PART I

Perspectives
1 Establishing the Public Legitimacy and Value of Scientific Knowledge in an Information Ether

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We now live, so we are told, in a knowledge economy, one in which the produce of our intellect is replacing the produce of the land and our own hands as the engine of prosperity. Although hardly a new idea, its contemporary statement is unique to the extent that it associates the characteristics and dynamics of knowledge explicitly with information technologies and the communication capabilities they support. The instrumental association between intellectual work, technological infrastructure, and public welfare inevitably raises questions about what knowledge is, how it interacts in human affairs, and how technical change might intervene in this process.

In this regard, it is impossible to avoid discussion of the public role of academic professions, whose primary function is not merely to produce and disseminate knowledge but also to evaluate its quality and significance and to act as the organizers and caretakers of various fields of knowledge and expertise. Historically, this function has been regarded to be in the public interest, at least for the most part. However, the contemporary knowledge economy-society construct assigns significantly new public responsibilities to the academy that complicate its status as a producer and arbiter of knowledge.
My aim here is to contemplate evolution in the function specifically of scientific knowledge in public life and to speculate on the future of the academic scientist in the role of public intellectual. This aim will be pursued in the context of evolution in the media through which scientists communicate with each other and with the public. For the sake of clarity, I will adopt a somewhat Popperian description of science as the practice of observing natural, human, and social phenomena systematically through the logically rigorous exploration of theories and hypotheses (Popper 1958). For my purposes here, a scientist is someone who is engaged professionally in such pursuits, and scientific knowledge is the result.

Science never unfolds so neatly, of course. As Watson (1968) observed, even if science is objective, scientists most certainly are not, or at least not all of the time. Moreover, not all knowledge comes from science, and not all academics are scientists. Academics produce knowledge in many forms that do not involve science as defined above. (These academics face their own challenges in the public sphere, but I will leave this discussion to others.) Also, the academy has no monopoly on intellectuals, who can emerge from any walk of life. We would do well to recall that Thomas Paine, whose revolutionary political tract *The Rights of Man* (1776) not only fuelled at least two major political revolutions but was also the very first international blockbuster bestseller, was by trade a tobacconist, tax inspector, and erstwhile inventor (Foner 1976).

In the contemporary knowledge economy framework, however, science is explicitly defined also as a wellspring of “intellectual raw materials,” which, it is presumed, will enhance public welfare in the form of new technology. This presumption casts science in a particularly narrow, instrumental role. Pragmatist philosophers have always argued that the evidence of whether genuinely new knowledge has been produced is that something genuinely different can be *done* as a result—in principle, a perfectly valid test (Pierce 1878; Dewey and Bentley 1949). The problems begin when questions about the utility of knowledge encounter the social dynamics of science as an institution and a political environment in which the legitimacy or reliability of scientific knowledge and evidence can, and often is, called publicly into question.

The contemporary imperative that science be useful generates a much broader group of direct stakeholders, each of whom may assess the value of science on the basis of different objectives, be they technological, political,
or social. When this expanded group of stakeholders is coupled with the now seemingly limitless technologically mediated possibilities for public engagement and with an expanding array of knowledge sources, it becomes a serious question whether academics still enjoy any edge at all in the public forum.

In this essay, I will pursue three related arguments. The first concerns the new media themselves. I will argue that it is now impossible to consider various forms of media in isolation, as standing apart from one another. The current media environment is more appropriately viewed as the product of a consistent historical pattern of development, in which each new feature is but punctuation in a much longer trajectory that always has been driven by many of the same forces. The second argument concerns the role of science and scientists in public life. Here my basic premise is that an essential tension exists between science and scientists that can be very difficult to reconcile in the public sphere. As I will argue, by nature, and somewhat counterintuitively, science fits rather uncomfortably into the space typically occupied by public intellectuals, and always has, even though the scientist may at times fare quite well in this role. My third argument, which pertains specifically to the context created by new media, is that issues surrounding the public role of scientific knowledge are much deeper than the simple question of how science is projected onto the public stage or communicated to the public. I will propose that in order to understand how scientists may fare as public intellectuals from this point forward, it is important to comprehend how media, old or new, are incorporated into the practice of science.

