

# The relevance of Peircean semiotic to computational intelligence augmentation

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## ABSTRACT

The aim of the present paper is, first, to describe the distinction of two types of computational intelligence research as Peter Skagestad has distinguished them: *Artificial Intelligence* or “AI” and *Intelligence Augmentation* or “IA”; then, second, to draw attention to a special sort of IA research, namely, computer programming which aims at supporting, augmenting, and perfecting the critical control of research communication and publication. Skagestad has been especially concerned to position Peirce as providing a theoretical basis for IA comparable to the foundational position of Alan Turing in relation to AI, and he does this by explaining what is implicit in Peirce’s dictum that “all thought is in signs,” which he construes as meaning that *all thought is materially embodied*, which he interprets as involving a recognition of the importance of exosomatic embodiments of mind. In developing Skagestad’s conception of IA further in the direction indicated I also ground this in Peirce’s dictum, but I do so by making explicit a different (but complementary) implication of it, namely, that *all thought is dialogical*. As an exemplary (but not prototypical) case of IA of this special sort, I use the automated archive and server system of primary publication created by the physicist Paul Ginsparg at Los Alamos National Laboratory some 12 years ago, which is presently in successful use in the fields of high energy theoretical physics and several closely associated fields in physics, astronomy, and mathematics. I argue that a proper understanding of the success of this system, which can be regarded as an IA application, reveals it to be an ideal implementation of computationally assisted primary (i.e. formal) publication. However, the interesting cases for development of IA in this area will be those that attempt to find out and design computational assistance for the many varieties of communicational practices involved in research activity that precede the stage of inquiry at which formal assertion of putative findings occurs. Interest in these less formal and rigorous types of communicational practices has yet to develop because they must be understood in relationship to the formal publication practices, and these latter have been so poorly understood that there has been no conceptual framework available for investigating these other and equally important practices as regards their rationale and needs.

## 1 INTRODUCTION

Peter Skagestad -- philosopher and Peirce scholar -- identifies two distinct programming visions that have animated research into computationally based intelligence which he labels, respectively, as: “*Artificial Intelligence*” or “AI” and “*Intelligence Augmenta-*

tion” or “IA”.<sup>1</sup> The aim of the present paper is, first, to describe the distinction of these two types of computational intelligence research for the benefit of those who might not be accustomed to recognizing these as co-ordinate parts of it, and then, second, to draw attention to a special sort of Intelligence Augmentation (IA) research which seems to me to warrant special emphasis and description, both because of its potential importance and because Skagestad’s account of the distinctive features of IA research does not seem to me to capture the most salient characteristics of this special part of it, perhaps because it may not have occurred to him that it is distinctive enough to require special attention in order to be recognized for what it is.

AI research can be characterized roughly as computer programming which aims at creating machines that can think as well or better than humans can think, whereas IA research is computer programming which aims at providing a computational basis for augmenting or increasing the effectiveness of human thinking by assisting it, as distinct from attempting to replace it by a machine simulation. The two can be regarded as being, in a general way, complementary in application, and the term “computational intelligence research” or “CI research” (as I will abbreviate it) can reasonably be regarded as embracing both. The particular type of IA to which I wish to draw attention here is computer programming which aims at supporting, augmenting, and perfecting the critical control of research communication and publication.

Although the philosophical work of Charles Peirce is relevant to AI as well as to IA,<sup>2</sup> Skagestad is especially concerned to position Peirce as providing a theoretical basis for IA comparable to the foundational position of Alan Turing as regards AI in virtue of the latter’s conception of the Universal Machine and of the so-called “Turing Test” for computer intelligence. Skagestad positions Peirce in this way by explaining what is implicit in Peirce’s dictum that “all thought is in sign,” construed as meaning that *all thought is*

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<sup>1</sup> Peter Skagestad, “The Mind’s Machines: the Turing Machine, the Memex, and the Personal Computer”, *Semiotica* vol. 111, no. 3/4, 1996, 217-243.

<http://members.door.net/arisbe/menu/library/aboutcsp/skagesta/semiotica.htm>

The same distinction is implicit in an earlier paper but is not drawn explicitly as a distinction between AI and IA, as so labeled. See: “Thinking with machines: Intelligence Augmentation, Evolutionary Epistemology, and Semiotic”, *The Journal of Social and Evolutionary Systems*, vol. 16, no. 2 (1993), 157-180.

<http://members.door.net/arisbe/menu/library/aboutcsp/skagesta/thinking.htm>

Whether Skagestad was the first to distinguish explicitly between Artificial Intelligence (AI) and Intelligence Augmentation (IA) in just these terms, treating it as a formal distinction, I do not know. The distinction itself can be said to have existed in some sense as far back as 1962 (if not earlier) when the idea of computationally based intelligence augmentation was first described as “intelligence augmentation” by Douglas Engelbart. The explicit drawing and labeling of a distinction as a distinction to be generally recognized thenceforth using a certain suggested label for it is of more importance than one might think, for it establishes a certain formal structure that can and often does function importantly thenceforth in the systematic organization of ideas. In any case, Skagestad uses the AI/IA distinction again in a later paper, “Peirce, Virtuality, and Semiotic”, in the Paideia Project on-line (1998), which is also available on-line:

<http://www.bu.edu/wcp/Papers/Cogn/CognSkag.htm>

<sup>2</sup> Two major areas of AI in which extensive application has already been found for Peirce’s work are knowledge representation and abduction, for example.

*materially embodied*. In developing Skagestad's conception of IA further in the direction indicated I also ground this in Peirce's dictum, but I do so by making explicit a different (but complementary) implication of the same Peircean dictum, namely, that *all thought is dialogical*.<sup>3</sup> As an exemplary (but not prototypical) case of IA of this special sort, I use the archive and server system of primary publication created by the physicist Paul Ginsparg at Los Alamos some ten years ago which is presently in use in the fields of high energy theoretical physics and several closely associated fields in physics, astronomy, and mathematics.

## 2 THE DISTINCTION BETWEEN AI AND IA RESEARCH

The present audience will require no reference to the literature on AI research, but the basis for the IA movement in computational intelligence research may not be equally familiar. The distinction is certainly implicit in much of the speculative literature on computational intelligence in the past few decades, but the overt recognition of these as two equally important developments within the broader category of computational intelligence programming seems to be relatively recent.<sup>4</sup> As background for the present paper I recommend three papers by Peter Skagestad on this topic which are easily available online. Links to these papers are to be found on the Peirce "ARISBE" website on the following web page:<sup>5</sup>

<http://members.door.net/arisbe/menu/library/aboutcsp/aboutcsp.htm>

All three of these papers are relevant, but I will only be touching here on a few of the points he makes in them, chiefly (though not exclusively) in the paper of 1993. In these papers, Skagestad distinguishes between *AI* or Artificial Intelligence and *IA* or Intelligence Augmentation as two distinguishable types of programming goals that correspond to what he regards as two distinct "computer revolutions", rooted in "two very different notional machines", namely, Alan Turing's Universal Machine, as described in his 1936

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<sup>3</sup> Skagestad is quite aware that this is a further implication of the dictum, but he does not make use of that in articulating the conception of the IA programming paradigm.

<sup>4</sup> In a message to the PEIRCE-L discussion forum (of 12-06-2002), Skagestad suggests that the explicit recognition of the distinction, using the tags "AI" and "IA", respectively, may be due to the computer scientist Frederic Brooks, who is quoted by Howard Rheingold, in his *Virtual Reality* (1991, p. 37) as saying: "I believe the use of computer systems for intelligence amplification is much more powerful today, and will be at any given point in the future, than the use of computers for artificial intelligence (AI)... In the AI community the objective is to replace the human mind by the machine and its program and its database. In the IA community, the objective is to build systems that amplify the human mind by providing it with computer-based auxiliaries that do the things that the mind has trouble doing." Note that Brooks speaks of "amplification" rather than "augmentation", though the conception would seem to be much the same.

<sup>5</sup> See Note 1 above for their individual URLs. See also Skagestad's "Peirce's Inkstand as an External Embodiment of Mind", *Transactions of the Charles S. Peirce Society*, Summer 1999, vol. XXXV, No. 3, pp. 551-561.

paper on computable numbers,<sup>6</sup> and Vannevar Bush's Memex, as described in a paper by Bush of 1945.<sup>7</sup> Skagestad says:

Both the Turing machine and the Memex attempt to mechanize specific functions of the human mind. What Turing tried to mechanize was computation and, more generally, any reasoning process that can be represented by an algorithm; what Bush tried to mechanize were the associative processes through which the human memory works. ...

The Memex, which attempts to replicate human memory, and hence may be aid to embody "artificial memory", was not intended to rival the human mind [as AI does] but to extend the reach of the mind by making records more quickly available and by making the most helpful records available when needed. This idea directly inspired the research program known as "intelligence augmentation" (IA), which was formulated in 1962 by Douglas Engelbart with explicit indebtedness to Bush, . . .

Skagestad remarks further that:

The Turing machine is the ancestor of the inference engine under the hood of the personal computer . . . , while Bush's Memex is the ancestor of many of those features we refer to, collectively, as the user interface.

And he reminds us that:

In the sixties computers were huge, expensive machines usable only by an initiated elite; the idea of turning these machines into personal information-management tools that would be generally affordable and usable without special training was advocated only by a fringe of visionaries and was regarded as bizarre not only by the general public, but also by the mainstream of the electronics industry. The second computer revolution obviously could not have taken place without the first one preceding it, but the first computer revolution could very easily have taken place without being followed by the second one.

Phenomena of this complexity are often explainable, as regards their origins, from more than one perspective. Real things have facets, and multiple complementary perspectives on complex historical realities is usually required in order to have a reasonably sophisticated account of them overall. In this case the role of visionaries like Turing and Bush is undoubtedly important, but there are other things to be said about the origins of the conception(s) of the computer as well, and my guess is that, as regards the origin of the conception of it as an instrument of personal use in augmenting the ability to produce text, to work with documents in various ways, and to communicate with others it originated, in part at least, as an unintended by-product of work designed to satisfy the need to document the programming involved in mainframe computing, the maintenance of which

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<sup>6</sup> Turing, Alan M. (1965). "On Computable Numbers, With an Application to the *Entscheidungsproblem*." In *The Undecidable*, Martin Davis (ed.), 116-154. Hewlett, NY: Raven Press. Originally published in *Proceedings of the London Mathematical Society*, 2nd Series, 42 (1936), 230-265.

