% and not 80%.” (p. 228). Weller (2002) examines about 65 studies related to the consequences of rejection rates and concluded that “the relationship between rejection rates and the importance of a journal has not been established. What has been established is merely that the more selective the criteria for including a journal in a study, the higher the rejection rate is for that journal. Almost every study discussed in this chapter – Weller emphasizes – has supported this finding, regardless of discipline. Each discipline has a set of journals with both high and low rejection rates; how these are translated into journal quality needs to be further investigated.” (p. 71).

Consequently, to select the first option for an acceptance policy in a conference organization, has **no proven quality benefit** (related to its respective high refusal rate) and **one proven quality risk**, i.e. to refuse good papers because the reviewers’ bias against new ideas or a new paradigm. Therefore, it seems evident that the second of the two options we stated above, will have, in conference organization, a probably better cost/benefit ratio regarding quality average, than the first option. This is especially true if we take into account that “**reviewers are twice as likely to agree on rejection than on acceptance**” as well as the time and other inherent restrictions existing in conference reviewing processes.

The low reliability of peer reviewing and the low level of agreement among reviewers of the same manuscript are some of the peer reviewing weaknesses which have contributed to the skepticism regarding its real value, effectiveness and usefulness. Some authors and editors went as far as to relate peer reviewing to chance. Let us show a sample of this kind of statements.

Peer Review and Chance

Lock (1985) admits that, “**Inevitably luck plays some part in getting accepted in a journal with high rejection rate**” (p. 40; emphasis added).

Fagan (1993) asserts that “**A review outcome may depend on the luck of the draw**” (p. 112. Cited in Speck, 1993, p. 58; emphasis added)

Glenn (1982) affirms that “**The outcome of submission depended largely on luck**”; p. 211; emphasis added).

Lindsay (1979) said that “**interrater agreement is just a little better than what would be expected if manuscripts were selected by chance.**” (Cited in Speck, 1993, p. 115; emphasis added) Nine years later, Lindsay was even more emphatic, titling his paper “*Assessing Precision in the Manuscript Review Process: A little better than a Dice Roll.*” (Lindsay, 1988. Cited in Speck, 1993, p. 113).

Ziman (1982), former editor of *Science Progress*, affirms that “The peer-review process seems not merely imperfect: **It is an entirely useless, if not positively harmful activity, based upon quite erroneous assumptions.**” (p. 245; emphasis added).

Sir Theodore Fox, former editor of *The Lancet*, referred to this issue saying “When I divide the week’s contribution into two piles - one that we are going to publish and the other we are going to return - I wonder whether it would make any real difference to the journal or to its reader if I exchanged one pile for another (quoted by the New York Times, February 25, 1986; and cited by Walker and Hurt, 1990, p. 28)

This frequently reported relation between peer review and chance, prompted some authors to suggest some solutions to this peer reviewing weakness.

Gordon (1982), who is a physicist, for example, proposed the “**optional published refereeing**” where authors would be given “the option of (and the responsibility for) publishing the manuscript provided it is accompanied by the anonymous unanswered comment of the referee where relevant... Such a system would not only leave the responsibility of publishing with the authors, to whom it must ultimately belong, but would be more consistent with the only real, and the only truly realizable, goal of refereeing process, namely to provide an evaluation of a submitted manuscript as is practical, but without any implication of infallibility on the part of the referees, journals and authors... In cases in which the referees were in substantial agreement with the manuscript, the manuscript would be published as under present refereeing process. Journals wishing to provide a minor, noninhibiting degree of referees’ accountability could require that the names of the referees be published at the end of the accepted manuscript, although authors would, of course, still bear the major responsibility for the manuscript.” (p. 214). An adapted version of Gordon’s optional published refereeing might be applied in the selection of papers for presentation in conferences, especially if the conference is a multi-disciplinary and/or inter-disciplinary one and, hence, a large one. When the reviews of a paper are non conclusive, the paper may be accepted, under the condition of accompanying its presentation at the conference and its publication in the proceedings with its respective reviewers comments. Gordon’s optional published refereeing may help with the time restrictions inherent to the reviewing of conferences

papers, which become a burden when the reviews of a paper are evenly split between recommendations to accept it and to reject it.

Effectiveness Ethics of Deception in Peer Reviewing

Peters and Ceci (1982a) “selected 12 already published research articles by investigators from prestigious and highly productive American psychology departments, one article from each of 12 highly regarded and widely read American Psychology journals with high rejection rates of 80%... With fictitious names and institutions substituted for the original ones...the altered manuscripts were formally resubmitted to the Journals that had originally refereed and published them 18 to 32 months earlier. Of the sample of 38 editors and reviewers, only three (8%) detected the resubmission. This result allowed nine of the 12 articles to continue through the review process to receive an actual evaluation: eight of the nine were rejected. Sixteen of the 18 referees (89%) recommended against publication and the editors concurred. The grounds for rejection were in many cases described as “serious methodological flaws.” (Peters, D. P. and Ceci S. J. Peer-Review Practices of Psychological Journals: The fate of published articles, submitted again. *The Behavioral and Brain Sciences*, 5, No. 2, June 1982, pp. 187-197)

Peters and Ceci’s study showed several weaknesses of the reviewing process. It showed at least the following Flaws:

1. Reviewing process is not effective in the detection of fraud and plagiarism: only 8% of the resubmissions were detected.
2. The very low reliability of the reviewing process as well as the very low agreement among reviewers: eight of the nine papers (that were accepted 18 to 32 months earlier) were rejected. This means 11% of agreement between previous and later reviewing processes. Furthermore, sixteen of the 18 referees (89%) recommended against publication and the editors concurred. So the agreement among previous and later reviewers is, again, just 11%. This percentage is in agreement with the experience of David Lazarus, Editor-in-Chief, in 1982, for the American Physical Society; who, as we indicated above, affirmed that “In only about 10-15% of cases do two referees agree on acceptance or rejection” (The American Physical Society, in 1982, which publishes *The Physical Review*, *Physical Review Letters* and *Review of Modern Physics*). The low reliability of the reviewing processes and the low level of agreement among reviewers are found in different disciplines, as we indicated above, and not just in journals in Psychology and Physics.

Referring to Peters and Ceci study, Horrobin (1982) affirmed that they “provided unusually sound evidence for something that those concerned with the reality rather than with the image of science have known for some time. **The referee system as currently constituted is a disaster.**” (p. 217; emphasis added).

Mahoney (1982) affirms that “the report of Peters and Ceci questions the assumption that ‘the review process is basically objective and reliable’...Their data are offered as empirical corroboration of potential reviewer bias, incompetence, and unreliability.”

3. Acts directed to deceive a reviewing process may be effective with a significant probability in journals, let alone the possibility of deception in conference reviewing processes, where the inherent time restrictions increase its effectiveness probability. Furthermore, the larger the conference, the more probable is the effectiveness of deceiving its reviewing process.

Referring to Peters and Ceci’s finding by means of deceiving journals editors and reviewers, David Lazarus (1982), past Editor-in-Chief for the American Physical Society, (1980-1990), emphasized that “...**the peer-reviewed system’s being of finite value, particularly when used deceptively...We [in the Physical Review] rely on the honesty and integrity of our authors** – and their own self-selection of the quality of the papers they send us – as much as on our referees and editors, to ensure the quality of our journals.” (p. 219; emphasis added) (The American Physical Society publishes The Physical Review, Physical Review Letters and Review of Modern Physics.)

Hopps (1983), in an article titled “Peer Review: A Trust, Not a Vault” asserts that **the submission of spurious manuscripts to evaluate a journal-review process is an example of “violation of trust between journal and author.”** (p. 97; emphasis added).

Commenting Peters and Ceci’s study, Horrobin (1982) affirms that “The [peer reviewing] system assumes perfect honesty and integrity”, consequently, “peer review is an open invitation to the crooked.” (p. 218).

Peters and Ceci’s study shows how effective the action of deceiving a reviewing process could be, and, consequently, how effective it could be for a bogus paper to get through a reviewing process of a journal, let alone through a conference reviewing process, especially if the conference is a yearly large one. With regards to this issue, Nelson (1982) asserts that “The focus of P & C’s [Peters and Ceci’s] report is on ‘false negatives.’ That is, the fate of publishable articles that are judged unacceptable. But it could just as well (and more appropriately I believe) have focused on ‘false positives,’ the publication of unacceptable manuscripts” or the publication of low quality or even bogus papers.

In a response given to those authors who made commentaries to their study, Peter and Ceci (1982b) affirm, in a section titled “*Ethics of deception*”, that “Seven of the commentaries explicitly criticized methods on the grounds that they entailed unwarranted deception...four other expressed the opposite opinion, expressing that they found deception justified under the circumstances...at least eleven have no quarrel at all with the ethics of our methods.” (p. 247). With regards to the ethical problem that rose with their studies they affirm that “The ethics of deception will not

be resolved by simple vote counts...or even by argument. There will always be moral relativists...who believe that a code of ethics cannot be violated in the abstract, but must be seen in context; who believe that anyone intending to practice deception must do a cost-benefit analysis...Conversely, there will be moral absolutists or non-consequentialists who will always view any deception, regardless of relative costs and benefits, as morally reprehensible.” (p. 247) We believe that ethical problems should be viewed with a moral perspective and not with a cost-benefit one. In our opinion, a relativist or a consequentialist ethics should be handled with care. Communists and fascists have also based their justifications on a relativist, consequentialist and cost-benefit-based ethics, where the ends justify the means used.

4. William Epstein, who studied editorial bias by sending fake papers to journals, faced censure by his professional society for submitting a deceptive paper. Daniel Goleman (1988a) wrote, in *The New York Times*, that “Dr. Epstein is being charged with two kinds of unethical conduct: deceiving his research subjects – the editors who received the articles – and failing to get their informed consent to be in the study.” Several weeks later, a panel found Epstein in violation of the two ethical precepts with which he was charged. Accordingly, Epstein received a letter from Claire B. Gallanty, chairwoman of the Society’s committee that ruled on ethics violation, informing him that “the panel had concluded that he violated two sections of the Association’s ethical code...The committee has recommended that a letter of censure be sent to Dr. Epstein, and that he write a letter to journals to which he submitted fictitious article, expressing his regrets. If he fails to do so, his membership in the organization will be suspended for one year.” (Goleman, 1988b). Some authors believed “that the charges brought against Dr. Epstein have less to do with ethical concern than the outrage of the editors he duped,” (Goleman, 1998a) and affirmed that some kind of studies in peer reviewing cannot be done without deception. To this kind of reasoning, John Schuerman, editor of a journal that Epstein’s paper could not fool, acknowledged that there are circumstances, in social science research, where it is accepted procedure not to obtain informed consent, but, he said, “you have to weigh the benefits of the research against the risk of harm to the subjects.” He added, referring to Epstein’s research, “Here the harm was the cost and the time to busy professionals, the large number of reviewers and staff involved.” Another cost he noted was emotional: “The chagrin and embarrassment of those editors who accepted the article.” (Goleman, 1988a). Michael Mahoney, who also, in the late 1970’s, sent a fictitious article to journals, and “three editors tried unsuccessfully to get him fired or see that he did not get tenure,” (Goleman, 1988a), affirmed, when evaluated Epstein’s study, that “Dr. Epstein abused the system he was studying.” (Mahoney, 1990; p. 54). In the context of Epstein’s study based on deception, (2002) affirms that “fabricated manuscripts will by their nature always bring up ethical issues: particularly the issues of the presentation of false data and the waste of editors’ and reviewers’ time.” (p. 237).
5. Alan Sokal’s hoax is a well known diction of peer refereeing. *Social Text*, a leading journal in cultural studies, published an essay suggesting a link between post-modernism and quantum mechanics, submitted by the physicist Alan Sokal. On the same day of publication, Sokal announced in *Lingua Franca* that the article had in

fact been a hoax. Sokal's objective was, in his own words: to "publish an article liberally salted with nonsense if (a) it sounded good and (b) it flattered the editors' ideological preconceptions." This publication caused an academic scandal for Duke University where *Social Text* is published. Sokal called his paper "a pastiche of left-wing cant, fawning references, grandiose quotations, and outright nonsense", which was "structured around the silliest quotation I could find about mathematics and physics" made by humanities academic (Wikipedia). Sokal's article has been seen as one "of the first direct attacks in a major journal against the institution of cultural studies." (Walsh, 1995). Sokal's critics emphasized on ethical issues and on the trust he violated, and which is necessary for the scientific enterprise. Stanley Fish (1997), professor at Duke University and executive director of the Duke University Press, which publishes the journal *Social Text*, after presenting his view on what Sokal attempted and achieved, concluded that "Alan Sokal, not his targets, [is] who threatens to undermine the intellectual standards he vows to protect. Remember, science is above all a communal effort. ... The large word for all this is "trust," and in his "A Social History of Truth," Steven Shapin poses the relevant (rhetorical) question: "How could coordinated activity of any kind be possible if people could not rely upon others' undertakings?"... In a 1989 report published in *The Proceedings of the National Academy of Science*, fraud is said to go 'beyond error to erode the foundation of trust on which science is built.' That is Professor Sokal's legacy, one likely to be longer lasting than the brief fame he now enjoys for having successfully pretended to be himself. **"The controversy also had implications for peer review. *Social Text* had dispensed with peer review,** hoping that this would promote more original, less conventional research, and trusted authors of prospective articles to guarantee the academic integrity of their work. *Social Text's* editors argue that, in this context, Sokal's work constituted a deliberate fraud and betrayal of that trust. They further note that scientific peer review does not necessarily detect fraud either, in light of the later Schön scandal and many other instances in the history of science." (Wikipedia; emphasis added).

6. 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. This affair has also been known as "a reverse-Sokal event". 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).

Van Wyk (1998) asserted that “In common with most other social systems, system stability depends on trust between participants” (p. 253). When the trust in a peer reviewing process is violated, the system is destabilized and even leading journals in physics end up publishing hoaxes or meaningless papers, and dissertations’ reviewers approve and grant doctorate degrees. If this can happen in leading journals in physics and in Ph. D. dissertations, it is not difficult to imagine what could happen in journals in other disciplines and in conferences, especially in large ones where about 2000 papers submissions should be handled, reviewed and accepted or refused in about 3 months. So, it is to think that in this kind of conferences, there would be no way to have security measures for quality control that can assure a probability of zero of not detecting low quality papers, or bogus submissions. Here, it is good to repeat the affirmation of David Lazarus (1982), former Editor-in-Chief for the American Physical Society, who emphasized that **“...the peer-reviewed system’s being of finite value, particularly when used deceptively...We [in the Physical Review] rely on the honesty and integrity of our authors** – and their own self-selection of the quality of the papers they send us – as much as on our referees and editors, to ensure the quality of our journals.” (p. 219, emphasis added) (The American Physical Society publishes The Physical Review, Physical Review Letters and Review of Modern Physics.).

We hope that the 2-3 tiers reviewing we are planning conferences organized by the IIS will significantly decrease the probability of accepting low quality or hoax papers. One of the costs of lowering this probability will certainly be the increasing probability of refusing good papers. Consequently, additional measures will be taken to lessen this cost, as for example, to accept refused papers as long as the respective author accepts the simultaneous publication of the reviewers’ comments.

Bias in the reviewing process

The Office of Scholarly Communications of the American Council of Learned Societies (ACLS) conducted a survey of 5,385 scholars. Morton and Price (1989) discussed its results, with regards to peer reviewing bias, and concluded that “About three out of four respondents think the editorial peer review system is biased...About 40% think bias is so prevalent in their disciplines that it merits reform...suspicions of bias appear to be held by scholars in all types of universities and among all disciplines sampled...the unease is pervasive, not an occasional outcropping of discontent...The question is, therefore, not whether bias exists in peer reviewing systems but whether it is prevalent and whether it systematically interferes with the free exchange of information and ideas by discriminating against particular subjects, opinion and classes of authors.” (Morton and Price, 1989; pp 7-9)

Many studies concluded that one of the weaknesses of peer reviewing is the frequently found bias in reviewers and editors. The most important ones are: affiliation, paradigm, country and gender biases.

Peters and Ceci’s study (1982a), mentioned above, showed irrefutable bias with regards to the affiliation of the authors. **89% of previously accepted papers were refused by the same journals that accepted them formerly, before Peters and Ceci had changed their authors’ names and affiliations.**

Honig (1982), commenting Peters and Ceci’s study, and based on his experience as an editor, asserts that “**there is indeed a definite preference and prejudice for papers from elite groups.**”(p.217; emphasis added).

Over (1982), also commenting Peters and Ceci’s paper, asserted that “One potential source of bias is the editor. **The editor is interested not only on quality control but also in the journal’s prestige.**...Blind review is one way to control bias, but typically the editor is not blinded during the review process.” (p. 236; emphasis added) The editor’s interest in prestige may, consciously or unconsciously, bias him/her toward manuscripts submitted from the 10, 20, or 50 top ranked universities; or toward elites groups or authors with good citation indexes. In conferences organized by the IIS, we tried to also blind the organizers by means of automating the random reviewers’ selection and semi-automating the papers’ selection. We will try to keep most of this procedure for future conferences; in spite of the 2-3 tiers reviewing we are thinking to implement in order to increase the security measures for a better quality control. To confuse “quality” with “prestige” may increase the probability of bias. So, we think that measures should be taken in order to reduce the probability of this kind of conceptual confusion, especially if we are interested in avoiding any bias oriented to elite groups or to highly ranked universities or research centers.

The Epstein’s study, which we mentioned above, has also shown the weakness of bias in peer reviewing. The New York Times wrote it in the following terms: “Dr. Epstein, a consultant in social policy in Washington, submitted the fictitious article to 140 journals

in social work and related disciplines. The article pretended to analyze the value of a 'social work intervention' in which an asthmatic child was temporarily separated from its parents in an effort to relieve the symptoms of an illness that is often psychosomatic...In half of the articles, the findings supported the effectiveness of a social worker intervening to remove the child while in the other half the intervention was judged ineffective...Dr. Epstein found that social work journals favored the positive version of the article, which supported the value of social intervention, and often shunned the negative version...50% of those receiving the positive version of the paper accepted it, but only 14 percent accepted the negative version." (Goleman, 1988; p. C1). This kind of bias may be counterbalanced with reviewers from other related disciplines, i.e. some kind of interdisciplinary reviewing. This type of reviewing might be applied in interdisciplinary journals and multi- and/or inter-disciplinary conferences, but the cost of it could be an increase in the reviewers' disagreement. Consequently, some kind of trade-off should be sought.

Mahoney (1977) conducted a study asking 75 reviewers to evaluate a paper that had identical experimental procedures. One version of the paper had positive results, another had negative results and a third one had mixed results. In his conclusion, Mahoney affirmed that reviewers were "**strongly biased against manuscripts which reported results contrary to their theoretical perspective**" (p. 161; emphasis added)

The bias against authors from developing countries is also a frequently reported one. "When Luis Benitez-Bribiesca worked in Boston and in Germany - asserts Van Wyk (1998) - he had no problem getting his work published, but since moving to Mexico, his papers are rejected out hand." (Van Wyk, 1998, p. 248; Gibbs, 1995). Jerome Kassirer, Editor-in-Chief of the New England Journal of Medicine, said that "Very poor countries have much more to worry about than doing high quality research. **There is no science there.**" (Editorial, 1997, Gibbs, 1995; Van Wyk, 1998; emphasis added). "To researchers in poor countries - asserts Van Wyk (1998) - this remark must seem exceedingly arrogant and insulting. Peculiarly enough, many of these 'second rate scientists' are alumni of First World institutions of 'excellence'. Hence, not only is it not good enough to be a talented researcher, but quality research may only be done at a small number of exclusive First World units." (p. 248). This is compounded by a "belief system according to which 'foreign' researchers are often led to believe that 'excellence' means acceptance by Western peers." (Van Wyk, 1998, p. 250). Among the possible solutions to the bias against researchers of the Third World, and to support their development with a more realistic and fair attitude, Van Wyk (1998) proposes that "Third World reviewers ought to be given the opportunity to review submissions of their First World colleagues in term of relevance to the large majority of humanity that it often excludes. In fact - Van Wyk adds - to improve the level of debate, we as systems practitioners ought to consider having in our editorial boards persons without academic accreditation or desirable affiliations to judge the practical relevance of the work and research we do and submit for publication. If they cannot understand what we say, ought we to plan for them?" (p. 255). This is a practical attitude as well as an ethical one. Fairness with scientists, engineers, consultants, professionals, intellectuals and practitioners from The Third World, may, on the long range, be an objective and pragmatically intelligent attitude, not just a fair one. When

organizing IIS's conferences we have in mind this important issue, and this is why we have a heavy component from the Third World among the conference reviewers, as well as among the invited sessions' organizers. We will keep this emphasis when organizing future conferences. The cost of this approach is the possible perception in some scholars – very few we hope – that the conference is not strongly related to the top First World's universities and research centers, and consequently, the prestige of the conference might somehow be lowered. This is a cost we are ready to pay as long as the quality can be kept in an adequate level, and not being confused with prestige. Quality is one thing; prestige is another thing, although they are related in some situations. Quality and prestige can co-exist in the same organization, but we can also have quality without prestige, and prestige without adequate quality.

Plagiarism and Fraud

The reviewing process is both, ineffective in detecting plagiarism and fraud, and a source of plagiarism. As we mentioned above, Magalhães (2004) affirms that “**Peer review does not prevent scientific fraud**, hardly detects errors, and only modestly improves scientific quality.” (Magalhães, 2004, emphasis added).

Peters and Ceci's study showed that peer reviewing is not effective in the detection of fraud and plagiarism: **only 8% of the resubmissions were detected**. Consequently, the effectiveness of the reviewing process in this study was just 8%: 92% of the “plagiarized” papers were not detected, not even by the same journal that published them the first time.

As we also indicated above, among the conclusions Weller (2002) made in her book, after studying more than 200 studies on peer reviewing in more than 300 journals, she affirmed that “Asking someone to volunteer personal time evaluating the work of another, possibly a competitor, by its very nature invites a host of potential problems, anywhere from holding a manuscript and not reviewing it to a careless review to **fraudulent** behavior.” (p. 306; emphasis added).

Chubin and Hackett (1990) also indicate the same kind of possible situations when a **competitor's manuscript is blocked or delayed, or its results or arguments are stolen**.

An epitome where peer reviewing resulted in plagiarism, where results or arguments were stolen, is what has been known as the **Yale Scandal**. In a two-part article in *Science* entitled “Imbroglia at Yale: Emergence of a Fraud,” William J. Broad (1980) thoroughly described such a fraud or plagiarism. Moran (1998) summarized it in the following terms: “A junior researcher at NIH [National Institute of Health], Helena Wachslicht-Roadbard, submitted an article to the *New England Journal of Medicine (NEJOM)*. Her supervisor, Jesse Roth, was coauthor. An anonymous reviewer for *NEJOM*, Professor Philip Felig of Yale [‘a distinguished researcher with more than 200 publications who held an endowed chair at Yale and was Vice Chairman of the department of Medicine’ (Broad (1980, p.38)], recommended rejection. Before returning its negative recommendation to *NEJOM*, Felig and his associate, Vijay Soman, read and comment on it. Soman made a

photocopy of the manuscript, which he used for an article of his own in the same area of research. Soman sent his manuscript to the *American Journal of Medicine*, where Soman's boss, Philip Felig, was an associate editor. Felig was also coauthor of the article. The manuscript was sent out for peer review to Roth, who had his assistant, Rodbard, read it. She read it and spotted plagiarism, 'complete with verbatim passages.' (Broad, 1980, p.39)...Rodbard sent a letter to *NEJOM* editor Arnold Relman, along with a photocopy of the Soman-Felig article. Relman was quoted as saying the plagiarism was 'trivial', that it was 'bad judgment' for Soman to have copied some of Roadbard's work, and that it was a 'conflict of interest' for Soman and Felig to referee Roadbard's paper (Broad, 1980, p. 39). Relman then called Felig, who said, according to Broad (1980), that peer-review judgment was based on the low quality of Roadbard's paper, and that the work on the Soman-Felig paper had been completed before Felig received the Rodbard manuscript (Broad stated that this last statement by Felig was incorrect)...Relman published the Rodbard paper, in revised form. Roth called Felig (a long-time friend from school days) and they met to discuss the two papers, for which they were either coauthors or reviewers. Broad (1980) stated that prior to the meeting 'Felig had not compared the Soman manuscript to the Rodbard manuscript' (p. 39), even though Felig was coauthor of one article and referee for the other! When he returned to Yale, Felig questioned Soman, who admitted he used the Rodbard manuscript to write the Soman-Felig paper...Broad (1980) reported that Rodbard and Roth began to express disagreement about the extent of plagiarism involved. Rodbard wrote to the Dean of Yale's School of Medicine, Robert Berliner, who did not believe all that she wrote. He was quoted as writing back to her, 'I hope you will consider the matter closed' (p. 38). NIH apparently put off (by dragging their feet or by stonewalling) an investigation. A subsequent audit of the records revealed, according to Broad, a 'gross misrepresentation' (p. 41). Soman admitted that he falsified, but claimed it was no 'significantly different from what went on elsewhere' (p. 41). After further investigations, at least 11 papers were retracted. Soman was asked to resign from Yale University, which he did. Felig became Chairman of Medicine at the Columbia College of Physicians and Surgeons." (Moran, p.69) "After two months [in this position], Philip was forced to resign...At issue was a scandal that rocked the laboratory of one of Felig's associates and coauthors back at Yale Medical school, where Felig previously worked." (Broad, 1980, p. 38) Helena Wachslight-Rodbard spent one year and a half writing letters, making phone calls, threatening to denounce Soman and Felig at national meetings, and threatening to quit her job. She wanted an investigation and she got it. (Broad, 1980; p. 38)

Several cases like the Soman-Felig scandal have been reported, but – as Moran (1990) affirms - it is "impossible to tell precisely how many attempts at plagiarism by means of peer review secrecy have been successful." (p. 118). It is to be thought that this kind of plagiarism is more frequent when the manuscripts are coming from what is called the Third World, to be reviewed by reviewers of the First World. Verbal reports abound on this issue. One of the explicitly stated functions of a conference and its proceedings is to be "a place to claim priority" (Walker and Hurt, 1990; p.79). This may counterbalance the plagiarism reported in journal peer reviewing, especially if we take into account that other explicitly stated function of conferences and their proceedings, is the informal

publication that may precede the formal publication of the respective research in a journal.

