New Research Norms for a New Medium[1]

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Many who have worked in a medium of ink and paper look to a future dominated by a digital electronic medium with a mixture of awe and apprehension. Scholars and researchers are among them, hailing the shift from the largely paper-based infrastructure of publication, communication and archiving to a predominantly electronic one as a boon, but also recognizing the problematic potential. Economic uncertainties likely to face publishers trying to devise business models for the new terrain are certainly worrying, but more central to scholars and researchers is the quality and integrity of the published corpus of their respective fields of inquiry and, by implication, the quality and integrity of research and scholarship itself.

This paper addresses an aspect of the relationship between electronic publication and the quality and integrity of research by focusing on one of the important mediating influences of quality and integrity, namely norms and conventions governing research and scholarship. What concerns me particularly is how we ought to be thinking about adapting those entrenched norms and conventions whose characteristics have been shaped for a realm of ink and paper, to an environment in which the electronic medium reigns. I use the example of establishing priority for research results as a case study for a general approach I develop in this paper. What I propose is that communities of scientific research should put potential normative shifts to a test, evaluating the extent to which proposed new norms are as true to research values as the entrenched norms they would replace. In the case of priority, I analyze whether commodification, a close

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cousin of priority, could feature successfully as a rival source of norms. On the basis of my test for evaluating new norms for the new medium, I argue that property norms [,] despite their long history and success in other spheres, are problematic as replacements for norms governing attribution of priority in spite of the latter's relative ambiguity and contextual dependence.

Research Norms: Attribution of Priority

Scientific research is constituted of an enormous array of practices. These practices are governed – implicitly and explicitly – by a set of interdependent norms, conventions and institutions that define standards and prescribe expectations within research communities. (From hereon I use "norms" or "conventions" as shorthand for "norms, conventions and institutions.") These norms regulate the basic methods of conducting research (in whatever the environments typical of various fields) and for reporting on the results of such research. In addition to procedural standards, norms regulate practices such as peer review, authorship, attribution of credit, research collaboration, mentorship, and more. In this paper, I am particularly interested in the subset of these norms that Seumus Miller, in his work on social norms and conventions, would conceive as having "moral or quasi-moral normative force" (1997, p. 213). According to Miller, norms, conventions and institutions acquire this moral or quasi-moral force in the extent to which they help people or communities realize moral or morally relevant values, ends and purposes.

Among these norms, some more than others are likely to be affected by a shift to the medium of electronic publication, communication and archiving – perhaps those governing authorship, peer review, and collaboration, for example. The goal of this paper is to offer a systematic approach to the challenge of devising new norms and reshaping old ones for the context of the new medium. I will use as a case study for this approach norms governing the attribution of priority, which, surprisingly, will call for adjustment as our reliance on the electronic medium of publication grows.

To have priority for a significant idea, result, discovery or invention (henceforth I use "result" as a generic term), is to have one's name associated with it and to be recognized as the first to have produced, invented, or discovered it. Many researchers, for whom priority is the "holy grail," will fight priority disputes with a fierceness and enmity not usually associated with the genteel hallways of academe. Some of these disputes have reached outside the research communities playing out in the public eye, for example, Robert Gallo of the National Cancer Institute fighting Luc Montagnier of the Pasteur Institute of Paris over priority for the discovery and isolation of the AIDS virus. The fight persisted for more than a decade and involved Gallo and his co-workers in accusations of gross misconduct. In another dispute a few decades ago, the South

African surgeon Christian Barnard performed the first ever heart transplant on a human, infuriating his mentors at Stanford University who believed that in exploiting the less stringent criteria for experimental medical treatment of his home country, Barnard had stolen their own clearly deserved priority.

Examples could be spun out indefinitely: the race to the double helix, Newton competing with Leibniz over calculus, Pasteur's dubious victory in the discovery of an anthrax vaccine and so on (See Geison). I will here briefly describe only one more, to use as a case study for the themes I wish to explore in this paper. The case involves mathematicians in the 19th century.

In 1832 a young mathematician, Janos Bolyai, published a treatise on a theory of non-Euclidean geometry, known today as hyperbolic geometry. Because at the time he had not yet established a reputation as a scholar, his father published the work as an appendix to an essay the father himself had written. When the senior Bolyai communicated his son's result to Carl Friedrich Gauss, the pre-eminent mathematician of their day, Gauss responded with words that mathematicians today are fond of quoting and frequently parodying. Reacting to Bolyai's work, Gauss wrote: "To praise it would amount to praising myself. For the entire content of the work ... coincides almost exactly with my own meditations which have occupied my mind for the past thirty or thirty-five years."

As it turns out, a third name, Nikolay Ivanovich Lobachevsky, is now also associated with the discovery of hyperbolic geometry. Lobachevsky had pre-empted both Gauss and Bolyai with his result published in 1829. Nevertheless, surprising as outsiders may find it, many mathematicians consider the attribution of priority for hyperbolic geometry to be genuinely controversial. Although some are ready to be strictly guided by publication date and find this enough to establish Lobachevsky as the discoverer, others argue for priority of one of the other two, or both, or all three. So the dispute continues to fester long after its protagonists have passed on. We will return to this case later in the paper.

Although disputes over priority are more common in science and engineering, they are not unheard of in other fields. In philosophy, for example, priority over the so-called "New Theory of Reference" is hotly disputed. Although most philosophers acknowledge Saul Kripke as discoverer, the philosopher Quentin Smith raised a storm of controversy by arguing that some of the fundamental ideas in Kripke's theory had been discovered and articulated eight years earlier by Ruth Barcan Marcus (Smith, 1995). As in the hard-fought priority disputes of science and engineering, this one involved hostile exchanges, careful retracing of historical milestones, and a thorough study and reanalysis of the disputants' relevant key works. At the writing of this paper there is still no closure to the debate, though several leading philosophers of logic and language have claimed to have debunked Smith (Burgess, 1998; Holt, 1997).

This paper, nevertheless, is tuned primarily to the context of science and engineering partly because priority and priority disputes are a more common feature of the science and engineering landscape, where research efforts regularly

converge on a number of widely recognized ends. Another, pragmatic reason for focusing on science and engineering is to reduce variation; even within the various sub-fields of science and engineering there is a degree of variability which I am not able fully to accommodate in what follows.