The Information Ether and the Public Function of Science

It is often claimed that the epistemological landscape of public life—what counts as knowledge and who counts as a knowledge producer or "knower"—is being transformed by the vastly more extensive capabilities of interactive media to engage individuals in various forms of discourse and to provide instant access to vastly greater resources of information and knowledge. I will introduce arguments that tend to cast doubt on this hypothesis, but I am more concerned to address the obvious corollary, namely, that the credibility of science and scientists in the public forum may
somehow be compromised by the so-called democratization of knowledge through electronic media.

For one thing, this notion overlooks the fact that scientists, too, are intensively engaged with these media, as is the practice of science. Indeed, the core technologies of contemporary public media—the Internet and the World Wide Web—began life as vehicles for collaboration between scientists and technologists. That they are no longer exclusive to science is, however, neither an indication that the public has become more scientific nor that science has become more public. My other objection, however, is that it is simply too convenient to lay this challenge at the feet of technology. Instead, I will lay it at the feet of science itself, where it has always belonged.

Eco (2005) noted perceptively that our age is characterized by a preference for technology over science—that technology has come to fill a deep-seated human need for magic, which, now as always, succeeds by short-circuiting cause and effect. As a method, magic is the antithesis of science, but I would propose that Eco’s observation is valid for scientists too, because so much of science is and always has been dependent upon technology (Petroski 2011). Moreover, as Borgmann (1984) observed, it is often those who are most engaged with technology who fail most completely to comprehend its role in their lives. Thus, we must avoid disassociating the fundamental dynamics of science as a community from the debate over the origins and evolution of electronic media.

In technological terms at least, the new media provide enormous potential for scientists to gain greater and more direct access to the public and also for the public to gain greater access to scientific knowledge. A marriage made in heaven? Perhaps. But a marriage made in Las Vegas to be sure, which, fittingly, is where most new electronic gadgets are introduced to the world. We should be under no illusions that the commercial goals of the new media are symmetrical with the loftier goals of a knowledge society or that the political economy of media has any necessary association with that of science or of public affairs.

The developers of new media have imposed a constant stream of innovation in the way that content can be generated, stored, and distributed by and among an ever more heterogeneous population of producers, intermediaries, and consumers. Today, we are catching a glimpse of the outcome: a media environment that is utterly pervasive but also indiscriminate—one whose internal logic is to become insidious in every aspect of human life.
The “cloud” has become the newest metaphor for this milieu, but in practice the term refers mainly to a technical architecture for the storage and distribution of data. To the user, it may signify subliminally the increasingly ephemeral nature of the network environment. But to the technologist, it describes only an engineering problem: how to make the links between any type or source of data and any type or source of medium completely transparent. Its motives are commercial and strategic, not visionary or philosophical, and certainly not scientific.

For these reasons, I prefer to describe the social functionality of this system in terms of an “ether,” which in my meaning incorporates both the archaic scientific concept of an invisible, featureless medium filling the space between particles of matter and the more poetic concept of something existing above and beyond the clouds—of something ethereal. The restoration of these concepts seems a rhetorically apt way to reconceptualize the current media environment as it relates to contemporary social and economic issues.

Since the 1930s, the political economy of media has been conceptualized mainly around infrastructure, with the problems defined in terms of how to create more “democratic,” or at least more equitable, access (see Mansell 2010; Ruggles 2005; Garnham 2000; Smythe 1981). More recent events, however, whether in the nature of the Arab Spring or the London riots, have forcefully demonstrated that the most pressing social issues have moved well beyond access as such. Rather, they now concern potentialities—opportunities to identify, configure, and deploy various “particles” of both data and media that hang in this ubiquitous yet amorphous ether such that the configuration can yield some kind of envisioned result. This is social action, not technological application.