If one of the functions of a conference is to be “a place to claim priority”, then conference organizers should consider adequate measures to avoid that their reviewing process generates opportunities for plagiarism from some of its reviewers. **One way to achieve this objective is to have a policy of “when-in-doubt-accept-the-paper” as opposed to the policy of “when-in-doubt-refuse-the-paper”.** Arnold Relman, editor of the *New England Journal of Medicine* had another reviewer suggesting him to accept Helena Wachslicht-Roadbard’s paper. If he had accepted the paper, there would have been no opportunity for the plagiarism made by means of his reviewing process. This reinforces what we stated above with regards to acceptance policy of conferences organized by IIS which mostly is based on agreements among reviewers recommending refusal, or non-acceptance. Papers with disagreement among the reviewers have usually been accepted based mostly on a majority rule. This policy might be improved by adding to it Gordon’s optional published refereeing.

This kind of acceptance policy has its benefits and its costs. The costs may be: 1) an increase in the number of low quality papers being accepted (which – as we argued above - is counterbalanced by an increase in the probability of accepting good papers which otherwise would have been refused); and, 2) an increase in the probability of effective deceptions, or bogus papers acceptance. The benefits may be: 1) an increase in the quality average of the papers (because the increasing of the probability of accepting high quality, paradigm shift, papers which otherwise would have been refused); and, 2) a decrease in the probability of plagiarism through some of the conference’s reviewers.

Rejection Rates

An acceptance policy oriented by the criteria “when-in-doubt-accept-the-paper” could produce a lower rejection rate than a policy based on the criteria “when-in-doubt-reject-the-paper”. This cost is at the perceptual level, it is not a real one. It has been repeated *ad nauseam* (wrongly in our opinion) that a high refusal rate is a necessary condition for high quality in journals and conferences. It is not so, it is not even a sufficient condition. Several editors and authors reported on this issue.

Gordon (1982) asserts that **“a high rejection rate does not in itself provide any guarantee that the accepted manuscripts are measurably better than the rejected ones.”** (p. 214, emphasis added)

Hogan (1982) affirms that **“the better journals do not necessarily have the highest rejection rates.”** (p. 216; emphasis added)

We can enumerate more affirmations like these. We found no study showing that high refusal rates are associated to high quality. It might be associated to prestige, but not necessarily to quality.

What studies did show is that there is a larger refusal rate in journals related to humanistic areas than in those related to areas in the natural sciences. Does this mean that humanistic journals have higher quality than journals in natural sciences, and specifically, in physics?

Meaning of “peer review”

Weller (2002) asserts that “Since editorial peer review is a process, its definition can and does vary according to how the process is envisioned.” There is a general obvious definition: “evaluation by one’s peers”, but the meaning of “one’s peers” differs according different editorial policies and according different universities’ regulations.

Pettigrew and Nichols (1994) explicitly stated the ambiguity of the term: “‘Refereed journal’ is not a precise term, but rather covers a continuum of peer/controlled quality assessment that reaches its most strict definition with double-blind peer review by several scholars working in the research area, and a minimal ability of the editor to override clear decisions by the peer reviewers”, so, depending on the meaning of “minimal ability”, the same journal’s editorial policy may be considered as “peer reviewed” or “not-peer reviewed”. Since conference proceedings reviewing is less formal than journals reviewing process, or informal, then the imprecision of the term is significantly higher.

DeBakey (1990) affirms that “ ‘Peer reviewing’ has become a stock term, but – he asks – is a reviewer of a manuscript...always a peer: a person who has equal standing with another, as in rank, class or age?” (p. 347, cited by Weller, 2002, p. 16) So, if we define “peer” as a person who has equal standing with another, as in rank (equal standing of academic rank, for example), many conferences organizers and Journals’ editors are definitely not making “peer reviews”, and this kind of “peer reviews” is definitely not the base of their paper acceptance policy. In many reviewing processes there is no feasible way of being certain if the reviewers have the same academic ranks as those of the authors of the papers being reviewed.

Chubin and Hackett (1990) state that “Chief among [the inherent difficulties of peer reviewing] is the changing definition of ‘peer’.” (p.193) Chubin (1980), referring to a study of peer review at the National Science Foundation, wrote: “Gender and current location are dominant factors in the selection of panel members [peers]. The implication is that panel members who are female or, say, reside in the southern U.S., conceptualize the world in a particular way, and thereby endow the panel or mail review with a balanced perspective otherwise missing.” Chubin and Hackett (1990) assert that nothing has changed since Chubin wrote the indicated statement, but only intensified. Then they continued saying: “add these proxy social (status) characteristics to the epistemological variation that distinguish disciplinary or technical ‘peers’ and altogether a new dimension emerges, that of ‘cognitive style.’” (p. 193), that is being pointed by several authors as it is the case of Mitroff and Chubin (1979) and Noble (1974), for example.

With regards to the meaning of “peer”, Chubin (1990) goes further and asserts that “Even if we restrict our focus to social characteristics and treat cognitive styles as random element in the review process, **we find the notion of ‘peer’ a misnomer**...grants peer reviewers are older, more accomplished, and more likely to work at prestigious institutions than are the rank and file of a field. These reviewers are certainly not every scientist’s *peers*, just as the members of a jury are exceedingly unlikely to be the defendant’s ‘peers’ except in the very loosest sense in which we are all one another’s peers. In both cases **the idea of peer decision making is a rhetorical device that lends legitimacy to the proceedings**”...While reviewers may, on average, be more accomplished than those whose work they review, they are sometimes less accomplished.” (p. 194; emphasis added)

Stumph (1980) asserts that “For most advanced scientists only a few or no peers exist. In their research new areas are explored, often with special techniques and approaches. There is thus a high probability that one or several aspects of a proposal will not be appreciated by the judging ‘quasi-peers’...for advanced scientists, a competent review cannot be achieved unless a reviewer who is working in the same field with similar amount of experience is consulted.” Consequently, “the closest scientific peer is a competitor. Even though reviewers try to be fair, nobody likes his or her programs for original ideas to be screened and judged by a real or potential competitor.” (p. 822).

Combining his arguments with those of Stumph (1980), Chubin (1990) concludes that “For the ‘best’ scientists *peer* review is unlikely...**Scientists are at the mercy of peer review systems that may offer neither ‘peers’ nor ‘review’.**” (p. 194; emphasis added)

It seems that there is no consensual definition of “peer” and we could find no consensual definition of “peer reviewing” or “peer refereeing”, or some kind of standard related to processes of peer reviewing. A minimum standard cannot even be found in among federal agencies. In a recent release from the USA Executive Office of The President (Office of Management and Budget: OMB) it was stated that “**there is no minimum, government-wide standards for peer review.**” (Office of Management and Budget, 2003; emphasis added)

After examining several definitions of the phrase “peer-reviewed journal”, Weller (2002) states that “These definitions contain a common element in that they each require some type of review of a manuscript other than the editor. Some definitions are more prescriptive than others, incorporating the number of processes and requirements. **These definitions do not address such issue as the percentage of material in a journal that should be peer reviewed**, or many other details of the process.” (p. 16) Weller consequently makes a definition which is “intended to be as inclusive as possible.” Accordingly, she states that “A peer-reviewed journal is one that has a portion of submitted manuscripts evaluated by someone other than the editor of the journal.” Again, this is a very elastic definition because it depends on the magnitude of the “portion” of submitted manuscripts evaluated by someone other than the editor of the journal. An acceptable “portion” for a given journal, or a given university, might be completely unacceptable for another. And, if we take into account that papers reviewing for

conferences proceedings are less formal than those of journals (or informal); and that some (or all, depending on the conference) of the paper acceptances are based on abstracts, and not on full papers; then the level of imprecision of the definition, for these cases, increases meaningfully.

Many other authors expressed similar concerns about the meaning of “peer reviewing” and the differences found in different peer reviewing processes. Manske (1997) asserted that **the “process of peer review has never taken a standardized form and continues to vary from journal to journal”**, let alone from conference to conference, and from proceedings to proceedings.

Consequently, being prudent and cautious with regards to what the term may mean to different scholars and practitioners, it is probably preferable not to use the phrase “peer reviewed proceedings” or “refereed proceedings” when organizing a conference, but to describe the reviewing process so each one would judge if it is applicable to his, or her, definition, or to the definition of her, or his organization.

Objectives of the Peer Reviewing or Refereeing

Several authors have pointed out the fact that peer reviewing processes seem to be having different objectives among different journals and even in the reviewing processes of the same journal. Some of these objectives are explicit and some are implicit. Conflicting objectives may generate conflicting results; and an effective peer reviewing/refereeing method for the achievement of an objective may be completely ineffective and inadequate for other objective.

David Kaplan (2005), for example, a highly cited author according to the Institute of Scientific Information (ISI), pointed out that “Peer review subsumes two functions. First, peer reviewers attempt to improve manuscripts by offering constructive criticisms about concrete elements such as the application of a technique, the strength of results, or the cogency of an argument. The second function of peer review is to render a decision about the ...significance of the findings so that the manuscript can be prioritized for publication. I propose reforming peer review so that the two functions are independent.” (p. 10).

Kaplan proposed to use different peer reviewing processes carried on with different kind of reviewers, and coordinated by different persons in order to meet both objectives. This proposition agrees, in our opinion, with Singer-Churchman’s **pragmatic-teleological truth** of the system approach. (Churchman1971). We will describe Kaplan’s proposition below, in the next section, and we will use the System Approach for proposing a systemic model for scholar publishing processes.

Walker and Hurt (1990) affirms also the plural objectives of peer reviewing/refereeing. They affirm that “Refereeing of manuscripts submitted to archival journals serves several purposes.” Although in different words, they refer, basically, to the two objectives cited by Kaplan, above.

Griffith (1982) also refers to two different roles in peer reviewing: “Judging document content versus social functions of refereeing.” He affirms “that referees make significant contributions to the quality of reported work.” And that a role of referees is that “of ‘certifying’ knowledge.” So, there seems to be some consensus that there are, at least, two different objectives in peer reviewing which can be stated using the terms Kaplan (2005) used. Simultaneously, peer reviewing objectives should also harmonize with those of knowledge communications where publications are means to achieve it, and peer reviewing supports the publishing process. Let us go into some details with regarding this issue.

Peer reviewing/refereeing is a sub-process which is, and should be, an integral part of the process of scholar/scientific publishing, which is a means for scholar/scientific communication. Consequently, the objectives of peer reviewing/refereeing should harmonize with the objectives of scholar/scientific communication. Before the first journal appeared, scholars, scientists and academics communicated through correspondence, exchange regarding experiments and findings through personal reports, mimeographed reports, private printing, etc. “The practice of journal peer reviewing began almost simultaneously with the founding of the first scientific journals...Publication in a journal under the auspices of a scientific society and the guidance of an editor, replaced the haphazard circulation and content of letters with generally available and more standardized journal article. It also solved the riddle of how to make new ideas and results available for general use while preserving a scientist’s claim to priority and credit—in effect, creating communal recognition of private intellectual property. And, because publications carried the explicit approval of a scientific society that sponsored the journal, a measure of quality assurance emerged...Publication also bestows a modicum of permanence to a manuscript, entering both the work and its author into the archives of a discipline.” (Chubin and Hackett, 1990, p. 85. These authors based their treatment on this issue on Zuckerman and Merton, 1973)

Being publication a means of communication, it shares the general purpose of scholar/scientific communication and has some specific objectives. Its *general* purpose is

1. To serve as a *method of communication* that allows others to *learn* about new discoveries, theories, experiments, hypothesis, innovations, solutions, problems, etc.

And, its *specific* objectives are, or may be,

2. To establish a scientific claim to *priority* and *credit* for having been the first to discover a new phenomenon, to identify a new theory, to find a new solution, to identify a new problem, to present a new innovation, to conceptualize a new experiment, etc.
3. To provide an adequate *permanence* to a manuscript.
4. To establish an adequate level of *quality assurance*.

Objectives 1 and 2 are *primordial* for scholar/scientific publishing. Regarding this issue, Chubin and Hacket (1990) affirm that “Publication is first a method of *communication* that allow others to learn of a scientist’s new theories, discoveries, and experiments, while establishing the scientist’s claim to *priority* and *credit* for having been the first to discover a new fact or phenomenon or to advance a novel conjecture.” (p. 85; emphasis done by the cited authors)

None of the first three objectives, from the four we stated above, involve peer reviewing. Chubin and Hacket (1990) are explicit about this fact. (p. 85). Objective 4, quality assurance, does require peer reviewing/refereeing. The first three objectives show “the advantages of publication over private circulation (Chubin and Hacket, 1990, p. 85). Papers published without its respective peer reviewing/refereeing, are not supported by the authority of the scholar society sponsoring the journal, or the authority of the respective editor and reviewers. When a scholar society and/or an editor publishes a paper without peer reviewing it, they publicly are “diluting the authority of the publication by invoking ‘the policy of **‘sit penes authorem fides’ [let the author take responsibility for it]**: We only set it down as it was related to us without putting any great weight upon it’.” (Chubin and Hacket, 1990; p. 86, quote from Merton. Emphasis added). Chubin and Hacket (1990) presented this kind of disclaimer when they referred to the evaluated and not-evaluated publications of the Philosophical Transactions of the Royal Society (86). They based their affirmation quoting (Merton, 1973; p.468), who referred to this issue in the context of the historical origins of journal’s publications. We think that this is the base and the rational supporting the present publications of non-reviewed papers as it is the case, for example, of several conferences (where acceptance of submission are done for reviewed papers as well as for non-reviewed ones.) In the same vain, it was established for the Database PubMed Central (following suggestion made by Harold Varmus, then Director of the National Institute of Health: NIH) that “the non-peer-reviewed reports will also enter PubMed Central...reports may never be submitted to a Journal for a traditional peer review, yet will be deposited in PubMed Central...” (Weller, 2002, p.320). Gordon (1978), p. 81) championed the idea of adopting an optional published refereeing where “the publication of almost everything will be guaranteed with the requirement that referees’ comments be published along with the articles.” (Quoted in Weller, 2002, p.317). Gordon was probably thinking 1) about meeting the essential three objectives of scholar/scientific communication avoiding jeopardizing them as a consequence of a process of peer reviewing implemented in order to achieve the forth objective, oriented to papers’ quality assurance; and about minimizing other observed weaknesses of peer reviewing/refereeing processes, such as bias, conflict of interests, etc. Robin and Burke (1987), as another example, affirm with regards to journals, that “Editors should reserve space for articles...that receive poor review...they should publish unreviewed material with a disclaimer...” (Cited in Weller, 2002, p. 317)) Robin and Burke are certainly referring to the same disclaimer we quoted above: **‘sit penes authorem fides’ i.e. let the author take responsibility for it.**

Peer review/refereeing processes, should be part of the means to be used into achieving the three basic objectives of scholar/scientific communication via publishing. It should not achieve one, or more, of these objectives, jeopardizing one or more of the other objectives. Consequently, there might be a need for several processes of peer reviewing

in order to effectively achieve several objectives. Kaplan (2005), for example, proposed two different peer reviewing/refereeing processes for the effective achievement of the two objectives he is proposing, where each process represent a method to achieve one, and only one of the objectives. Again, this may be seen as a solution for peer reviewing/refereeing problematic in the context of the Systems Approach and its pragmatic/teleological basis. In this context, means should not be confused with the ends, and if one single means cannot be effective in the achievement of multiple objectives so multiple means should be considered, designed and implemented.

On the other hand, some characteristics, intrinsic to peer reviewing/refereeing processes, generated other objectives, which are different to those stated above in the context of scholar/scientific communication. Any action taken in order to achieve these newly generated objectives should not interfere in the achievement of the original ones. Means should not be confused with ends. Means should be subordinated to the ends, not vice versa. The expectations, based on the intrinsic characteristics of peer reviewing/refereeing, that generated new objectives, are the following:

- A. The “anticipation of peer review may raise the general quality of scientific research because scientists may draw on their prior experience as reviewers and reviewees to *imagine* the criticism that reviewers might have of their work and take measures to forestall and to remedy them.” (Chubin and Hacket, 1990; pp. 88-89)
- B. Peer reviewing is expected to provide constructive feedback that probably will improve the quality of the article being reviewed. As we indicated above, Griffith (1982) refers to this issue as the “social function of refereeing” affirming “that referees make [or are suppose to make] significant contributions to the quality of reported work.” This social function of the peer reviewing might be seen as an integral part of scholar/scientific communication, in the most general sense of the term. It is arguable if this fact makes “social function of refereeing” an objective of the scholar/scientific communication, but it definitely is a highly valued by-product of peer reviewing processes that should be cared about. As we will describe below, an informal Participative Peer-to-Peer Reviewing might fulfill this social function, and might be decoupled (but still complementing) a more formal peer reviewing.
- C. Peer reviewing/refereeing increases the confidence with which non-specialists, including laypersons, may use scientific results.” (Chubin and Hacket, 1990; p. 89)
- D. Peer reviewing/refereeing “reinforces the stratification system of science providing referees, who are more likely to be established, productive scientists, with a steady flow of privileged information about the work of others...Few scientists resist making some use of knowledge gained from reviewing.” (Chubin and Hacket, 1990; p. 89; citing Lievrouw, 1988) This intrinsic characteristic is a coin of two sides. On one hand reviewing supports more knowledge

communication among scholars and scientists. In the other hand, if the system is abused, it can generate bias, unfairness, conflict of interest, plagiarism and other very undesirable effects generated by this intrinsic characteristic. In the latter case, peer reviewing/refereeing not just interferes with the essential objective of scholar/scientific communication but it corrupts, degenerates and vitiates the entire process. As we saw above, more than few cases have been reported on this kind of corruption.

Actions oriented to meet expectations/objectives A, B, C and D should not interfere, compromise or put at risk the achievement of objectives 1, 2, 3 and 4. Otherwise, the whole peer reviewing/refereeing process would be denaturalized; case in which it might be less harmful to exclude it or to embrace some form of optional published refereeing, as Gordon (1978) suggested, and we mentioned above.

The modeling or the design of any system, process or procedure of peer reviewing/refereeing should start by making explicit the objectives to be met by it, as well as the priorities among these objectives. In our opinion, the priorities might have the following sequence: 1, 2, 4, 3, B, C, A, D. Other sequences may also be legitimate. The important thing, in our opinion, is to make explicit the objectives to be achieved, and to design as many processes of peer reviewing as it is necessary as to have the assurance of not putting at risk objectives of higher priority by means of the implementation of a reviewing process oriented to achieve an objective of a lower priority. It is highly probable that it might not be feasible to meet several objectives with the same peer reviewing method or process. In such a case we can choose between removing some objective(s) with lower priority and having several peer reviewing methods in order to achieve different objectives. All this should be done taking into account the cost restrictions of the reviewing process. A very good example is the suggestion made by David Kaplan (2005) by means of which he proposed two different methods of peer reviewing/refereeing for the two objectives he thinks should be addressed in scholar/scientific publishing. Above we referred to the two objectives he stated, and below, in the next section, we will describe the two methods he is proposing for effectively achieving the two objectives he mentioned.

Proposed Solutions and Alternatives

Faced with the huge evidence regarding the multiple weaknesses of peer reviewing/refereeing, authors and editors have been proposing several solution, modifications and even alternatives to peer reviewing. We will try here to briefly enumerate several of them. Later, we will combine some of them in a systemic we will try to elaborate regarding scholar/scientific publishing, in the context of knowledge communication.

As we said above, Kaplan (2005) affirms that “Peer review subsumes two functions. First, peer reviewers attempt to improve manuscripts ... The second function of peer review is to render a decision about the ... significance of the findings.” Consequently Kaplan proposes “reforming peer review so that the two functions are independent.” In a

first phase “Review of a manuscript would be solicited from colleagues by the authors. The first task of these reviewers would be to identify revisions that could be made to improve the manuscript. Second, the reviewers would be responsible for writing an evaluation of the revised work. This assessment would be mostly concerned with the significance of the findings, and the reviewers would sign it. After receiving the final assessments from several different reviewers, the authors could decide to submit to a journal, sending the manuscript and the signed reviews together. The editors, carrying out the second function of peer review, would then decide to publish or not based solely on this material. The reviewers' identities would be revealed in the publication.” (p. 10)

We might infer that Kaplan’s solution or method places objectives A, B C, and D (the intrinsic ones) in the hands of the reviewers of the first phase, while objectives 1, 2 and 3 (the extrinsic ones) are to be met in a second phase by the editorial reviewing process; and the achievement of objective 4 (quality assurance) might be shared by both kinds of reviewers. In our opinion, Kaplan’s solution or method would significantly lessen, if not eliminate, some of the weaknesses of peer reviewing. In Kaplan’s words “This revision of peer review would change the incentives for all involved. The authors would tend to publish results that represent more complete findings and be more satisfied with the outcome, because they could exert lots of control over the review process. The reviewers would tend to be more honest in their evaluations, not wanting to praise work they consider flawed, because their names would be attached to it. Reviewers would not give a cursory and willfully negative evaluation, because the authors could simply not forward their comments. It would be in the reviewers' best interests to help improve manuscripts that have flaws but are potentially important... **Peer review is broken.** It needs to be overhauled, not just tinkered with. The incentives should be changed so that: authors are more satisfied and more likely to produce better work, the reviewing is more transparent and honest, and journals do not have to manage an unwieldy and corrupt system that produces disaffection and misses out on innovation.” (p. 10; emphasis added)

The idea of a peer reviewing as a broken system which requires a complete overhauling or changing is shared by many authors and editors. Mitroff (1982), for example, affirms that the natural tendency of solving this problem is “always to specify more of the same: tighter reviews procedures, and the like. However...why should new procedures of essentially the same kind as the old be expected to alleviate the problem, especially if one begins to suspect that the very form of the procedures may themselves be part (or at least a reflection) of the problem?” (p. 228)

Glenn (1982), being editor of *Contemporary Sociology*, affirmed that “If substantial improvements are not made, we should **stop pretending that the review process is something that it is not**...Editors, department heads, deans and other concerned persons should acknowledge the weaknesses of the reviewing process and act accordingly.” (p. 212; emphasis added)

Some editors and authors even proposed to completely eliminate the reviewing process, as it is currently known. With this approach pre-publication reviewing would be

substituted by post-publication reviewing made by the readers of the paper. Some authors proposed this solution (or dissolution in Kenneth Ackoff's term) of the problem seriously and others did it semi-facetiously. Perloff and Perloff (1982), for example, referring to peer reviewing weaknesses and specifically to the research made by Peter and Cesi (1982) on this subject (which was briefly described above) recommend three possible solutions: blind reviews, paid reviews and no reviews at all. "Not entirely facetiously – they affirmed – it could be proposed that one way to remove the bias associated with the reviewer's awareness of the author's identity and affiliation [and other peer reviewing weaknesses we might add], is to have no review at all. This caveat emptor [in Latin: "let the buyer beware"] approach might be viewed as a nod to the free market of ideas. Let million of flowers bloom. All one needs to get published is to write an article, submit it for publication, and pay for its publication. In this way, all individuals, whether from recognized or unrecognized institutions, would be assured of having their words immortalized. Those articles that catch fire and are cited might come from beggars, thieves, princes and future Nobel laureates. Let it all hang out: the garbage, mediocrity, and the crown jewels. One could argue that all people are "created equal," endowed with such inalienable right as the pursuit of truth via totally unrestricted opportunities to publish what they wish." (p. 233).

This "not entirely facetiously" affirmations (as their authors qualified it) have serious conceptual support in liberal philosophies. David Shatz (2004), a professor in philosophy, dedicated the whole first chapter of his book entitled *Peer Review, A Critical Inquiry*, to analyze this proposition under the light of liberal philosophies. Referring to Perloff and Perloff's suggested solution, cited above, Shatz affirmed that "Essentially, the (partly facetious) proposal is to replace 'closed peer review' with 'open peer review.' Open peer review is review by the scholarly community at large, instead of a few anonymous referees with an editor or board." On behalf of the open peer review "one may argue that closed peer review, with its emphasis on selectivity and limitation conflicts with the pursuit truth as conceived by liberalism. In his famed argument for free speech, John Stuart Mill asserted that the expression of multiple and diverging viewpoints is more likely to produce truth than would suppressing some viewpoints. Through the proliferation and collision of ideas, truth will emerge; and as for those individuals who already have the truth, they will hold it in deeper, less dogmatic way by dint of having been challenged...Yet pre-publication peer review of articles and books *prima facie* runs contrary to Mill's argument...Universities and journals seem to differ sharply in their hospitality to proliferation" (pp. 16-17). Examining the relationships of liberalism to peer review Shatz (2004) says "In short, the practice of peer review seems to sit uneasily with liberalism's commitment to the proliferation of ideas and to Mill's marketplace conception. It may be added that the publication process limits free expression even for authors whose work has been accepted...The final product in some sense is not the author's work. In this aspect, too, the peer review process does not allow for completely free expression." This is one of the reasons why in WMSCI Conferences, as well as in its collocated conference and other events organized by the IIS, authors of accepted papers are neither compelled to follow the reviewers suggestions nor to be constrained by established paradigms.

It is in the spirit of this kind of intellectual and scientific liberalism that, Gordon (1978; 1982), advocated the idea of adopting an **optional published refereeing** where authors would be given – as we textually indicated above -- “the option of (and the responsibility for) publishing the manuscript provided it is accompanied by the anonymous unanswered comment of the referee where relevant... Such a system would not only leave the responsibility of publishing with the authors, to whom it must ultimately belong, but would be more consistent with the only real, and the only truly realizable, goal of refereeing process, namely to provide an evaluation of a submitted manuscript as is practical, but without any implication of infallibility on the part of the referees, journals and author.” (p. 214). Robin and Burke (1987), also mentioned above, represent another example of approaches and solutions inspired by intellectual liberalism stand. As we said above, they affirmed with regards to journals, that “*Editors should reserve space for articles...that receive poor review...they should publish unreviewed material with a disclaimer...*” (Cited in Weller, 2002, p. 317; emphasis added) Other authors, as it is the case of Carta (1978), for example, suggested the publication of all abstracts of all rejected papers, so the reader can contact an abstract’s author if he/she have any kind of interest in the paper. (Cited in Gordon, 1998; p.213).