Publication and Priority

Before considering how attribution of priority may be affected by electronic publication, let us first consider the relationship of priority to publication generally. To earn priority, researchers (or a team of researchers) typically need, in the first place, to persuade their communities that their results are correct, sound, valid; that is, meet the standards of their fields for establishing the integrity of results, processes, discoveries, procedures, etc. One of the most common ways to establish integrity of results is to submit a written report for publication so that reviewers – usually accomplished members of a research community – may evaluate it. In judging a work worthy of publication, these respected peers accord public recognition of its quality. In the second place, researchers must persuade their peers of the primacy of their claims; that is, that they are the first to have proved, discovered, demonstrated, etc. the result. Publication frequently plays an important role in this, too, as researchers typically demonstrate that a thorough search of published works has not yielded the same result.

People credit Henry Oldenberg, who served from 1662–1677 as secretary of the Royal Society of London for Improving Natural Knowledge, with cementing the connection between priority and publication. Oldenberg, who opposed the secrecy that shrouded scientific discoveries and believed in the power of critique-and-rebuttal, started the Society's *Philosophical Transactions: Giving Some Accompt of the Present Undertakings, Studies and Labors of the Ingenious in Many Considerable Parts of the World* to promote these commitments. To encourage scientists in Europe and England – among them the young Isaac Newton – to publish their results in the *Philosophical Transactions*, Oldenberg offered prompt publication. He also promised that the Royal Society would stand behind these scientists' claims to being first to discover the phenomena in question. Thus, in March 1665, Oldenberg initiated the convention that priority goes to the person who publishes first (or now, more precisely, the person who first submits the finding for publication), rather than the person who first makes the discovery.[2]

[2] Discussed in Whitbeck, C. (1998). *Ethics in Engineering Practice and Research*. Cambridge University Press, New York p. 267; and *Dictionary of Scientific Biography*, Volume X, 200–203, American Council of Learned Societies, New York: Charles Scribner 1974. In reading up on Oldernberg, during my stay at the Institute for Advanced Study, I had the remarkable

A few qualifications and caveats are in order: (1) What I have described is a somewhat idealized version of actual practice. In the real world of research there may be significant compromises – some necessary, others regrettable: reviewers may not be as careful as they ought to be, acknowledgment of prior work not as comprehensive as it ought to be and so forth. (2) The details of practices may vary among distinct fields and disciplines: for example, in some fields conference proceedings (or other pre-publication reports) do not "count" as publications as much as polished journals, whereas in other fields, articles in polished journals are viewed as already "old." I do not expect that all such variations will affect my central claims. But in the cases where they do, it is quite possible that the broader picture I describe below will not apply. (3) Although publication is one of the predominant vehicles for establishing priority for results, I do not maintain that it is the only vehicle. Particularly in applied or practical fields it may be that what matters is not publication but being first to build a prototype, or perform a procedure (e.g., heart transplant surgery). Other exceptions may occur in fields that are willing to attribute priority even when researchers have not offered the full disclosure of publication but have only managed to convince key peers through informal channels that their priority claims are valid.[3] I merely claim that publication, for many fields of research in science and engineering, is one central vehicle for establishing or brokering priority and it is important, at least in these fields, to understand the expected impact of a shift to the electronic medium.

The Puzzle

Consider how electronic publication may affect conventions for establishing priority. As we have heard repeatedly, electronic publication "democratizes" publication, meaning that there are many more avenues for publication, including self-publication, that do not involve clearing the usual hurdles of peer review. This prospect has caused widespread concern that in eroding the traditional means of control, electronic publication will lead eventually to a declining quality of published work, resulting in an enormous but untrustworthy body of work unless we find ways to carry forward mechanisms or conventions of review. Although there are good reasons to worry about this problem, it takes us too far from the

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experience of paging through several of the delicate, yellowed three-hundred year-old volumes of Transactions, published around 1667. I am grateful to the librarian for pointing these out to me in the Institute's rare books collection.

[3] Robert Merges (1996) discusses several such scenarios, especially in high-stakes biomedical research.

current thread. Its relevance to priority is that the loss of one of the established mechanisms for assessing the correctness of results means we undermine one of the two crucial elements involved in validating the priority claims of aspiring researchers and scientists.

Although of enormous importance, it will not be the first but the second element – namely, establishing primacy – that will be my primary focus. Here, electronic publication poses a curious puzzle. Here, too, the backdrop is what people call the "publication explosion." If we construe publication in terms of its natural meaning – to cover the activity of preparing material for presentation and distribution, sometimes for sale, in public – we see that information and communications technology throws wide open the possibilities for publication.[4]

Currently the most prominent venue for publishing works in the electronic realm, the World Wide Web ("the Web")[5] has inspired a sense of excitement and prospects of revolutionary change. For many, the Web represents an ideal – a readily accessible medium without entry barriers imposed by the traditional gatekeepers, such as commercial publishers. Everyone who has access to the Web and some know-how can publish. Cited as key evidence for the democratizing tendency of information technology, this broad access to a medium of publication enables vast numbers of people to speak out, be heard by others, hear others speak and communicate to one and to many. Taking advantage of this novel opportunity, participants and contributors have built the Web into a virtually endless mélange of text, sound, images and information. It ranges over the polished, commercially produced websites of corporations (some costing millions of dollars) and government agencies, as well as the eccentric, highly personal websites of lone individuals. All of this constitutes the realm of electronic publication. These compelling but phenomenologically-based impressions of size and scope are given quantitative measure by researchers like S. Lawrence and C.L. Giles who, in 1999, using statistical sampling methods, estimated the lower bound size of the total indexable web to be 800 million.[6] In two years, this number has surely grown to above one billion. (see Ziman, 1998).

As noted earlier, scientific researchers claiming priority need to convince their peers that they have scrutinized the relevant body of published works and have ensured that they are first, that no others before them have obtained the same or very similar results. In opening wide the possibilities for publication, the electronic medium poses enormous challenges to locating the relevant body of published works. In addition to the standard journals, of which many are published on-line,

[4] In using the term "natural," I refer to the meaning of the term as conveyed in general conversation and captured by definitions given in any of the standard English dictionaries, such as Oxford or Webster.