<sup>7</sup> Bush, Vannevar, As We May Think, in the *Atlantic Monthly*, 176(1) (1945); reprinted in Nyce, James M. and Kahn, Paul (1991), eds. *From Memex to Hypertext: Vannevar Bush and the Mind's Machine* (San Diego, CA: Academic Press, Inc., 1991), 641-649. Available also at the Arisbe website: <http://www.theatlantic.com/unbound/flashbks/computer/bushf.htm>

required that records be kept both for one's own use as a programmer and for the use of other programmers as well. This in turn required the ability not only to record information but also to communicate it, which could be facilitated by making use of the powers of the computer itself as the instrument for doing such recording and transmitting.

It was by no means necessary to make such use of the computer for this purpose, though, since the recording and communicating of programs and programming notes could all have been done in ways previously used for recording and communicating things like that, namely by writing them down on sheets of paper either by hand or by use of a typewriter. But once the use of the computer itself for such purposes was recognized as a possibility and regularly practiced, it is not surprising that there would be a few people here and there perceptive enough to grasp much broader and more exciting visions of possible use, for the purpose of actualizing what Vannevar Bush had envisioned in Memex, which was, among other things, the prototypical vision for what later became the conception of hypertext linkage.

In any case, Skagestad himself draws three preliminary conclusions from his historical account of the difference of the two visions:

First, the Turing machine and the Memex each provided an indispensable piece of the technology that has become known as the personal computer, which we may today opt to conceptualize either as a personal Turing machine or as a computerized Memex;

Second, the two constructs are not rivals in the sense of offering conflicting solutions to the same problem; Bush and Turing were attacking entirely different problems, and so their respective solutions do not directly conflict with each other; but:

Third, the two constructs embody different conceptions of the human mind in general and of human-machine interaction in particular.

He continues, saying:

Turing regarded the human being as essentially indistinguishable from a machine; Bush regarded the human being as essentially a machine user, and sought to construct symbol-manipulation machines that would be "thinking machines" in the sense of machines to think with, not machines that think. While Bush's vision has served as the inspiration for a vast industry that is rapidly transforming our culture and society, Turing's vision has become the governing paradigm of the research program known as artificial intelligence (AI), and indeed for the entire interdisciplinary field known as cognitive science. So pervasive is the influence of this paradigm that one frequently hears it said that the computational model is the only comprehensive and fully articulated model of the mind available. There is, however, a different model of the mind available—one which, while not articulated by Bush, is fully supportive of the research program Bush initiated, the program today known as "intelligence augmentation" (IA). The model I have in mind is one which was articulated in the nineteenth century by Charles Peirce, and which has recently been advocated by James Fetzer as the semiotic model of the mind.

To summarize to this point, Skagestad's basic argument is to the effect that computational intelligence research (CI research) has thus far worked chiefly from two distinctive visions of what might be achieved -- AI (Artificial Intelligence) and IA (Intelligence Augmentation) -- which are capable of being regarded as complementary rather than ex-

clusive alternatives of CI development, but which may tend to be at odds with one another because of the importantly different conceptions of mentality which lie at their respective bases. Skagestad's primary aim thus far, though, has not been to encourage research development in which they are capable of being mutually supportive, though he is doubtless in favor of this, but rather to make clear that the second paradigm for research into computational intelligence is conceptually independent of the first, such that what we refer to as if it were one thing, the computer, is in reality two importantly different things at once: on the one hand, an algorithm-embodiment mechanism capable of mimicking mentality functionally to an extent yet to be determined; on the other, an instrument for coordinating factors variously involved in human intelligence insofar as these can be supported mechanistically in such a way as to augment human intelligence instead.

Skagestad regards the theoretical basis for the AI conception as lying in Turing's conception of the Universal Machine, but he does not regard the corresponding historical figure in Intelligence Augmentation, Vannevar Bush, as providing the theoretical basis for the IA tradition generally. His view is rather that although Peirce did not envision its actualization in the concrete way Bush did in his conception of the Memex machine, Peirce's philosophy does provide a theoretical basis for the IA tradition generally in a way that Bush's more limited vision does not. Skagestad also recognizes others whose conceptions are supportive of this theoretical basis as well, most notably Karl Popper and his conception of the exosomatic evolutionary development of mind, as is explained at some length in Skagestad's 1993 paper. But he regards Peirce's work, which was prior to Popper's, as being theoretically more adequate.

### **3 IS THERE A UNITARY PRINCIPLE FOR IA RESEARCH GENERALLY?**

I agree with Peter Skagestad both as regards the need to recognize that two distinct research projects have actually been at work in the development of computational intelligence technology, and as regards the claim that Peirce's philosophy can provide a theoretical basis for the second kind of computational intelligence project as well as contributing importantly to the first. I take this basic agreement for granted here, but before going ahead to explain the further aspect of the IA research tradition which especially interests me, I should note first that I do not think that Skagestad has succeeded thus far in identifying precisely enough what it is that is fundamental in the IA tradition that runs from Bush through Douglas Engelbart, J.L.C. Licklider (internet development), Ivan Sutherland (computer graphics), Ted Nelson (hypertext), Alan Kay (interface design), and other stellar figures up through Tim Berners-Lee, who both invented the conception of the world wide web and at the same time established it as an actual world wide hypertext system, beginning around 1989, and who still continues with his development work on the so-called "semantic web".<sup>8</sup> That is, I do not find any place where Skagestad de-

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<sup>8</sup> The semantic web is the world wide web as augmented by programs providing machine-readable descriptions of the resource material available as the referential content of websites along with programs for processing this material regarded as information in ways useful to users of the web. Berners-Lee explains his larger vision of the web in an informative account of the way the web actually developed both in concept  
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Queiroz, J. and Gudwin, R. (Guest Editors)

scribes IA in a way that seems to capture what the various facets of it to which he appeals in his account have in common which would justify regarding this second controlling vision as itself a single or unitary vision, though I believe there is indeed some such unifying factor to be appealed to.

Thus Skagestad at times refers to IA as being based on the conception of *the personal computer*, in contrast with the conception of the computer exemplified in the kind of computing characteristic of mainframe computing. This could perhaps be firmed up by identifying some trait or traits essentially characteristic of personal computers that could be shown to involve the rest by implication, but I do not find that this is done satisfactorily. He also frequently mentions the problematics and purposes of *user interface design* as of the first importance, and that, too, is certainly to the point but also is not itself satisfactorily defined. In using Bush's vision of the Memex machine as an historical basis, he is, in effect, privileging the principles of *hypertext* as fundamental, and this surely is of basic importance, too. But, again, I find no attempt on Skagestad's part to demonstrate that these principles are somehow at the bottom of it all. *Networking* is still another possible candidate which he uses as illustrative of the second revolution in computing, but the general idea at the basis of networking would have to be made clear and shown to be conceptually basic relative to the other factors mentioned as characteristic of IA research and this has not been done either.

My own hunch -- and it is little more than that, but it seems worth mentioning in a suggestive spirit here -- is that the key to the identity of what Skagestad characterizes as the IA tradition in computational research lies in the conception of *interactive* computing, which he does indeed recognize in passing but does not linger on. One reason for thinking this might be the key factor is that the conception of the personal computer seems to have developed historically in large part from the attempts of the early hacker community at MIT to take advantage of the DEC machines that came into competition with the IBM mainframes, as being more responsive to the programmers' needs than the monoliths that preceded them. These needs included the need to *play* -- the fountainhead of creativity in the development of the computer generally, in my opinion -- and the games devised were interactive ones involving the production of text to be produced by the player and interpreted by the computer, and text produced by the computer to be interpreted by the player, in a continual response and counter-response which simulated human interactivity with things in one's environment in the context of a structure of inquiry which gave sense to it all. I am referring, of course, to the "adventure" games in particular, which were games of discovery as based on clues provided by textual descriptions of what items were to be found in the labyrinthine tunnels of the "Colossal Cave" in which the adventurers found themselves.

With this, the paradigm of the computer as an algorithm-enacting machine was implicitly displaced by quite another vision of what these things were all about; for regardless of what was happening on the side of the *machine* -- let us assume it was nothing but the use of algorithms in application to data structures -- what was happening on the side

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and in implementation in his *Weaving the Web: The Original Design and Ultimate Destiny of the World Wide Web*, with Mark Fischetti (HarperCollins Publishers Inc., 2000).

of the game *player*, who was an integral part of the overall interactive system, was not algorithmic, with the result that the overall system of interaction could not itself be understood simply as the orderly triggering of algorithms and bore little overall resemblance to what the machine appeared to be in the perception of the mainframe programmer, who was accustomed to thinking in terms of the machine as dedicated to the enactment of purely deductive routines operating on data supplied to it for purposes of drawing just such deductive conclusions from it. Finding your way out of the Colossal Cave required a lot of deduction, to be sure, but algorithmic deduction was not the overall form of the activity of the interactive person-and-machine, which, in effect, humanized the latter by informing it with human spontaneity in the service of discovery.

Human and machine interactivity in the solution of problems arising in the context of discovery is the point from which I would start, then, in attempting to get a clear and unitary vision of the essence of what Peter Skagestad regards as the second computer revolution and identifies with the project of IA or Intelligence Augmentation.<sup>9</sup> Skagestad might agree with me on this -- I am not suggesting any disagreement here -- but as best I can make out from what he does say in the articles mentioned, the starting point for understanding IA philosophically for him has been rather with the idea of the “exosomatic” location of mind in the material environment.

Let me explain now how this relates to the Peircean dictum that all thought is in signs, which he regards -- rightly, in my opinion -- as the key conception for understanding Peirce’s semiotic as capable of providing a theoretical basis for IA generally.