In the same spirit, it was established, as we indicated above, that for the Database PubMed Central (following suggestion made by Harold Varmus, then Director of the National Institute of Health: NIH) that “the non-peer-reviewed reports will also enter PubMed Central...reports may never be submitted to a Journal for a traditional peer review, yet will be deposited in PubMed Central...” Analogously, and for similar reasons, arXiv (archive for electronic preprints of scientific papers in the fields of physics, computer science, mathematics, and biology which might be accessed via Internet) “provides **pre-review dissemination**” as it was stated by Ginsparg (2003; p.317; emphasis added). Paul Ginsparg (1994; 1996a; 1996b; 2001; 2002a; 2002b; 2004), the original developer of arXiv, was awarded a MacArthur fellowship in 2002 for his establishment of this archive (Wikipedia). Ginsparg’s arXiv, PubMed Central and the solutions provided by authors and journal editors like Gordon (1978; 1982), Robin and Burke, (1987), Carta (1978) etc. are examples of intellectual liberalism in the context of scholar communications. One of their commonalities seems to be the same disclaimer we quoted above: *‘sit penes authorem fides’* i.e., **let the author take responsibility for it.**

Paul Ginsparg had also indicated that peer reviewing has several objectives and it should be more focused for its own improvement. Based on his vast experience in publications in Physics, Grinsparg affirms “that current peer review methodology...strives to fulfill roles for two different timescales: to provide a guide to expert readers (those well-versed in the discipline) in the short-term, and to provide a certification imprimatur for the long-term.” But, he adds “ the attempt to perform both functions in one step necessarily falls short on both timescales: too slow for the former, and not stringent enough for the latter...Before considering modifications to the current peer review system, it's important to clarify its current role in providing publicity, prestige, and readership to authors. Outsiders to the system are sometimes surprised to learn that peer-reviewed journals do not certify correctness of research results. Their somewhat weaker evaluation is that an article is a) not obviously wrong or incomplete, and b) is potentially of interest to readers

in the field. The peer review process is also not designed to detect fraud, or plagiarism, nor a number of associated problems -- those are all left to posterity to correct. In many fields, journal publication dates are also used to stake intellectual property rights (indeed their original defining function... But since the journals are not truly certifying correctness, alternate forms of public distribution that provide a trustworthy datestamp can equally serve this role.” (Ginsparg, 2002b; 2003, pp. 314)

Consequently, Ginsparg proposes decoupling the two basic roles he identified that peer reviewing has. This can be done, he affirms, by implementing a system for self-archiving and instant “pre-review dissemination” of submitted articles, as his arXiv does in Physics and Mathematics (and lately in Quantitative Biology, non-Linear Sciences, and Computer Science). This kind of self-archiving for immediate dissemination remove the time pressure on peer reviewing processes so they can get focused on their actual second role related to authentication and certification. Ginsparg does not propose the elimination of peer reviewing, as other authors suggested as a way of solving the problematical situation created by the frequently reported and large variety of peer reviewing weaknesses. On the contrary: he is proposing a non-reviewed self-archiving depository in order to improve peer reviewing by means focusing it in its principal role.

Both, Kaplan and Ginsparg indicate that the way of solving the problematical situation created by peer reviewing weaknesses, failure, and pitfalls is by means of meeting the different objective that peer review processes try to achieve, implementing different peer reviewing methods (Kaplan) or by differentiating pre-reviewing publishing from post-reviewing publishing (Ginsparg). Both perspectives agree with the Singer-Churchmanian pragmatic-teleological truth, which form part of the epistemological infrastructure of the System Approach. Both authors suggest improving the effectiveness of peer reviewing by means of not confusing the means with ends, by means of not inverting the means with the ends, by means of identifying different methods, procedures or systems for achieving different objectives. Both authors subordinate, subject the means to the ends. Although both proposals for solving the peer reviewing problematic are quite different, both do not contradict or exclude each other. On the contrary they can complement each other. Ginsparg’s solution allows peer reviewing processes to focus by means of differentiating between pre-reviewing publishing and post-reviewing publishing and hence, removing the time constraint and pressure from the peer reviewing processes. Kaplan solution focuses on the peer reviewing processes separating in two different methods the means of meeting two different objectives as that of providing an adequate feedback for the improvement on the article and that related to editorial decisions. Consequently, both solutions might be combined as to complement each other. Ginsparg differentiates between pre-reviewing and post-reviewing publishing and Kaplan differentiates between two different methods for meeting different objectives in the specific case of post-reviewing publishing. We will use the combination of both solutions to the peer reviewing problematic in the experimental solution we will describe below.

There is another very important issue treated by Ginsparg which is also important for the experimental solution we will be describing below. The concern of the academic and scientific communities regarding the pre-reviewing or publishing of non-reviewed

articles, because publications are at the center of the academic prestige and are an essential part of academic promotion and grant decisions. The best way to present this issue is by means of the same text used by Ginsparg.

“When faculty members are polled formally or informally regarding peer review - indicates Ginsparg - , the response is frequently along the lines "Yes, of course, we need it precisely as currently constituted because it provides a quality control system for the literature, which is necessary for deciding job and grant allocations." But this conclusion relies on two very strong implicit assumptions: first that the necessary signal results directly from the peer review process itself; and second that the signal in question could *only* result from this process. The question is not whether we still need to facilitate *some* form of quality control on the literature; it is instead whether given the emergence of new technology and dissemination methods in the past decade, is the current implementation of peer review still the most effective and efficient means to provide the desired signal.” (Ginsparg, 2003; p.314)

This is a very important statement. It goes to the essence of the problem and uncovers some implicit suppositions that are not necessarily true. The academic community does need a quality control, but peer reviewing or refereeing is not always a reliable method by itself, as an overwhelming number of studies and experiences have shown; some of which we described above. In the other hand, other indicators or measures might be more reliable than peer reviewing alone. Peer reviewing is a means for quality control, among others. It is no The Means. It is one among others. The number of downloads of a web-published article, the citation index, author’s career reviewing (as different than article’s peer reviewing), etc. are examples of alternative means for assessing quality in the published literature. New technologies create new methods and procedures for publication and scientific information disseminations, from which new quality assessment means might emerge, as actually they are emerging.

“Appearance in the peer-reviewed journal literature – Ginsparg continues - certainly does not provide sufficient signal: otherwise there would be no need to supplement the publication record with detailed letters of recommendation and other measures of importance and influence. On the other hand, the detailed letters and citation analyses *would* be sufficient for the above purposes, even if applied to a literature that had not undergone that systematic first editorial pass through a peer review system. This exposes one of the hidden assumptions in the above: namely that peer-reviewed publication is a prerequisite for entry into a system that supports archival availability and other functions such as citation analysis. That situation is no longer necessarily the case... There is much concern about tampering with a system that has evolved over much of the past century, during which time it has served a variety of essential purposes. But the cat is already out of the bag: alternate electronic archive and distribution systems are already in operation, and others are under development. (Ginsparg, 2003; pp. 314-315).

Ginsparg emphasizes in the complementary relation that exist, or might exist, between non-reviewed and reviewed publishing. Journals augment non-reviewed publishing by “some measure of authentication of authors (they are who they claim to be), and a certain

amount of quality control of research content.” Journals provide at least a minimum certification of “not obviously incorrect, not obviously uninteresting.” (Ginsparg, 2003; p. 315).

Conference contributions to their respective proceedings are somewhere in between the non-reviewed publishing (that characterizes the self-archiving and immediate dissemination procedures of archives like Ginsparg’s arXiv) and journals publishing. This “in between” is due to the conception of conferences as informal or semi-formal means of scholarly communications. Consequently, there is a high diversity in the level of peer reviewing among the conferences being organized, and there is a large variety of conceptions among scholar societies about the functions, roles and objectives of a conference.

There is a huge difference among the conceptions of conferences even among the most prestigious, old, large and veteran scholarly societies and institutes. These types of required submissions and their respective reviews encompass a very wide specter going from the extreme of requiring from authors to submit just full papers which would be reviewed through methods and processes very similar to the ones used by Journal’s editorial processes (several ACM and IEEE conferences are examples of this extreme) to the other extreme of accepting submission of no more than 50 words abstracts requiring no reviewing process (The prestigious and large INFORMS’ and IFORS’ Conferences and annual meetings are examples of this other extreme.). Acceptance rates have as well a very wide range going from 14.9% ACM’s SIGGRAPH 98, or lower, to about 100%, which it is what explicitly stated in INFORMS 2006, for example, in the following terms: “Contributed abstracts are not reviewed and virtually all abstracts are accepted.”

Proceedings are conceived as a means of relieving authors of the burden of distribution of their papers. Walker and Hurt (1990) assert that “the ‘real’ value of conferences and other meetings lies in the informal communication that takes place during, between and after the formal presentation of prepared conference papers. They also affirm that “Often all the contributed papers to be presented at the meeting...are provided to those attending the meeting as it convenes or even beforehand. Such pre-publication has many advantages and disadvantages. On the plus side it provides a simple way of ensuring the integration of the meeting into a communication process; it also relieves the authors of the burden of distribution of their presentations. Additionally, it ensures earlier and wider distribution - to more than those assembled for the meeting. However, wider and earlier distribution of material of untested quality may not be desirable. To wait until the presented material meets the test of public exposure and discussion may be preferred.” (Walker and Hurt, 1990; p. 86).

Since an essential function and value of the proceedings provided to those attending the conference as it convenes, i.e., as pre-publication, or more precisely, pre-journal-publication is to relieve authors of the burden of distributing copies of their presentations, it is evident that the respective authors have the complete responsibility of the contents of the papers included in the proceedings as if they would have distributed their own copies of their presentation or the papers which are the basis of their respective presentations.

This is similar to what Ginsparg call pre-reviewing publication, i.e., pre-journal-publication, which is the content of disciplinary repositories, as it is the case of the arXive.

This complete responsibility of the authors with regards to the papers included in the proceedings is being manifested in a clear and explicit way in an increasing number of proceedings. As example is the following text that is being included in the proceedings of several conferences organized by the IEEE:

“The papers of this book comprise the proceedings of the meeting mentioned on the cover and title page. They reflect the authors’ opinions and, in the interest of timely disseminations, are published as presented and without change. Their inclusion in this publication does no necessarily constitute endorsement by the editors, the IEEE Computer Society, or the Institute of Electrical and Electronics Engineers, Inc.”

Walker and Hurt (1990) emphasize it saying “don’t confuse the purpose of a proceedings with that of a Journal” (p. 94), and they add (quoting UNESCO’s Bulletin for Libraries 24:82-97, 1970) that “Valuable oral exchange does not usually become valuable publication simply by printing it”. They insist that “An important element in the process of transmission of scientific information and knowledge is oral communication” (p. 95).

This kind of communication, not just among scientists, but also between them and their socio-economical context is stressed by several authors. Walker and Hurt (1990) affirm that “Communication is not only necessary among colleagues and peers...but is a necessary part of the interplay among several disciplines or branches of science, between the scholarly community and industry, between the scholarly community and the government, between the scholarly community and the lay public, and among all these segments of society” (p. XVI). Conferences which main purpose is to serve as a forum for interaction of scholars, practicing engineers, consultants, managers and users of science and technology are contributing to this kind of communication, cited by Walker and Hurt. Such conferences are more informal than scientific conferences, which are, according to Walker and Hurt, in a middle point between the formality of the journal and the informality of other means of scientific communication. These conferences cannot follow the middle level formality of the scientific conferences without risking their main purpose, which is to relate scientists of different disciplines and engineers, consultants, practicing professionals and managers. A conference that announces, as its main purpose, to be a forum for scientists and non scientists cannot be valued according to scientific conferences standards, if there is any.

Conferences of this kind would not be, for example, peer reviewed or peer refereed, and in such a case, their proceedings should not be announced as such. Such kind of conferences may accept just very short abstract submissions. And, if this is the case, there is no more reviewing process than the required for the topic of the presentation, whether it fits some area of the conference or not. Very important and prestigious conferences have been repeatedly and explicitly stating that the papers included in their proceedings

cannot be considered as peer refereed. For example, The Thirty-Seventh Annual ACM Symposium on Theory of Computing (STOC 2005) clearly stated that:

“The submissions were not refereed, and many of these represent reports of continuing research. It is expected that most of them will appear in a more polished and complete form in scientific journals.” Ronald Fagin (IBM Almaden Research Center), Forward, STOC 2005 Program Chair.

.Resuming, we can say that the expected, and the planned informality, or semi-formality of conferences generate a large continuum of conceptions which goes from the extreme of similar-to-journal-reviewing to similar-to-non-reviewed-publishing that characterizes disciplinary self-archiving of instantaneous dissemination, as it is the case of Ginsparg’s arXiv.

Different levels of authentication and certification is suggested by Ginsparg, and different levels of authentication and certification are also found in the diversity found in conference conceptions. A diversity of levels of authentication and certification is an essential feature of the systemic model of scholarly publishing we will briefly describe below.

Is more research/reflection needed in the area of scholarly publishing and peer reviewing?

In the brief research we did to produce this paper, we noted that:

1. Many studies and research have been done on the weaknesses of peer reviewing, but we could not find studies or research oriented to show its strengths. Weller (2002) concluded her book (based on large research on the subject) in a similar way, asserting that “Studies that documented weaknesses of editorial peer review proved easier to identify than studies that document its strengths.” (p. 312). This is why
 - a. Some authors suggested the elimination of peer reviewing, offering some alternatives to it. Regarding this issue Weller (2002) affirms that “There are those who suggest that the traditional role of editorial peer review in the publication process be eliminated.” (p. 321).
 - b. Other authors offered some ideas about improving or reforming it, but we found no research/reflection on these ideas.
 - c. Faced with this situation, authors who are still supporting the editorial peer review recognize its inherent weaknesses but recommend accepting it because they cannot find anything better. An epitome of what this kind of authors suggest is Relman’s conclusion: “Despite its limitations, we need it. It is all we have, and it is hard to imagine how we could get along without it” (Relman, 1990; p522). Weller (2002) finished her book in the following terms: “Like a Democracy, editorial peer review is messy and

does not always work as it should, but it is essential to the integrity of scientific and scholarly communication.” (p. 322)

2. Given this situation, shouldn't more research be done, not on what peer review actually is, but on what peer review should be, or how it should be done? It was not easy for us to find this kind of research in the literature. Almost all research is oriented to what peer review actually is and how it has operated in the past and in the present. Research/reflections are strongly needed on:
 - a. How peer review might be improved?
 - b. How scholarly publishing might be achieved more effectively?
 - c. How peer review might be completed and complemented?
 - d. What alternatives to peer reviews may have less weaknesses and more strength, and in which cases or conditions?
 - e. What alternatives to the present scholarly publishing system might be better?
 - f. How can we measure the quality or the effectiveness of a given peer reviewing process?
 - g. Is there any consensual standard on the effectiveness of peer reviewing? If not, how can we identify or produce it?
 - h. How new technologies in Informatics and Cybernetics (communications and control) can be used to improve the efficacy (efficiency and effectiveness) of scholarly publishing and scientific information communication?
 - i. How new technologies in Informatics and Cybernetics can be used to improve the efficacy of knowledge generation, communication and management?

3. Most research/reflections we found in the literature are related to journals' peer reviewing. Some research has also been conducted on grants and books peer reviewing. We found, up to the present, no research done on conference peer reviewing. We will continue our search for this kind of studies, but meanwhile it is good to suggest that research and reflection-based studies should be done on these conferences peer reviewing as well as on conference organization methodologies. Regarding this issue, research/reflections may be oriented to answer general questions as the following:
 - a. When authors affirm that reviewing processes in conference organizations are informal, or semi-formal, as compared to journal peer reviewing, what does this informality or semi-formality mean?
 - b. How amplified are the weaknesses found in journal peer reviewing, in the case of conference papers reviewing.
 - c. Should journal peer reviewing be applied to Scientific conferences?
 - d. Should journal peer reviewing be applied to Engineering conferences?
 - e. Should journal peer reviewing be applied to Management conferences?

- f. Should journal peer reviewing be applied to conferences which its purpose is to be a forum where scientists, engineers, managers, consultants, professionals and practitioners meet to interchange ideas and share experiences, besides presenting and publishing papers?
 - g. Should all kind of conferences be ruled by scientific conferences' standards? If so, what are these standards? Who decide them?
 - h. Who decide the quality level of a conference? Their participants? Their reviewers? Their session organizers? External self-appointed assessors? All of them? If so, what weight should have each kind of assessor in the final assessment?
4. For the three kinds of research/reflections we are suggesting above, we propose to use the approaches of:
- a. Action-Research, and/or
 - b. Action-Reflection, and/or
 - c. Action-Design.

This is because peer reviewing, be it for journals, grants, books or conferences, is a social process, based on social systems and on knowledge generation and communication. Since it is not just a thinking activity, but also a decision/action one, we should not base its research/reflection just on thinking processes, but also on action/decision ones; not just on knowledge, but also on experience earned through action and its pragmatic consequences. It is not just a matter of truth, but also of efficiency and effectiveness.

A first step that could be given in the research/reflection suggested above, in (3), might be a participative symposium on conferences' quality as the one we tentatively show below.

Inquiry for a Possible Solution

A First Step in a Process of a Possible Solution

The problem of quality control has been addressed with more frequency for journals than for conferences, symposiums, colloquiums, etc. Consequently, it is important to increase the interchange of opinions in the area of quality control in Scholarly and Professional Communication in areas different to the journals'; to promote personal and group reflexive processes on the subject; to establish ways of communicating these opinions and reflections; and to publish them and to consider the possibility of research projects on the subject. A first step regarding this issue is the organization of symposia and conferences regarding effective and quality symposia and conferences.

So, we decided to make this first step organizing symposia on the indicated topic, accompanied by informal sessions with a conversational format. Consequently, initially small symposia were organized around topics related to the indicated issue and sessions

with a conversational format were organized around the subject, in the context of the conferences organized by IIS in 2006.

Questions and issues that were posited in the context of these symposia and informal conversations were the following:

- Who determines the quality of a conference? The participants? The organizers? The Program Committee? Is it possible to have a quality conference with no quality authors but with good quality reviewers and organizers?
- What is more indicative of the quality of a conference: the quality **average** of its papers and their respective presentations? The quality of the best papers? The papers and presentations of the lowest quality?
- As we mentioned above, Campanario (1995) documented many cases of peer-review rejections of works that later turned out to be accepted as cases of important discoveries, including eight authors who eventually earned Nobel prizes after their prize winning ideas were initially rejected by reviewers and editors. Furthermore, studies show that 20% of published articles were previously rejected and over 50% of rejected manuscripts were eventually published, and some of them are among the most referenced papers. Accordingly, isn't it better to lower the probability of rejecting good papers, with the disadvantage of taking the risk of including some low quality papers? Who should decide the trade off implied in this situation? The conference participants? The organizers? The invisible hand of the market?
- Is it possible to establish quality for all kinds of conferences, no matter their respective disciplines, no matter if they are interdisciplinary or not, no matter their established purpose? If so, is it possible to establish these standards statically? Should they be dynamic? Should they change with their environmental changes? If so, who should make the respective changing decisions? The participants? The organizers?
- Is it **desirable** to organize conferences with the purpose of bringing together researchers and professional practitioners, academics and consultants, scholars and managers? Is it **feasible** to do it just following academic quality standards?
- Should the conferences be classified and, accordingly, have different kinds of quality standards? In such a case, who is to decide the quality of a conference? The conference participants? Some external group or board?
- Is it desirable to organize multi- or interdisciplinary conferences? If they are desirable, shouldn't different quality standards be followed according to the different disciplines being included in the conference? Is it feasible to have an adequate level of tolerance among scholars from different disciplines with different conceptions about quality standards?

- Can we apply the concept of “**bottom-up quality**”, as it is done in several social endeavors (including software and information systems development) to conferences organization? Should all conferences be organized with the “**top-down quality**” approach which characterizes journals’ standards? Are “bottom-up quality” and “top-down quality” contradictory opposites? Or, are they polar opposites? If they are polar opposites, can’t they be related in such a way as to produce a synergy between both of them?
- Is it thinkable to organize a conference with a bottom-up approach in such an organic way as to generate quality as an **emergent** property? Is it possible to think that the Systems Approach and Complex Adaptive Systems theories and methodologies may support the idea of organizing conferences with emergent quality? Could it be thought as a research project or program?
- Should a conference, as a techno-social system, be an open system? Should it be a closed one? What would be the advantages and disadvantages of being open and closed?
- Is it desirable to open a conference in such a way as to take the risk of including some low quality papers or presentations, in order to reduce the probability of refusing good papers?
- Can a publishing organization hold conferences in order to publish their proceedings and the respective best papers in the journals they are publishing? Or should the conference organization be limited to a group of scholars and learned societies?
- Several authors have concluded that students and young scholars are, in average, better as reviewers than more senior scholars. Accordingly, should they be part of conferences program committees? If so, what should be their participation percentage?
- Is it desirable for conferences to follow journal standards of peer-reviewing? Is it feasible with the timing restrictions of a conference? Is it possible with the functions of the conferences as an informal, mostly verbal, communication process to follow journal’s standards which have different functions according to authors? Is it feasible? Is it desirable?
- Is it legitimate to try different approaches for papers acceptance in conferences? As we said above, several studies, and journal editors, concluded that there are significant weaknesses in journal’s peer reviewing and peer refereeing processes. Consequently, several alternatives have been proposed for journal publishing processes. These weaknesses may be amplified in conferences’ organization. A frequently used alternative in conference organizations is to base the acceptance for presentation in abstracts, no more that 50 words abstracts in some prestigious conferences. Other frequently used alternative is to organize focused conferences related to a scientific discipline and to apply a peer reviewing process similar to those

used in journals but, perhaps, in a more informal way. The first alternative is usually used for large conferences and the second one for small focused ones. Are there other alternatives? Is it possible to combine both of them? Shouldn't other alternatives be explored, especially if we take into account the frequent studies conclusions and expert opinions with regards to the weaknesses of the peer refereeing process? Can the reviewing process be opened to non-peers? Can a user or a potential user give an opinion with regards to the usefulness or importance of a given technological solution? Should conferences be oriented just by epistemological perspectives? Should conferences be oriented just for original research? May they be also oriented to innovations? If yes, shouldn't practicing engineers, consultants, managers and users also participate in the conference with presentations? Many questions like these can be formulated, and the possible alternatives to peer refereeing in conferences would surely depend on the answers that may be given.

A very first step has already been done in the context of the conferences organized in 2006 by The International Institute of Informatics and Systemics: IIIS. This first step is a first increment in an action-Reflection, Action-Design and Action-Research Project, in the context of an Incremental Planning and an Incremental-Evolutionary Methodology (Callaos, 1992; 1995a; 1995b; Callaos and Callaos, 1992; 1994; 1995a; 1995b). This first evolutionary incremental step included the following aspects:

1. A small symposium on Knowledge Communication and Conferences (KCC 2006) was organized in the context of 10th World Multi-Conference on Systemics, Cybernetics and Informatics WMSCI 2006) which papers were included in the WMSCI 2006 proceedings.
2. A small symposium was organized on Knowledge Communications and Peer Reviewing (KCPR 2006) in the context of the 4th International Conference on Information Technologies, Systems and Applications (CITSA 2006), which papers were included in the CITSA 2006 Proceedings.
3. Eighteen (18) informal conversations, of two hours and a half each, were held with regards to the problematic of increasing the effectiveness of conferences and peer reviewing.
 - Appendix A contains a paper draft which served as input to these informal conversations, where the nature of such kind of conversations were explained and its characteristics were contrasted with those of traditional conferences.
 - Appendix B contains the very open and flexible guidelines given to the moderators, and co-moderators of each of the 18 conversational sessions held.
 - The output of these 18 conversational sessions were resumed and collected in Appendix C.

4. An experimental first step was given in the implementation of a Participative Peer-to-Peer Reviewing (PPPR) in the context of the conferences organized by IIS in 2006. This experiment provided the experience, the information and the knowledge to apply PPPR, in a non-experimental environment, as part of the reviewing process in the organization of the conferences being arranged by IIS in 2007. PPPR is also being considered for its application in the reviewing process of the Journal of Systemics, Cybernetics and Informatics (JSCI). PPPR will be explained with details below, as part of the model and solution we are proposing in the context of the action-reflection, action-design and action-research paradigm.
5. An experimental first step was given implementing electronic pre-conference and post-conference virtual sessions associated with the traditional face-to-face sessions held at IIS' Conferences in 2006. This experiment provided the experience, the information and the knowledge to implement this kind of virtual session for IIS' future conferences as an expansion of the usual conferences.

This 5-dimensional first step generated the experience, the information and the knowledge required for the design of the second incremental step, part of which is

- A. To attempt a first step in the modeling of an integral, integrative and integrated small system for scholarly publishing. This model will be described below. This model will be based on both: the experience gotten in the implementation of the 5-dimensional first step described above, and the literature review that was prompted by this new experience and the information gotten, especially from the 18 conversational sessions.
- B. To find the financial support to implement an experimental system developed for IIS according the model indicated in A, which will be briefly described below.

A Systemic Model of Scholarly and Professional Publishing

As integral part in the design and implementation of a second methodological increment in the context of action-reflection and action-design approach, and with the experience gotten in the implementation of the first step, as well as with the information and knowledge generated in the symposia and the informal conversational sessions organized on the subject, the following activities have been conceived:

- To continue organizing the symposia organized in 2006, and to organize a more general one on Knowledge Generation, Communication and Management, in order to provide context to the symposia we referred to above.
- To try an initial model for a systemic model of scholarly and professional publishing.

- To implement a prototype of the model in order to test it and to gain the experience, information and knowledge that would allow an improvement of the model, and its associated implemented system, in the context of an evolutionary incremental methodology, and according the approach of Action-Reflection, Action-Design and, probably, Action-Research.

Our objective in this session is to briefly describe this initial and experimental systemic model for scholarly and professional publishing.