[5] Although electronic publication may occur in locations other than the Web, this possibility only exacerbates and does not diminish the puzzle I describe below.

[6] S. Lawrence and C.L. Giles, "Accessibility of Information on the Web," *Nature*, 400, 107–109, 1999.

there is a wide range of new kinds of publications whose very possibility was unthinkable in the paper-based medium. Specialists in library and information science have identified various forms of emergent non-traditional species of electronic publication. B.J. Wyly calls attention, for example, to "wildcat electronic publications," a term he coined to describe the products of "small, single journal electronic publishers of free electronic journals" (1998). Like his namesake in the oil business, an "independent entrepreneur who has the daring to drill an exploratory well on a hunch about a possible oil field," the wildcat publisher creates exploratory scholarly communication tools based on some sense of the needs of scholarly communities and based on a hunch about the possibilities of electronic publishing. A generic term of trade, "gray literature" covers a broad range of material found on the Web that is not published by established publishers. This includes electronic preprints, locally published reports placed on the Web by institutions like universities or corporate research centers, and reports of work that individual scientists and engineers place on their personal web pages (see Gelfand and King, 1998; Sonkkila, 1998).

The puzzle of priority is this: in the face of the dramatic publication explosion, what will become of the norms governing priority? What will become of the systematic expectations we have of researchers claiming priority? How do we revise entrenched norms, how do we devise new norms, for a research environment built upon an electronic substructure? Will the burden upon those claiming priority be that they now must search all electronic publications to assure colleagues that they have a well-grounded conviction that no one, anywhere, has previously published similar results? Do we expect them to conduct a search of literally all publications, including every John Doe's and Jane Roe's personal Web pages? If not, then what?

Research Norms and Research Values

Rather than answer the question about priority norms directly, consider the question in terms of research norms (conventions and institutions) generally. Setting out to understand the rationale for existing norms, we surely will find a complicated picture. Sure to feature into this picture is the role of research values, ends and purposes in determining the shape of these norms, conventions and institutions. (From hereon I mainly use "values" as an abbreviation for "values, ends and purposes.") Take for example, the practice of peer review in which a paper is distributed according to certain rules for evaluation by several experts in the field. We can understand norms governing peer review as a way to promote and maintain the soundness and integrity of a body of research results. Blind review, a special form of peer review, seeks similar ends, trying in addition to

correct for the tendency of personal ties to obscure or bias objective considerations. Norms that prescribe replication of experimental results serve to promote the ends of objectivity and fidelity in a world where researchers, even in good faith, are all too vulnerable to bias and accident. Conventions of authorship can be understood as designed to nourish and give recognition to promising researchers.

There is an important distinction to bear in mind between values, ends and purposes attributable to all humanity, serving, as it were, general welfare and those of subgroups within the general. In imagining ends of scientific and engineering research serving general welfare we might include the creation of a body of reliable and trustworthy work, the capacity to respond effectively and engage productively within the world, the ability to manipulate the world in service of important human needs. Even more fundamentally, we may also include ends such as: knowledge, truth, progress, social welfare, and the betterment of the human condition.[7]

By contrast, one can imagine a different set of purposes serving specialized subgroups like scientific researchers, universities, industrial organizations and non-governmental funding agencies, whose interests are not always identical or even compatible with those of the general. Any one of the subgroups may wish to steer scientific research practices, via norms, conventions and institutions, in directions that serve its own parochial interests, and, as a matter of fact, probably has succeeded in doing so in many cases. A descriptive or historical study of scientific research could chart the important place of parochial interests in determining the shape of conventions and institutions (and no doubt parochial interests have carried the day for particular practices). This paper, however, is primarily normative and primarily concerned with conventions responding to the general welfare. In other words, it is concerned with norms and conventions that carry moral weight; those that serve parochial or special interests, to the extent that they serve solely special or parochial interests, do not.

Although in some cases, it may be possible to understand a given norm as optimal, or near-optimal, in achieving certain ends or promoting certain values, it may be the case that the shape of research and publication norms will require more than an accounting of the relationship of norms, on the one hand, to values, on the other. Many other factors can influence the character of norms – some quite idiosyncratic. In some cases – ones I would judge pathological – a community may

[7] There is bound to be considerable disagreement over precisely what constitutes the goals, ends, values and purposes of scientific and engineering research, especially given skeptical appraisals of many sociologists, historians, philosophers and anthropologists. A discussion and defense of my proposed list extends beyond the scope of this paper and I must, therefore, let it stand as a basic assumption. For readers who fundamentally disagree with my characterization of the "values, ends and purposes" of scientific research, the central claims of this paper will most likely appear inadequately defended. Others who merely find the list incomplete, or mistaken in detail, may find that this does not detract from the central claims and arguments.

accept norms that are not optimal for achieving stated values because, for example, members systematically misjudge the way the norms operate on practices, or because it is corrupt, or because it is excessively influenced by parochial interests. Although in these pathological scenarios we should take corrective steps, in others, there are unavoidable but legitimate co-determinants of scientific norms and conventions that are non-moral in nature. To these I now turn.

The context for scientific research is ultimately the real world. If we wish to understand the factors that shape, or co-determine, the character of research norms, we must look beyond underlying values and ends, to contextual constraints of the research environment itself. These constraints may be as general as the political and educational backdrop of research, or as mundane yet as compelling as economic contingencies and speed of the local postal service. For example, the conventions governing recognition of scientific work and career development in the Soviet Union, several decades ago, systematically would take into account whether a researcher was Jewish. In other words, one would understand the shape of these norms only if one took into account certain features of the political context. In understanding norms of peer review, one needs to bring into account a number of detailed, practical factors such as overwork and the cost and difficulty of distributing copies. These factors may work against an ideal driven only by considerations of, say soundness and integrity, and can determine limits on features like the numbers of reviewers, degree of diversity and level of expertise, amount of time, etc. Similarly the convention calling for replication of experimental results is softened by practical factors such as funding limits and the general sense in many scientific communities that those who choose to replicate results are not engaged in what is seen as the exciting pursuit of novel research.[8]

In sum, successful norms (successful as norms with moral force) are ones that for a given environment are optimal with respect to values; they are likely to be shaped also, but not overly compromised by, external factors including environmental constraints and parochial influences. At any given time these norms will reflect a balance between the idealized pursuit of values, on one hand, and contextual constraints, on the other.