#### **4 THOUGHT IS IN SIGNS – THOUGHT IS EXOSOMATICALLY EMBODIED (SKAGESTAD)**

Peter Skagestad understands the dictum “All thought is in signs” to mean that thought is not primarily a modification of consciousness, since unconscious thought is quite possible in Peirce’s view, but rather a matter of behavior -- not, however, a matter of a thinker’s behavior (which would be a special case) but rather of the behavior of the publicly available material media and artifacts in which thought resides as a dispositional power. The power is signification, which is the power of the sign to generate interpretants of itself. Thinking is semiosis, and semiosis is the action of a sign. The sign actualizes itself *as* a sign in generating an interpretant, which is itself a further sign of the same thing, which, actualized *as* a sign, generates a further interpretant, and so on. As Skagestad construes the import of this -- correctly, I believe -- the development of thinking can take the form of development of the material media of thinking, which means

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<sup>9</sup> Note that I am referring to human-machine interaction, as distinct from other types of input of information originating from some source external to the machine. As usually conceived, artificial intelligence seems to be distinguished by its concern with developing machines which have “stand-alone” intelligence, in the sense that whatever intelligence the machine has can be attributed to it without implicit reference to the human-and-machine relationship in a way that would allow its intelligence it exhibits to be regarded as due to the human with whom it is interacting. For example, the ability of my home computer to beat me at Reversi almost every time we play seems to be a clear case of AI, but not of IA.



such things as the development of instruments and media of expression, such as notational systems, or means and media of inscription such as books and writing instruments, languages considered as material entities like written inscriptions and sounds, physical instruments of observation such as test tubes, microscopes, particle accelerators, and so forth. The evolution of mind means that cognition is still developing, not primarily in the nervous system and brain and not in some mysterious kind of immaterial mind-stuff, but rather in the material instruments and media of cognition. Thus Peirce says, for example:

A psychologist cuts out a lobe of my brain (*nihil animale a me alienum puto*) and then, when I find I cannot express myself, he says, 'You see, your faculty of language was localized in that lobe.' No doubt it was; and so, if he had filched my inkstand, I should not have been able to continue my discussion until I had got another. Yea, ***the very thoughts would not come to me*** [emphasis added]. So my faculty of discussion is equally localized in my inkstand.

Let me quote Skagestad's comment on this:

As is indicated by the emphasized sentence, Peirce is not making the trivial point that without ink he would not be able to communicate his thoughts. The point is, rather, that his thoughts come to him in and through the act of writing, so that having writing implements is a condition for having certain thoughts -- specifically those issuing from trains of thought that are too long to be entertained in a human consciousness. This is precisely the idea that, sixty years later, motivated Engelbart to devise new technologies for writing so as to improve human thought processes, as well as the idea that motivated Havelock's interpretation of Plato.

I am sure you can readily see the connection of this with the development of computer graphics, the user interface, the use of the mouse, word processing, hypertext, and so forth, which is what primarily interests Peter Skagestad. The theoretical grounding of all of this in Peirce lies in his locating of thought in the media of its expression, as expressed in the dictum that "all thought is in signs."

## 5 THOUGHT IS IN SIGNS – THOUGHT IS DIALOGICAL (RANSDELL)

With this as preface, then, let me explain something about my own interest in Intelligence Augmentation as a Computational Intelligence project and indicate how it relates to his interests. I agree with Peter Skagestad as regards what has been said thus far, and my interests certainly include those computational mechanisms that constitute and control the interface both with document and data materials and with other persons, and which include or enable the many powers of manipulation of text and graphics that have been developed in recent years, that enable us to make and follow hypertext links (i.e. to associate freely and to trace associations already made), that enable us to exchange messages with others and communicate with them in a number of different ways, and so forth. But there is a further and equally valid interpretation of the dictum that "all thought is in signs" which also has implications for computationally-based Intelligence Augmentation, namely, that ***thought is dialogical*** -- hence ***communicational*** -- in form. If thought is to be found in signs, and is actualized in their actual generation of interpretant-signs of themselves, then it is the flow of discourse as asymmetric dialogically-structured interpretation calling forth further interpretation that constitutes the flow or process of

thought, and the development of intelligence is at least in part a matter of the development of *critical control practices* that conform to *communicational norms* which make discourse more efficient and effective relative to whatever ends it may have.

Since the discourse or communication in question is to be made more effectively intelligent, it seems reasonable to start out by working with communication as it occurs especially in processes of *inquiry*, where the function of the norms of critical control is to make inquiry more successful in the sort of results it specifically aims at. The ability to be successful in this way is certainly an important part of what we regard as intelligence and this is, of course, a natural place to begin for any philosopher who has been influenced by the work of Charles Peirce and John Dewey, as I have. Now, by far the most effective kinds of inquiry that have been humanly devised are those that occur in *research traditions* of the sort which began developing in antiquity as early as six or seven centuries B.C, and have ramified and given birth to many further such traditions especially during the past five or six centuries, which include both what we now call the “sciences” and what we usually think of as “scholarly” traditions as well. In these traditions research ability is embodied in practices, habits, and skills of the inquirers which can be divided into two types: on the one hand, there are what might be called the “*material* skills” of inquiry which have developed in the given field, some of which will be field-specific but of which many will be common to a number of such fields, and some common to all. On the other hand, there are also what I will call the “*discursive* skills” of inquiry, meaning by that the mastery of those practices, habits, and skills of discussion and communicational interaction that control the flow of discourse in the context of inquiry, according to the communicational norms developed in the various research traditions generally: I mean such special practices as asserting, suggesting, questioning, making critical response and counter-response, raising objections, elaboration of points made, etc. These skills have been largely overlooked thus far, and I want to bring them particularly to your attention here and try to convey some idea of why I think they are important, in spite of being largely ignored as a distinctive type of IA research at present.

The sort of Intelligence Augmentation I am chiefly concerned with, then, is that which would be achieved by devising mechanisms and programs that would increase the effectiveness of the communicational norms which encourage successful inquiry as these have developed in research traditions whose ancestral forms sometimes go back more than two and a half millennia ago, and also those that would facilitate and inquiry into the norms themselves for the purpose of identifying those the conformity to which would indeed result in more successful inquiry. The project of development of any computational devices that could be helpful in this would qualify as a contribution to IA research of this special kind.

I should remark, though, that whether the focus on communication in inquiry in particular will provide us with an adequate basis for understanding the potentialities of IA programming designed especially to make communication in general more intelligent is another matter. The approach via concern with inquiry in particular is a natural place to begin but it might take us only a certain distance, beyond which we will need to consider other and importantly different types of communication as well if our aim is to develop Intelligence Augmentation of this sort as extensively as we can. We can leave that ques-

tion aside here, but understanding something of the potentialities and problematics of IA in this respect should at least provide us with a more sophisticated understanding of the role of communicational norms in intellectual life than we presently enjoy, and it will also enable us to take extensive advantage of the philosophical work of Charles Peirce -- himself a master of inquiry in a number of different fields -- in developing analytical conceptions for this purpose.

## 6 INQUIRY AND ASSERTION

The support for this in Peirce's philosophy is primarily in his theory of inquiry, which is the general framework he relies upon in developing his logic. Logic includes the development of notations and derivation techniques for deduction, and the development of methodologies of induction and abduction as well, but Peirce situates these traditional logical concerns within the framework of inquiry conceived in such a way that it can be regarded, for some purposes, as *a general theory of assertion*. However, I am hesitant to call it that because it could be more misleading than helpful to do so in view of the way speech act theory, which was pioneered by Peirce, has been developed in the past century after his death, which has taken an importantly different approach to understanding what assertion is by minimizing the social aspect of the speech act as much as possible. This is done by considering the role of the addressee of the act to be limited to whatever is implicit in recognizing the given speech act as being the sort of act it is. "Uptake" is the usual term for this sort of constitutive acknowledgement of the speech act as being of this type or that, and by focusing solely -- and usually with great brevity -- on that one aspect of the involvement of the community in general in every act of serious assertion the role that assertional acts in particular actually play in a *community* of inquirers has been left largely unexplored and undeveloped. This is not what Peirce had in mind in conceiving logic as a general theory of assertion, however.

If you are already acquainted with Peirce's work you will know that he prefaced his first systematic overview account of the logic of science with a pair of essays -- "The Fixation of Belief" and "How to Make Our Ideas Clear" -- which situate logic in the narrower sense in which it is taught in logic classes within the general framework of a process of inquiry<sup>10</sup> which might roughly be described overall as follows: A particular inquiry which occurs within an ongoing and enduring inquiry process is not to be regarded as having an absolute moment in time when it first begins nor a moment in time when it completely and definitively ends, but to be thought of rather as coming into existence when the ongoing process has become informed by two or more conflicting tendencies toward *acceptance* of something which has resulted in a stalemate or conceptual impasse (*aporia*) of the sort we would describe logically in terms of two or more contradicting assertions of opinion at once. A given inquiry is constituted by the inability of the inquirers to resolve a disagreement about what is to be accepted. This disagreement will have come about as a result of the accumulation of understanding up to that point, and the overall direction of inquiry is given by the attempt to take such steps as are required to

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<sup>10</sup> The reference is to the six-paper series entitled "Illustrations of the Logic of Science", published in 1877-78 in successive issues of *Popular Science Monthly*.

get past the initial impasse or *aporia* in order to arrive at a shared and non-conflicting acceptance of results or findings. This shared acceptance, if it occurs, will enable further inquiry into the same subject-matter to proceed, using, when relevant, whatever is then accepted as the basis for achieving still further understanding of the subject-matter. The typical patterns of agreement, disagreement, and research strategy this can involve have been rather thoroughly explored as regards its logical import by both Peirce and Dewey in particular and will not be of special concern to us here.

Now, to regard logic as a theory of assertion is to take a certain and rather special perspective on the inquiry process,<sup>11</sup> regarding it particularly from the point of view of the individual inquirer considered as motivated qua member of that research community by the aim of making a contribution to the shared understanding of the subject-matter which has already developed within the research tradition. The actual act of assertion occurs, then, when the individual inquirer, having prepared him/herself sufficiently to be willing to take the risk involved in doing so, actually attempts to capture the attention of others in the research field in such a way as to cause them to come to the same conclusion which he or she has already come to and thus to contribute to the research tradition by shaping it in the direction of an ultimately stable and shared understanding of the subject-matter.