The model will be a systemic one because the focus will be on the **relations** among its parts and the **interactions** with its environment. Consequently, the system by means of which the model will be implemented will be **open, integral, integrative and integrated**. As more of these characteristics are achieved the model and the system will be more systemic, and the generation of **synergies** and **emergent properties** will become more probable

There are several models, with different levels of comprehensiveness and details. The most comprehensive, detailed and complete models we identified could be found in Björk's "Scientific communication life-cycle model" (2005a; 2005b) and in Hars' "Object-oriented model of scientific knowledge" Björk used IDEFO (derived from the Structured Analysis and Design Technique: SADT; and developed by the US Air Force under its ICAM (Integrated Computer Aided Manufacturing), from where it got its acronym: Icam DEFinition zero: IDEF0). Hars used the Unified Modeling Language (UML) because it is the most used in object-orientation one. IDEFO is mostly used in manufacturing modeling, and especially in Integrated Computer Aided Manufacturing. UML is mostly used in Software Engineering, especially in the specifications of software requirements and design in an object-oriented programming and/or object-oriented database environments.

Björk's model is mostly an *empirically descriptive* one, while Hars' is basically a *theoretical* one and oriented to future possibilities that might, and probably would emerge with the new advances in information technologies. The first is mostly oriented to describe *what* the present scholarly system actually is; the second combines theoretical considerations of *what* scientific knowledge is with *what* might be possible with information technologies in order to achieve more effectiveness in the knowledge generations, communications and management. Our model is dynamically between both models and oriented to *how* we might go from what the scholarly publishing system is now to what it could become by means of using the information technologies to make it more effective including the improvement of peer reviewing and the inclusion of alternative quality control means which are feasible with an adequate use of present information technologies. Our model is a first step in a possible path that might take us from the *present* scientific communication life-cycle to a possible future knowledge network, as, for example, the one conceived by Hars (2004). The implementation of the first step (or "increment" using the language of Incremental Planning), briefly delineated below, will provide the experience, information and knowledge for conceiving the next step or increment, and the implementation of the second increment will provide the input

for conceiving and designing the third increment, and so forth. This is why we think that our model is *dynamically between descriptive and prescriptive models*.

Our modeling process is systemically and cybernetically oriented. Its basic activities, conception, analysis, design, development, implementation and deployment are not done in series but in parallel, which allows for *cybernetic loops of negative feedback, positive feedback and feedforward* among them. Consequently, the dynamic process of going from where we currently are to where we could be in the future (with a more effective scholarly and professional communication), could be an adaptive process, supported by a learning process, in the context of action-reflection, action-learning, action-design and action-research. Details regarding this kind of cybernetically systemic methodology can be found in Callaos, 1992; 1995a; 1995b and in Callaos and Callaos, 1992; 1994; 1995a; 1995b). This evolutionary incremental methodology proved to be especially effective in about 80 academic projects in information systems development, and in about 130 real life projects of information systems development, implementation, deployment and maintenance in the largest private and public corporations in Venezuela in the last 18 years. About 20 millions of software's lines of codes were developed and deployed with this methodology, and the first million of lines of code had been in operation and in maintenance for about 10 years, which is not very usual in the domain of information systems development, especially in Venezuela where the corporative environment is highly dynamic and volatile. So, it is highly probable that this methodology will also be effective for the implementation of the below briefly delineated first step, as well as for the implementation of the next steps and increments. The experience we have regarding this methodology is mostly in corporative contexts. We have had less experience with this methodology in the case of global information systems, and this is why it is advisable to be prudent and cautious in its application in this domain. Prudence and cautiousness, in the context of this kind of methodologies, means that the conceiving-designing increments should be as small as possible, specially the initial ones. This is why the first increment is a humble one, especially if we relate it to what has already been achieved by very known and prestigious systems as it is the case of Ginsparg's ArXiv, the increasing number of institutional and disciplinary repositories that are being developed and implemented by the most prestigious universities and research centers, as well as the increasing number of successful collaboratories, and other knowledge infrastructures, as on line encyclopedias and dictionaries. What we are attempting here is quantitatively smaller, a lot smaller in both, the number of articles to be archived as well as in the initial search facilities. But, we hope it will be larger in integrating related applications to the central archive, as it is the case, for example, of the integrated Participative Peer-to-Peer Reviewing, which, in our opinion, will help in lessening the weaknesses of traditional peer reviewing, not substituting it, but complementing in order to allow it to be more focused and serving less objectives than what it is attempting to meet currently in the traditional publishing life-cycle.

Along with an increasing number of authors, we think that the peer reviewing weaknesses and ineffectiveness is intrinsic to a knowledge communications model restricted by the limiting factors of paper-based publishing, and that electronic publishing and an adequate use of Information Technology would certainly solve, resolve or dissolve most of these

weakness and lessen the rest of them. Consequently, our proposed model will be based on Information Technology but with components oriented to the required bridging function with present paper-based publishing and present academic culture and values regarding knowledge generation, communication and management.

A long range project is required for the conception, design and implementation of a comprehensive Global Information System for Knowledge Generation, Communication and Management (GIS-KGCM). A first conceiving-designing-implementing step will not be a small one and it will be applied to the areas of Systemics, Cybernetics and Informatics (SCI). So a tentative name for an Internet-based Information System for Knowledge Generation, communication and Management might be Scibrarium, i.e. a cyber-librarium in SCI (Systemics, Cybernetics and Informatics). If the experiment is successful SCI might start also representing other areas in Science. Alexander Hars' (2003) Cybrarium, which is collaboratory in the domain of Information Systems, inspired us for possibly naming Scibrarium the knowledge infrastructure we are envisioning in a long range project. Scibrarium will probably have the following general characteristics:

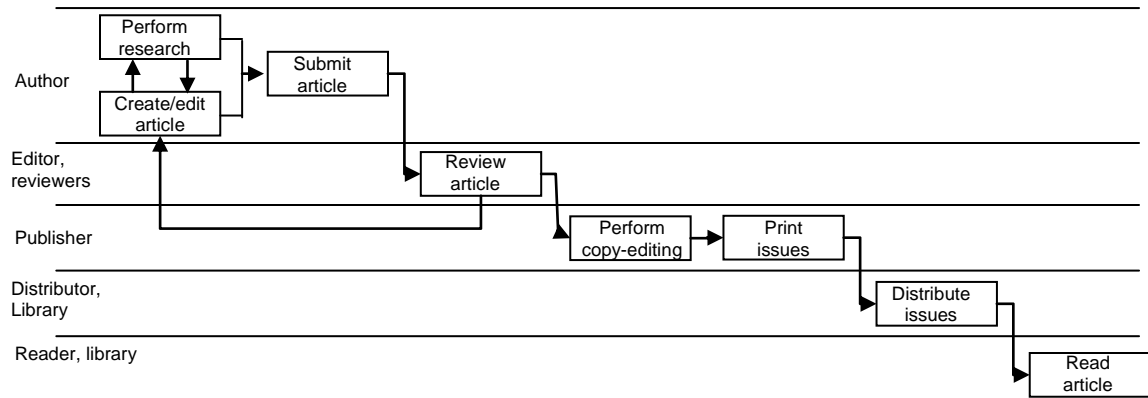
- A Global Information System (GIS) with the general function of supporting Knowledge Generation, Communication, Management (KGCM) by means of integrating different kinds of Knowledge Infrastructures and different Information Technology's tools: pre-print and post-print archive, collaboratory, Participative Peer-to-Peer Reviewing, electronic journals, hybrid electronic journals, electronic conferences, electronic encyclopedias, electronic dictionaries, electronic seminars, electronic meta-journals, etc. In the first step, IIS will sponsor the design and implantation of a web-based information system that would contain essential functionalities of pre-print archives, Participative Peer-to-Peer Reviewing (PPPR) which IIS has been using in their conferences' organization, some basic aspects of collaboratories, post-print services of the articles published in IIS Conferences' Proceedings and in its hybrid journals. In this first step, support for Readers' Appraisal (RA) of the articles they are reading will be provided. RA complements PPPR in the context of a Participative Reviewing (PR) and bottom up quality assessment and control. A general reader might not be a peer of the article's author he, or she, is reading, but they still have an opinion regarding the article that is worth to be collected. But, since the reader might not be an author's peer the data collected with this regards should be differentiated from that of peer reviewing. Information collected fro PPPR an RA might have different meanings and might lead to different conclusions, so they must not be confused. They should be separated even the fact that both might complement each other. Authors, readers and decisions makers should be provided with these two kinds of information so they can improve their performance. The reader will have the alerts we might need to give and he might need in order to have a better understanding of the article. The author should have information in order to improve, in future version, the content and the form of her, or his, article. Academic and grants decision makers might need both kinds of information in order to improve their decision making which might depend more in peer reviews' result, or final user assessment (the rest of the academic community and the society at large), or both kinds of information, depending on the nature of the

decision to be made, the purposes of the decision and the regulations of the organization in such a problematic context.

- The GIS in KGCM, or Scibrarium, is conceived as systemically and cybernetically integral, integrated and integrative. Implementations In initial phases might have a mixture of different kind of services usually provided by different kinds of knowledge infrastructures, but this mixture or hybrid services will continually be integrated with the support of the system's users. More levels of integration will be achieved in an incremental-evolutionary process.
- The GIS in KGCM, or Scibrarium, will be based on a systemic model electronic scholarly communication and publishing where it becomes feasible to parallelize activities that had to be done necessarily in series in paper-based publishing environment. The importance and usefulness of this parallelization cannot be underscored because it allows for **cybernetic loops** among them. **Negative feedback** will provide the regulations and check and balances required for different levels of quality control. Adequate **positive feedback** is necessary for the generation of synergy and constructive emergent properties, which is a basic purpose in human collaboration in general, and a basic function of collaboratories. **Feedforward** loops are necessary in learning and adaptive processes. These three kinds of cybernetic loops will be applied in:
 - Conception of the model dynamically being proposed for academic and professional Knowledge Generation, Communication, and Management (KGCM).
 - Methodology supporting the conception-design-implementation in an incrementally evolutionary process.
 - In the structure and architecture of the system being implemented (GIS in KGCM or Scibrarium), as well as in its use.

As we indicated above the traditional model, being mostly followed currently in academic communications, or scholarly publishing, is product of a paper medium used in its implementation. An increasing number of authors are emphasizing this issue, and, in our opinion, it represents one of the principal causes of the weaknesses and ineffectiveness of peer-reviewing.

Several peer reviewing weaknesses are associated to the required serialization of most activities used in traditional scholarly communication and publication. A direct consequence of this serialization (product of the paper-based publishing media) is the generation of a linear model, or a highly linear one. The traditional general model was very well represented by Hars (2003) and it is shown in Figure 1 (Hars, 2003; p12).



Source: Hars 2003, p. 12

The Traditional Publishing Process

Source: Hars 2003, p. 12

Figure 1

Notice that in this traditional model there are only two cybernetic loops, one is internal to the researcher/author and only one is external to him, or her. Furthermore, this unique cybernetic loop is from the editor and the reviewers to the authors, so it is a negative feedback, a regulatory one. It regulates what might be published and what must not be published in a top-down kind of quality control. Quality is decided and regulated by authorities (editors and reviewers). For profit publisher appoint editors who appoint reviewers. These scholars are what has been called “science gate keepers”. But a frequently raising question is “Who control the controllers?” In a strictly top-down quality control it is not easy to identify meta-control or auditing procedures. Even if we have an adequate answer to this question, and we do have meta-control and auditing procedures, what is evident is that there is just one negative feedback loop in the traditional publishing process. There is no synergic positive feedback, at least no explicit ones. There is no feedforward loops, at least not explicit ones. Consequently, learning processes are not supported and the structure is not adaptive, at least not sufficiently adaptive.

Linearity, seriality, and lack of explicit cybernetic loops have been explicitly present in most – if not all – descriptions and models of the traditional scholarly publishing communication process. We think, and we hypothesize, that most weaknesses of peer reviewing – if not all- are caused by this linearity, lack of explicit cybernetic loops and a low degree of activities parallelization; all of which will be explicit and essential characteristic of the model we will suggest and the information systems we will be developing for the knowledge infrastructure we are envisioning.

The literature review Björk (2005a; 2005b) did regarding the traditional publishing process explicitly showed a high degree of linearity even in models that take into account the new possibilities allowed by new information technologies, including the Internet. Based on Swisher (2005), Björk (2005b) presented “An illustration of the scientific communication process including facets of both Garvey/Griffith [1965] model and Hurd’s [1996; 2004] additions to it.” The diagram of Figure 2 is the same one Björk

(2005b) and Swisher (2005) showed, and it visualizes the very high degree of linearity and the complete lack of cybernetic loops. This kind of models which are non-adaptive, non-flexible and with highly serialized activities is a legacy of paper-media based scholarly communication and publishing. We think there are no more reasons why this model, that showed its ineffectiveness, especially in peer reviewing and quality control, should continue to be followed in the Internet, specially in the web-based knowledge infrastructure.

Beside the cybernetic loops, other important relationships missing in the traditional publishing process is its desirable – even necessary – interaction with the industrial and corporative sectors, as well as with the society at large. Björk (2005a; 2005b) adds this feature in his “Scientific communication life-cycle model”. It can be seen from Figure 3 that for Björk the most essential and general of what he calls *stages* of the scientific communication life-cycle are the following:

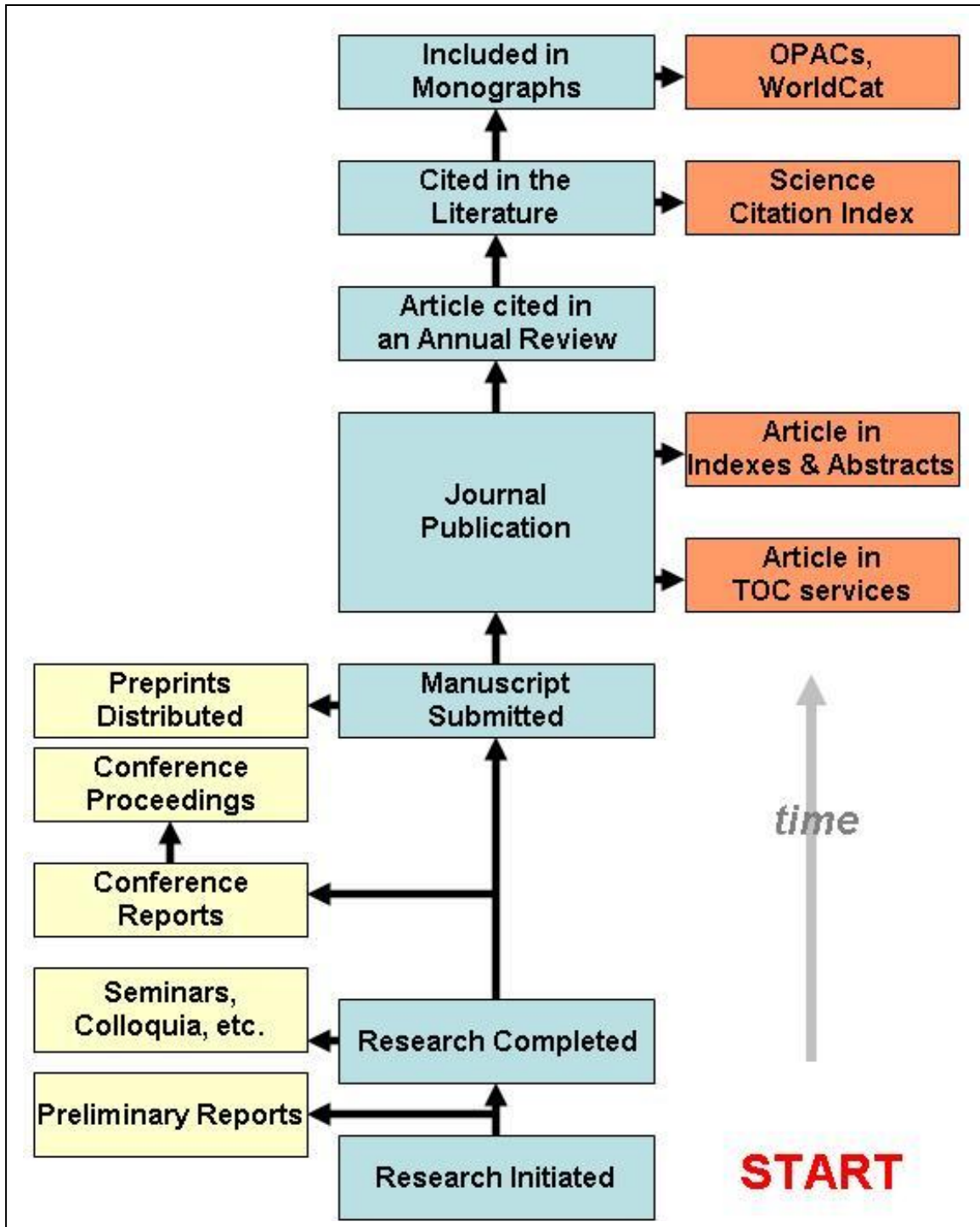
- Perform the research
- Communicate the knowledge
- Evaluate the research or the researcher
- Apply the knowledge

According to Björk’s (2005a; 2005b) the input to the “apply the knowledge” process is the “Disseminated Scientific Knowledge”. Is scholarly journals dissemination adequate for applying a new knowledge? Is a different kind of dissemination required for an adequate communication of scientific results to the practitioner community? Is a different kind of dissemination required for an adequate interdisciplinary communication of scientific results so analogical thinking is enabled for hypothesis formulations? Our opinion is that a knowledge infrastructure should address these questions and implement associated solutions if the ‘application of knowledge’ is to be addressed in a more explicit and effective ways than it has been addressed up to the present.

Björk’s (2005a; 2005b) model includes two general cybernetic loops, and both of them are of the negative kind of feedback, a regulatory one. Can’t we generate a design model where more loops and more variety of these loops relate the different processes of the academic communication? Can’t we design a model where more explicit interactions relating synergically the academic activities and its social, societal, industrial and corporative environments? We will try to answer these questions in the next paragraphs.

A Systemic-Cybernetic Model of Academic and Professional Communication

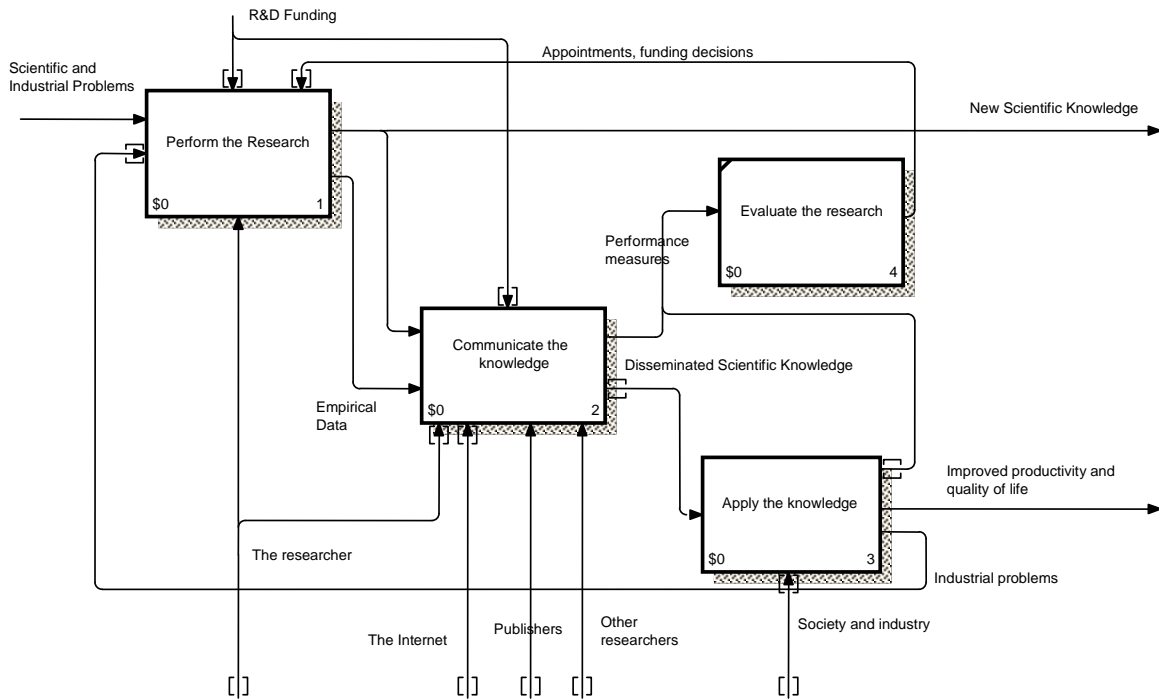
In the model we are proposing, usually serialized activities will be parallelized in order to allow the emergence of regulative, synergic and learning cybernetic loops. An essential parallelization is the one related to publishing and peer reviewing activities or processes. Our experimental hypothesis is that this kind of parallelization will solve, resolve or dissolve most of the problems we identified in the first part of this article with regards to the faulty traditional peer reviewing.



An illustration of the scientific communication process including facets of both the Garvey/Griffith (1965) model and Hurd's (1996, 2004) additions to it

Copied and pasted from Björk (2005b, informationr.net/ir/12-2/paper307.html) and Swisher (2005, www.ou.edu/ap/lis5703/sessions/s06.htm) showed. Both URLs were accessed on March 11, 2007)

Figure 2



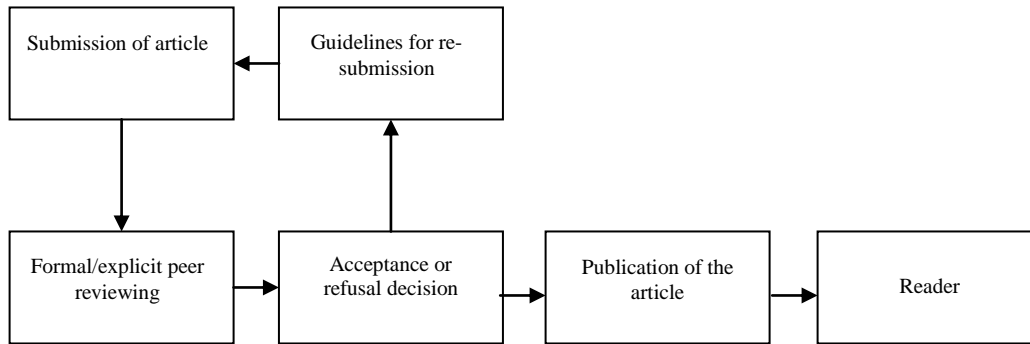
The Scientific Communication Life-Cycle Model, Björk (2005a; 2005b)

Extracted from www.jisc.ac.uk/uploaded_documents/Bo-christer%20Bjork.ppt, on March 11, 2007

Figure 3

The traditional formal and explicit peer reviewing is done *before*, of the publication of the article being published. But informal and implicit, but not less real, peer reviewing has always been done *after* the article had been published. The readers of the articles are actually peer reviewing the article they are reading, and according to this implicit and informal review the reader decides whether or not to use the content of the article in his, or her, teaching, research, consulting activities, etc. If readers do not use an article in any of their activities, the article is dead. The more readers use the content of an article in any of their academic, consulting or professional activities, the more its author's objectives are met. The Citation Index is an example of the consequences of the informal peer review that follows the traditional model of "peer-review-first-publish-after-acceptance". Informal but more important and more real peer reviewing follows the traditional publishing which follows the formal, explicit and usual peer reviewing. Figure 4 visualizes the explicit and the implicit sequence related to the explicit/formal peer reviewing that precedes a publication and the implicit/informal one that follows it. Figure 4a shows the explicit and formal submission-peer-reviewing-publishing serial processes which usually have one cybernetic loop, a negative feedback by means of which the output of the publishing quality is regulated according to a top-down approach. This well intended regulative process is not having the required effectiveness and it is producing non-desirable, questionable and sometimes unethical consequences, as we have showed with details in the first part of this article.

On the other hand, we can observe that there are implicit and informal peer reviewing that it is so real and effective that it is still supporting a flawed formal/explicit traditional peer reviewing. The informal and implicit peer reviewing (shown in broken arrows and lines in figure 4b) has the required cybernetic loops (quantity and variety) as to keep alive the traditional model and operating with – up to the present – accepted effectiveness and efficiency.

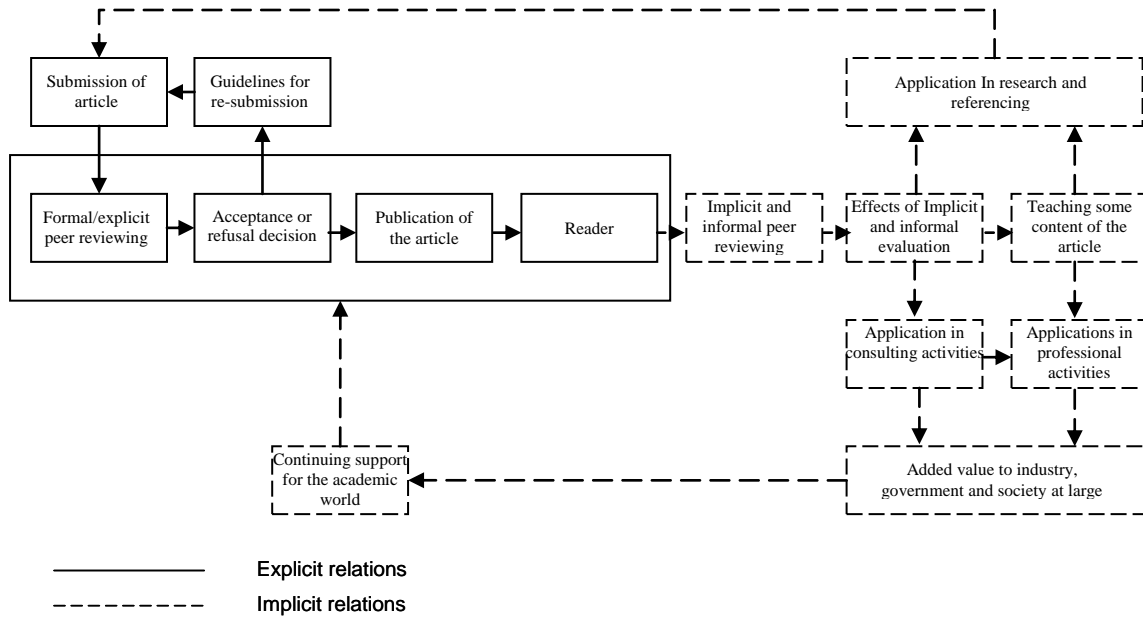


Formal/Explicit Peer reviewing in the Traditional scientific communication

Figure 4a

But with the new possibilities provided by Information Technology, these levels of efficiency and effectiveness might not be any more acceptable ones. Indeed, the huge success that other scientific communication models and systems have had (as it is the case of the arXiv, for example), it is highly probable that the traditional scientific communication restricted by the paper-media will see degraded its usefulness and consequently its importance. The new model should be tried and these models should take into account the legacy of the traditional models in order to design not just new knowledge infrastructures but to also design the required bridges to the actual paper legacy as well as the actual academic values based mostly on paper publications in paper journals, and on quality measures based the paper-media, as it is the case of the Citation index. Electronic Knowledge Infrastructures might provide metrics as good as, or better, than the Citation Index. A new model should take into account this kind of new quality metrics, but keeping also an eye on the traditional ones because there are an essential part of the actual academic values.