Over time, norms – even those that are for the most part successful – may change, evolve. From the model I have described, one can predict what some of the precipitating factors of change might be. It may be that a research community, finding that its record of stated ends and promoting recognized values falls short, makes a dedicated effort to improve via an adjustment of norms. It may be that change is precipitated in a shift in the nature of the values themselves. Or it may be

[8] David Baltimore discusses this point in his Tanner lectures (1992).

that non-moral features of the research context change, requiring a shift in norms.[9] These changes in norms may be gradual, occurring over the course of years or even centuries; or they may be sudden, a response to a significant discontinuity in either context or values.

In the case of attribution of priority, we have a case where changes in non-moral features of the research environment challenge entrenched conventions. These changes, as mentioned earlier, occur as a result of the shifting from a mode of publication, communication, collaboration and archiving that is paper-based to one that is largely electronic. If prevailing commentary and predictions are borne out, the shift will be swift and radical (some might even say revolutionary) and will demand swift and sometimes radical adjustments in norms, conventions and institutions. The puzzle of priority is a symptom of the need to adjust entrenched norms because of changes brought about by electronic publication. In this case we shall not have the luxury of waiting centuries for new norms to evolve, but will need to take explicit and active steps to adjust them to the new context. In the following section, I suggest a strategy for responding to such needs to revise norms.

The Test

I put forward attribution of priority as an example of a practice that is likely to be disturbed as a result of a move to a digital electronic medium of publication, communication and archiving like the Internet and the World Wide Web. Conventions that worked reasonably well in the past may give rise to challenges, like the puzzle, when they are embedded in a new context. Instead of a quick resolution to the puzzle, I outline a systematic approach to devising and evaluating new research norms for a digital age, drawing on the model I have sketched above of the relationship among norms, values, and contextual factors. The terms of the model offer a vocabulary for framing a fundamental test of any proposed norms (that is, new norms that would replace entrenched norms). The test to which we put new norms is to evaluate whether, and to what extent, they promote the values, ends and purposes that inspired the entrenched norms in the first place.

In practice, establishing that norms have "passed" the test is not a straightforward affair. To demonstrate that the proposed new norms serve the values served by the norms they replace, and serve them as well as, or better than,

[9] In The United States we are currently seeing a spate of interest in norms and conventions governing scientific research coming in the wake of much publicized cases of so-called scientific misconduct. A large literature discusses this trend. See as an example, Caroline Whitbeck (1998).

these norms, calls for an acquaintance with, and at least some understanding of, complex social, intellectual and historical factors driving the research endeavor, as well as an ability to reason about considerations of value. Once we have explicated the relationship between proposed new norms and values and ends, and compared this with the relationship of entrenched norms and values and ends, we must be prepared to consider how efficacious proposed norms would be in comparison with competing contenders. (Readers familiar with traditional utilitarian analysis will be familiar with the formal characteristics of this demand: a moral prescription to act, or not to act, in a certain way should not only maximize the balance of happiness over unhappiness but must do so optimally.)

In spite of these complexities, it is worthwhile submitting proposed norms to the test because the alternatives are in all likelihood worse. One alternative is that under pressure a community would adopt new norms prematurely without consideration for values and ends. This would leave the community, and society at large, vulnerable to the possibility that these new norms are suboptimal in a variety of ways, involving, for example, rigors that are arbitrary and not necessarily conducive to the values, ends and purposes of scientific research. Or they may involve an excessive compromise of values in favor of practical convenience. Another danger of adopting norms without submitting them to the test is that the scientific community might allow a tilting of purposes away from general societal needs toward parochial interests of specialized groups. In any of these cases we will have weakened the moral content, the moral force, of norms, conventions and institutions governing scientific and engineering research.

One final point to note is that the test need not be conservative; it need not limit possibilities to norms that do as well but no better than predecessors. Indeed, the appeal of information technology as a medium for research publication, communication and so forth is that it promises more; it offers the prospect of a less constraining research environment and potentially a more effortless, less costly, more efficient path than previously possible to realize the ultimate ends and values of scientific and engineering research.

Now, armed with the test, let us return to the puzzle of priority, at the same time trying to learn something about adapting research norms, in general, to contextual changes brought about by new and powerful technologies. In order to demonstrate how the test may be applied, I have narrowed my focus to two possible lines of response that both seemed to be plausible contenders. The one that occupies most of my attention is a wholesale shift in normative framework to that of intellectual property as a means of reducing some of the fuzziness and ambiguity in current norms of attribution of priority that the puzzle demonstrates. I will argue that this move fails the test and will briefly consider an alternative approach. This plan is inspired by two goals. One is that it offers a vivid illustration of how one may use the test as a tool for crafting and evaluating new research norms for the information age. The other is to mount a challenge, in this one small corner, against the looming dominance that intellectual property seems

to have acquired as an omnibus solution to a broad range of problems in this era of information and information technology.[10]

Allure of Intellectual Property

There are a number of reasons why intellectual property may seem attractive as a replacement regime. Norms and conventions in research and scholarship are unquestionably imprecise, even sometimes ambiguous. As has happened in other domains, the arrival of information technology can spell opportunity for much needed change. In this case, one could argue, it triggers a need to impose order on an archaic, motley family of norms, particularly those guiding the allocation of entitlements, under the rubric of our rational system of property rights. Entrenched conventions that have guided the attribution of priority, as well as authorship, peer review, collaboration and so forth, evolved in a far different context and cannot automatically be expected to remain effective in the electronic realm. Why force from traditional conventions more than they are naturally able to yield when the conceptual framework of intellectual property, refined and adapted for the infrastructure into which research is sinking its foundations, is ready at hand? The institution of intellectual property has the further advantage of many years of explicit formal development within the legal system. Through it we could avoid the fuzziness and ambiguity of many of the entrenched conventions guiding scientific research that evolved only implicitly through years of use and practice.