This is done by making a claim of a finding or -- if deemed important enough -- a discovery, this being done by putting forth a research report. The occurrence of such an act, when it is recognized for what it is, is the intentional triggering of a complex set of non-terminating communicational obligations and permissions that apply not merely to the researcher making the claim but to everyone in the research tradition addressed by the assertion.<sup>12</sup>

## 7 SERIOUS ASSERTION AND PRIMARY PUBLICATION

As we will see shortly, It is necessary to distinguish between assertions made in a serious spirit and in a playful or at least nonserious spirit. Initially, though, I focus upon serious assertion, both because it is easier to characterize in a brief space than the many varieties of nonserious assertion that typically occur in the course of inquiry --constituted by a number of importantly and sometimes subtly different ways in which the force of an assertion can be qualified or made conditional -- and because of the unique role of serious

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<sup>11</sup> Another perspective that might be taken would be to regard logic as the general theory of the nature of a question, inquiry then being regarded as a protracted form of a question: the question as quest or hunt or search (= research). Still another overall perspective would be to think of it from the perspective defined by focusing attention primarily on the nature of research acceptance, which is a variant way of describing it in terms of the "fixing of belief" or the "settlement of doubt", as Peirce himself frequently expresses it. These would all have to account for the same general features of inquiry, but different aspects of it would be dwelt upon in more or less detail according to which perspective is taken.

<sup>12</sup> This is a special kind of assertion, to be sure, because it occurs within the context of communication in an ongoing research community, but it may provide helpful clues to understanding what assertion is outside of this special context.

assertion in the ongoing communicational interactions that are continually structuring and restructuring the inquiry process through the effect of conformity to the norms of permission and obligation they involve. Regarded from a rather detached or esthetic perspective, the course of inquiry in a lively research tradition exhibits what might fairly be called a kind of choreography of conversation, though the participants do not normally think of it in that way; and in the dance of research, acts of serious assertion provide a kind of emphasis that has a unique organizing effect on the process when it occurs.

For present purposes, let me characterize serious assertion as obtaining whenever the person making the assertion takes full responsibility for making a claim which, taken seriously by the others in the research community, will put upon them the obligation to take what has been claimed seriously enough to allow themselves to be persuaded to the conclusion which the claimant has already come to, *if* the claimant has actually made the case for it in the claim in a way that is found to be rationally persuasive. (Found to be so by whom? By each member of the given research community taken distributively, i.e. taken one by one, as distinct from the membership regarded as a collectively constituted individual. The research community is not to be regarded as a collective entity.<sup>13</sup>) Other obligations involving both the claimant and the fellow researchers addressed by the claim are involved in serious assertion as well.

For example, the claimant is required to be sincere about actually having arrived at the conclusion him/herself; those addressed by the claim are obligated to make known to the claimant and to the research community any serious objections they have to the claim made in case they see a serious flaw in it and think it important enough to warn others about; anyone addressed by the claim -- i.e. any member of the research community -- is permitted to respond appropriately to the claim in any other way they see fit, insofar as it bears on the question of whether the claim should be accepted; the person making the claim is required to include enough information about the method of replication of results to enable it to be tested according to the claimant's own specifications; the claimant is expected to have some explanation in case an objection is made to the effect that replication has been attempted but failed; and so on.

This describes what I have been calling "serious" assertion, and it obviously plays a special role in the inquiry process because of the power of a seriously made research claim, regarded as such by all concerned, to affect the actual course of research in a given research community in virtue of its ability to impose such obligations on those in the same community and thus, at times at least, to compel the members of the community in general to a common conclusion. This is, at least, what the claimant hopes to be the ultimate effect of making such an assertion, though there is no way of doing this that can have the regular effect of actually achieving such an agreement. Indeed, the number of those that do succeed in that respect will often be decidedly in the minority. There can be

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<sup>13</sup> The reason for this lies in considerations having to do with the conception of a research peer.

no such thing as an algorithm for achieving research acceptance, and computational programming which took that for its goal would be futile.<sup>14</sup>

Now, assertion of this sort is, of course, much the same as what is usually referred to as “publication”. But the word “publication” is often used in reference to ways of making things public which does not entail or carry with it the kinds of strong and definite norm-triggering associated with research claims proper, so let us refer to the making of serious research claims, in the sense just described, as acts of primary publication. (“Formal publication” would often be a contextually adequate synonym, and I will sometimes use it as such here, but there are reasons why a distinctive term of art for this is desirable, and there is a special motivation for adopting the word “primary” for this purpose.<sup>15</sup>)

## 8 NONSERIOUS ASSERTION

But the inquiry process is not simply a matter of being serious in this sense, but also involves much -- indeed, far more -- communicational activity of a preparatory sort which also affects its outcome importantly but does so differently since what is said, not being asserted seriously in that sense, does not trigger the same rigid and rigorous obligations as an act of primary publication triggers. (This does not mean that it triggers the applicability of no norms at all: all speech acts trigger some generally recognized norms, and even the most playful of discourse in the context of inquiry is norm-governed.) Seriousness or nonseriousness, in this special sense, is not a matter of how anyone feels: people can, in a nonserious way, argue about matters with great passion and intensity of conviction as regards their opinion at that moment, but still be arguing nonseriously in that it is understood by all concerned that what is being said is not to be taken as invoking the application of the rigid and rigorous communicational norms associated with what is identified as a serious claim to a research finding. What makes assertion serious, in the relevant sense, is the *de facto* recognition and acceptance of the intent that the special rules of discourse that constitute the obligations and permissions attendant to a serious research claim obtain, and this is not a matter of how anyone feels but of the willingness to accept

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<sup>14</sup> The attempt to develop a “rational reconstruction” of research acceptance is also futile, in the view being explained here. This does not mean that acceptance is irrational but only that it is not representable algorithmically.

<sup>15</sup> Namely, to mark the origin of the conception of primary publication, as a distinct analytical conception of unusual value for our purposes, in the work of Joshua Lederberg, who is, as far as I know, the first to have seen clearly the special role which what he calls the “primary literature” plays in the normal course and choreography of research, referring by that to the documents which function as the material vehicles of primary publication proper. See his paper “Options for the Future”, *D-lib Magazine*, May 1996:

<http://www.dlib.org/dlib/may96/05lederberg.html>

Lederberg is not, of course, to be held responsible for all of the ways in which I construe or misconstrue the conception.

the application of the especially rigorous communicational norms associated with such claims.<sup>16</sup>

As research traditions have developed across time, various kinds of communicational practices have developed within them which in one way and another qualify them as non-serious in the sense indicated: for example, informal discussions of an occasional nature with research colleagues casually encountered, including correspondence by mail; more or less loosely structured group meetings of various kinds (which can range from local discussion groups with more or less set topics and discussion agendas, to international conferences, congresses, and the like); coordinated team efforts as part of complex research projects such as are becoming increasingly common in the hard sciences; messages and sometimes long and complex threads of discussion posted to public forums and newsgroups; and even self-communication, as when we are working out our ideas in momentary isolation from others in the tradition with which we identify ourselves, which can be regarded as limit cases of the social. And so forth.

I have no idea how many different sorts of communicational practices might turn out to be worth recognizing, but they will obviously vary greatly as regards the controlling norms governing what is regarded as communicationally appropriate, depending on what the communication is supposed to accomplish in contributing to the general aim of learning more in breadth and depth about the subject-matter of the research tradition. Sometimes people are in need of an opportunity to try out new ideas simply in order to find out whether they are worth exploring further; sometimes they are in need of exposing their thinking to others to get some rapid critical feedback, negative or positive; sometimes ideas are being put forth to lay some groundwork for establishing a possible future claim to priority in discovery; sometimes certain things are being discussed simply because the participants think their overall view of the research topics that interest them are in need of vitalization by being set within in a different context than usual; and so forth.

Which of these would be the most important as regards research aims? Are the cases of serious assertion -- primary publication -- the most important? The answer is surely that one cannot make such a judgment a priori and apart from any actual context of concern, or apart from an understanding of the extent to which the given research tradition is flourishing or is still in a stage wherein it is not clear where it is going yet. Sometimes a primary publication claim can be of the first importance, and they frequently are. But a casual conversation in a hallway between a couple of unusually talented researchers might well make a far greater difference to the future of the given research tradition than any single act of publication does and thus be more important in that sense. Primary publication has a unique role in the process, which we will be considering further shortly, but "importance" is not the right word for it. And this should be stressed since there is a strong tendency not merely to overemphasize its importance, but to put so much emphasis on it as to ignore the possible importance of other communicational practices altogether, and thus to reduce one's conception of what inquiry is to a misleading caricature.

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<sup>16</sup> Compare the differences between promising, not promising, and pretending to promise or acting as if one is promising though it is understood only to be an act, e.g. such as an actor on a stage might perform. The differences are too subtle and complex to discuss here.

Research is a kind of hunting activity, and to identify publication as the most important thing in research communication is like saying that the most important thing in the hunt is the coordinated attack on the prey at the close of the hunt, which is no doubt true in some cases but cannot be said to be generally true inasmuch as the complex process of hunting may well involve activities preliminary to the climactic attempt to capture or kill the prey (and be only loosely connected with it) which are actually far more important in its success than the acts of actually attacking or capturing it, which can sometimes be little more than *pro forma*.

In what follows I will be illustrating what I have in mind by Intelligence Augmentation of this special type by reference to a concrete case of unusual interest, namely, the automated publication system devised by the physicist Paul Ginsparg for the benefit of his own research community (high energy theoretical physics) and several others closely associated with it. The special interest that attaches to it is due in part to the fact that understanding it requires recognizing the need to distinguish between serious assertion or primary publication and other kinds of communication that occur in the course of research. It will be important to bear in mind in understanding the case, then, that it not be interpreted as being used by me as a paradigm of research communication in general. It is used rather because of the way in which it illustrates the special role which primary publication has come to play in research, and also because acquaintance with how it has been received by people in various research communities who are interested in the uses of the internet in scientific and scholarly communication reveals the massive confusion that presently exists in the general understanding of how critical control works in research communication. This confusion is based largely on a misunderstanding of the nature and function of peer review.