Consequently, an adequate level of hybridization between the new electronic media and the traditional one based on paper should be part of the new model, at least in a transitory way. This hybridization might not be a transitory one. It might even be better than the purely electronic, non-hybrid one. New technologies do not always substitute the traditional ones. Most of the time new and traditional technologies complement and complete each other creating synergic relationships, and this might be the case for the new information technology and the traditional paper media.



Formal/Explicit and Informal Implicit Peer Reviewing in the Traditional scientific communication

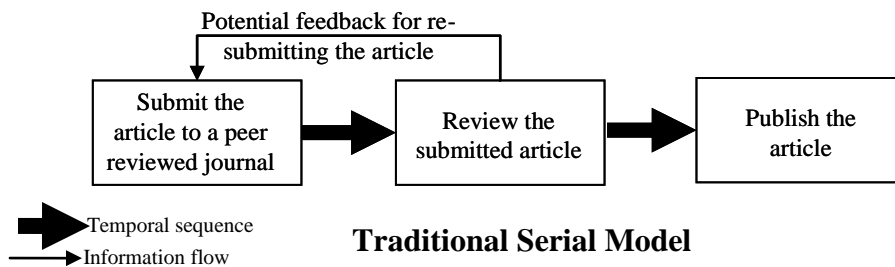
Figure 4b

Ginsparg's success is, in our opinion, partly caused 1) because he provided an electronic knowledge infrastructure that formalized and made explicit processes that were actively implicit and informal, and 2) because his solution did not try to substitute the traditional paper-media publishing but, on the contrary, it actually complemented it. Scientists, especially physicists used informal ways of communicating before or in parallel to submitting their papers to journals for their potential publications. First they used the postal mail for these informal submissions to colleagues, then they used the fax, then when the email, when it appeared, represented a quick and costless way of sending their articles previous to their possible publication in a printed journal. This is why these kinds of informal submissions were called pre-print. When the Web Technology emerged Ginsparg immediately envisioned its use as a very efficient media to disseminate pre-print. Then he conceived and supported the implementation of the Arxiv which supported, a formal and explicit way, what has been going on for a long time in by informal and implicit means. Pre-arXiv's dissemination of the pre-prints was done directly by the article author without any kind of editors or reviewers intermediation. Consequently, Ginsparg implemented the arXiv as a self-archiving knowledge infrastructure and physicists all around the world started self archiving their articles while simultaneously submitting the same article for journal publication after being peer reviewed. So, a first degree of parallelism emerged between peer reviewed publishing processes and non peer reviewed pre-print arXiv's publications.

One of the most essential features of the model we are proposing is to provide a knowledge infrastructure support for the *parallelization of peer reviewing and publishing*

processes. Peer review is undoubtedly necessary for quality control. What it is definitely not necessary is to do it before publishing. An opposite approach showed to be more efficient: immediate dissemination of knowledge once the article has been self-archived by its author at about the 1% of the traditional publishing process. This fact has been shown by the huge success of the arXiv. This kind of immediate knowledge dissemination might support peer reviewing instead of contradicting it. The informal/implicit peer reviewing (shown with broken arrows and lines in figure 4b) that usually happens after traditional publishing (based on a previous peer reviewing) might become explicit and more formalized with an arXiv-style repository of such a repository is accompanied by supporting the reader for possible comments, positive feedback he or she might have, or for possible evaluation he or she might be willing to make with regards the article he or she read. In figure 5 we tried to visualize this basic and simple idea which is at the heart of the model we are proposing.

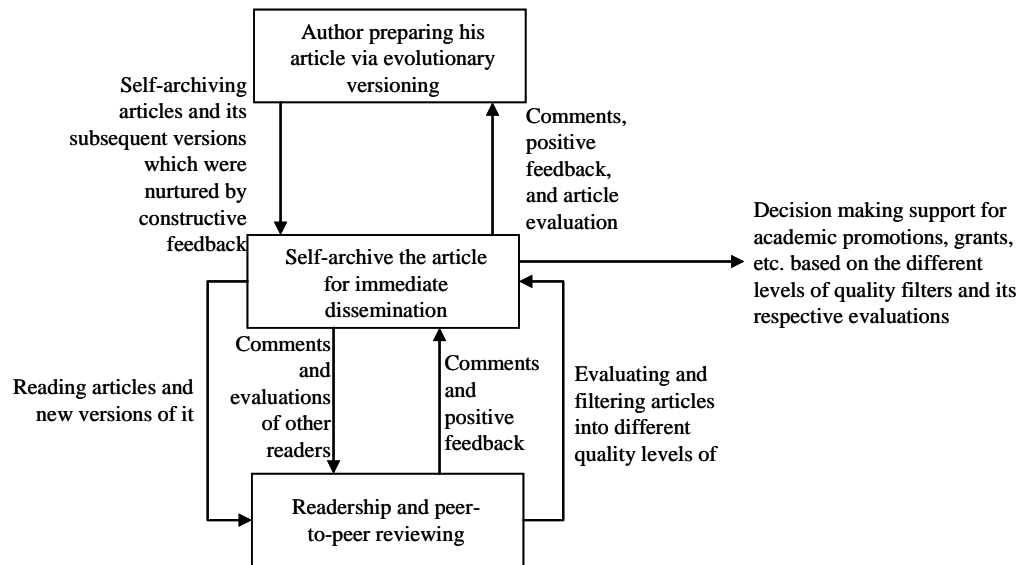
The systemic-cybernetic model shown in figure 5b has no temporal sequential arrows but just information flow ones. Publishing, peer reviewing and readership processes are conceived as parallel and simultaneous processes relating to each other by means of cybernetics loops, including positive and negative feedback as well as feedforward loops. This is why this model provides the necessary – although not sufficient – conditions for evolutionary learning and the emergence of synergies between its stakeholders, especially between authors and readers, authors and reviewers, reviewers and readers and reviewers/readers and those making decisions regarding academic promotions or grants.



Traditional Serial Model

Figure 5a

As we indicated above the modeling process will be an incremental-evolutionary one in the context of action-design, action-learning and action-research. The initial model, as well as the initial web-based system or prototype will be small enough as to allow the users of the system to participate in its design, but large enough as to provide some useful service to its initial users. We will describe below the initial model and the associated experimental system or prototype being implemented.



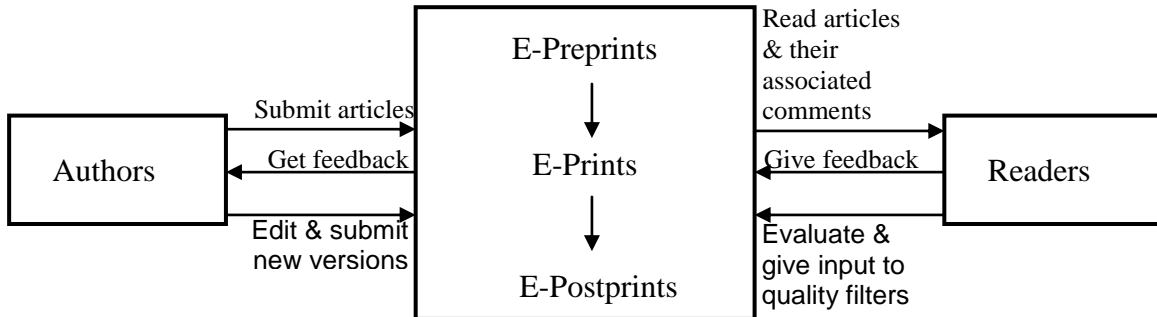
Systemic-Cybernetic Model

Figure 5b

Accordingly, the initial stage of the project will make emphasis in the following features:

- To provide a potential support for authors who as researchers need to be informed about:
 - Other authors working in the same areas;
 - Other articles published (with or without peer reviewing) in the same or related areas;
 - Opportune constructive feedback for the continual improvement of his or her paper, before sending it for its potential publication in a journal, or before sending the final version of an accepted paper for its publication, or before its presentation in a conference.
- To take into account the traditional scientific communication via paper-based peer reviewed publications and the academic values as well as the measures that will be employed in the decision making for his or her academic promotion or for the grants he or she applied for. Consequently, the first stage of the project will be oriented by a *hybrid* conception of the publishing process.
- As a hybrid system, it will include the traditional paper-based publication, which means that the online repository accept and will include *pre-prints* and *post prints*. A comprehensive hybrid model of academic and professional communication should also include printed paper-based publications, hybrid journals, hybrid proceedings, etc.

From the perspective of a hybrid model, including pre- and post-print, as well as prints, the essence of the model is shown in figures 6 and 7.



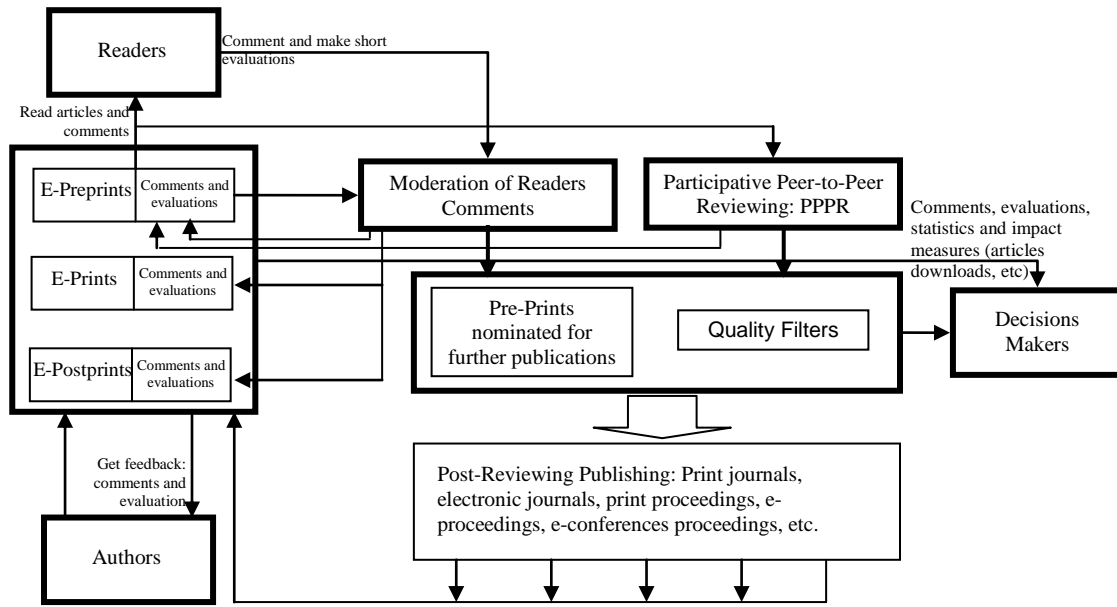
Basic Scheme of the Model Proposed

Figure 6

The basic characteristics of the model being proposed have already been implemented, partially (as it is the case of the arXiv, for example) or more comprehensively (as it is the case Cybrarium, for example). What probably is new is the level of integration, in the same knowledge infrastructure, as well as some of the means used to support its functionality as it is the case of the Participative Peer-to-Peer Reviewing that we will briefly explain below.

In figure 6 we tried to provide the core idea of the model we are proposing. There will be several stakeholders of the knowledge infrastructure that would implement the model. But, the most important among these stake holders are the *authors* and the *readers*, though a high percentage of the users will be both: readers and authors. These users who will have both roles probably are, at least initially, the most important users. They probably will be scientists, academics, engineers, professionals, consultants and practitioners. We are hoping that in a not too far future the system will also be useful to academic managers and decision makers, corporative and industrial managers, grants institutions, and in the long range the system will also be useful and usable by the general public and society at large.

The basic features of the model, which will be implemented in the initial prototype, or the initial version of the system, are schematically shown in figure 7. These essential features provide the users with the following supports:



Basic Features of the Model

Figure 7

1. Support authors

- a. In the immediate dissemination of their articles (research results, ideas, working articles, working hypothesis, etc), which eliminate the possibility of plagiarism and fraud on behalf of reviewers, as it was the case of the Yale Scandal, for example. Authors may self-archive their article while they are working the final version to be sent for its potential publication in a journal, or for its potential presentation in conferences, or they may self-archive the same final version they are submitting to a journal or to a conference.
- b. Since the authors might get a more *plural* feedback (comments, constructive criticism and evaluations) from their peers, which because its plurality will be more representative, there will be a significantly low degree of possible bias in the peer reviewing of their article. Other peer reviewing weaknesses will also be lessened – or even disappear – with the increasing plurality in the number and the kinds of reviewing that the article might receive.
- c. Get feedback (comments and evaluations) on their articles sooner than usual (as well as more plural and more representative of their peers) so they can comment on the comments they get, interact with their commenting peers and get into a learning process which might lead to new and improved versions of the submitted article, possible collaborative research or publishing with some of the readers of their articles who are researching in the same area,

and other possible synergic relationships by means of the interaction of their readers and other authors who might have submitted articles in the same area or topic.

- d. Get information about the number of readers and peers who downloaded the submitted and self-archived article, which would be a good measure of the interest raised by such an article.
 - e. Authors will get emails informing them about new articles that were self-archived in the same area or topic where they archived their article(s). This would provide them with information that might be important as input for a next version of their article, or before sending it to for its publication via the traditional process.
 - f. Items a, b, c, d, and e provide authors with an adequate support in the process of *preparing* their article while avoiding simultaneously peer reviewing weakness and pitfalls. Authors would be supported in the process of PREParing their articles by means of self-archiving the initial version of their article via electronic PREPrint. This is why we called the system implementing the first stage of this project e-PREP: It helps to prepare the publication of a paper by means of e-preprinting it and by means of getting comments and evaluations of it from both, readers of the articles and other authors who submitted articles in the same topic or area. These authors would provide constructive feedback and evaluation by means of a Participative Peer-to-Peer Reviewing, which will be described below.
 - g. This support would continue after the paper has been published according to the traditional publishing process. E-prints and e-postprints may still receive comments and constructive feedback that might hint an article's author to make an improved or more completed version than the one he, or she, had printed in a paper journal or in a printed conference proceedings. Authors may not be able to modify articles already printed in journals or proceedings but they may still modify, or send a new version, of the respective post-print if it was archived in e-PREP.
2. Provide **readers** with:
- a. Comments evaluation made for the article being read, so the reader who is not expert in the area may have critical comments related to the article being read by him, or her.
 - b. The possibility with commenting, or making short evaluations, of the article that he, or she, just read.
 - c. The possibility of interacting with the author of the article.

3. Provide the potential **reviewers** of the article with more means for a better reviewing of it. Access to comments and evaluations made by readers and other authors in the same topic would improve meaningfully the reviewing to be made by a reviewer. This reviewer could be an *internal* one, i.e., another author who self-archived an article in the same topic and is in the process of a Participative Peer-to-Peer Reviewing, which will also be supported by the e-PREP system, or the reviewer might be an *external* one, i.e., a reviewer appointed by an editor of a journal, where the article was also submitted, a Conference's reviewer or organizer considering the same article for its possible presentation at the conference.
4. If the article is also submitted to a journal, the respective **editor** may get, from e-PREP, some editorial support, especially in the initial screening phase and when and acceptance, or refusal, the decision should be made in the context of no agreement among the reviewers that the editor appointed for the paper review.
5. Academic and grants **decisions makers** will have plural and more comprehensive information regarding articles archived in e-PREP in any of its forms (preprint, prints and post-prints). They may have access to the output of the different quality filters. Their decisions will not be based just on previous discrete few decisions that few reviewers made with regards to an article, or a set of them. They would also have similar kind of information from more sources that are more continuous and more plural. Academic and grant decision maker will still have the kind of information they have right now, but they may, if they wish, complement it with higher and hence more representative variety of opinions, perspectives and evaluations. They would have not just access to previous decisions, but the opinions and evaluations that supported similar decisions regarding the same article under consideration.

Let us now describe with some more details the system that would be implemented for the initial stage of the project which we called e-PREP. Since this system will be initially oriented to the areas of Systemics, Cybernetics and informatics, we will specifically call it e-PREP-SCI.

Electronic Pre-Prints Archive and Participative Reviewing Support System in Systemics, Cybernetics and Informatics: e-PREP-SCI

What is e-PREP-SCI

The main purpose of e-PREP-SCI is to support Scholarly communication, especially in its initial phases, including a quick dissemination of its initial or intermediate products. We are trying to achieve this purpose in e-PREP-SCI by means of a modest first step inspired and oriented by Ginsparg's arXiv and combining it with a Participative Peer-to-Peer Reviewing (PPPR) support.

e-PREP-SCI is an electronic archive where authors may upload their papers or extended abstracts in order share his/her recently acquired knowledge or conceived hypothesis, to establish credit for the ideas included in the submitted papers or abstracts and to pre-publish his/her paper, or abstract, in order to get constructive feedback, through peer-to-peer reviewing, oriented to improve his/her paper before submitting it to a conference or to a journal to be considered for its publication.

As we indicated above, e-PREP-SCI might be considered as a supporting service in the process of “PREPreparation for a publication in SCI” (Systemics, Cybernetics and Informatics) [Click here for more details regarding e-PREP-SCI.](#)

e-PREP-SCI has two basic sub-systems:

- SCI-Archive, which contains the submitted papers and abstracts, and
- support for a participative peer-to-peer reviewing (PPPR), based on the spirit of “Pay It Forward”. This principle has been applied by Ed Yourdon for the achievement of bottom up quality in software development, and in PPPR is applied for the achievement of bottom up quality in scholar publishing. [Click here for more details regarding PPPR and its related bottom up quality.](#)

The initial version of the system supporting e-PREP-SCI service will be a very first experimental prototype (V.0.0) that will continuously be developed including, with an evolutionary-incremental methodology, more of Ginsparg’s Arxiv and implementing more requirements suggested by its users, especially in its PPPR sub-system. Other authors’ ideas on this subject will also be incrementally added. The site is under continuous development as financial restrictions and time allows our team to add and refine content.

The initial web site will be a means for a beta test of the e-PREP-SCI concept and its present implementation through this first experimental prototype. Consequently, visitors will find that some pages and links remain under construction. We ask for your patience as we continue to develop both the concept and its implementation. We also invite suggestions and comments.

The conception of Electronic Pre-Prints Archive and Participative Reviewing Support System in Systemics, Cybernetics and Informatics: e-PREP-SCI, is based on the integration of several ideas or concepts:

- The Arxiv, originally developed by Paul Ginsparg in 1991, as an archive for preprints in physics, that extended its scope as to include mathematics, computer Science, non-linear science and quantitative biology.
- The Cybrarium which was conceived by Alexander Hars (2003) as a collaboratory that “provides extensive support for the creation and synthesis of

knowledge as well as for its dissemination. Participants can create contributions. Annotate existing contribution and publish either instantly.” (Hars, 2003; p.187)

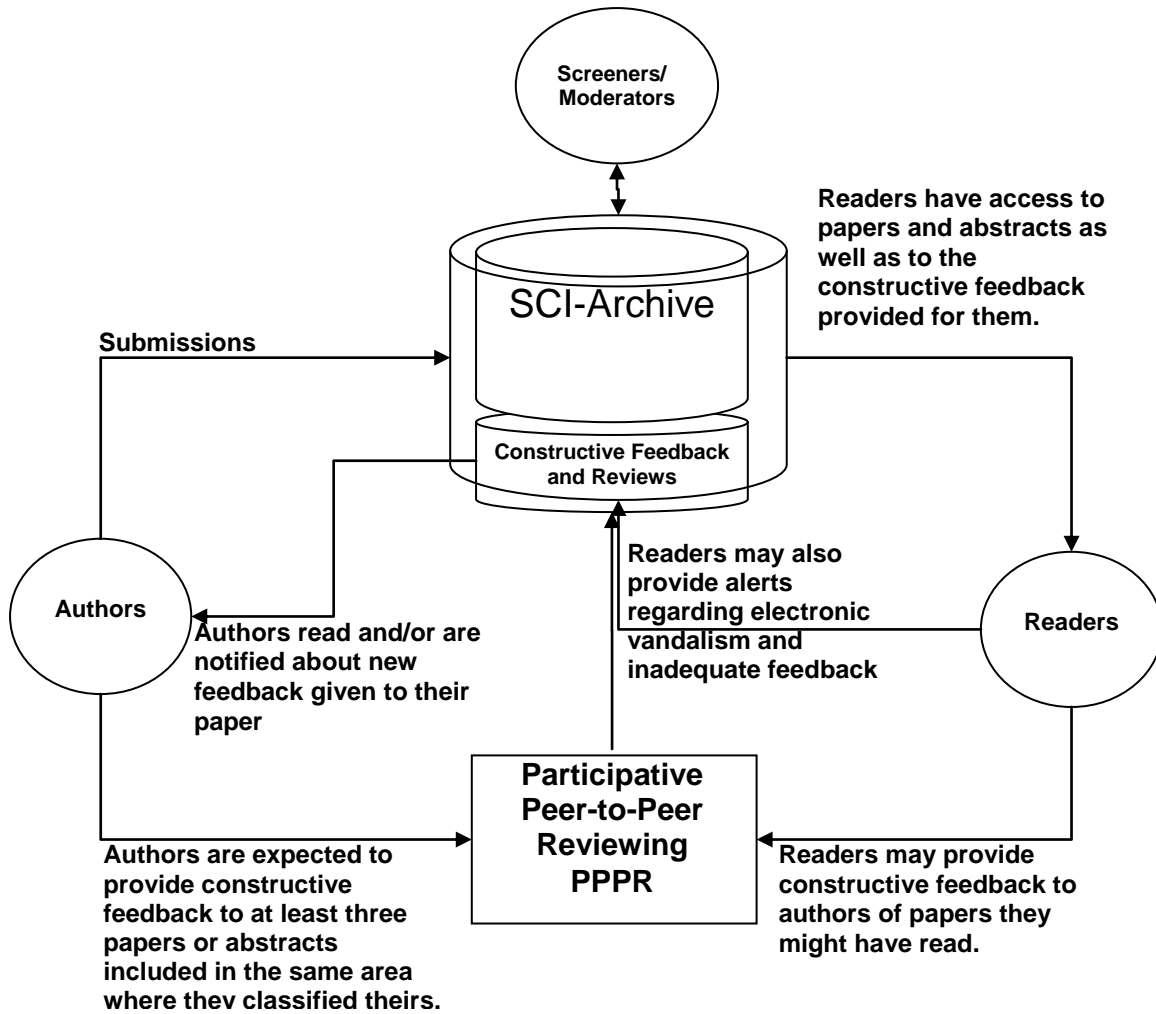
- The Participative Peer-to-Peer Reviewing (PPPR) that is being applied in the conferences organized by the International Institute of Informatics and Systemics (IIS) as a bottom-up quality control conceived as a complement of traditional top-down processes: double-blind reviewing, non-blind, open, reviewing, etc.

The preprint archive of the e-PREP-SCI will be non-refereed electronic archive where authors may upload their papers or abstracts in order share his/her recently acquired knowledge, to establish credit for the ideas included in the submitted papers or abstracts and to pre-publish his/her paper, or abstract, in order to get constructive feedback, through peer-to-peer reviewing, oriented to improve his/her paper before submitting it to a conference or to a journal to be considered for its publication. e-PREP-SCI might be considered as a supporting service in the process of “PREParation for a publication in SCI” (Systemics, Cybernetics and Informatics). In Figure 8 we tried to make a very basic scheme of e-prep-SCI, and in Figure 9 we tried to schematize e-prep-SCI basic processes.

Basic Functions of e-PREP-SCI

Initially, e-PREP-SCI will have four basic functions:

- General Readership, where user in general access the SCI-Archive in order to read the papers and abstracts submitted. The general reader may also comment and/or review what he, or she, is reading, providing a constructive feedback to the author (s). In a first phase the potential readers will be limited to members of the International Institute of Informatics and Systemics (IIS), and other pre-approved ones. In a second phase readership will be opened to the general public.
- Paper or abstract submissions, where an author may submit his, or her, paper or abstract. Submitted papers or abstract may be edited only by the author who submitted them in order to improve them according the provided feedback. Submitting authors are expected to review at least three other papers or abstracts included in the same area where they classified their paper or abstract. This review will be done in the spirit of “pay it forward” applied by Ed Yourdon for the achievement of bottom-up quality in software development.
- Submission Endorsement: Papers and abstracts will be displayed as received without a previous reviewing process, but the submitting author must present at least one endorser who should endorse both the content of the paper or abstract as well as its classification in the respective area or sub-area. Endorsement does not include reviewing, and it is not paper review.