Another reason to consider a role for intellectual property in rationalizing research norms and conventions is less normative and more historical. It points to the prominence of intellectual property in our thinking about information technology generally. In the past roughly two decades, intellectual property has been a central preoccupation of a number of constituencies involved in the development and use of information technology. Almost as soon as the commercial promise of information technology was recognized, lively debates erupted over the nature and extent of private ownership that could and ought to be exercised over a variety of aspects of the technology. Hardware and software developers, creators and distributors of content, economists, philosophers, scholars and practitioners of law and policy, motivated by concern over their and other stakes in the question, argued over the ownability of everything, from chip design to source and object code to algorithms. These arguments gained momentum and urgency as their scope extended to the many forms of intellectual

[10] It has emerged as a solution – misguided, I think – also in the context of protecting privacy.

work expressible in electronic form such as text, images, movies, music, information and more. The digital electronic format offered unprecedented tools for creating works of art and intellect embedded in a technology whose very nature is to provide easy, widespread and virtually instantaneous access to these works, as well as a power to manipulate vast troves of material. A clear blessing for many, such capabilities create headaches for those who seek to control and profit from these creations. The institution of intellectual property was seen, and continues to be seen, as a robust answer to this conundrum.

There remain several unsettled controversies, several highly charged questions about the appropriate extent and strength of intellectual property rights in cyberspace (see, for example, McCullagh, 1997; Samuelson, n.d.; Samuelson et al., 1994), and some important challenges to traditional regimes of intellectual property, most notably the open source movement. Nevertheless, it is fair to say that norms and policies defining the institution of intellectual property are – for better or for worse – a defining part of the culture that has emerged with the electronic medium as we know it today. As anecdotal evidence of this progression, I note the considerable familiarity many academic computer scientists have with the intricacies of intellectual property, as compared with their relative innocence of such matters, say, fifteen years ago. Also anecdotally, with other norms of authorship, it is common for people to express their disgust over plagiarism as a wrong involving a violation of property rights.

In progressively moving its center of gravity to the electronic media, the research enterprise is not merely adopting a set of bare tools and technological capabilities; it is adopting a technological system that is rich with values, culture and conventions of its own.[11] This shift to an electronic infrastructure of publication is not merely a matter of the world of scientific research adopting and adapting a set of neutral tools for its use. It is a matter of two worlds meeting – one, research, the other, information technology – both replete with their own sets of values, purposes, norms, practices, conventions and meanings. In the case of information technology this culture and these values powerfully reflect those of the communities that have developed the technology, as well as the communities in whose service the technology was initially developed. For the most part these communities have embraced intellectual property into their culture and viewed it as a boon.[12]

Another reason is more specific. The reason we think of intellectual property at all has to do with the very nature of a priority claim, which is after all a claim of

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[11] The idea of values embedded or embodied in technological systems has been discussed quite actively in recent decades, first within the area known as Science, Technology and Society (STS) and more recently applied specifically to computer systems. (Bijker et al., 1987, Nissenbaum, 1998; Winner, 1986).

[12] I say "for the most part" because this is not true universally. Richard Stallman, through the Free Software Foundation, and many others continue to propound a view of computer software as a public good.

entitlement that an individual (or group) makes over an intellectual work. There are suggestive similarities between attribution of priority, on one hand, and allocation of intellectual property rights, on the other, more specifically, between the meaning conveyed in a priority claim and that conveyed in a patent claim. Both attach to the authors of inventions serving as public recognition of those authors. Both place demands on the quality and nature of a work in question. Both demand primacy of discovery. Both demand evidence of a search for prior similar work.

Intellectual Property and Research Norms

On the basis of these considerations, importing a patent-like system into the realm of research and scholarship holds promise. Moreover, in replacing norms governing priority, it offers needed precision to deal with difficult questions raised by our "puzzle." Beyond the potential efficiencies of such a switch, we may be interested in whether it is morally compelling. And to satisfy this interest, we need to submit the proposed change to the test; that is to say, evaluate whether the proposed new norms sustain and promote values, ends and purposes as well as their entrenched counterparts.

I see two perfectly good strategies for carrying out the test. In one, we first spin out the relationship between entrenched conventions and values and new conventions and values, respectively. Next we compare the results. In another – the one I pursue here – we take a shortcut by first identifying key differences between proposed (new) conventions and entrenched (existing) conventions and next analyzing how these differences might impinge upon the attainment of ends and values. In carrying this out, this second mode of analysis, I note two differences between patents and priority:

> Difference I: Patents do not tolerate independent invention, discovery, performance, etc.[13] By contrast, under entrenched conventions, priority can be shared.

[13] The prevailing patent system in the United States is committed to a distinction between discovery (e.g. discovery of pre-existing facts of nature) and invention (of processes, methods, machines, etc.). The second delimits a category of patentable works the first delimits one that is not. Although there may be utilitarian reasons for insisting that some works ought not be patentable, I do not see that the distinction can sensibly be based on the distinction between discovery and invention which has been given dubious interpretation, in my opinion, in the context of patents. It is not central to my paper that this matter be settled so I propose to finesse it.

Difference II: A patent must be formally applied for and registered with a legally sanctioned body (in the United States, The Patent and Trademark Office). No strict equivalent exists in research, where publication serves a similar function.

These differences, in themselves, do not imply superiority of one approach over the other. We need to look beyond the mere presence of difference, beyond formal disparities between institutionalized conventions of patents, on one hand, and conventions of priority, on the other, toward the question of their respective functional implications for values, ends and purpose. Before taking the two points of difference in turn, I need to prepare the ground by acknowledging key substantive presumptions upon which my analysis rests.

The first concerns the moral foundations of intellectual property, and property in general. While acknowledging the enormous depth of thinking on this subject, for the sake of simplicity (and progress on core issues of this paper), I adopt a utilitarian approach.[14] Accordingly, I take general welfare as the key measure for deciding among alternative regimes of intellectual property.