Basically, I am referring here to the dynamic organization of networks, not just around the fixed capabilities of a technological medium but in the service of some human purpose, whether frivolous or profound. The problem with dynamic systems is that they teeter unpredictably between conditions of stability and chaos (Bak and Paczuski 1995; Agar 2004; Perrow 1986). The traditional industrial solution to this problem in the various media sectors has been to impose an artificial stability through a mixture of market concentration, proprietary technical platforms, and sympathetic public regulation (Melody 1987; Mansell 1993; Ballon and Hawkins 2009). In truth, however, this strategy is itself unstable, and we can track the
outcomes of significant perturbations in a succession of iconic, archetypal, and to a large extent conflicting corporate cultures—from IBM “machine” culture, to Microsoft “control” culture, to Google “search” culture, to YouTube “producer” culture, and now perhaps to Facebook “clan” culture.

The point is that all of these perturbations are the product of the same forces. In the ether, one culture does not disappear with the appearance of another, and each tends to recede to the characteristics of previous cultures—for example, all tend eventually toward monopolization. Moreover, these cultures have been primarily defined not by changes in technology but by innovations in how technology, content, and the consumer are configured (Napoli 2010; Hawkins and Vickery 2008). A glimpse into the ultimate logic of network building is provided by user-generated content. Consumers produce content, at their own expense, that is distributed by a commercial intermediary for consumption by other consumers. Commercial value is created for the intermediary—YouTube or Facebook, for example—but not for the producer, who works for free.

Such developments continue an historical process by which more and more forms of human communication become commodities, dependent upon a commercial intermediary in order to be realized in their social context (Smythe 1981; Jhally and Livant 1986; Cohen 2008). For, indeed, although much of the content in the ether is “free,” the ether itself is not. The ether is a commercial environment within a gigantic global machine that has been building for nearly two centuries and that embodies the most massive single financial investment in any industry in human history. To be sure, it is also a public space, but only in the way that a shopping mall or sports stadium is a public space.

A critical question for what follows is whether what we might call the emerging “ethos-of-the-ether”—its evolving social conventions and practices—bodes well or ill for the role of science and scientists in the public arena. The roots of the problem were illustrated well by New York Times columnist Randall Stross (2008) in his exploration of the origins of Google as a company. Unlike IBM or Microsoft, which were built up, respectively, by engineers and hackers, Google was founded by a couple of academic scientists whose motivations were only partly commercial or even technological. Rather, they were normative, aimed at realizing a presumed imperative to exploit the full capabilities of digital technology in order to make all information available to everybody.
This is very much an activist’s imperative, and it forms a kind of post-Cartesian dictum: *If something is possible technically, then it must be.* However, it may also represent a new form of tyranny—one that imposes information on the public rather than restricting it. With respect to any putative public role for scientists in creating, organizing, and validating systems of knowledge, this is no idle point. Information theorists have long noted that no linear relationship exists between information (data) and knowledge. Not only does an increase in information not yield a corresponding increase in knowledge, it may, in some situations, actually function to reduce knowledge (see Bialek and Tishby 1999). A much used metaphor is the narrative of infinite length. As a narrative gets longer by introducing more information, it accumulates meaning, until it arrives at a critical length, at which point adding yet more information will cause the entire narrative to deform, until at some point all of its meaning is lost. The implication would seem to be that if all of the information in the world were to be imposed upon us, nobody would know anything at all. Epistemological apocalypse!

All of this is, of course, highly theoretical. In practice, the information ether does not relate every particle to every other particle such that one potentially infinite and meaningless narrative would ever emerge—this is precisely why the ether metaphor is apt. The point that information theorists are trying to make is that knowledge emerges not from information as such but from the boundaries that are placed around it. Boundaries change as knowledge increases, and these changes constitute evidence of learning.