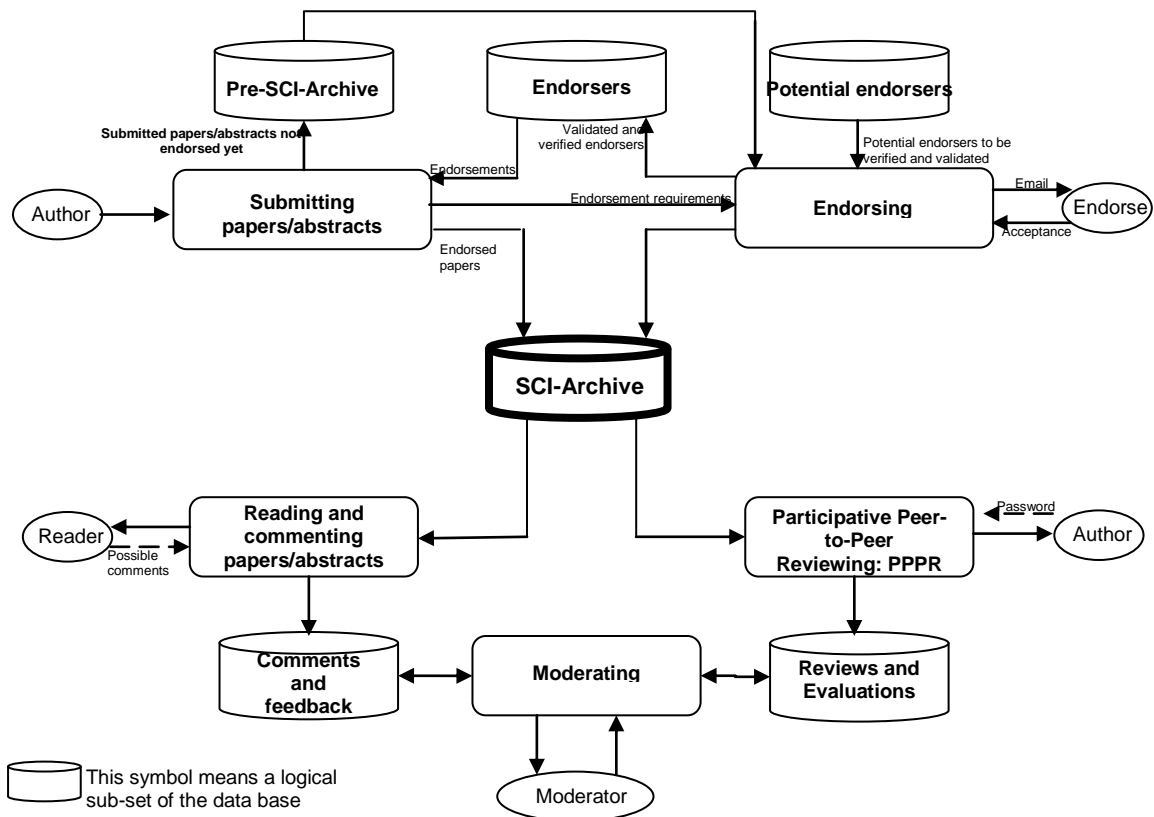


Basic Scheme of e-prep-SCI

Figure 8

- Moderators will take care for the feedback made by readers so it would be a constructive one. They can remove inadequate comments, harsh words, insulting phrases, non-ethical statements, electronic vandalism, bogus comments, etc. Moderators might also be in charge of suggesting changes to be made in the classification of a paper or abstract, or suggest changes in the wording of the feedback so it gets more useful and constructive.
- Participative Peer-to-Peer Reviewing where general readers might provide constructive feedback and where each author of each submitted paper or abstract should review at least three other papers or abstracts submitted to the same area where he or she classified his or her submission. Along first three months of participative peer-to-peer reviewing, the submission might be:

- Selected for its publication in the Journal of Informatics, Systemics and Cybernetics (JSCI), if its author (s) accepts the invitation for such a publication and its publication is recommended by the Journal’s reviewers.
- Selected for its publication in another kind of publication (a multiple authors book, for example), if its author (s) accepts the invitation for such a publication, and the paper is accepted by the respective editor.



Basic Processes of e-PREP-SCI

Figure 9

A paper or an abstract included in the e-PREP-SCI can be excluded at any moment after its publication if the participative peer-to-peer reviewing generates strong negative comments and/or a respective moderator suggests its exclusion from the SCI-Archive.

A Short Description of the ArXiv.

As it was stated above, the arXiv, founded by The National Science Foundation, was originally developed by Paul Ginsparg in 1991, as an archive for preprints. The Los Alamos National Laboratory originally hosted the arXiv (his name then was LANL

preprint archive), but it is now hosted and operated by Cornell University, with mirrors around the world.

The arxiv was formed as a means for scientists to rapidly disseminate new discoveries and theoretical developments to the worldwide scientific community. Publishing in journals requires a process that can drag on from months to years until approved for publication. In the present fast changing world it might not be enough just to publish one's ideas in scientific journals, rapid communication of all plausible new ideas to the academic community through an easily accessible internet archive is essential to the progress of science. This was the main purpose of the arXiv, and this is one of the main purposes of e-PREP-SCI.

The arxiv is not peer-reviewed but, since January 2004, a paper requires previous endorsement for its inclusion in it. This endorser may be an rXive author. Endorsers are not reviewers. They are not asked to review the to-be-endorsed paper, but to check if the paper is appropriate for the selected subject area. e-PREP-SCI would also require endorsements for the inclusion of a paper in the archive.

Some academics or researchers consider a hindrance the lack of peer-reviewing, but those authors who are using the arXiv do not seem to care about it. Many authors, although not all of them, are very careful in what they post, because they are interested in the publication of the paper in peer refereed journals or proceedings as a next step. But, not all authors who post papers in the arXiv are interested in peer-reviewed publications. Grigori Perelman, for example, posted in November 2002, a paper related to a potential proof of Thurston's Geometrization conjecture, and he appears not to be interested in its publication in a peer-reviewed publication. According Wikipedia, Perelman affirmed that "If anybody is interested in my way of solving the problem, it's all there [on the arXiv] - let them go and read about it."

While some consider that the arxiv does contains some dubious e-prints, others, as for example Nobel Laureate Brian Josephson complains his papers "are not accepted or are forcibly recategorised by the administrators of the arXiv due to the controversial nature of their work, amounting to intellectual censorship." (Wikipedia). There is a tradeoff that should be decided between restricting the papers to be posted with the potential cost of refusing to post paradigm shift papers, and removing restrictions with the cost of posting dubious papers and submissions made by some persons with an ethical levels low as to engage in electronic vandalism. We hope that the Participative Peer-to-Peer Reviewing (PPPR), which possibility will be included in the e-PREP-SCI, will allow less required top down restrictions in order to lower the probability of continuing the posting of dubious papers or submissions made in the context of electronic vandalism.

Since we strongly believe in academic and intellectual freedom, and we profess a high level of respect to diversity in practitioners' experience, in ideas plurality and professional reflections, we will try to sacrifice in an increasing of the probability of including (mostly for a short period of time) dubious papers, and submissions made by

electronic vandals, in order to avoid top-down papers' rejections. All papers submitted to e-PREP-SCI will be posted, and those considered dubious or generated by electronic vandalisms will be removed after they had gone through the process of Participative Peer-to-Peer reviewing. In this way papers will be removed by means of a bottom-up quality control, instead of blocking the inclusion in the archive of papers considered dubious via top-down or because its authors were included in a black list. In this way we would like to avoid the increasing critics that are being made in the context of academic and intellectual freedom. An example of these critics can be found at <http://www.archivefreedom.org/> where the issue of Freedom in Scientific Research is addressed. A short article titled "Repression of Physicists in the 21st Century" resume the purpose of the mentioned Archive Freedom's web site. In this article, the following text could be read regarding the critics being made to the arxiv:

"Presently hosted at Cornell University under the direction of physicist Paul Ginsparg, it [the arXiv] blocks certain physicists from posting their papers to this archive. The arXiv administrators maintain a list of physicists whom they have blacklisted or ostracized so that any paper those individuals attempt to submit is systematically rejected regardless of its scientific content. Usually these blocked papers have already been accepted for publication in reputable peer refereed science journals or in other cases are undergoing review for journal publication which indicates that these papers are serious and well thought out. **The list of suppressed scientists even includes Nobel Laureates!** One characteristic that these ostracized physicists share in common is that they have written or published papers in the past which propose new ideas that challenge traditional physics dogma. In other cases their published works just happen to run counter to the particular theory preferences of the small political clique administering the archive." (emphasis added)

"Our world is experiencing serious problems such as exponential population growth, environmental pollution, impending energy shortages, nuclear proliferation, and climatic change. We cannot afford to suppress the works of those seminal minds whose new ideas could revolutionize the way we interact with the world. What if a paper described the discovery of a new source of energy that could help to alleviate the coming energy crisis? Or, what if a paper brought to light a serious environmental hazard which, if unheeded, would result in a substantial loss of life? And, what if arXiv.org moderators censored one such important paper because of a possible personal dislike of its author or because it conflicted with a theory they personally favored? Society cannot afford this kind of behavior."

We will try to avoid this kind of critics and concern with regards to academic and intellectual freedom by means of the Participative Peer-to-Peer Reviewing (PPPR) made after the paper had been posted. When bottom-up unanimity is achieved regarding the necessity of removing a paper, such a paper will be removed. Papers that have been posted with no previous screening or reviewing, will be flagged as such, in order to alert the reader with regards of the possibility that such paper might be a dubious one or generated and submitted by electronic vandalism.

Participative Peer-to-Peer Reviewing: PPPR

As we said above, e-PREP-SCI is a combination of the essential functionality of the arXiv and Participative Peer-to-Peer Reviewing (PPPR). A prototype system supporting PPPR has been used in the conferences organized by The international Institute of Informatics and Systemics (IIS) in 2006. It also be used in the conferences that will be organized by IIS in 2007. So, a description of the system, and service, is important for presenting the functionality of this PPPR subsystem, one of the two principal subsystems of the e-PREP-SCI system and service.

Conferences being organized by IIS since 2007 have a Peer-to-Peer Reviewing (PPPR) as an integral part of a multi-methodological reviewing which we think is a highly desirable, and perhaps necessary, in the organization of a Multi- and Inter-disciplinary conference. The reasoning which supports the multi-methodological approach can be found at the web pages of the conferences organized by IIS and in Appendix D. Let us here provide a very short description of it in order to provide and adequate context to the PPPR being used in IIS' conferences organizations, where PPPR is an integral part of the multi-methodological approach. Right after this brief description, we will provide more details on the essence and general characteristics of PPPR.

This multi-methodological approach might be achieved by means of adequately combining:

1. **Formal, linear, systematic** methods, for the achievement of what is called **top-down quality**, as it is the case of the usual and conventional **double-blind** reviewing, and/or
2. **Informal, nonlinear, systemically interactive** methods, for the achievement of what is called **bottom-up quality**, as it is the case of **non-blind, or open reviewing**, recommended by Kaplan (2005).

In the conferences organized by IIS, both methods have been used since 2006, and combined with **Participative Peer-to-Peer Reviewing (PPPR)**, as another **Informal, nonlinear, systemically interactive and bottom-up** reviewing method, since 2007, where each draft paper or abstract has been reviewed, evaluated and constructively commented by other authors who submitted draft papers or abstracts in the same area, sub-area or topic.

Consequently, each draft paper or abstract submitted to IIS' conferences went through through these three kinds of reviewing

1. It was sent to at least three reviewers, randomly selected, for its double blind reviewing. Some of the submission has been sent to 5-8 randomly selected reviewers, in order to get at least 3 double-blind reviews.

2. Draft papers and extended abstracts have had non-blind, open reviewing by means of 1-3 reviewers suggested by the submitting authors, and approved by the respective Organizing Committee.
3. Submissions were posted, without previous screening, in the conference web site in a way that they could be accessed, reviewed, commented and evaluated just by the authors who sent draft papers or abstracts in the same area or topic. Authors will get a login and a password in order to have this kind of access.

Final acceptance, in this three-tier reviewing methodology, depends of the three kinds of reviews but a paper should be recommended by non-blind reviewers AND blind reviewers in order to be accepted for presentation and to be included in the respective proceedings of any conference organized by the IIS. A recommendation to accept made by non-blind reviewers is a **necessary** condition, but it is not a **sufficient** one. A submission, to be accepted, should also have a majority of its double-blind reviewers recommending its acceptance. This double necessary conditions generate a **more reliable and rigorous** reviewing than a those reviewing methods based on just one of the indicated methods, or just on the traditional double-blind reviewing.

IIS' non-blind reviewing is based on the essence of what Kaplan (2005) proposed in order to fix peer reviewing problems. Kaplan affirms that "Peer review subsumes two functions. First, peer reviewers attempt to improve manuscripts by offering constructive criticisms about concrete elements ... The second function of peer review is to render a decision about the ... significance of the findings so that the manuscript can be prioritized for publication. I propose reforming peer review so that the two functions are independent." With regards to the first function of peer reviewing Kaplan proposes that "**Review of a manuscript would be solicited from colleagues by the authors.** The first task of these reviewers would be to identify revisions that could be made to improve the manuscript. Second, the reviewers would be responsible for writing an evaluation of the revised work. This assessment would be mostly concerned with the significance of the findings, and the reviewers would sign it" (emphasis added).

IIS tries to achieve the first function via Kaplan's non-blind peer reviewing and the second function by the traditional means of double-blind review. This is why submission acceptance by the non-blind reviewers is a necessary condition but not a sufficient one. The submission should also have favorable recommendations by the majority of the double-blind reviewers in order to be accepted by IIS for its presentation and inclusion in the respective conference proceedings.

As we informed above, a third reviewing tier is the participative peer reviewing, which complements the two tiers described above but it is not a necessary condition for accepting a submission. An article submitted to a conference being organized by IIS is immediately displayed for review to those authors who submitted articles in the same theme, area, or sub-area. Accordingly, each submitting author have access to all submission submitted to the same area where he/she submitted his/her article and can comment and evaluate any submitted article to the same area. This kind of reviewing

(PPPR) provides additional input to the selection process and assists all participants in placing their presentations in context. It is not a necessary condition but it has a complementary function, especially in those cases where the non-blind reviewers have a strong disagreement and no majority of recommendations are for accepting or not accepting the respective article.

In general, peer reviewing/refereeing in scholarly publishing is an instrument of quality control and assurance by means of which journal's editors, conference's organizers, book publishers, etc. achieve the quality level required by their objectives. Since the functions of different means of scholarly publishing are not necessarily the same, indeed they might be highly diverse, peer reviewing may differ among different means of scholarly publishing.

Regarding this issue Walker and Hurt (1990, *Scientific and Technical Literature*), for example, affirm that **“it is not practical to require the same kind of refereeing of conference papers as for journal articles because of time constraints, and the process would modify, if not eliminate, one of the most desirable characteristics of such meetings: informal exchange of ideas and preliminary findings of new research as well speculative and even nonconventional presentations of information, both intended to promote innovation and creativity.”** This is why “Even for conferences sponsored by societies that have high standards for their journal publications, there is no assurance high standards are also applied to the publications of all conference contributions.” (p. 97; emphasis added)

Although the informal or the semi-formal nature of conferences, as well as their time constraints, might be the cause of a less effective quality control and a lower level of quality assurance, communications and information technologies are making feasible some reviewing methods that might help in increasing the level of quality control and assurance. Communications and Information Technologies are making bottom-up quality a real and practical possibility. Participative peer-to-peer reviewing, via computing mediated communications, is a means for achieving bottom up quality in papers' peer reviewing or refereeing.

Bottom-up methods and processes are opposites to top-down ones, but, in our opinion these both opposites are not necessarily contradictory. Indeed, they might complement each other creating synergic relationships where the whole is more than the sum of its parts. They might even be polar opposites, where each opposite requires each other as a necessary condition for its own existence.

Quality control used methods has been oscillating between these two opposite methods. The movement of Total Quality is fundamentally based on participative peer-to-peer bottom-up methods, which emerged in opposition to the supervisory top-down approach based on the applications of rules and systematic and formal procedures. Bottom up quality processes are highly interactive, informal, non-linear and systemic (but not necessarily systematic) where personal creativities are merged in a collective web of team

creativity. Analogously, papers' reviewing/refereeing may be made by means of **interactive, informal, non-linear and systemic bottom-up methods as opposed to formal, linear and systematic top-down methods.**

Bottom-up quality has been achieved not just in manufacturing, and in other areas where Total Quality methods were applied with a significant effectiveness, but also in participative management of organizations, information systems development and software engineering. Ed Yourdon, for example, the very well known consultant in Software Engineering, and creator of the best known top-down methods in programming and software development is actually embracing and advocating top-down quality in the areas of software engineering and information systems development. Interviewed by Carol Deckers (from Quality Plus Technologies) in a Quality Plus E-talk affirmed that "in the context of quality assurance in the computer field, that is a bottom-up grass roots approach to making things better as opposed to the top-down approach that you see in most business organizations." Regarding Software Engineering Institute's Capability Maturity Model, oriented to quality assurance, Yourdon affirms that "It is interesting that, at this point [November, 2000], after a full decade that only 15% of American IT organizations have even bothered going through an assessment to find out where they are on that scale. When it happens, it is usually done on a top-down basis, i.e., a senior vice president or a CIO says we better do it and it is very important for the long-term good of the company to achieve a level 3, 4 or 5 on this scale...while this may be very beneficial for the company as a whole, it often has short-term negative consequences for the practitioners and the computer professionals and the engineers down at the bottom because they end up having to work harder and longer in order to achieve these worthy goals. **One of the interesting things that was done four or five years ago by the same organization, the SEI, was to develop a bottom-up approach.** This is something that could be practiced at the grass roots level by individual computer engineers." (Transcripts of the Quality Plus E-talk where Ed Yourdon made these statements can be found at <http://www.stickyminds.com/sitewide.asp?Function=edetail&ObjectType=ART&ObjectID=2244>.) Yourdon, creator, about 30 years ago, of the Yourdon's Top-Down Structured Analysis and Design in Software Engineering affirms convincingly that most software engineering projects are feasible just with a bottom-up quality approach. The Software Engineering Institute, developer and promoter of the Top-Down Capability Maturity Model also conceived a bottom-up quality model. Information Communication Technologies (ICT) are providing the means to make possible and feasible the bottom up approaches to quality control and assurance. Bottom-up approaches to quality control are being increasingly applied in an expanding diversity of areas.

In our opinion, bottom-up approaches to peer reviewing/refereeing should also be designed, implemented and tested in the context of action-design, action-learning and action-reflection, in order to take advantage of the opportunities being generated by ICT. Consequently, The Institute of Systemics, Cybernetics and Informatics (IIS) is implementing a Participative Peer-to-Peer Reviewing (PPPR) as a complement to the traditional top-down processes where journal editors and conference organizers select reviewers for submitted papers and whose comments and evaluation regarding the papers they are reviewing support the decisions to be made regarding the acceptance or non-

acceptance of the submitted papers. Integrating systemically traditional top-down and bottom-up reviewing method will, very probably, improve the total quality of peer reviewing and might help in overcoming the frequently reported weaknesses of peer reviewing/refereeing.

In this systemic framework, IIS has implemented the Participative Peer-to-Peer Reviewing for the conferences that has been organized since 2007. Accordingly, draft papers and abstracts submitted since 2007 to any conference has be posted as received, without any previous screening, by the Organizing Committee, on the conference website where, as we informed above, it can be accessed, via password, by the authors who made submissions in the same area, sub-area or topic. This kind of bottom-up quality control and assurance needs the real and effective participation of the authors submitting papers in order to provide a workable solution for increasing the quality level of the papers to be accepted for their conference presentation. Yourdon used, at the 10th International Conference in Software quality, the phrase “**Pay it Forward**” to refer to what it is necessary in bottom-up quality. According to our experience, since 2007, the more authors participate in the PPPR of other articles, the higher the reliability of the respective accepting (non-accepting) process and the higher the quality of the final versions of the respective papers, as well as the quality of their presentation at the conference. Regretfully, the participation frequency in PPPR is not, up to the present, an adequate one

Interviewed by Carol Dekkers regarding the phrase “**Pay it Forward**” in the context of achieving bottom-up quality, Yourdon affirmed “I want to point out a thought that is something that I picked up elsewhere; I did not invent it at all. It is the title of a book and actually a movie...It is a very simple idea that if someone does you a favor rather than paying it back or ignoring it altogether, that you might reciprocate by paying it forward. You know, passing it on but in kind of an expanding chain. If somebody does you one favor, you pass on the favor forward to three other people and each of those three passes it on to three others and so on. The reason why I was suggesting it, **particularly in the context of quality assurance in the computer field, that is a bottom-up grass roots approach** to making things better as opposed to the top-down approach that you see in most business organizations, and frankly in many government and social movements as well. The idea that the president, or the boss, or the CEO is going to figure out how to make things better and then the issue of edicts and orders that will ripple downward through the hierarchy to cause things to be done in a different fashion. Sometimes, that is important, particularly if you have a charismatic leader who can help break some kind of stalemate or paralysis in an organization. But I think in a lot of cases, it is going to have to come from the bottom upwards, and that was what I was trying to suggest in that conference and to help reinforce it. I made sure that everybody in the conference had a copy of the book. I also told them that I was prepared to follow my own advice by offering a "Pay It Forward" favor to two or three people in the conference.” (Emphasis added. Transcript of this interview can be found at the URL we informed about above).

Participative Peer-to-Peer Reviewing requires a **Pay it Forward attitude** from the authors submitting draft papers and abstracts in order to be effective. Paraphrasing

Yourdon we can say that each author receiving, or to receive, constructive comment for the draft paper or abstract he, or she, submitted to the conference, he, or she, should reciprocate and “pay it forward” making constructive reviews for three draft papers or abstracts. The authors of these three papers would “pay it forward” reviewing nine draft papers or abstract, and so on, in a kind of expanding chain that would generate a continuous quality increasing of each paper, and the quality of all the papers to be presented at the conference, as a whole. Regretfully, it has been our experience that few authors have the “**Pay it Forward**” attitude. Consequently, a reflection-action project would be advisable to implement in order to identify how a “Pay it Forward” attitude might be generated or enhanced.

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Appendix A

Is it possible to integrate Conventional Conferences and Conversational Meetings in order to achieve more effectiveness?

(First Draft)

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Abstract

Academics, professionals and practitioners have increasingly been using the conversation format as an **alternative** to the conventional conference format. We think that the conversational format might also be used, not just as an alternative, but concurrently with conventional conferences in a way as to generate synergic relationships between both formats/models. In this short draft we will briefly describe the characteristics of each model, highlighting their opposite features, suggesting the benefits of their possible integration, a methodology to design and implement this possible integration and the initial steps that might be taken in the context of reflection-action and design-action, which can end up in action-research projects.

Origins and Characteristics of Conversational Meetings

The largest meetings with the conversational format are The Fuschl and The Asilomar Conversations. The Fuschl Conversations have been organized every second year, for 25 years by the International Federation of Systems Research (IFSR) and The International Systems Institute (ISI) has organized 25 meetings with the conversation format since the early 80's, being the Asilomar Conversations the core of them. Bela H. Banath, former President of the IFSR and the ISSS (International Society for Systems Research) and the founder of these two series of meetings with the conversational format was the first Honorary President of The WMSCI Conferences. The experience gathered in these conversations will support the organizing process of conversational meetings in the context of The WMSCI Conferences and other conferences organized by the International Institute of Informatics and Systemics (IIS). Organizing conversational meetings in the context of conventional conferences might support the generation of ideas with regards to the possible synergies that might be generated by means of combining both models and the ways of implementing them with the purpose to increase the effectiveness of conventional conferences. Indeed, the conversational format was conceived as an alternative to the conventional one in order to improve the effectiveness of scholar, academic, professional and /or practitioner meetings.

T. G. Frantz (2006), for example, affirms that The International Systems Institute (ISI), organizer of the Asilomar Conversations, “was born out of the recognition that academic, scientific and professional conferences seem to offer scant opportunities for colleagues to confer, to converse. Typically, a minority of participants deliver prepared presentations to a relatively passive majority. Except for brief Q & A opportunities, interchange among participants is rarely found on the official schedule... Presenting is almost always more prestigious than listening, and some presentations carry greater prestige than others. Traditionally, the prestigious experts disseminate pre-packaged new ideas to the others, who are encouraged to take home and use whatever they find valid or promising. Such hierarchical knowledge distribution systems greatly constrain us in addressing humanity’s most pressing and complex issues, issues about which we are not merely concerned, but also outraged. Of course, at traditional conferences it is understood that scholars should approach issues objectively - without emotional involvement. Bela H. Banathy had a different vision for scholarly gatherings, one which could more fully harness the collective potential of groups...As Banathy puts it, “We aspire to reap the ‘reflecting and creating power’ of groups that emerges in the course of disciplined and focused conversations on issues that are important to us and to our society.”

One of the basic drives B. H. Banathy had for the organization of the Fuschl and the Asilomar conversations was the organization of Social Systems. Since the design and organization of conferences, even the conventional ones, has implicitly been a social system design, we would like to make this design explicit by means of the conversation format which open doors for collaborative design and group creativity (synectics). This is one of the basic reasons driving us to start a process of organizing conversational meetings in the context of conventional conferences. Another basic reason is to provide the participant of conventional technical sessions with the possibility of having conversational meetings on the technical topic that supported the conventional presentations of their papers.

As it was stated in the Follet Conversation, this format “supports integrated diversity, produces much more mutual learning and new knowledge, and builds long-lasting networks. It also represents recognition that the most interesting part of typical conferences has been the conversations that occur in doorways, in hallways, and over coffee and meals between presentations.” (Follet Conversation, 2002) On the other hand, and as B. H. Banathy affirmed “It is the basic right of individuals, groups and communities to be involved in making decisions that affect them.” Consequently, Conferences participants should be involved in the decisions that affect them with regards to the effectiveness of the conferences as related to the achievement of the objectives they might have while participating in the conferences as, for example, knowledge communication, learning, networking, etc. A participatory process should complement the hierarchical one used in conventional conferences for knowledge communication.

It is our experience-based opinion, that bottom up and top down methodologies might be synergistically combined in the design of some social systems as it is the case of information systems development teams. Similar possibilities might exist for conferences design organizations. Indeed, they actually are social and information systems for

knowledge communications and learning processes. Among the methodological roots we are proposing is B. H. Banathy's (1996; 1999) description of social systems design as "future creating disciplined inquiry" (Banathy, 1996) p. 45). Interpreting Banathy methodological conception, Kathia Castro Laszlo affirms that social systems design "is a purposeful and creative process through which a human activity system can transcend its actual situation by translating an ideal image of the future into reality - it is concerned with that which ought to be. As an interactive and participatory process, SSD [Social Systems Design] is based on the premise that we cannot design *for* others: we can only design *with* others. Were we to do otherwise, we would not be engaged in authentic design but rather in the imposition of our visions, values, and proclivities. Systems design involves the use of scientific and intuitive knowledge, rationality and creativity, theory and practice, thinking and conversation, analysis and synthesis, participation and collaboration, evaluation and experimentation. Rather than relying on deduction *or* induction, as traditional sciences and the humanities do, systems design uses deduction, induction, *and* abduction - the latter involving the creative generation of new knowledge from what is already known." (Banathy, 1996, p. 34; Castro Laszlo, 2001).