According to utilitarian reasoning, intellectual property produces utility by investing intellectual property owners with control over intellectual works. This they exercise by limiting access to the work, and setting conditions of sale, transfer, duplication and so on. In the case of patents, control is monopolistic – at least, for a period of time. Received wisdom has it that investing property holders – in this case patent holders – with control creates favorable conditions for investment because property holders, assured of protection, are ready to invest in bringing inventions, etc. to the public – paying for development, manufacture and so forth – will be protected. In turn, the promise of profit and other success, serves as encouragement to further inventiveness, creativity and bold investment in projects that might otherwise seem risky. The potential gains are thought far to outweigh the recognized costs to individuals and societies in the forms both of restricted freedoms and also the costs of enforcing property rights. In the patent system, for example, many see a system that has been finely calibrated (and continues to be adjusted) to yield on balance – through the direct benefits of innovative products and indirect benefits of wealth – a substantial increase in social welfare.[15]

My paper also rests on certain beliefs concerning the ends and purposes of research in science and engineering, and conditions necessary to nurture them. As noted earlier, I take the ends and purposes to include such things as the advancement of knowledge and understanding; attainment of truth; the ability to interact with and successfully manipulate the world; and so forth. They are best

[14] A large and daunting literature addresses the question of how to construe moral justifications for intellectual property. Jeremy Waldron (1988) provides an excellent historical overview.

[15] For an elaboration of these and other aspects of intellectual prop", see, for example, two recent articles (Eisenberg, 1989; Hughes, 1988).

served by an environment of openness, that is, an environment in which researchers freely share ideas, results and even primary data (with suitable caveats and limitations); where scientific researchers engage in active evaluation and critiquing of the work of others; where researchers may learn from others and teach them; where they are allowed to build, or piggy-back, freely upon the ideas, results, efforts and accomplishments of others. Such conditions of open exchange offer the greatest hope for successful cumulative efforts and rapid progress and have been expressed, or embodied, in the design of many of the dominant institutions in which cutting-edge research is housed and discussed – most notably, the university, professional organizations, research conferences, and open research publications.[16]

By contrast, at the heart of the institution of intellectual property, in general, and patents, in particular, is a fundamentally different idea. The insight here is that monopolistic control over intellectual property gives a head start to patent owners, thereby encouraging both intellectual effort and investment. This head start is important, if not essential, for the commercial viability of investment in innovative technologies. In the realm of research, however, such control is neither universally needed nor is beneficial for the attainment of values and ends. Progress in research is facilitated not by allowing researchers to limit and control access but by the possibility of free and ready access to the results of others, and the ability to build, without obstacle, upon them. Even as we hail the pursuits of commercial activity, and accept patents as an essential vehicle for promoting it, we can question the efficacy of patents in research. The key difference, the reason for the mismatch, is the divergence of their respective purposes, ends and values. The test thus implies caution against norms and practices being welcomed into the realm of research that were developed in the service of commercial viability, for these norms and practices are unlikely to promote the distinctive ends of the research realm.

By now I am sure many readers will be bursting with a mixture of cynicism and doubt. Even those who are not adherents of so-called postmodern or social constructivist depictions of scientific research and its ends might characterize my account as an impossible idealization, a roseate caricature of the real state of scientific and engineering research. A nuanced and historically detailed account would yield a different picture, shaped by the following observations:

1. Scientific researchers, as a matter of fact, are not nearly as generous with their results and know-how as I have suggested. This is due in part to the intense competition among scientists and laboratories, which is driven by the ambition (some may say even egomania) of scientists. But it is also due in part

[16] See Clarisa Long (1999) for an excellent and balanced discussion of the efficacy of intellectual property norms in a research context, problems as well as promise.

to what Robert Merges has described as a "creeping propertization" of scientific research. Contrary to traditional views, researchers and the institutions that house them are eager to jump onto the property bandwagon and embrace an intellectual property framework for the products of their research. Perhaps because they perceive in it the possibility of personal advantage – material wealth, control over further research, professional advancement and so forth – they are increasingly seeking property rights in fundamental ideas and discoveries.[17] This notion that intellectual property rights might serve the personal or "home team" interests of scientific researchers might well have featured as an important consideration in my discussion above of the allure of intellectual property.

2. Again, as a matter of fact, patent authorities seem to show an increasing tolerance for patent applications that by conservative standards might have been judged to contain unpatentable content (too distant from application, containing fundamental ideas, etc.). Controversial areas in which such patents have emerged include the biomedical sciences and "the patenting of life," and in computer science and engineering, the algorithms. This also demonstrates that the sharp line I draw between commercial efforts and research, even if it once did exist, is fast fading.
3. Although some research – especially "basic" research – is conducted in educational institutions and patronized by government (that is, public) funds, a great deal of research is – and has been for many decades – conducted for profit corporations. Although some may even argue that public funding does not necessarily imply a duty to forgo property-like control over their work, few have expressed doubt that research conducted in corporate, industrial settings is proprietary.[18] John Ziman, who applies the label of "academic science" and "industrial science" to identify not only two distinct funding sources but two distinct cultures, notes that, "In recent years, however, these two cultures have begun to merge. This is a complex, pervasive, irreversible process, driven by forces that are not yet well understood." (1998, p. 1814) If true, this observation would mean that there is, at best, only a fuzzy line between commercial, proprietary research and open, free academic research. This suggestion should, at least, serve as a caution against sweeping idealizations concerning the ends of scientific research and the motives of its researchers.

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[17] Merges (1996) offers both historical and legal insight into this trend toward reliance on property for establishing claims over scientific results. Although Merges' discussion is clearly inspired by the model of research found most commonly in the biological and biomedical areas, it has implications for all science. Also see work by Rebecca Eisenberg (11989) which warns of potential harm to research resulting from excessive propertization.

[18] Merges discusses this claim in his article (1996).

Each of these three observations conveys important insights about contemporary institutions and practices of scientific research and deserves a more extensive and detailed response than I shall provide here. I do not find them to be fatal, however, with respect to the core claims of this paper though a full rebuttal (not possible here) would require at least that I would need to qualify the scope of my conclusions, to recognize fuzzy borders, and admit variation at the level of individual cases. Because a complete discussion would take me too far from the main course of the paper, I offer some stopgap responses; I hope they satisfy most readers, at least for the moment.