## 9 THE WAY THE GINSPARG PUBLICATION SYSTEM WORKS

We turn now to the case of the automated archive and server system for pre-print distribution of publication in high energy theoretical physics, and in several related fields in physics, astronomy, and mathematics, which was first developed at the Los Alamos National Laboratory by the physicist Paul Ginsparg some twelve years or so ago.<sup>17</sup> The system was recently moved to Cornell University when Ginsparg took a position there, and the official name of it now is simply “arXiv” -- the “X” is a visual pun on the Greek letter *chi* -- but I will refer to it here as “the Ginsparg system” in order to keep the focus on the work of Ginsparg in setting it up, which is the instance of IA application of special interest to us here. Since inquiry is a form of learning, the success of which is an increase in

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<sup>17</sup> The publication system itself is at

<http://arXiv.org>

I suggest browsing the site thoroughly, particularly the following page:

<http://arXiv.org/blurb>

where you will find both some interesting statistics on its use, and also several papers by Ginsparg which are quite straightforward in making clear how both he and at least some of his associates from early on have regarded what they are doing. Read the earlier material first. I hope in another study of this to give a much more informative account and analysis of the Ginsparg system as a project which is still underway, though precisely how it is presently conceived by Ginsparg himself is not altogether clear.



the understanding of things, anything which contributes to the efficiency and effectiveness of inquiry is *ipso facto* an augmentation of intelligence. The interest in Ginsparg's work does not lie, however, in any special sophistication or novelty involved in the programming, considered simply as computer programming, but rather in the way the programming was developed as a material support for communication governed by certain controlling norms believed to be conducive to the furtherance of inquiry in the fields it was originally intended to serve. These norms are those just discussed above as those governing what I have thus far referred to as "serious assertion" or "primary publication". The Ginsparg system was obviously quite consciously set up to be a venue serving this purpose and it has in fact done so ever since.

The way the system works is simple. If one wants to make a claim to a research result to one's research peers in the field in question, one writes up the claim being made and the basis for it, considered as a conclusion, in the form commonly understood to be dictated by whatever would be required for purposes of testing or replication, whether that involves an appeal to a priori reasoning, as in the case of mathematical proof claims, or to observational or experimental procedures. The generic form of all such papers can be described quite specifically, if necessary, but there is no need for our immediate purpose to say more than that there is nothing unusual about the expectations of the people in the fields which use the Ginsparg system as their medium of primary publication as regards the form they expect such publications to take, which does not differ significantly from the form which primary publication takes in any other research field, most of which can be deduced from the fact that the researcher must make clear what would be required for purposes of replication of results. The archive is programmed to accept several special formats, such as postscript, pdf, LateX, and HTML. It is left up to the person depositing the paper to do the formatting and encoding required (or to arrange for having it done properly). The act of depositing is understood by the research community to whom it is addressed as intended as a serious assertion, i.e. as an act of primary publication. and if it meets certain minimal conditions (e.g. includes specifications for replicability of results) it may actually be recognized as being such.<sup>18</sup>

In addition to the paper itself, one also prepares an accompanying abstract, usually involving the use of topical key words, and one deposits both paper and abstract in the archive. The abstract -- not the paper -- is then automatically distributed by email to those users of the publication system who have previously indicated, by a description of their own research interests, that they are interested in reading all papers that might contain material especially pertinent to their research concerns. (Since the archive is divided up by fields and subfields, one might simply signify to the machine that one is interested in any abstracts deposited that pertain to one's field.) If a reader of the abstract decides that the paper it describes might be of interest, then he or she can click on a link which will

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<sup>18</sup> Whether the act actually does count as a primary publication depends upon it actually being regarded as an act of primary publication by the research community addressed, i.e. it depends upon "uptake" actually occurring. In that case, there is no "uptake" by the community and without any such recognition no primary publication has actually occurred: the attempt at asserting a research claim has "misfired", as J. L. Austin would say. Presumption plays a role in uptake just as it does in recognition of a peer as such, but this is a topic which cannot be explored here.

cause the whole paper to be sent to them or downloaded by them. The entire process of deposit, notification, and retrieval of papers is automated.

If one disagrees with the claim made and it seems important enough to do so formally, then one can deposit a reply to it in the same place that will also be formally correct, as that is generally understood in the field in question. Thus critical dialogical interchanges can occur in the system which are of the same general type as those which might occur in a traditional professional journal which permits “replies” as a normal part of the publication process. But it is important to understand that the arrangement is not conducive to the kind of informal discussion typical of, say, a listserver based forum or an organized discussion group or among the members of a special project team, or a “bulletin board” or “news” group discussion, much less with the kind of discussion that might occur on a real-time “chat” line. Inappropriate responses might well be made and deposited in the archive -- there is nothing which precludes this -- but the system is designed to discourage that by making it necessary to deposit an abstract if one wants others in the field to know that one has made a reply. This helps to insure in practice a kind of formality which is of the essence of what I am calling “primary publication”. Too much is at stake professionally in what appears under such an understanding to make the communicational mores of, say, an informal discussion group appropriate. In the case of the Ginsparg archive and server system there is no “policing” to insure this, as it has been found not to be necessary.

## **10 THE OBSCURED IMPORTANCE OF THE GINSPARG SYSTEM**

From a narrow point of view, the Ginsparg archive-and-server system is nothing more than an automated form of a system of communication that had already existed for decades in the research fields it serves, namely, the practice of distributing copies of pre-prints to others in the field, meaning by “pre-prints” papers embodying primary publication claims distributed to research peers prior to their appearance as papers published in the editorially controlled journals in the field, often prior to submission to such media of publication, and sometimes not even destined to be submitted. Pre-prints are not the same as drafts, though, since the term “draft” implies a lack of polish and completeness that would be inappropriate in something distributed as a pre-print. On the other hand, something regardable as a pre-print can also be regarded as a revisable version, and most pre-prints that are subsequently published in a journal are probably going to be revised to some extent before they appear there, even if only at the behest of the journal editor, who is often under pressure to economize on the space occupied by the paper in the journal.

Before the establishment of the Ginsparg system at Los Alamos, pre-print distribution usually meant distribution only to those well-enough connected professionally to be on the mailing list for distribution of preprints by those at the “leading edge” in the field, which of course tended to insure that those on the distribution list would be strongly advantaged thereby in their professional success as researchers. Thus there were actually two distinct venues of primary publication in such fields: the pre-print distribution system and the system of editorially controlled and “peer-reviewed” professional journals, corre-

sponding to the distinction between well-connected and thus advantaged researchers and those not-so-well-connected and thus not in position to participate in leading edge research. The time delay involved in publication in the professional journals usually means that, by the time those who depend on the journal literature for understanding what is at the “leading edge“ find out what is happening there, the edge will already have moved on to other matters. Any field which puts great stress on priority of discovery will tend to resort to pre-print distribution as a means of primary publication unless there is something that hinders it, and the domination of the direction of research in many fields by those in the privileged position of being able to participate in primary publication of this kind -- sometimes discussed in terms of the domination of research by “invisible colleges”<sup>19</sup> of the communicationally privileged -- was a matter of growing concern in the sciences by the time Ginsparg established his automated and unrestrictedly accessible pre-print server system at Los Alamos.

Ginsparg and his associates seem to have been aware from the beginning that something of potentially momentous importance had been accomplished by the relatively simple act of installing the archive and server system on the internet with a policy of unrestricted access to deposit and retrieval. Judging from such discussion of this as I am acquainted with, the most important thing for them seems to have been that in adopting this new system they were making a transition from a system of publication which was primarily serving the special interests of just those physicists who, like themselves, happened to be in the advantaged “in-group”, to a system serving the needs of all physicists the world over who are capable of accessing the internet, even if only at a minimum level of efficiency, without limitations based on special qualification or collegial connections. I will refer to this as the cosmopolitan motive in their idealism.

At the same time, though, they seem to have understood that something else was being accomplished as well which had to do somehow with an exposé of the peer review practices of the journals as being impertinent to the critical control of leading edge research. Since it is part of the received and conventional wisdom that peer review is the one thing that insures that “standards of quality” will be recognized in research and in control of publication, their typically contemptuous dismissal of it as impertinent was construed by many people as dangerously subversive of science and scholarship, especially in view of the fact that the scientific disciplines from which it was emanating are high on the scale of professional prestige and thus cannot simply be written off as complaints of the sort to be expected from people who can’t meet the supposedly high standards of peer review. This can be regarded as the *anti-authoritarian* aspect of their idealism, not because they explicitly construe it in that way but because it is in fact a rejection of the authoritarian conception of the role of peer review in research, and I think they have had some understanding of that even though I find no attempt to think the concept of peer review through to figure out what, exactly, is or is not happening in it and what the basis for critical control actually is or should be.

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<sup>19</sup> The word “college” is used in the sense of “group of colleagues”.

Thus Ginsparg and his associates, who created and developed the publication system, took a highly idealistic view of it for the reasons just indicated, and this idealistic zeal initially took the form of claiming that what they had accomplished at Los Alamos for their own fields can be accomplished across the board in the sciences, and not only there but in research traditions generally. Limitations of time and space do not permit a description here of what has happened in the past few years as this idealistic zeal met with increasingly hardened resistance, which finally took the form of a deflationary rhetoric which has been highly successful, at least temporarily, in inducing a kind of obscurantist confusion about the Ginsparg publication system that has now largely silenced it as a reform movement.<sup>20</sup> This was achieved by invoking and promulgating a certain important misunderstanding about the nature of peer review while at the same time forbidding the discussion of peer review reform in the most influential public forum devoted to the topic of free on-line scholarship. This effectively reduced the apparent significance of the success of this system of publication to a minimum by encouraging a refusal to recognize the Ginsparg system as a system of primary publication.

When the existence of the Ginsparg system became widely known, beginning some five or six years or so ago, it generated much “viewing with alarm”, and dire predictions about the inevitable decline in quality of research in the fields using the system were common.<sup>21</sup> It seems reasonably clear by now, however, that this predicted decline has

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<sup>20</sup> The process of obfuscation is viewable as a public record in the archives of the September Forum sponsored by the professional magazine *American Scientist*, which has been managed almost from its beginning by Stevan Harnad, who has been the most influential figure in shaping opinion about the nature of publication especially as regards the prospects for getting all research publication on-line with free -- i.e. unrestricted as well as financially free -- public access. This has been the chief public locus for discussion of these matters because of the extraordinarily influential character of so many of those who are subscribed to the forum: the “movers and shakers”, as well as some in powerful opposition to the movement to on-line availability of research publication. Harnad, a computer scientist with a high (and well-deserved) prestige as a researcher in cognitive science, has been the chief agent of obfuscation of the significance of the Ginsparg system, though his stance has always “officially” been that of a strong advocate of it. (He does not, of course, regard himself as doing any such thing.) But as the cliché goes, with a friend like Harnad who needs enemies? I have reported occasionally on the PEIRCE-L list over the past four years about what I thought was actually going on there, but this is not the appropriate place even to try to provide an overall summary statement of that. But if you have the time and energy required to work through the course of discussion from the time Harnad took it over, shortly after it was established, until the present, you will find it remarkably informative as an exhibition of the strategies used to nullify the threat to the established way of controlling research publication which the Ginsparg system initially seemed to pose. You will find some scattered and largely ineffectual attempts by me to throw up some resistance to this, but Harnad’s willingness to shut down discussion on any topic he finds threatening to establishment sensibilities made it impossible for me to accomplish anything in that respect. The following URLs will yield threaded versions of the entire archives for the forum from more than four years ago:

<http://amsci-forum.amsci.org/archives/september98-forum.html>

or

<http://www.cogsci.soton.ac.uk/~harnad/Hypermail/Amsci/index.html>

I hope to give a somewhat detailed account of what occurred there and in related forums and situations in another paper.