By means of analogical paraphrasing we might affirm that for those conferences participants who know about peer refereeing weaknesses and conventional conferences' low level of effectiveness, conferences design and organization need "future creating disciplined inquiry" so future conferences might be increasingly more effective than the actual conventional one. The design and the organization of a conference, as a social and information system for knowledge communication, networking and learning, should be a "purposeful and creative process through which a human activity system can transcend its actual situation by translating an ideal image of the future into reality," in such a way as to increase its effectiveness with regards to the objectives that its participants might have. As an interactive and participatory process, conference design and organization might be based on the premise that **we should minimize the design *for* others: and maximize the design *with* others.** Were we to do otherwise, we would not be engaged in authentic design but rather in the imposition of our academic visions, epistemological values, and disciplinary proclivities. Conference design and organization should involves "the use of scientific and intuitive knowledge, rationality and creativity, theory and practice, thinking and conversation, analysis and synthesis, participation and collaboration, evaluation and **experimentation.**"

Opposition between Conventional Conferences and Conversational Meetings

The conversational and the conventional conferences formats oppose each other in several aspects. The table A below resumes some of them. It might be thought that because of these opposite aspects of both models, the respective meetings have been held separated from each other. But, in our opinion, this opposition does not necessarily mean a **contradiction**; it might be handled as a **polar** one from a synergic perspective, or a complementary opposition, where each opposite requires each other to generate a **synergic relationship** or to produce positive **emergent properties**, where the whole is more than the sum of its parts.

	Conventional Conferences	Conversations Format
Input	Paper based on a solution or an answer , which will be presented by an individual (its author).	A problem or a question , which will be addressed by a group .
Output	Knowledge or information communication.	Sharing of Knowledge, reflections, ideas and opinions in multi-directional communication.
Flow of Information	Basically unidirectional .	Multi-directional .
Sequence	Serial : one presentation after another, in a lineal format.	Serial/Parallel : multiple short presentations by each individual interacting with similar shorts presentations of others in a non-lineal interchange of ideas.
Cybernetic Loops	None or very low level of feedback in the small time period of questions/answers.	High levels of feedback and feedforward loops in a highly interactive environment.
Formal/Informal	Papers are presented in a formal environment and informal interaction is limited to coffee breaks.	More informal sharing of ideas and reflections with more possibilities of group creativity and ideas emergence.
Creativity	Individual (or group creativity) previous to the meeting.	Group creativity during the meeting nurturing and being nurtured by the individuals in the group in positive loops of feedbacks.
Order	Pre-established fixed order of papers presentations. Plan-based order.	Post-established, emergent and dynamic order. Rules-based order.
Process	Systematic	Systemic
Implicit general Objective	Oriented to efficient knowledge or information communication	Oriented to effectiveness in knowledge communication, sharing of ideas and reflections, solving problems, answering questions, achieving consensual designs, etc.
Whole/Parts	The whole is basically equal (or sometimes even less) to the sum of its parts	The whole is basically more to the sum of its parts
Guiding Metaphor	Mechanism	Organism
Methodological and Epistemic Approach	Mostly, but not uniquely, oriented by Reductionism and Mechanicism	Oriented by the Systems Approach and its Pragmatic-Teleological epistemology and methodologies.

Table A

On the other hand, the opposite features of both meeting models do not make any of them better than the other in an absolute form. Each model has its own advantages and disadvantages and depending on the objective of the organizers any one of them might be more or less adequate. If an appropriate combination is made of both of them we might amplify the advantages of each model and diminish its disadvantages. To identify some kind of an adequate combination, some tradeoffs should be made. These tradeoffs are, by its very nature, more subjective than objective, so they require subjects to do them with the objective of finding the most consensual one. In our case these subjects are the scholars, academics, researchers, practitioners, consultants and professionals who are the participants of our conferences and the users of their respective organizational process.

We think that to generate a good level of consensus with regards to this kind of tradeoff, between the two models, a meeting (or meetings) with a conversational format might be a good starting point. Consequently, our purpose is to begin with the very small step of organizing this kind of conversational meetings in the context of The WMSCI 2006, and other conferences organized by IIIS without any additional cost for the participants in these conferences. The input of these conversational meetings will be related to the possible ways of integrating both conference models, as well as to the conference relationships to knowledge communication, scholarly publishing and the possible solutions to the weaknesses of the required peer reviewing, which are amplified in conference submission reviewing as related to Journal's or grant's peer refereeing or reviewing.

As we will describe below the participation in the conversational meetings might end up with a paper, written after the conversational meeting and published in the respective post-conference volume of the conference proceedings, in a multiple-author book or in special issue of the Journal of Systemics, Cybernetics and Informatics (JSCI). It will depend on the willingness of the participants of the conversational meetings to work out the respective papers, as well as on the quality and quantity of these papers.

A Systemic (not systematic) Methodology will be applied

The methodology we will be following for the achievement of our purpose is a Systemic (not systematic) one, for the Systems Analysis and Synthesis, oriented basically (but not uniquely) to Social Systems Design as well as to Information Systems Development, Software Engineering Projects Management and Collective Decision Support Systems. This methodology has been continuously designed and re-designed, in a cybernetic process, using feedback and feedforward loops for its continuous improvement, generalizations and application to different kind of systems. This methodology has been successfully applied, in the last 25 years, to more than 120 design and implementation processes of different classes of systems, and it has been explained with details elsewhere (see, for example, Callaos, 1992; 1995a; Callaos and Callaos, 1995d). In a coming second version of this draft we will specify the way we will apply the mentioned methodology to the project of finding out if there is some way to improve the effectiveness of conventional conferences by means of integrating with them conversational meetings.

In this first version of the draft we can inform that this methodology we be based on the combinations of:

- A. The System Approach, in general, and specifically, the Singer-Churchman epistemology regarding the Pragmatic-Teleological perspective of the truth.
- B. The Incremental Approach to Planning.
- C. Cybernetic principles and specially the application of positive and negative feedback loops as well as feedforward loops to relate the increments of the incremental planning.
- D. Biological and ontological evolutionary concepts in the context of an analogical thinking that generated the evolutionary-incremental methodology when applied to B and C. (See, for example, Callaos 1995b). This methodology has been applied and taught in the last 25 years in several Venezuelan universities and many industrial seminars. The evolutionary-incremental methodology (which has been successfully applied to more than 100 projects in Information Systems Development) is a special case of the General Sytemic Methodology for Systems Analysis and Design mentioned in A. (See, for example, Callaos and Callaos, 1992; 1994; 1995a, 1995b, 1995c)
- E. The Mathematical Solution we found to The Voter Paradox (see, for example, Callaos, Callaos and Lesso, 1980) when a collective preference or decision is needed in any phase of the general and the specific methodology, and especially in the context of Collective Decision Support systems.

Next steps to be taken with the purpose of achieving the stated objective

In the more general methodological context we briefly mentioned above, we will try to establish a very small first increment and its implementation and the learning process that will certainly be generated will give us the orientation to establish the next planning increments and phases. With the first increment we will try to trigger a dynamic process that can provide an adequate level of adaptability and support the required learning process for the design and the implementation of the project by means of which we can achieve our stated objective. Consequently, we will plan just the first steps in order to allow the first conversational meetings and its respective collective preference to set the framework for the next steps. Accordingly, the first steps will the following:

1. To consult the participants of the next 2006 conferences with regards to the desirability and feasibility of making a first step in the next conferences by means of including in the respective conference programs at least one conversational meeting oriented to few initial questions which need some collective reflections and some consensual answers. If the participants think it is not feasible to trigger

- the mentioned process in the next 2006 conferences, it will be started in the 2007 conferences.
2. If an adequate number of academics, researchers, practitioners and professionals participating in the next conferences think that it is a good idea to initiate the processes in the next conferences, a first conversational meeting will be included in the conferences programs.
 3. After the conversations are over, each scholar, academic, practitioner or professional who participated in the conversational format may write an extended abstract based on his/her reflections as well as on the opinions, information and reflections shared by others in the same conversational meeting.
 4. These extended abstracts will be posted in an electronic forum as to continue with some kind of electronic conversations.
 5. Those authors of the extended abstract who might be interested in writing a full paper on the subject, trying to integrate their reflections with those of the other participants in the conversations (citing the content of the extended abstract and what they might have heard in the conversational meeting or what they might have read in the electronic forum)
 6. If there is an adequate number of full papers on the subject, the best of them will be selected for the publication of a multiple-author book or an issue in the Journal of Systemics, Cybernetics and Informatics (JSCI).
 7. Since the initial meeting and some papers can make suggestions about the next steps that might be taken, the following phases of the projects will be designed accordingly and the participants of the conversational meetings will be informed about it.

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Appendix B

Guidelines for the Conversational Format

Prepared by

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Richard Saul Wurman affirms that “You begin all the conversations with questions.” (Conversation: What are conferences for? *Harvard Business Review*, June, 2006, p.26). Other authors think that the conversational format might begin with “a panel, a video presentation, or a reading”. We will begin this conversation in the context of this 2006 conference with the question written in the conference program.

The Role of the Moderator

This role of the moderator of a conversational meeting can be both challenging and rewarding. She or he is the key to an open, proactive and productive conversation, enabling people to explore ideas and thoughts and seek common ground, consensus and understanding.

Here are a few suggestions for bringing out a range of opinions, exploring the topic thoroughly, and encouraging a rich and lively discussion. A good moderator:

- serves as a facilitator rather than a lecturer and encourage everyone to speak, creating opportunities for everyone to do so, and helping participants look for common ground.
- remains impartial, encourage tolerance to opposite ideas, shows respect for all opinions, and does not use the position to influence the outcome of the discussion,
- keeps the conversation moving and on track by occasionally summarizing points, and bringing the discussion back to the topic if it wanders unproductively,
- asks questions that challenge old assumptions,
- if a ground rule is broken, asks the group to help re-establish the rules,
- proposes changes in commentator role that might help improve next conversations,
- registers the names of the participants in the conversations, and, if possible, makes a very short description of the most important issues that were raised in the conversation.

Setting Ground Rules

Ground rules will help keep the conversation on track. Here are some suggested ground rules:

- Participants will be engaged listeners and contributing speakers, and will show respect for the views expressed by others.
- Although a frank exchange of conflicting views is encouraged, participants should be careful not to become argumentative or dogmatic in the expression of their point of view.
- Participants understand that their comments should be **brief no more than 5 minutes**. This is a basic rule to follow because it is essential to the interactive and participative nature of the conversational format. The group can change the limit of five minutes via consensus or, at least the majority rule. No one will monopolize the conversation. The moderator will ensure that all who wish to speak have a chance to express their views.
- Comments will be directed mostly to the group as a whole rather than to the discussion leader or another individual. Disagreements will not become personal; name-calling and shouting are not acceptable.
- Participants will not interrupt when others are expressing their views. If needed, the group can establish a mechanism (e.g., raising hands) to enable everyone to speak or respond to someone else's comments.

Wrapping up a conversation

To wrap up a conversation, within the specified time announced, two steps are suggested.

1. the moderator should give a fifteen-minute warning as the session draws to a close. This will give some time to the participants to make some individual conclusions and/or to share their opinion regarding the most important issues, ideas, answers, possibilities, etc. raised during the conversations. Or participants may be asked to comment on the efficacy or impact of the conversation for them. If a consensual conclusion is reached it must be made explicit, but consensual conclusion should not be forced on the group. Where different or opposite conclusions are in conflict then this is what should be made explicit.
2. Even though a thought-provoking dialogue is, in itself, a good outcome of any conversation, and this is one of the most important outputs of the conversational format, participants may want a sense of closure to the discussion. To achieve this objective the moderator may challenge the participants to identify areas of common ground as well as areas where opposing and different perspectives have been communicated.

Evaluation of the conversation

Participants can evaluate the conversation after the conclusion of the conference. They will be able to evaluate and make recommendations to both the process and the content of the conversation.

Post-Conversation publishing

Each scholar, academic, practitioner or professional who participated in the conversational format may write an extended abstract based on his/her reflections as well as on the opinions, information and reflections shared by others in the same conversational meeting.

These extended abstracts will be posted in an electronic forum as to continue with some kind of electronic conversations.

Those authors of the extended abstract who might be interested in writing a full paper on the subject, trying to integrate their reflections with those of the other participants in the conversations (citing the content of the extended abstract and what they might have heard in the conversational meeting or what they might have read in the electronic forum)

If there is an adequate number of full papers on the subject, the best of them will be selected for the publication of a multiple-author book or an issue in the Journal of Systemics, Cybernetics and Informatics (JSCI)

Appendix C

Output of the conversational format sessions in WMSCI 2006 and its Collocated Conferences

Prepared and semi-structured

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As planned, the sessions with conversational format held in the context of WMSCI 2006, and its collocated conferences (EISTA, CCCT, CISCI, PISTA, SOIC 2006), were purposefully run with no pre-established structure or structuring. The input of all these sessions had some related questions: How conferences might be organized in a more effective way? How peer reviewing might be done more effectively? etc. Each session generated its own level of structure-ness. One session had a high level of structure-ness because one of its moderators proposed a method based on the Nominal Group Technique, and it was accepted by the session's participants. Other sessions were maintained structure-less by their participants in despite of their respective moderator's efforts oriented to achieve some level of structure-ness. Other sessions had intermediate and occasional levels of structured-ness, although in an intermittent and in erratic ways.

Since, purposefully and explicitly, no structure, o semi-structure were externally pre-established, or enforced, several structures, semi-structures, or non-structure-at-all were internally generated in what we might call an emergent property of each sessions, determined by its moderators and its participants. But, to briefly inform about the output of these sessions, which is the main objective of this report, it should and need to be done with a minimum of structure-ness. Otherwise, this report will be an ineffective communicative tool. There is no way to describe comprehensively, with any kind of report, the ideas, knowledge, information and the emotions that were shared in the conversational sessions, as well as there is no way to substitute or to generate reflection processes similar to the ones lived by the participants in the conversations. What we are attempting with this report is:

1. To distill the essence of what has been said and shared in the conversational sessions.
2. To have a document that, although it cannot communicate the ideas and reflections shared in the sessions, it might serve as a brief written reminder, a minute, of the essential information generated at the conversational session. As with a minute written after a meeting, just those persons who participated in the meeting can understand, comprehend and integrate in its context the content of minute or the memorandum.

3. To serve as a communication tool for those who attended the conversations.
4. To support further interactions and communications regarding the basic objectives that accompanied the idea of organizing conversational format sessions.
5. To support a post-conference electronic communications regarding the issues involved in the conversational face-to-face sessions.
6. To bridge the conversational face-to-face sessions and the electronic conversations or forums on the subject, generating continuity among them.
7. To bridge these conversations, held in the context of past conferences, with conversations to be held in the context of future conferences to be organized by the International Institute of Informatics and Systemics: IIS.
8. To start the preparation of the material required input for the publication of papers on the issues involved in the conversational format.
9. To try to use the results of these conversations as an initial trigger for a possible research project on the involved issues; to use the results of the conversational **informal search** in a more structured and **formal research**, from the perspective of the action-research approach, action-learning and action-design, relating these three processes in a systemic whole. Indeed, several specific ideas that emerged in these meetings will be implemented for the 2007 conferences in order to action-learn about their possible efficiency and effectiveness (efficacy) via action-design through their actual implementation. This might be the starting point of an action-research in some of the involved issues.

As we said above, one of the conversational sessions was relatively structured, but most of the other sessions were highly unstructured conversational processes. Consequently, the set of results of all conversational sessions is highly unstructured. To try to semi-structure the output generated we will take the suggestion made, by Dr. Gary Metcalf, right at the beginning of the first session. Dr Metcalf is one of the most – if not the most – knowledgeable and experienced scholar in the area of conversational meetings (as they were theorized and practiced by Professor Bela H. Banathy). Dr. Metcalf suggested: “changing the input question to the conversational session from “**How** conventional conferences can be more effective?” to “**What** are the characteristics of an effective conference? Separating the ideas related to the “what” from those related to the “how” is a good starting point for attempting some kind of initial and gross classification-based structuring. Indeed, according to Plato, classification is a first step recommended for understanding reality, by means of a philosophical reflections or scientific research processes.

The “what” and the “how” can be related via analogical thinking to two of the four Aristotelian causes. They are analogically related to what Aristotle called the *formal* and

the *efficient* causes, respectively. The other two Aristotelian causes are the *teleological* and the *material* ones. These two causes might analogically be related to the “why” and the *resources/barriers* for making reality the “what”, via the “how”, in order to achieve the “why”, the final end or purpose. In a second version of this report, we will use these four conceptual categories to try a first classification of the ideas distilled from the conversational sessions, as a mean to have an initial semi-structuring of the results achieved in these conversations. In this report we will restrict ourselves to separate the “what” from the “how”. We will add the perceived barriers in achieving the characteristics of an effective conference, and we will differentiate between how to achieve effective conference from how to achieve effective peer reviewing.

1. What are the characteristics of an effective conference?

Most communicative phrases for this question, given by the conversational sessions’ participants, are the following:

“What a conference look like that you would love to attend/participate in?”

“What would an ideal conference look like?”

The answers given to this kind of questions and the reflections generated by them were distilled, mostly by the participants, with the following phrases and statements:

- 1.1. Learn in a relaxed environment
- 1.2. Theme or subject-matter of the conference.
- 1.3. Provision for significant interaction. Meet with other persons with similar interests. Meet right people. Interaction with similar and dissimilar people. Egalitarian. Make relationships.
- 1.4. Quality of attendees.
- 1.5. Possibilities of collaborative research and/or reflections.
- 1.6. Join informal conversations.
- 1.7. Be touched by stories.
- 1.8. Find academic technology transfer.
- 1.9. Ability to influence design of next conference.
- 1.10. Efficiency and effectiveness in achieving the conference’s objectives.
- 1.11. To verify/validate the known and to learn the unknown.
- 1.12. To participate in a conference should be an effective way of knowing what is going on in a field. Conferences are not for training.
- 1.13. Academic-Industrial conferences are there to learn from each other.
- 1.14. Plant seeds that grows later.
- 1.15. Venue with focus and spontaneity.
- 1.16. Electronic discussions before and after the face-to-face meetings.
- 1.17. Invitation to have a real role.
- 1.18. Quality facilitation.
- 1.19. Real-time capture and display of ideas and information.
- 1.20. Exceeds expectations based on clear expectations.

- 1.21. Peer-group interactions within informal and formal sessions.
- 1.22. Meet funding requirements.
- 1.23. Possibilities of emergence of communities where conference participants can serve as members.
- 1.24. Adequate conference infrastructure and logistics.

In one of the conversational sessions the answers to the question “What would an ideal conference look like?” were distilled and clustered in the following areas:

- a) Theme and content
- b) Enchantment
- c) Ownership and membership
- d) People and processes
- e) Housekeeping.

Using this distilled cluster we might suggest the following one which would take into account the output of other conversational sessions regarding to this issue.

- a) **Enchantment and learning in a relaxed environment.** To verify/validate the known and to learn the unknown, plant seeds that grows later, Real-time capture and display of ideas and information
- b) **Theme, content and subject matter of the conference**
- c) **Participants:** quality, similar and dissimilar interest and people, egalitarian, Peer-group interactions within informal and formal sessions.
- d) **Informal useful processes:** interaction, relationships, Join informal conversations.
- e) **Participation, Integration, continuity, membership and ownership:** Collaborative research, ability to influence design of next conference, Invitation to have a real role.
- f) **Adequate environment and venue:** Venue with focus and spontaneity, Possibilities of emergence of communities where conference participants can serve as members, adequate conference infrastructure and logistics.

2. **New questions that emerged while trying to answer the input questions to the conversational format.**

As it is known, conversational sessions are triggered by a question, not a solution, by a problem, not a solution; and they may produce answers or more questions. The conversational sessions held at the 2006 IIS' conferences generated the following questions:

- 2.1. Who should decide what the best conference model is? Academic self-reference?
Self designated elite?
- 2.2. How to decide what is important?
- 2.3. How we decide which mode of knowledge communications should be used?
Conventional an? Formal meetings? Conversational format? Hybrid meetings?
- 2.4. What is quality? Who define it?
- 2.5. What kind of peer reviewing should be used? Who should decide it?

3. Barriers for the achievement of the characteristics of an ideal conference.

- 3.1. Limited human, technological, managerial and financial resources.
- 3.2. Two narrow themes. Highly specialized technical sessions. [Interdisciplinary communications is lacking from most disciplinary and multi-disciplinary conferences.
- 3.3. Cultures and epistemological diversity present “language” and communicational barriers isolating disciplines and even sub-disciplines, sub-sub-disciplines and sub-sub-.....-sub-disciplines.
- 3.4. Confusion and miscommunications created by diversity of meanings, paradigms, and epistemological and methodological values.
- 3.5. Diversity in the meanings of ownership, memberships, enchantment, which is amplified in multi-cultural conferences.
- 3.6. Non-show would be presenters.
- 3.7. Fixed regimented presentations, in formal session presentations.
- 3.8. Tenure and academic promotions requirements: publications, publish-or-perish academic reality interferes with many desirable conferences’ characteristics.
- 3.9. Tight ownership by organizations. Lack of influence on outcomes. Inflexibility for entry.
- 3.10. Lack of meetings oriented to generate ideas regarding the issue of how to make feasible what it is unfeasible with the present means.
- 3.11. “Certification” Vs. participative learning.
- 3.12. Constraints vs. Possibilities. Resources restrictions limit the objectives achievement level. Resources efficiency vs. conference effectiveness.
- 3.13. Lack of support from reviewers.
- 3.14. Passive nature of conventional conferences.

As it could be noticed, most of the issues considered as barriers, are basically **what** conventional conferences **are not**. Consequently, in section 1, of this report, we collected opinions a bout what conferences should be. In number two we collected opinion **what** conferences **are not**, and what are the barrier to organize an ideal conference. A thing is usually defined by **what it is**, but, when defining a concept, some authors also include, **what it not**. This kind of definition is called negative definition, what a concept or a thing is no, while a positive definition is oriented to what a concept or a thing is. Positive and negative definitions complement each other, to describe **what it is** something, complements the description of what **it is not**.

The participants of one of the conversational sessions distilled and clustered the barriers they enumerated (which were included, in this report, in section 2 above) in the following areas or themes, all of which refer to traditional conferences as opposed to some new innovation in academic and professional meetings, as it is the case of the conversational format:

- a) Traditional format (as opposed to unconventional ones, as it is the case of the conversational format)
- b) The metaphor of Microsoft Vs. Linux, as a support for analogical thinking conducive to the conception of identifying new ways of conceiving academic and professional conferences.
- c) Opposing advocacy Vs. Inquiry would distill the characteristics of the traditional format Vs innovative ones, as it is the case of the conversational format.
- d) Opposing delivery vs. production may also serve the same purpose of contrasting the traditional format to the conversational one.
- e) Answers Vs. Questions
- f) Lecture Vs Laboratory
- g) Left Vs. Right Brain.

4. How the effectiveness of conferences might be improved?

The means that might be used or actions that could be taken in order to achieve the kind of conferences described in section 1 and to overcome the barriers enumerated in section might be some of the following:

- 4.1. Invite more conference participants to the conversational sessions.
- 4.2. Increase the promotion of the conversational format.
- 4.3. Review and benchmark enchanting conferences.
- 4.4. Format for next year combining both formats with more adequate balance.
- 4.5. Create Synergy [between both formats]
- 4.6. To increase the potential of inter-disciplinary interactions ask for papers oriented to inter-disciplinary communication: literature research, state of the art and cutting edge developments. This may also be followed by conversational format.
- 4.7. Visualize diversity: Language, Culture, Discipline, etc.
- 4.8. Reinforce Chairs and co-chairs as facilitators.
- 4.9. Survey of no-shows.
- 4.10. Align learning and formal processes of traditional conferences.
- 4.11. Find ways to add art and music.
- 4.12. Find a more adequate balance among different kinds of participants:
 - Academy and Industry meetings (or sessions) balanced in about 50%-50%.
 - Find a better balance in the academic area: Students and Faculty.
 - Promote the papers presentation by students
 - Find a more adequate balance among faculty, students and Industry.

- More participation of students as reviewers [there are several studies finding that graduate students and assistant professors do a better job in papers reviewing than senior scholars.
- Students as volunteers].

- 4.13. Create a mission for the conference.
- 4.14. Best papers prizes: industry sponsors, focus on students. Institute a Gold Medal for the best paper, including evaluation of papers presentations.
- 4.15. Invite “big” names speakers, “big” reputation schools, departments. Everyone else: Competitive review.
- 4.16. Over-emphasis on high status institutions might be an insult to low status institutions.
- 4.17. Inform “big” names of local achievements and Interests. Balance with local invitations to speak. Make connections.
- 4.18. Implement reflective closing session.
- 4.19. Create recommendations on findings generated from conference.
- 4.20. Reflect on what was significant.
- 4.21. Provide local tour
- 4.22. Reduce no-show.
- 4.23. Provide a learning environment.
- 4.24. Provide a stimulating Da Vinci environment.
- 4.25. Symposium, in the etymological sense of the word, in the sense of a platonic symposium.
- 4.26. Try, as experiment in the context of action-research and action-learning, poster sessions in an environment similar to a platonic symposium.
- 4.27. Organize conference every two years, instead each year.
- 4.28. Define effectiveness in conferences [In order to do so conferences’ objectives should be clearly identified and weighted]
- 4.29. Conversational sessions in different languages due to barriers and because it would improve generation of ideas. In such a case, the respective moderator should easily communicate in both languages in order to share with other conference’s participants the ideas generated in the respective session.
- 4.30. Different levels of meetings where people would feel more comfortable to present papers and share ideas and opinions.
- 4.31. Motivate the presenters to do their best communicate their knowledge as by clear and motivational methods of presentation [Some guidelines might help. Some prizes for the best presentations, not necessarily the best papers regarding their content, might also help in motivating the presenter in doing better presentation] One of the sessions recommended the following, which might be included in the guidelines that might be prepared for the presenters:
 - to add dynamic examples in order to present the knowledge clearly while using the unique characteristics of the computers.