As a start, I would deflect at least some of the force of the observations by reiterating that my picture of research practices and institutions and of ends, purposes and values is sketched in broad brushstrokes only; it is intended to map compelling trends in general terms only and cannot, at the same time, account for the full range of variation within the realm of actual practice. Furthermore, it will not accurately reflect the substantial body of research that is conducted squarely within the corporate, for-profit sector (Ziman's "industrial science").

Second, I note that my purpose here is normative. When I declare the values and ends toward which scientific activity is directed, I describe not necessarily that which actually motivates every individual scientist and research institution, but the values and ends to which the endeavor of scientific research, and the surrounding society, publicly subscribe. I aim to identify those values and ends held out in the public eye as ideals both by participants in scientific research and by surrounding societies that sponsor it. It is in relation to these ends that I evaluate norms, conventions and institutions governing research – both those entrenched and those proposed to replace them. That some (or even a majority) of scientific researchers embrace property rights because they see them as an effective vehicle for transforming ideas and other intellectual accomplishments into material wealth, or personal advantage, does not in itself lend moral weight to an observed trend, or current, in the direction of increased propertization.[19] (Just because it is so does not imply it ought to be so.) To the detractor who points out that certain conventions would be preferred by scientific researchers and scientific research institutions, because they serve to advance parochial interests by promising personal and institutional wealth or power, I would argue that such conventions are simply not morally compelling. My prescriptions are not proffered as prudential advice for any particular interest group within scientific research, but as suggestions of general policy for a society intent on furthering general social welfare through scientific and engineering research, and for members of the

[19] Merges also discusses the regrettable scenario where even researchers who are in principle opposed to staking private ownership over basic scientific results may find themselves compelled to do so or be driven out of the business of research altogether, if others around them "defect" to a property framework in significant numbers (1996).

scientific community (individuals and institutions) who remain eager to ground their norms and conventions in morally compelling terms.

We now return to consider the two points of difference between the conventions of priority and the conditions of patents, and to evaluate how these differences bear on values and ends.

Concerning Difference I: In this point we observed that built into the patent system is protection against the threat of independent co-discovery. Conventions governing priority, by contrast, regularly admit shared priority. Priority is readily shared when discovery is simultaneous, but may be shared even when one discovery indisputably precedes the other. This may occur under one, or more, of the following conditions: the second discovery was independently achieved; the time difference was not excessively large; the method of achieving the result was significantly different; and the second researcher was not negligently unaware of the first's result. In the discovery of hyperbolic geometry; for example, Bolyai is acknowledged as one of the discoverers even though there is no doubt that his work was published three years after Lobachevsky's. Should we consider this as evidence that conventions for the attribution of priority are vague, misguided, internally inconsistent, etc.? I think not. In the commercial realm it is arguably essential for patent holders to know that there will be a period in which no others will compete in the manufacture of their inventions. Without such assurance, investment – especially large investment – might always seem excessively risky. In research the need to identify a unique discoverer may at times be outweighed by the advantage of linking more than one researcher with a result. The possibility of shared priority serves important ends: it highlights, or helps reify, successful lines of attack, effective or powerful techniques, and promising schools of thought. There need not be only one and indeed, if there are more, then it serves scientific research if all are kept in clear sight.

Another reason for embracing shared priority is that priority serves to highlight promising members of a community, constituting an important currency of community approval. Priority thereby helps forge careers in research. Whereas the monopolistic aspect of patents helps attract investment in a particular invention, priority contributes toward the needs of a research community to identify its ablest members – those who produce consistent, excellent, reliable work, those who will ultimately form the corps of peers whose judgment is trusted and who undertake the responsibilities of mentorship, review and so forth. These trusted members also serve as leaders not only in defining the directions of research that should be patronized but in carrying out, or managing, the work that happens to be patronized. The demand for uniqueness that is characteristic of a system of patents – and works so well to realize its ends – could, therefore, work against the purposes served by the looser requirements of conventions of priority.

Concerning Difference II: It may be difficult to appreciate how the second point of difference – the absence of an authorized body, and an explicit, formalized procedure for recognizing priority – reflects anything but a weakness in conventions

of priority. Is it not due to the vagueness in the conventions guiding priority that we are led to such strange results as the confusion over priority for the discovery of hyperbolic geometry? Skeptical outsiders may want to set "straight' the misguided mathematicians who continue to maintain that Gauss be recognized as a co-discoverer, based only on his flimsy claim that the work "occupied his mind" for several decades. Explicit guidelines, or an authorized body, could draw clear and unambiguous lines between acceptable and unacceptable grounds for priority claims.

The test guides us to frame a question along the following lines: does the explicit, authority-driven procedure of patent allocation serve the same ends of research, and as well, in the electronic age, as the entrenched norms have served until now? The answer, I think, is no. What appears as ambiguity and unclarity in conventions for attributing priority is, rather, a sensitivity to contextual factors that is well-suited to promoting ends for which these conventions were designed. In the case of hyperbolic geometry, we observe an exceptional tolerance to the unusual priority claim made by Gauss. Mathematicians who show forbearance for Gauss' priority claim do so not because they are confused by a fuzzy convention but, rather, because they are swayed by Gauss' extraordinary stature within the mathematical community. These mathematicians could argue that allowing for convention to be sensitive to such considerations is neither irrational nor grossly unjust (as we might say of a patent system that allowed for such favoritism). In any given time communities of researchers recognize a few of their members as enlightened leaders, who usually have earned their stature on the basis of exceptional work. The community accords these leaders tremendous trust – trust that is not easily, blindly or irrationally accorded. The rewards the community expects to reap are what all of us seek from enlightened leadership. We value it not because the paths it takes are already evident to those who follow, but because it takes paths and offers the possibility of insights that are frequently not evident to others. For many mathematicians Gauss' well-earned stature means that even his mere meditations promise fruitful avenues of study.