Traditionally, many of these boundaries have been defined by academic science in ways that have given science a significant role in public life, if not always an overtly activist role. The significance of the ether is not merely that information becomes available to more people. Rather, it is that the ether itself becomes not only the primary public interface with information but also the cauldron within which information producers and consumers can construct a potentially infinite number of boundaries that might reinforce or undermine the boundaries defined by academic science. Although in many respects the ether is an outcome of organized science, it functions as a naïve analogue of the social arena in which humans constructed knowledge before science and from which science emerged. Most of the questions about the future public role of science revolve around much deeper questions concerning the continuance of this cycle.
Science and Scientists in the Public Sphere

Currently it is fashionable to promote linking science more closely with political and economic life. For example, the doctrine of “evidence-based policy” would seem to compel decision makers to act in accordance with the findings of rigorous, systematic, and independent investigation. This can appear very attractive to scientists, but also dangerous (Pawson 2002). The obvious corollary is that if there is no evidence, or if the evidence is incomplete or ambiguous, which in science it often is, then an easy excuse is generated for taking no action at all. Or, worse, the evidence provider becomes a convenient scapegoat should the action fail.

In practice, evidence-based policy may be just another example of how science can be neutralized by co-opting it into politics (Jasanoff 1995; Leiss and Chociolko 1994; Majone 1989; Salter 1988). In the present context, however, it well illustrates how injecting science into the public debate has always presented special problems, owing to the many intrinsic limitations imposed by the conventions of science itself. Ironically, these may weaken the power of science to affect public opinion, often for the simple reason that the conventions of academic science are unfamiliar outside the academy and often do not conform to what non-scientists perceive scientists to be and to be doing.

Science as a “Problem” in Public Life

Regrettably, the only lasting outcome of the Copenhagen Earth Summit may be the debacle created by the infamous Earth Summit e-mails. These appeared to show that environmental scientists were being picky about the findings they chose to publicize in order to support only one side of the global warming debate. Political professionals skilfully transformed the disclosure of these messages into a general impression that all climate science was suspect or bogus. However, regardless of the motives in this particular case, it is actually quite typical for scientists to be strategic about what is and is not disclosed and where and when. In preparing critical editions of the lab journals of seminal figures in twentieth-century physics, Holton (1986)—himself an eminent physicist—concluded that, in choosing which findings to publish first, scientists normally privilege those that tend to support the theories in which they have the greatest confidence and to suppress those that do not. Holton suggests that it could not be other—that unlike
most of us, who either choose to “believe” or “disbelieve,” scientists must often “suspend disbelief,” which is not a deception but a challenge to others to come up with better explanations.

The point is, however, that these climate scientists were not undone by the ether, which they could exploit as adeptly or ineptly as anyone, but by the persistent reluctance of science as a community of practice to deal with criticism and controversy that come from outside science and by a chronic failure to tell the public exactly what science is and how it works (Feyerabend 1993; Smolin 2007; Latour and Woolgar 1979). If, indeed, the public face of science is being transformed by the new media, the cause lies not in the imposition of new media on science but in the internal relationship between the scientific community and these media.

But if the practices of science itself can be twisted so easily in the ether in order to discredit it in the public eye, how can scientists possibly construct or maintain any credibility in the same space? One obvious strategy is to humanize science by associating it publicly with the personalities of selected scientists, thereby disassociating it from the practice of science itself. This may or may not lead scientists into the role of public intellectuals, but it does create celebrities, who, by definition, are famous simply for being famous. Arguably, however, neither status reflects the actual nature of science as a profession or practice. Smolin (2007) observes that the most characteristic thing about doing science is its absolute tedium—the Eureka! moments coming rarely, if ever, and always in the wake of what in most professions would be considered a mind-numbingly impossible number of mistakes, blind alleys, and failed attempts. Whether it be Dr. Frankenstein, Louis Pasteur, or Sheldon Cooper, the popular image of the scientist through history has been far from the realities of science as a vocation and as a career.