<sup>21</sup> But how can one say this? How can I, in particular, not myself a researcher in any of the fields the system serves, make such a claim? The short answer is that if the quality of research really was in a decline in those fields in virtue of its being a flawed system of primary publication, that would manifest itself in in-Special Issue on Computational Intelligence and Semiotics  
*S.E.E.D. Journal (Semiotics, Evolution, Energy, and Development)*  
Queiroz, J. and Gudwin, R. (Guest Editors)

not occurred and these pessimistic assessments seem to have given way generally to an admission, sometimes grudging, that it does seem to be working for those fields for which it was originally designed. On the other hand, it has also become increasingly clear that there is no tendency yet toward general adoption of it as a model for publication practices in the sciences generally, as Ginsparg and some of his associates had once thought might occur, much less toward emulation of it in scientific and scholarly research publication generally. Consequently, the initial interest in it as a revolutionary new internet-enabled publication system has now all but disappeared.

Indeed, as I remarked above, it is now commonly regarded as not being a publication system at all, notwithstanding the fact that it has continued to be the chief system for primary publication -- as defined here -- in those fields which it was originally invented to serve. Yet the only value of it for publication practices generally is now usually considered to lie in the fact that it has provided the model for the development of internet archival systems of a type which might be replicated at any number of different nodal points on the internet -- university based archives of this type are now being touted as ideal nodal replications of it -- the virtue of which is that anything deposited at any such archive becomes ipso facto available as a document in a single world-wide virtual database of such documents, which can be searched and subjected generally to programs designed for purposes of retrieval of material from it, for keeping track of what is there as any master library must do, and for purposes of analysis of the documents it contains in the interest of sorting them out and describing them according to any number of different sets of criteria corresponding to various interests which someone might have in them as publications with a history. The value of this is indisputable, but it is not, in my opinion, the most important thing to understand about the Ginsparg publication system.

Thus although the rhetorical disinformation about the system as a publication system has had no effect on its use in the fields for which it was originally designed, which still continue to flourish with the use of it, it has effectively diverted attention from its idealistic aspect and the potentialities for encouraging reform implicit in the automated system, and the major significance of it has come to appear (quite misleadingly) to be only that it is an example of how it is possible to make the transition from paper-based journal publication to on-line publication without raising any reform issues that might disturb the already prevailing systems of hegemony exercised by the various institutions and cabals that control research by supporting and controlling publication. With this, the significance of the success of the Ginsparg archive for the development of what is potentially a very important part of IA research has been obscured in such a way as to amount to a kind of "dumbing down" of our understanding of the conditions of success in scientific and scholarly research. To reverse this it is necessary to insist upon the challenge which

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creasing difficulty in communication in the field and a continually increasing frustration of the sort that would be apparent in such things as the abandoning of proper form and a tendency to sloppiness in preparation of the papers for publication, in increasing lack of restraint in the sobriety of criticism, in the formation of warring factions, and so forth, which would in turn result in the abandonment of the use of the system by serious researchers, who would simply revert to the prior and more satisfactory system of pre-print distribution. But it has shown no such signs of degeneration but been, on the contrary, in a state of constant growth in usage at a remarkably uniform rate of increase from the beginning until the present.

the Ginsparg system has posed and continues to pose for peer review as that is presently understood.

## 11 PEER REVIEW AND THE CONCEPT OF A PEER

I should emphasize that the view of peer review being proposed here is not in opposition to regarding peer review as of fundamental importance in the critical control of research. The view is rather that what has come to be called “peer review” is not peer review proper but rather a crippled form of it which is not only of limited value at best as a critical control principle but is also a subversion of the peer principle that underlies the practice of authentic peer review. Why? Because it treats peer review as a system of elite control, which is directly contrary to the conception of a peer. According to the view being presented here, the working of *authentic* peer review is in fact best observed in action by studying the practices exemplified paradigmatically by the Ginsparg system (or any equivalent system) of primary publication.

When I first became interested in this issue, I thought it would be best not to disturb the present usage of the term “peer review” as referring to editorially commissioned pre-publication peer review, especially since the early enthusiasts for the Ginsparg publication system typically regarded peer review, in that sense, as of little real importance because leading edge research seemed to have little use for it: by the time a paper appears in a journal, the leading edge has moved on, and so the function of the journal as a venue cannot be to control what appears at the leading edge in the form of research claims. This is not to say that the editorially controlled journal cannot be of service to those at the leading edge, but it would have to be in virtue of the uses of retrospection, of getting clear on what has been accomplished, as distinct from the accomplishing of it, which does not require editorial validation based on peer review. Something like this, at least, was and perhaps still is the view of the researchers supporting the use of the Ginsparg publication system. The possibility that the primary control function of peer review is precisely that of control at the leading edge and that the unfiltered automated archive is actually where authentic peer review is to be found does not seem to have occurred to the physicists, however, any more than it occurred to the partisans of the editorially controlled journals, who regard the journal as the natural home of peer review and thus of the primary critical control factor in scientific research. With this sort of agreement among the antagonists as regards what peer review is -- namely, editorially commissioned pre-publication peer assessment -- it seemed to me that it would be folly to challenge this, even though I already believed that it involves a serious misconception, and the best bet would be to leave that usage of the term “peer review” undisturbed. After all, it was basically just a verbal matter -- or so I at first supposed -- and perhaps no more, at bottom, than a quibble about usage.

But in this I was mistaken. What I did not yet realize was that it would be in virtue of the exploitation of the physicists’ rejection of peer review -- as they understood it in common with their opponents -- as a primary critical control factor that the supposedly dangerous radicalism of the automated, unfiltered, and unrestrictedly accessible publica-

tion system at Los Alamos would be neutralized and rendered innocuous as regards any vested interests that might be threatened by the challenge to peer review as presently understood. I have since realized, though, that since it is respect for the peer principle that lies at the basis of the critical control of research communication generally, this was a rhetorical mistake that has enabled the success of those who deny the significance of the success of the Ginsparg system by denying that it has the status which it actually does have as a venue for primary publication. With this status denied, what actually takes place in the Ginsparg system can be and now commonly is in fact dismissed as being no different in kind from what happens on any bulletin board, listserv based forum or discussion group, chat line, or any other informal medium not regarded as important enough to the hegemony of legitimacy claimed by the editorially controlled journal to be a challenge to it.

What has been missing thus far in this is an understanding of the conception of a peer which would explain why peer review, wherever located in the process, can be taken for granted as the basic critical control function in research. Yet for this very reason what is now commonly regarded as its paradigmatic occurrence -- editorially commissioned pre-publication peer review -- should be recognized as a false pretender, since properly understood peer review is best exemplified at present by the routine workings of the Ginsparg automated, unfiltered, unedited, and unrestrictedly accessible archive and server system. Or so I will argue. Why, then, is it thought important that the acceptance of research claims in a given field be something that happens in consequence of *peer* assessment of claims made?

A research peer is, of course, an equal. To be more exact, a peer is a *presumptive* equal, not someone who has been demonstrated to be *de facto* equal in this or that respect but rather someone who is regarded, presumptively, as someone whose informed opinion about the subject-matter of research is to be taken as seriously as one's own opinion is insofar as that depends on the *status* of the researcher, as distinct from its dependence on *the justification provided* by the researcher for the claim. A peer is someone whose disagreement with one's own view requires to be explained and should not simply be ignored, this being assumed prior to any examination of what the reason for the disagreement may be; a non-peer is someone whose opinion about the matter in question makes no difference to you, poses no challenge to you either to explain the disagreement or to accept the fact that you haven't made your case if you can't explain it. The question of what gives a person peer status will not be addressed here. Its importance is understood, but it is not possible to do justice to it here. Let it suffice for present purposes to say that, in practice, what determines peer status is the same thing that determines who it is that is being addressed in the research claim.

This is one of many places where the concept of primary publication shows itself to be of value. In primary publication, the researcher making the claim is always addressing a more or less definitely envisioned research community consisting of all persons who share the same serious concern with the same subject-matter, the purpose of making the claim being to appeal to such persons to recognize the validity of the research conclusion which one has drawn and is putting forth as a conclusion which, one argues, should be accepted henceforth as a premise or presupposition in their further thinking about the

subject-matter in question. In putting forth one's claim in the form of a primary publication one is attempting to trigger a process controlled by the complex set of obligations and permissions which I alluded to and sketchily described above, in Sec. 7, which one hopes will eventuate in the acceptance by those addressed of one's claim. Those addressed are one's peers -- that is, being one's peer and being a member of the group addressed is the same thing -- and thus all persons addressed are regarded as being equal in the sense of being subject to these norms, the set of which applies to the researcher making the claim as well since they are the role-defining rules which the dialogue of publication is to conform to.

Peer review proper, then, is what occurs in the inquiry process when one makes such a research claim and the research community addressed responds according to the communicational norms that then obtain. All communication that occurs within this normatively constituted dialogical space that pertains to the claim at issue is peer review.