We must conclude, then, that a patent-like system does not pass the test. An explicit authority-driven procedure, not sensitive to such needs, offers decisiveness at the cost of inspiration and serves different ends to the ones that have guided entrenched norms. This is not to say that some ends might not be served by a patent-like system of priority for intellectual work, but they are not likely to be the same ends that norms, conventions and institutions governing practices in research currently serve. The test indicates that researchers should not rush to these new property-like norms simply because they are already embedded in information technology, or because they hold the allure of personal profit.[20]

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[20] Contextual factors are an important backdrop for this claim. It assumes, for example, that some form of public support is available for scientific research. Robert Merges describes a compelling scenario in which strong property claims to results have become the norm and researchers may be forced to assert strong property rights or be driven out of research altogether (1996).

Other Approaches

Where does this conclusion leave us? It is all very well to reject the more rigorous patent-like system for the idealistic reason that values and purpose of research will not be as well served by it as they are by the current norms, but this does not make the puzzle go away. Pressure to adjust priority norms for the electronic medium is still with us; what alternatives do we have?

A direct approach to the puzzle is to hone in on the scope of a search for prior work. At one conservative extreme, we may propose simply to "bite the bullet," making no change whatsoever in the literal framing of the norms and conventions governing priority. Naturally, although the stated requirements remain unchanged, their meaning has changed considerably: researchers would still be responsible for searching for prior work in all publications, but now the search ranges over a great deal more than before, including not only traditional works published electronically, but also all the new forms of publication such as gray literature, wildcat publications, and every last arcane homepage upon which someone may have posted materials. This position is problematic.

Although it adheres to the letter of the old convention, it involves a radical semantic shift. Because the term "publication" now spans a far wider domain, a hapless priority seeker would face the daunting challenge of searching a virtually endless domain of publication. To make matters worse, according to the study by Lawrence and Giles cited earlier, the technology for conducting these searches is still relatively unthorough. In addition to estimating the sheer size of the Web, Lawrence and Giles studied how effectively the major search engines indexed it.[21] Of the search engines they studied, they found that none, taken singly, indexed more than one-third of the Web, and, in combination, all covered approximately two-thirds. This means that even the most vigorously pursued search is unlikely to yield all relevant material.

Aside from these practical obstacles critics could argue that the requirement is unfair: in pre-electronic days no-one expected priority seekers to hunt for results in desk drawers, privately owned filing cabinets, backs of envelopes and past conversations. Similarly we should not now expect them to search, literally, *all* electronic publications. Even if improved and refined search engines could assure better results (whatever that may amount to), we may still worry about the quality and reliability of the publications in the gray literatures and personal home pages. Should they count? Because these considerations indicate that conventions based on a bite-the-bullet approach would in all likelihood hamper progress, the test would weigh against its adoption.

[21] When users run a search using a search engine, the search will yield results only from the sites indexed by that search engine.

An attenuated or less extreme version of the approach may seek to define a more restricted scope of search; a revised convention in this spirit would demand a search for prior work only within a circumscribed range of pre-approved publications. Although more reasonable than a bite-the-bullet version and less disturbing of fundamental values than a framework of intellectual property, this promising alternative would stand or fall on the basis of satisfactory answers to questions like what the subset would be, how it would be defined, and who would be responsible for defining it. Philosophical analysis alone is unlikely to offer much more than general guidelines, for example, that the definition ought not be arbitrarily drawn, and that it should not be so limiting or conservative that it stifles the exciting, novel creations of electronic publication. It might also point out that care should be taken in identifying who has discretion over setting boundaries: a professional organization, a committee, an individual. Finally, any proposal must pass the test. That is to say, careful consideration must be given to the consequences on values and purposes of research.

Beyond these quite general directives, any concrete interpretation – I believe – requires a degree of familiarity with actual practice that most likely inheres with participants themselves in the various areas of research, including researchers and high-level administrators. Specific answers will be forged by those with the deepest understanding of the real meaning of various options, familiar with practices and conventions, and best able to anticipate how particular conventions for delineating the scope of the search would affect the attainment of values. To the extent that this paper can contribute to the important process of revising and adapting to the demands of the new situations and challenges of electronic publication, communication and collaboration, it is to warn against rushing to new norms and conventions merely because they appear most similar to the old, nor because they happen to come along with the technology (though developed to serve values that are not their own). Nor should they pursue parochial interests, however alluring these prospects might be. They should approach important questions about revising norms, practices, institutions and conventions with an eye to underlying values, ends and purposes. Where the electronic environment demands new norms, or new interpretations of traditional norms, these values, ends and purposes should be their paramount consideration.

Conclusion

A great deal of social activity is governed by norms and conventions, some trivial, some grave, some regulated by law, some not: from stopping at red, to driving on the right (or left), to addressing strangers in particular ways, to "ladies first", to not talking in theatres. Many conventions are so entrenched and so effective that we

barely are conscious of them, we rarely question their hold over us, and in some cases we forget that we adhere to them not as ends in themselves but as means of promoting other valued ends. It is frequently only as a result of change or disruption that we "notice" a convention and understand its operation. Disruption may force us to dissect a convention and gain insight into its functioning.

This paper has drawn attention to conventions of scientific research publication, suggesting that the shift toward the electronic medium constitutes one of these convention-disrupting changes. The puzzle of priority is an example of conventions disrupted and the need to put new ones in their stead. Calling attention to the need is an important first step, but as important is delineating a reasonable process for determining the shape of new conventions. In what I call "the test," I attempt to do so by outlining a systematic approach to evaluating proposed new norms. The test demands that we dissect entrenched conventions to discover the ends, purposes and values they serve. When proposing conventions to replace entrenched ones, we must evaluate proposed conventions according to their likelihood of achieving these same ends, purposes and values. When, for example, traffic lights are replaced by traffic circles, we establish new rules by making sure that the ends are served as well if not better; when gender roles are re-examined for ideological reasons, we try to re-engineer conventions of who goes first that "work" to serve other ends and so forth. We may happily discover that the new conventions serve valued ends even better than the old, but we should be wary of results showing that proposed conventions are inefficient, arbitrary or biased. In the case of electronic publication I have argued that conventions will maintain moral force only if they continue to serve, and serve justly, the fundamental ends of scientific and technical research.

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