Moreover, in public affairs, it is doubtful whether scientific celebrity enhances the stature of science as knowledge. It certainly has not provoked any new fervour to engage with science at a professional level. Enrolment in science degree programs has fallen dramatically in most OECD countries since the 1970s. Not that long ago, the Los Angeles Times reported that some California schools were displacing science altogether in order to make more room for instruction aimed at raising basic math scores (Watanabe 2011).

Nevertheless, the celebrity scientist phenomenon has flourished, and it provides a useful illustration of the major questions inherent in any process whereby scientists become established in the role of public intellectual. Do
they assume these roles by dint of their scientific credibility? Or by extra-scientific criteria, where their credibility claim is not as clear-cut? In today’s media-rich and celebrity-obsessed culture, what is known appears to count for much less than who appears to know it and who, or what, brings it to public attention.

The Role of Public Intellectual as a “Problem” for the Academic Scientist

In one banal sense, all professional academics are “intellectuals” by definition in that their job description is to engage in intellectual pursuits like teaching, research, and writing. Most are also “public” intellectuals in the sense that in most countries (the United States being the only significant exception) they are also typically public employees. In almost all cases, much of the knowledge they produce is supported by public finance and is nominally public property. Nevertheless, few academic scientists ever become public intellectuals in the sense that they acquire an identifiable personal voice that they then exercise in a public forum in order to advocate certain positions, including some that may go beyond the specific content of their scientific work, with the intent of influencing events and public opinion.

When Albert Einstein, still the most iconic of all scientific personalities, arrived in the United States just before World War II, he was already a public figure because of the notoriety of his theory of relativity, which, although understood scientifically by only a few dozen academic physicists worldwide, had already become the first “celebrity” theory of twentieth-century science. Everybody knew who Einstein was, even though almost nobody understood anything about what he had done. However, the Einstein of the 1920s and 1930s was not yet a public intellectual, at least not in the sense that he used his scientific reputation to engage and influence public opinion in any great cause. Arguably, this role came about in the first instance not because of his radical thinking about the universe, although ultimately that sustained him in this role, but because of his warnings about Nazism and the possibility of weapons of mass destruction. The former may have won him a Nobel Prize and the status of public figure, but, arguably, the latter won him the front page of the New York Times and the status of public intellectual. Einstein also knew the dangers of taking this step. That he understood the physics of nuclear weapons was beyond question, but he
could claim no equivalent understanding of international affairs. As he later famously remarked, “politics is more difficult than physics.”

The waters are made murkier still in that, within the paradigm of a knowledge economy, economists and politicians argue that science and scientists are now also agents of economic growth, or anyway that they ought to be. This adds a completely new dimension to the public role of science, one that seriously complicates the question of how to establish the quality, objectivity, and significance of scientific knowledge and how to organize and govern science as an institution (Ziman 1991; Feller 1990; Mowery et al. 2004; Sampat et al. 2003).

The traditional equivalent of both “property” and “profit” in science has always rested in the reputation scientists achieve by contributing insights that can be uniquely attributed to them as individuals (Dasgupta and David 1994). Replacing reputational with economic criteria, even indirectly, has many implications and creates additional moral hazards for the conduct of science and the integrity of the peer-review process upon which its credibility depends. For example, stories have emerged in the media about the apparently common practice whereby pharmaceutical companies hire ghost writers to prepare papers for publication in reputable scientific journals such that the papers spin their findings in commercially useful directions. Medical researchers and practitioners are then induced to lend their names as authors in order to obfuscate the corporate origin of the paper and its commercial agenda.