## 12 THE RATIONALE OF THE PEER PRINCIPLE OF CRITICAL CONTROL

To get a clear understanding of what peer review is and why it is rightly regarded as fundamental in the critical control of research, we have to understand why it is thought important that the acceptance of research claims in a given field be something that happens in consequence of peer assessment of claims made. As remarked earlier, a peer is a presumptive equal, not someone who has been demonstrated to *be de facto* equal in this and that respect but rather someone who is regarded, presumptively, as one whose informed opinion about the subject-matter at issue is important enough -- because competent enough -- that any disagreement of one's own opinion with that of the peer's opinion yields a situation in which both opinions cannot be true but neither can be decisive in respect to which of them is mistaken in virtue of one of them having superior status in such matters. In other words, there is no relationship of authority among peers -- unless we are talking about *Animal Farm*,<sup>22</sup> where some peers are recognized as being more peerish than others.

The concept of a peer appears in many different contexts in modern society. A familiar example of the way peer status works can be illustrated by a case where a physician is asked by a patient of another physician for a second opinion. Physicians usually do not object to a patient's request for a second opinion provided it is understood that both opinions are on par as professional assessments in the sense that the second opinion is simply one more opinion to be duly considered rather than a definitive or determining opinion relative to the first: there is and can be no general presumption in favor of one peer's opinions relative to another based on the status of the physician. They are in that sense equal. This does not imply that one of them cannot make a better case than the other, but that is something which the patient has to judge for him/herself. In case the two opinions conflict, the question of which to follow cannot be settled by turning to a third physician

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<sup>22</sup> The reference is, of course, to George Orwell's fable about authoritarianism.



who will settle the matter by telling the client which is right: all that the third can do is to offer a third opinion, on par with the other two, and if it agrees with one and not the other, there is still no implication that the one agreed to by two of the three is the better opinion in virtue of that. In other words, there is no authority status recognized as obtaining among physicians, all of whom are on par -- are peers -- in this sense. In general, there are no authorities among peers, no superiors or inferiors. Recognition of peer status is a procedural matter, not recognition of a matter of fact.

If this is so the question is, then, why the egalitarian conception of a research peer should be regarded as a part of the normative rationale of research, as it has in fact come to be conceived in modern times. Is this due merely to a sympathetic extrapolation from a commitment to political equality? Although this topic cannot be addressed here in depth and detail and with the rigor it deserves, at least this much can be said, namely, that the adoption of this normative conception in the practice of inquiry is based on the underlying assumption that in perceptual interaction with the subject-matter -- in experience of it, in other words -- the subject-matter itself will compel us to a belief or conviction about itself, provided we have made ourselves properly receptive to it conceptually and perceptually. The assumption is that there must be a causal relationship between the subject-matter of research and the researcher in which the researcher is passive in the sense of receiving the action of the object, such that the researcher's convictions are shaped by the subject-matter itself.

A common sense illustration: What color does a certain object have which is presently outside of my range of vision? I take steps to observe it and when I do this I see that it is red, let us say, and glaringly so: there is just no doubt about it. I can think anything I want, but the object itself insists upon impressing its redness on me, in that particular situation, at least, whether I like it or not. Experience is what interaction with the object impresses upon you; it is what you emerge from your encounter with the object as having learned from it. Now, it is the empiricist principle that requires recognition of the peer principle. Let me explain sketchily how this works.

Real things are faceted in the sense that they are perceivable from multiple complementary points of view, each of which is a facet or aspect of the appearance of the same thing. As the perceiver varies in his or her relationship to the object the shift in perspective or point of view reveals other facets of the object, each of which must be taken duly into account and reconciled with the rest as different facets or aspects of the same thing. The reason we must respect others as our peers in inquiry into things is that we cannot possibly build up an adequate general understanding of our subject-matter in a research field without trusting the basic competence of others in the field except where we have definite reason for doubting it, provided we have some *prima facie* reason for supposing that competence to exist. Otherwise our attention would have to be constantly diverted into an investigation of the competence of each of our colleagues rather than into the subject-matter. A peer is -- logically regarded -- equivalent to a respected perspective (or set of perspectives) on the subject-matter, and to treat a peer as other than an equivalent

of oneself -- whether as a superior or as an inferior -- is to undermine and derange the coordination of perspectives which is the constant task of the ongoing science.<sup>23</sup>

Peirce describes the coordination of the perspectives of the individual inquirers, which assumes an equal respect for each such perspective as having its own role to play in providing the composite substance of the data being reconciled in the coordination, in a striking passage in "How to Make Our Ideas Clear":

One man may investigate the velocity of light by studying the transits of Venus and the aberration of the stars; another by the oppositions of Mars and the eclipses of Jupiter's satellites; a third by the method of Fizeau; a fourth by that of Foucault; a fifth by the motions of the curves of Lissajoux; a sixth, a seventh, an eighth, and a ninth, may follow the different methods of comparing the measures of statical and dynamical electricity. They may at first obtain different results, but, as each perfects his method and his processes, the results are found to move steadily together toward a destined center. So with all scientific research. Different minds may set out with the most antagonistic views, but the progress of investigation carries them by a force outside of themselves to one and the same conclusion. (*Collected Papers*, 5.407)

The force outside of themselves as individuals is the manifestation of the reality of the object as a causal agent, determining the understanding of the community of inquirers by compelling the initially differing opinions of individual inquirers in such a way as to contribute toward the accumulated composite understanding in which, by theoretical coordination and reconciliation, a shared and commonly perceived unitary subject-matter is achieved. When only some members of a research community are actually treated as having a right to provide input into the theoretical reconciliation that is constantly being constructed in the ongoing course of inquiry, the community of inquirers shrinks, in effect, to the size of those so privileged, and the properties of the subject-matter that are effectively being accessed and taken duly into account for purposes of arriving at an understanding of the subject-matter are correspondingly diminished, and, with this, the theoretical understanding being erected on that basis is increasingly less determined by the real and therefore increasingly likely to be merely fanciful as properties of it which might otherwise be made available to the inquiring community are not experientially accessible. The empiricist principle that the subject-matter must have its say-so in the results arrived at, if the results are to be reliable, thus requires acceptance of the peer principle in research.

Speaking generally, it is possible to produce an understanding of a higher order of magnitude of intelligence in a research community or tradition than is achievable at the level of the individual as such: it is as if each person in the research community, each seeing something the others do not perceive in exactly the same way, adds his or her indi-

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<sup>23</sup> It is true that some people's opinions will inevitably be weighted more heavily in practice than others-- and no doubt should be if they establish a track record that warrants it--but this must remain at the level of individual judgment and not be confused with the shared public understanding of a given scientific community, which is always concerned only with characteristics of the subject-matter since it is that and that only which constitutes the concern constitutive of the particular community of inquirers as such. In other words, no community of scientific inquiry as such can legitimately concern itself with ranking its own members in terms of their status and worth in the community because to do so is to lose sight of its subject-matter by lapsing into group introspection instead.

vidual perceptual faculties and what they can yield to those of his or her peers, putting him/herself in service to others in that way: the research community as such has as many eyes and ears and individual minds as it has mutually trusted members, and is incomparably more intelligent than any of its members considered singly, provided its communicational practices enable its members to work together in constructing a commonly accepted and reliable theoretical understanding in which their individual contributions are reconciled and coordinated.

This can be done, though, only on condition of there being basic relationships of trust of the competence and honesty of others in the research community being taken for granted, and this trust is in turn based on attitudes of presumption toward one another which eliminate the need for each person to be constantly verifying the reliability of the views of the others. Try to imagine the case of a researcher in a research community who supposed that the opinions of everybody else in the community of inquirers should be assumed to be acceptable only after being verified or corroborated by himself. Without a presumption of competence and integrity on the part of others, on condition of recognizing -- presumptively -- each of the others *as* peers, there is simply no community at all and no available comprehensive view of the subject-matter. That condition of peer recognition is actualized in the actualization of the norms governing the communicational process, many of which will be found upon analysis -- or so I would argue but can only suggest here -- to be based on the peer principle in one way or another, which provide the framework of presumed obligations and permissions that enable the coordination of opinion to obtain and to sustain itself across time. Anything which augments the efficiency of these norms of communicational conduct is *ipso facto* augmenting intelligence and doing so, moreover, at a rate of magnitude of increase which cannot be estimated in the abstract but which is manifestly so great at times that it would be difficult if not impossible to exaggerate just how greatly human intelligence is magnified through this sort of collaborative cooperation.

### 13 AUTHENTIC AND PSEUDO-PEER REVIEW PRACTICES

What Ginsparg accomplished with his automated archive and server system was to establish a dialogical space within which peer review in its purest form occurs. I don't mean in its *perfected* form, which is an ideal possibility that will doubtless never be actualized, human limitation in all matters being what it is, since it would require a research community that was perfected in its ability and willingness to live up fully to such extraordinarily rigorous requirements as one cannot reasonably expect to be fully actualized. But the form is pure in virtue of the absence in it of manipulation of the discourse -- and thus of the results of it in what is eventually accepted or not accepted by the research community as valid findings -- by a human mediator controlling its content or occurrence *from without* by filtering it or otherwise shaping it in ways not available to a peer participant in the process on par with the rest, functioning as such in virtue of being under the constraints of the generally well-understood communicational norms governing peer-to-peer relationships.

Contrast this -- peer review proper, peer review in its pure form -- with what is at present misleadingly regarded as being peer review, assuming -- as is commonly assumed by its defenders -- that only editorially controlled access to the claims of researchers should be permitted, insofar as that is possible.

(1) When peer review proper is in effect, the researcher makes his or her research claim by a direct appeal to his or her peers to take notice of it, leaving it to the members of the research community addressed to live up to their responsibility to take due note of one's claim insofar as it pertains to what interests them as peers of the claimant. The providing of the abstract is the formal method by which this is effected in the Ginsparg system. In the case of "peer review" so-called as usually conceived and practiced, though, the researcher making the claim is not permitted to make the claim at all except insofar as the managing editor of the journal decides to permit this occurrence, which is a right not available to a mere peer or equal of the researcher making the claim, hence a violation subversive of the peer review process in virtue of being a violation of its normal workings as based in the peer principle.

*Comment:* Note that this assumes that the paper (the research claim) is not being made available to the particular peer community addressed in some other and concurrent way. If it is, then the editorial action is simply impertinent to the peer review process as such, which is going on quite apart from this particular editorial act, provided access is genuinely open.