- to involve the audience during the presenting process by asking them questions (at least one question) dealing with their thoughts regarding the main issues of the subject/research.
 - to combine Asynchronous and Synchronous modes of communication while conferencing.
- 4.32. Creative people use their knowledge in different context. Consequently, interdisciplinary communication should be fostered.
- 4.33. Hybrid format: 5-7 minutes of presentation followed by conversational format. This would require preparing and distributing the material before the meeting.
- 4.34. Conversational presentations, instead of poster sessions.
- 4.35. Integrate conversational and conventional formats. Hybrid conferences. Conversational format allows dynamic learning with pre-conference marketing of the paper and follow up conversations regarding the session's papers. Some specific ideas in this context are the following:
- Have a conversational follow up after a formal session. For technical papers, in narrow sub-sub-disciplines, ask authors: who would benefit from your paper? What might these benefits be or look like? What Society impact might have the implementation of your paper's ideas? That may trigger inputs for conversational follow up of the conventional format.
 - Proposal that authors "market" their papers previously and/or during session stressing specific contribution.
 - Ask for abstracts that everybody understands, even of the most technical papers.
 - Select the most outstanding papers as input to conversational sessions. Questions made in the context of those papers may be the conversational trigger. The respective authors should make the presentation more accessible for general audience.
 - Ask authors to add questions to their papers as a way of increasing the probability of the selections of their paper for a conversational format.
 - Panel presentations followed by a conversational format. Group reflections will follow ideas presentation made by the panelists.
 - Last day of the conference might be used for conversational sessions on some topics where papers were presented in a conventional format.
 - Conversational format in a right after the conventional conference.
- 4.36. Conversational sessions at a different regular sessions' time.
- 4.37. Promote more endogenous trans-disciplinarity and interdisciplinary interactions.
- 4.38. Add art, culture and promote activities oriented to a XXI Century Renaissance with Renaissance men and women.
- 4.39. Show interest in the identification of the impact of Science and Technology in Society. Ask authors to include in their papers and their presentations the possible Society impact of their solution **or** of what they are proposing. Remember that Science is neutral, not the evolution of Science.

- 4.40. The symposium on Knowledge Communication and Conferences (KCC 2006) is a very good idea, especially for improving the effectiveness of conferences. So, keep organizing this kind of conferences. It also might be organized as a separate conference, along with Knowledge Communications and Peer reviewing (KCPR 2006).
- 4.41. Exit poll at the end of sessions.
- 4.42. Have a fraud and a plagiarism policy.
- 4.43. Have different registration fees: for presenters and non-presenters.
- 4.44. Bundles for membership.
- 4.45. Find ways to overcome resistance to new ideas from reviewers or conference organizers.
- 4.46. Video conferencing.
- 4.47. Provide vegetarian food.
- 4.48. Board for messages from / and to conference's participants

5. How the effectiveness of peer reviewing might be improved?

- 5.1. Define effective conference and effective peer reviewing, as a necessary condition to increase effectiveness.
- 5.2. Work with review managers. Meta-reviewers.
- 5.3. Work with stem manager or stem chair.
- 5.4. Provide different kinds of reviewing methods according different disciplines' requirements and different authors' expectations.
- 5.5. In peer reviewing, reasoned or, commented judgments should be mandatory.
- 5.6. In reviewing of papers check for significance.
- 5.7. Fair review.
- 5.8. Bring in practitioners.
- 5.9. Provide reviewers with guidelines
- 5.10. Provide interaction between reviewers
- 5.11. Test the relevance of the paper for the themes of the conference and target audience.
- 5.12. Standards for peer reviewing should be worked out. There should be worldwide standards for different kind of conferences with different objectives. These standards should be voted by worldwide delegates. There should be at least two kinds of standards according the objectives conferences' participants are looking for when register as conference's participants: while academics looks basically, but not uniquely, for publications, participants from industry are looking for ideas, new ideas that might represent innovative products, services, methods or solutions.
- 5.13. Reviewer selects the paper that he or she will review.
- 5.14. Reviewing manager, o meta-reviewer select the paper which review will be reviewed by him or her.
- 5.15. Guidelines for reviewers. Train the reviewers.
- 5.16. Define paper quality and review quality.

- 5.17. Not a policy of accepting or refusing, but a policy of different kinds of acceptations for different kinds of presentations. For example:
- Papers accepted for 20-30 minutes of presentation time and others accepted for 5-10 minutes of presentation time.
 - Poster sessions with 5 minutes of presentation previous to the poster session.
 - Poster session followed by a conversational format.
- 5.18. Identify the objectives of peer reviewing. The users of peer reviewing should participate in this identification.
- 5.19. Organize a Doctoral Workshop on Reviewing.
- 5.20. Combine or integrate Democratic vs. Professional reviewing, Participative Vs. elite reviewing, bottom-up vs. top-down reviewing. These dualities are not necessarily conflictive or contradictory. They might represent polar opposites where the opposites complement each other in a synergic relation if an adequate integrative method or procedure is identified and applied.
- 5.21. Reviewers from corporations. Specifically ask for them for it.

Appendix D

A Multi-Methodological Reviewing Process for Multi-Disciplinary Conferences

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The main purpose of this brief paper is to present different actual conceptions and models of academic/professional conferences and their respective papers/abstracts reviewing processes as a means to show the desirability, if not the necessity, of using a multi-methodological processes in order to review the submissions made to a multi- and inter-disciplinary conference.

There is a high diversity in conference conceptions with a considerable number of different models and papers/abstracts reviewing process, even among the most prestigious, old, large and veteran scholarly societies and institutes. The types of required submissions and their respective reviews encompass a very wide specter going from the extreme of requiring from authors to submit just full papers which would be reviewed through methods and processes very similar to the ones used by Journal's editorial processes (several ACM and IEEE conferences are examples of this extreme), to the other extreme of accepting submission of no more than 50 words abstracts requiring no reviewing process (The prestigious and large INFORMS' and IFORS' conferences and annual meetings are examples of this other extreme.). Acceptance rates have as well a very wide range going from 14.9% ACM's SIGGRAPH 98, or lower, to about 100%, which is what is explicitly stated in INFORMS 2006, for example, in the following terms: "Contributed abstracts are not reviewed and virtually all abstracts are accepted."

Mediating between these two extremes we find a plethora of different conferences' models and conceptions. The following are some representative examples:

- The prestigious ACM's STOCs (ACM Symposium on Theory of Computing) and IEEE's FOCS (IEEE Symposium on Foundations of Computer Science) require submissions of extended abstracts (no more than 2000 words) and explicitly inform that they will not be accepting full papers submissions.
- The International Society for Systems Sciences: ISSS (previously named International Society for Systems Research: IFSR) the oldest and the most prestigious scholar association in the Systems movement, founded, 50 years ago, by Ludwing Von Bertalanffy, Kenneth Boulding, Ralph Gerard, James Grier Miller and Anatol Rapaport, accept abstracts as submission to its yearly conferences.

- The 43rd Annual Technical Meeting of the Society of Engineering Science held at PennState required the submission of abstracts and explicitly announced that “The proceedings of the 2006 SES conference will consist in one-page abstracts”
- IEEE’s 31st International Conference on Electronics Manufacturing and Technology (IEMT 2006), invited to the submission of extended abstracts of about 500 words and once the abstract is accepted, after its reviewing, a full paper would be included in the proceedings.
- DEANZ 2006 conference Enabling E-Learning Approaches held at the Auckland University of Technology (AUT) in New Zealand, informed that “Submissions may be **reviewed papers** which will go through a double-blind review process to ensure quality, **non-reviewed papers** or **posters**.” For the non/reviewed papers, the organizers asked for an abstract of 200 words which deadline was the same as the deadline of the Final paper submission of the reviewed option.
- The 22nd International Free Electron Laser Conference held in Duke Free Electron Laser Laboratory in Duke University, announced that the “The proceedings will consist of two parts: **Refereed Papers:** (Part 1) comprising the invited talks and a selection of contributed papers...and **Non-Refereed Papers:** (Part 2) will contain contributed papers in camera-ready form with the same page size.”
- The 2006 International Conference of Higher Education Research and Development Society of Australasia (HERDSA) announced the inclusion of refereed and non refereed papers for their presentation at the conference, held at the University of Western Australia. Submissions for non refereed presentations are required as abstracts limited to 250 words.

These are just examples of the huge variety of the conceptions of peer reviewing that are supporting conferences’ models used by conferences’ organizers. This variety is a consequence of the diversity of objectives or functions that conferences might have, which explicitly or implicitly orient conference organizational processes and methodologies.

These conferences’ objectives and functions differ significantly among scientific disciplines, between scientific and engineering, between epistemological and professional values, between academy and industry, between scholars, and consultants and managers, and even among different academic elites competing for the same resources. So, significant inconveniences should be expected organizing multi- and inter-disciplinary conferences, especially in those cases where an explicit purpose of the conference organizers is to bring together, not just scientists, from different disciplines, but also engineers, professionals, practitioners, consultants, managers and people from industrial, corporative and governmental realms. One way to deal with this kind of problem and hindrances in multi- and inter- disciplinary conferences is to use a **hybrid model** and a **hybrid methodology for peer reviewing**, integrated by the most used models and peer reviewing methods applied in conventional, established and habitual conferences,

organized by prestigious and/or age-old academic or professional associations or institutes. Such a hybrid model may amplify the very known weaknesses of peer reviewing and, for being open to different epistemological values and teleological variables, may be more vulnerable for electronic vandalism. Consequently, additional security measures and procedures should be added to a hybrid model with hybrid methodology for peer reviewing.

There is a very limited literature with regards to what conferences are or should be; what are the most used models; what are, or how should be, the peer reviewing methods to be used, etc. This limited literature along with the increasing number and variety of conferences being organized is a baffling paradox. A significant and an increasing number of publications (papers and books) are being produced regarding journals' peer reviewing, but very few publications has been made dealing with conferences' peer reviewing. Indeed, there are also very few publications related to conferences and symposia as such. It is a paradox that so many conferences are being organized with such a high diversity of objectives, models and peer reviewing methods and the publications are scant regarding these issues.

In Walker and Hurt (*Scientific and Technical Literature*, 1990) we find one of the very few publications made regarding what conferences are, or should be. The first chapter of their book is about *Journals*. The second chapter is about *conference proceedings* where the nature and the objectives of conference proceedings are differentiated and contrasted to journals publishing. Walker and Hurt affirm that scientific and technical written communications are produced on a continuum between the two extremes of formal and informal communications. They characterize the **formal** communications as (p. XII)

1. Public
2. Permanently stored
3. Typically retrievable
4. Relatively "old" information
5. Primarily user selected
6. Non-interactive

And they characterize **informal** communications as (p. XI)

1. Restricted
2. Temporarily stored
3. Difficult to retrieve
4. Recent and current information
5. Disseminator selected
6. Same work repeatedly reshaped in various informal media
7. Interactive

After establishing these characteristics of formal and informal written communications, Walker and Hurt affirm that "The formal media may be the more permanent means of recording ideas but they are only part of the overall enterprise...The formal media are

also not all equally formal...**The informal exchange of information among colleagues is much more important at certain periods of the research process than the formal media.**” (p. XX; emphasis added). It is this informality what represents the teleological essence of conferences. The degree of formality or informality is what generates such a plethora of conferences models and their inherent peer reviewing methods, if peer reviewing exists at all. Walker and Hurt affirm that conference proceedings, along with patents and technical reports, belong to what it is named “grey literature” which they associate with three features, as follow:

1. “it is not necessarily literature with original scientific contributions, but of an informational nature.” (p. XXI). This is why in many conferences potential participants are invited to present position papers, case studies, white papers, etc. and in other panels are organized. This is also why we find an increasing number, in the last 25 years, of meetings with conversational formats, where no paper presentations are made at all. As it is known the input of conversational meetings are questions and problems, not answers and solutions as it is the case of conventional conferences.
2. Literature “is distributed in a nonconventional manner and not within the normal marketing systems that make up the publishing and book trade.” (p. XXI). This is why some conferences have no proceedings at all, others have just electronic proceedings with no printed version, and still others have just abstracts in their proceedings. In these conferences some authors distribute copies of their papers to those who attended the session where their paper was presented and to colleagues with whom they informally interacted in coffee breaks, lunches, etc.
3. Literature which “is not bibliographically controlled and therefore difficult to locate and procure.” (p. XXI)

Walker and Hurt affirm that “the sources, kind and amount of grey literature are each increasing and at present [1990] may represent as much as 50% of all technical and scientific literature.” (p. XXII). If in 1990, grey literature represented 50% of all technical and scientific literature, it is to suppose with a high level of certainty that now it represents a larger percentage due to the exploding publications via Internet.

Walker and Hurt affirm as well that “Along the informal-formal continuum, conference papers may rest at or near the middle...Depending on the field, papers presented at conferences may not have gone through the development or review process as that required of the journal article...In some fields the conference is a place where ideas are tested, a place to distribute research results, or a place to claim priority. This last point reduces the journal to the role of repository of the archival record.” (p. 79).

According to these authors the main conference functions are the following:

- “To learn of the latest work being done
- To learn of the latest available equipment

- To make contacts with fellow researchers
- To discuss details with colleagues” (p.81)

They add that “Other purposes are served by conferences...there is a general sense that the “real” value of conferences and other lies in the informal communication that take place during, between, and after the formal presentation of prepared conference papers.” (pp. 81-82) The wide specter of the purposes that a conference might have plus the high plurality of means which might be used to facilitate informal communications, which are the real value of conferences, generate the huge variety of conference models as well as the high variety of reviewing methods (or no reviewing at all) used in different conferences. This variety of conference models, approaches and perspectives require tolerance from the advocates of competing models in order to allow the emergence of the real purposes of conferences. The intolerance that sometimes emerges between advocates of different conference models and their reviewing approaches and methods, or between elites from different disciplines, or between scientific and engineering conferences, or between academic and industrial/managerial conferences is really harmful to the global effectiveness of the conferences. This kind of problems is amplified in multi- and inter-disciplinary conferences, specially in those where one of the main purpose is to facilitate inter-disciplinary communications as a means of fostering cross-disciplinary fertilization and the analogical thinking that precedes logical thinking in scientific research, engineering design and technological innovations.

In our opinion, this is why multi- and inter-disciplinary conferences; especially those trying to foster cross-fertilization and interdisciplinary communications, should accept different kind of submissions (papers, drafts, extended abstracts, short abstracts, etc.) and implement different reviewing processes, from the formal papers reviews, to semi-formal extended abstracts review, to short abstract informal review, and even to a completely informal screening of very short abstracts of no more than 50 words (as it is the case of the age old INFORMS and IFORS conferences and meetings).

We introduced, in the conception, design and organization of conferences organized by IIS since 2006, in the context of action-design, action learning and action-reflection (that may conduce to an action-research project), different kinds of submissions. Four kinds of submissions, for example, were implemented for 2007 conferences, along with their respective four different kinds of reviewing methods and processes. In our opinion (a hypothesis in the context of action-research), this multi-methodological approach is highly desirable – and it might be even a necessary one – for an adequate organization of a multi- or inter-disciplinary conference. When we first began with this multi-methodological approach, we stated that these different ways of accepting submissions and reviewing them (four methods in a multi-methodology for a Multi-disciplinary conferences) might change in the future because the same dynamic nature of action-learning, action design and action-reflection. After implementing these four methods for the 2007 conferences, and after experiencing the implementation of them we learned (in action learning context) that Kaplan Method might not be feasible for conference organization. It was conceived for Journal reviewing and in this context it might be a desirable and feasible method. We also learned that short abstract reviewing is not always

desirable. Consequently, we removed, for the 2008 conferences, these two methods, placed at the extremes of the formal-informal specter of reviewing.

Let us first describe the 4 methods that were applied to submissions made to 2007 conferences, and then we will describe the three methods that will be applied to submissions made to 2008 conferences.

These four ways used to make submissions to the WMSCI 2007, and its collocated conferences, are the following, corresponding to review methods which go from semi-formal to the formal ones.

- **A brief Abstract** (100-400 words, not a full paper and not more than 400 words)
- **An Extended Abstract** (500-2000 words, not a full paper)
- **A draft paper to be reviewed by a Modified Kaplan's Method.**
- **A draft paper pre-reviewed according to Kaplan's Method** (Kaplan, 2005, , "How to Fix Peer Review", *The Scientist*, Volume 19, Issue 1, Page 10, Jun. 6)

In the context of action-learning and action-reflection, we decided for the 2008 and the 2009 conferences, as we said above, to remove the two extreme methods: the most formal and the most informal one, and following some suggestions made in conversational meetings we decided to add the option of accepting submissions for presentation only, which will not be included in the pre-conference proceedings.

Hence, for 2008 and 2009 conferences the 3 submission options that authors had were the following:

- **A draft paper to be reviewed by a Modified Kaplan's Method.** Accordingly, authors submitting a draft paper should suggest 2-3 colleagues for the non-blind reviews of their paper.
- **An Extended Abstract** (500-2000 words, not a full paper). Accordingly, authors submitting an extended abstract should suggest 1-3 colleagues for the non-blind reviews of their paper.
- **An abstracts** (200-500 words, not a full paper and not more than 500 words) for **presentation only**. Accordingly, authors submitting a brief abstract for-presentation-only may suggest 1-3 colleagues for the possible non-blind reviews of their paper.

Each brief abstract had also been sent to at least three reviewers for its double-blind review as well. Acceptance decisions were based on both kinds of reviewing: Non-blind and double-blind ones. If brief abstract were accepted only-for-presentation, their respective articles were not published in the pre-conference proceedings but authors had the option to publish them in the post-conference volume of the proceedings if:

- Their respective presenters were willing to include them in the post-conference volume of the proceedings; AND
- The full paper is received, according to the required format, by the respective deadline (about 20 days after the conference is over); AND
- The chair of the session where the paper was presented recommends its inclusion in the post-conference volume of the proceedings, supporting his/her recommendation on the opinions of the session's attendees.

Through 2008 and 2009 conferences we learned (from experience and from authors feedback) that the “for-presentation-only” option was not cost-effective for authors nor for the Organization Committee; and, generating confusion among authors, it was a source of undesirable entropy and miscommunication between authors and the Organizing Committee. We also learned, mainly from the conversational sessions and from informal feedback given by authors that it was a good idea to explore the option specifically oriented to support **inter-disciplinary communications**. Consequently, and in the context of the process of action-learning and action-reflection we were following, we decided to remove the option of “for-presentation-only” and to add an option oriented to specifically support inter-disciplinary communications.

Virtual Participation

Given the Global Recession, and thinking of those scholars, researchers and professionals related with the conference topics but unable to attend it personally (usually due to insufficient funding for the traveling costs) a **Virtual Participation** mode has been established, with the same peer reviewing and validity than face-to-face ones.

Submissions made for Virtual Participation are going through the same reviewing processes of the regular papers (double-blind, non-blind, and participative peer reviewing) and, if accepted (according to the same acceptance policy), they will be included in the proceedings and will be eligible for journal publication, **with no additional cost**, if they are, according to their reviewers, among the best 10%-20% of those physically and virtually presented at the conference.

Each regular session, included in the conference program, will be associated to a corresponding **virtual session** where all final versions of the articles to be presented will be displayed and authors can comment them via electronic forums. Registered authors of virtual participations will have access to all conference program sessions (and papers). Their article will be displayed as the regular ones. Virtual authors also have the option of sending, besides the final version of their article in a PDF document, an electronic presentation (PowerPoint, flash, etc. and/or a 15-20 minutes video)

Submission Options for the 2010 Conferences

According to what we described above, for de 2010 conferences (and following conferences if no more changes are made) there will be three submission options for

face-to-face participation, and three corresponding options for **virtual** participation. These options are the following (and are explained in more detail below):

- FA. A Draft Paper (2000-5000 words), for face-to-face participation
- FB. An Extended Abstract (400-2000 words), for face-to-face participation
- FC. An Abstract written for Inter-Disciplinary Communication (200-600 words), for face-to-face participation
- VA. A Draft Paper (2000-5000 words), for virtual participation
- VB. An Extended Abstract (400-2000 words), for virtual participation
- VC. An Abstract, written for Inter-Disciplinary Communication (200-600 words), for virtual participation

Authors should fulfill certain requirements for each of these six options as indicated below:

FA. Full Draft papers (2000-5000 words) submitted for their presentation at the conference and for inclusion in the conference proceedings, in their hard copy and CD versions. This kind of submissions will be reviewed by a **Modified Kaplan's Method**, where the submission's author should suggest at least two scholars, researchers and/or professionals for the **open, non-blind** review of his/her paper. Each paper will also be sent to at least 3 reviewers for its **double-blind** review as well. Acceptance decisions will be based on both kinds of reviews: Non-blind and double-blind ones. [David Kaplan's article titled "How to fix Peer Review" (The Scientist, Volume 19, Issue 1, Page 10, Jun. 6, 2005) can also be accessed at <http://www.scienceboard.net/community/perspectives.142.html>

FB. Extended abstracts (400-2000 words, not a full paper) submitted for presentation at the conference and for inclusion in the conference proceedings, in their hard copy and CD versions. Authors submitting **Extended Abstracts** should suggest at least one scholar, researcher, or professional for the **open, non-blind** review of his/her abstract. Each extended abstract will also be sent to at least three reviewers for its **double-blind** reviewing as well. Acceptance decisions will be based on both kinds of reviewing: Non-blind and double-blind ones. **"The submission should contain a scholar [or a professional] exposition of ideas, techniques, and results, including motivation and a clear comparison with related work."** (as it is indicated for submissions to be made to the Annual IEEE Symposia on Foundations of Computer Science: FOCS).

FC. Abstracts, written for Inter-Disciplinary Communication (200-600 words), may be submitted for presentation at the conference and for inclusion in the conference proceedings, in their hard copy and CD versions. The purpose the Organizing Committee seeks by allowing this kind of submissions is to foster communications among different

knowledge domains, different disciplines, and different kinds of experiences, as for example between academic and corporate knowledge/experience. Authors submitting abstracts for Inter-Disciplinary Communication should write both, the abstract and the full paper in a way as to be understood by scholars from other disciplines, i.e. they should be written in non-technical, non-disciplinary terms, and should clearly state the contributions the authors are making in their respective disciplinary or interdisciplinary field, and/or the potential impact of the article's content in other disciplines. Analogical thinking is suggested for these articles as complement of the usual logical-disciplinary one. Consequently, this kind of articles may contain inter-disciplinary analogies, expressional metaphors, analogical inferences, communicational analogies, analogy-based hypothesis formulations, design proposals, etc.

Authors submitting **Abstracts** may suggest 1-3 scholars, researchers, or professionals for open, non-blind reviewing of their respective abstract. Each abstract will also be sent to at least three reviewers for its double-blind reviewing as well. Acceptance decisions will be based on both kinds of reviewing: non-blind and double-blind. The submission should be similar to the abstracts or introductions usually written at the beginning of a full paper, containing "a scholarly [or a professional] exposition of ideas, techniques, and results, including motivation and a clear comparison with related work" (as it is indicated for submissions to be made to the Annual IEEE Symposia on Foundations of Computer Science: FOCS).

VA. Full Draft papers (2000-5000 words) submitted for **Virtual Participation** at the conference and for inclusion in the conference proceedings, in their hard copy and CD versions. Similarly to the face-to-face option above (indicated as FA), "this kind of submissions will be reviewed by a [Modified Kaplan's Method](#), where the submission's author should suggest at least two scholars, researchers and/or professionals for the **open, non-blind** review of his/her paper. Each paper will also be sent to at least 3 reviewers for its **double-blind** review as well. Acceptance decisions will be based on both kinds of reviews: Non-blind and double-blind ones. [David Kaplan's article titled "How to fix Peer Review" (The Scientist, Volume 19, Issue 1, Page 10, Jun. 6, 2005) can also be accessed at <http://www.scienceboard.net/community/perspectives.142.html>]"

VB. Extended abstracts (400-2000 words, not a full paper) submitted for **Virtual Participation** at the conference and for inclusion in the conference proceedings, in their hard copy and CD versions. Similarly to the face-to-face option above (indicated as FB), "authors submitting **Extended Abstracts** should suggest at least one scholar, researcher, or professional for the **open, non-blind** review of his/her abstract. Each extended abstract will also be sent to at least three reviewers for its **double-blind** reviewing as well. Acceptance decisions will be based on both kinds of reviewing: Non-blind and double-blind ones. **"The submission should contain a scholar [or a professional] exposition of ideas, techniques, and results, including motivation and a clear comparison with related work"** (as it is indicated for submissions to be made to the Annual IEEE Symposia on Foundations of Computer Science: FOCS)."

VC. Abstracts, written for Inter-Disciplinary Communication (200-600 words), submitted for **Virtual Participation** at the conference and their inclusion in the conference proceedings, in for hard copy and CD versions. Similarly to the face-to-face option above (indicated as FC), “the purpose the Organizing Committee seeks by allowing this kind of submissions is to foster communications among different knowledge domains, different disciplines, and different kinds of experiences, as for example between academic and corporate knowledge/experience. Authors submitting abstracts for Inter-Disciplinary Communication should write both, the abstract and the full paper in a way as to be understood by scholars from other disciplines, i.e. they should be written in non-technical, non-disciplinary terms, and should clearly state the contributions the authors are making in their respective disciplinary or interdisciplinary field, and/or the potential impact of the article’s content in other disciplines. Analogical thinking is suggested for these articles as complement of the usual logical-disciplinary one. Consequently, this kind of articles may contain inter-disciplinary analogies, expressional metaphors, analogical inferences, communicational analogies, analogy-based hypothesis formulations, design proposals, etc.

Authors submitting **Abstracts** may suggest 1-3 scholars, researchers, or professionals for open, non-blind reviewing of their respective abstract. Each abstract will also be sent to at least three reviewers for its double-blind reviewing as well. Acceptance decisions will be based on both kinds of reviewing: non-blind and double-blind. The submission should be similar to the abstracts or introductions usually written at the beginning of a full paper, containing “a scholarly [or a professional] exposition of ideas, techniques, and results, including motivation and a clear comparison with related work” (as it is indicated for submissions to be made to the Annual IEEE Symposia on Foundations of Computer Science: FOCS).”