(2) If the journal editor actually makes scrupulous use of a peer reviewer or a panel of such reviewers in deciding on what is to be published in the journal and what is and is not to be included in the published paper itself, this is done by selecting what the editor deems to be a peer of the claimant, typically without attempting to make sure that the claimant is actually addressing the person the editor chooses to perform the function of a peer. It is quite possible that an editor will now and again mistakenly choose as a peer of the claimant someone who in fact lacks the concern for the subject-matter and its problematics which is an essential condition of being a peer reviewer relative to that claimant. In authentic peer review, on the other hand, to the extent that any selection of participants in the peer review process occurs at all, its participants are essentially self-selective, insofar as it is possible for anyone to confidently engage in the dialogue as an addressee whose pretensions to being such will be honored by the other participants. Whether any given individual is actually functioning as a peer in the authentic review process, though, is a matter of fact that can be determined only empirically by careful interpretation of the dialogical process to ascertain whether or not a given putative contribution to the dialogue is actually functioning as such. Thus there is actually no selection of participants by persons at all in the authentic peer review process, the selection being a function of the ongoing course of the dialogical process itself. In a sense the selection is made by the research community itself, insofar as it is actualized in the discursive process.

*Comment:* Imagine a case where somebody not qualified to function as a peer in a critical dialogical process that follows upon the making of a certain research claim nevertheless attempts to do so. If the lack of qualification does not show up in what is actually said by the interloper, identifiable as such by those who are proper peers, then it may be because it is inconsequential in that context and it is as if it never actually occurs, just as

in ordinary conversation when someone attempts to “butt in” and is simply ignored by those already engaged. The words uttered simply fail to engage. Of course, this can happen with something said by someone who is in fact a qualified peer, too, but that is one of the natural imperfections of a dialogical process. More importantly, it can also happen that the interloper is not identified as being such and consequently ignored, as would be proper in the case of a pseudo-peer, but rather is treated mistakenly with the respect due to a peer and that person’s supposed contribution is given a status and weight it should not be accorded. This can happen, certainly, but the reason why editorial intervention is especially deleterious is that the prestige of the editor will tend to cause the peer participants to take seriously supposed but fake peer contributions which they might otherwise simply ignore as impertinent, giving weight to a putative objection or corroboration from that source, say, which it simply does not deserve to have accorded to it, this being done in a mistaken effort to pay due respect to the editor, thus interfering with the normal workings of the process which the rules of peer critical dialogue are intended to promote.

(3) In editorially controlled peer review, the peer reviewers actually used in the process are at best no more than a miniscule sampling of those who would ideally be involved in any peer review process, hence their contribution is given a weight by the editorial use of them which is entirely disproportionate, given the number of peers actually available in principle for review purposes. The rationale is, of course, that the editor is to use his or her judgment in selecting the peer reviewers, which may be no more than one but is never more than a handful, but with no commonly understood basis for doing so other than the assumption that the editor is a person of good judgment. There is no doubt but what many editors do in fact have good judgment, and that their selection of reviewers can be counted on to be reasonably just. But inasmuch as the opinion of the reviewers is actually operative in the publication process only via the confidence the editor places in them, and it is the editor who selects them to begin with, there is no getting around the fact that this is an elitist system in which the editors, who must themselves be peers of the readers of their journals, are functioning as Orwellian peers, peers more peerish than the peers whom they nominally serve. Such servants are really masters in disguise, regardless of their quality as thinkers, and the support of the journal system as the only legitimate mode of access of researchers to research in their fields is authoritarian and radically opposed to the basic spirit of modern research, which takes the peer principle as fundamental for reasons explained above.

(4) In the crippled form of editorially commissioned peer review, the function of communication is stripped of all logical force since the paper is not even made available to the peer community until after it has been adjudged to be sound. Thus there is no reason for interest in the question of how inquiry is rendered intelligent by the development of norms of communication or dialogue, and indeed there is in fact little interest in such matters, which are regarded as pertaining only to questions concerning the efficiency of the “delivery” of the “product” of inquiry. The study of the rhetoric of inquiry -- the dialogical process by which acceptance is encouraged or discouraged -- then becomes purely political in conception since it pertains only to the practical uses of knowledge already produced, and the institutions supporting research are naturally conceived as factories

whose improvement should take the form of maximizing productivity through the use of whatever carrots and sticks are most efficient in producing such results.

## 14 LESSONS TO BE LEARNED FROM THE GINSPARG PUBLICATION SYSTEM

What is usually defended as peer review in publication is actually Orwellian “peerhood” insofar as it actually functions as a critical control practice, where a class of persons is systematically afforded a status in the professional communicational process that positions them as functionally authoritative while seemingly functioning as mere peers in service to their peers. The privileged class in question is not, however, the class of commissioned peer reviewers (who actually have little power in their own right) but rather the class of managing *editors* in control of the various media available as publication venues who commission peer reviewers and decide what weight, if any, to put upon their opinion case by case in the process of their own decision-making about acceptability, revisability, and actual publication of research claims.

Although the approach taken here may suggest the contrary, this is not said as a general negative criticism of the role of editors, whose selective and organizing function in research is indispensable and who deserve far more appreciation for their efforts than they commonly receive. It is merely that we have regarded them thus far only as they are regarded from the misleading perspective provided by those who mistakenly regard them as functioning primarily to control the flow of research discourse from a position superior to that of the researcher proper, which is a position thrust upon them heretofore in consequence of the de facto impossibility of establishing media of distribution that would make it possible to provide universal and unrestricted access to research claims. That limitation no longer exists, as was first recognized in practice by Ginsparg when he decided to establish at Los Alamos the modified form of the pre-existing system of preprint distribution. In consequence of this the editorial functions in physics in general have undergone a rapid and continuing reform over the past decade or so which suggests that the problem of the extending the peer principle in such a way as to include the editorial function instead of setting the two at odds with one another will first be addressed straightforwardly and solved or resolved in a correspondingly straightforward way there before it occurs elsewhere in the world of research. At present, though, apart from developments there, the confusions about the critical control of research that have been generated by the mistaken conception of peer review as being paradigmatically exemplified by editorially commissioned peer review make it all but impossible to perceive clearly what the authentic editorial function in research communication actually is. But that will have to be addressed in another study.

In any case, one of the things we can learn from reflecting upon the import of the success of the Ginsparg publication system is that when a research tradition has reached a mature state it does not require editorial leadership at the leading edge of research in the field, and that, conversely, when a research tradition is unable to make effective use of such a system it may be because the need for editorial guidance at that point in the re-

search process is too great for the people in the field to function effectively in the authority-free and highly austere and formal communicational environment provided by the Ginsparg system of primary publication. Of course there may be other reasons why a given research field is incapable of making effective use of an authority-free primary publication environment of this sort:

- For example, it might be that an “invisible college” of private pre-print circulation of papers is in exclusive possession of the leading edge in a given field and the appeal of their own self-interest as members of a privileged class is simply too great for those so privileged to want to take advantage of the opportunity to make the radically egalitarian move that Ginsparg and his associates made in establishing their automated and unrestrictedly available publication system at Los Alamos. There was surely a gamble there, and there must have been a substantial number of physicists among those who initially adopted the new system who were initially resistant to the establishment of the open access pre-print server in the belief that idealism is nice but the quality of work which would appear there, under the conditions of unrestricted access, could only result in a decline in the field. It seems reasonable to suppose that this will be the case with at least some fields that probably could successfully adopt the Ginsparg system now but lack enough boldness of leadership among the most respected leading figures to make the transition from the protected environment to which they are accustomed to one that they can only “view with alarm”.
- Or it might be that the field is one in which the funding arrangements are such that much important research must be kept secret and primary publication must be carefully and skillfully censored to insure that nothing is discussed in it that could jeopardize the relationship with major funding sources for the field because of unintended “security” violations, whether they be commercial or governmental in character. The extensive -- and increasing -- control of research by private interests as well as by clandestine governmental funding of research for purposes of national security is no doubt sufficient to explain why a number of fields cannot possibly make use of such a system and must depend to a high degree on editors in the role of censors.
- Or it might be that the field or subfield is too inchoate and unfocused overall for a primary publication medium such as the Ginsparg system provides to be regarded as being of much value as a venue. For such a field, the automated archive could only be a collection of papers that might or might not be of interest, but it would have too little rationale as a collection to be of much interest since there would be no dialogical process to which the collection would be functioning as a contribution. In that case there would be little reason, if any, for regarding it as a system of primary publication

There are perhaps other reasons as well, but these are enough to make it not at all surprising that the Ginsparg publication system works well only for those fields that are sufficiently matured to be able to make effective use of it because the communicational practices that were already governing those fields finds its arrangements congenial.

I suggest, though, that any serious study of the communicational practices even of such successful fields as these will find that the communication that occurs on the basis of such an austere and formal system is in fact but a very small part of the research communication practices even in those fields, and this is true a fortiori of the many research fields which have yet to develop to a comparably sophisticated level. What makes the Ginsparg system unusually interesting, then, is not that it presents a paradigm of what research communication is all about, but rather that in virtue of the success of their development of such a pure system of primary publication, we are enabled both to see what authentic peer review is actually like with an unusual clarity because of the simplicity of the system and thus to understand what the principles underlying practices of formal publication are, and also, in virtue of that, enabled to see as well that if we want to understand how research communication in general works, in the interest of developing and augmenting it, we have to set ourselves the task of finding out what other sorts of communicational practices are actually operative in the inquiry process in such fields and in other fields as well, since we cannot reasonably suppose that the success even of such highly developed research fields as these is due exclusively or chiefly to their primary publication practices.

While I believe that a proper understanding of the success of the Ginsparg system, which can be regarded as an IA application, reveals it to be an ideal implementation of computationally assisted primary (i.e. formal) publication, the most interesting prospects for development of IA in this area lie in designing computational assistance for the many varieties of communicational practices involved in research activity that precede the stage of inquiry at which formal assertion of putative findings occurs. Interest in these less formal and rigorous types of communicational practices has yet to develop, however, because they must be understood in relationship to formal publication practices, and these latter have been so poorly understood to date that there has been no conceptual framework available for investigating these other and equally important practices as regards their rationale and needs. My discussion of this case has been motivated by the aim of taking a first step in that direction by developing some conceptions useful for understanding what has and has not been achieved by Ginsparg in establishing that system successfully. The present account is only intended to be suggestive of what a more rigorous and thorough-going account of this will be like, which I hope to make available in the near future. Critical feedback will of course be more than welcome.