However, corruption is hardly unique to commercial motives. Creating and enhancing scientific reputations is every bit as open to abuse (see Smith 2006). Anyone who takes on the eccentric task of trying to assess the demography or impact of science by looking at publication patterns is immediately bedevilled by self-citation, citation pooling, editorial-board stacking, and countless other reputation-enhancing devices that are as old as science itself. Again, however, such conceits are not products of the publication media as such, even though they may be facilitated by them.

More critically, there are serious questions as to how much credibility or influence even an uncompromised scientific reputation actually has in a public sphere dominated by the forced chatter of 24-hour news or the opinion mills of the blogosphere. It would be fairly safe to say that very few individuals owning a smart phone would know that contained within these gadgets are theoretical advances in physics that have won at least two
Nobel prizes. The actual prize winners are largely unknown to the public, and, for the most part, they have derived very modest commercial proceeds from these ideas, if any at all. Should they become concerned enough about an issue of the day that they feel compelled to use their Laureate status to attract an audience for their views, the question is whether their reputation would be built upon public comprehension that they have won the ultimate scientific accolade or upon some distant association with a rather mundane electronic gadget that can be mastered easily by any six-year-old. More to the point, if competing for public attention on any issue, how would these scientists fare against a Bill Gates, a Steve Jobs, or a Mark Zuckerberg, who would be associated much more personally with the same gadgetry by the same public constituency?

Today, it is very easy to forget that, even up to the 1950s, the prospect that science might have economic value was quite a radical new idea. Although Bernal (1939) first proposed this idea in the 1930s, it took another couple of decades to catch on. Even Schumpeter (1934, 1942), now revered (mostly wrongly) as the prophet of the modern high-tech economy, was initially dismissive of the idea that science or technology had any intrinsic economic value, a view he never really abandoned. The organization of experimentally based science in the research university as we know it today, along with links to industry, came about only in the mid-nineteenth century (Freeman and Soete 1997). It was not until the post–World War II period of economic reconstruction and conversion that massive and completely unprecedented public investments were made in basic science, most with the intention of weaving closer and more intricate linkages between science and the industrial fabric (Bush 1945; Ruttan 2006).

Interestingly, however, despite increasing political pressures to maximize the economic return-on-investment from the university system, independent surveys of the interactions between academic and non-academic communities, whether in commercial or non-commercial contexts, suggest that such interactions remain confined to a relatively small, stable, and quite specialized segment of the faculty, with the notable exception of those in medicine and, to a lesser extent, information technology (Cohen, Nelson, and Walsh 2002; Fini, Lacetera, and Shane 2010). Again with the main exception of medical science (which is vertically integrated with the health industries), they also tend to indicate that academic institutions rank among the least significant sources of knowledge that industrialists consider to be important.
for creating new products and services (Cosh and Hughes 2010). The situation is, of course, complex, and such findings require much contextualization and explanation. All the same, they do not suggest the emergence of any particularly noteworthy “axis” of coordination and cooperation between academic science and industry, especially not one that is attracting academic scientists in significant new numbers.

The reasons are not difficult to fathom. For the individual scientist, public engagement carries many dangers and pitfalls of a purely professional nature. Undoubtedly, they conflict with long-established and institutionally powerful ideals about the purpose and practice of science. Moreover, this inertia is sustained by long-established processes of academic career building, which in the modern academy typically do not award much merit for public engagement. Quite the opposite: severe penalties can attend an academic who dares stray too far from the Ivory Tower, or for too long.

Once firmly established, however, some scholars succeed in gaining additional stature from public exposure. For example, Stephen Hawking’s admirable career as the public face of cosmology—as challenging and baffling a branch of science as ever entertained the human mind—does not appear to have harmed his credibility as a theoretical physicist. But then Hawking is remarkably careful not to stray very far from talking about science, even when speculating about sensationalist topics like alien invasions or the existence of God. Probably wisely, he leaves to others any discussion of the possible implications of cosmology for the public interest. Then again, Paul Krugman (1994) once noted that even though Hawking might be the most famous physicist since Einstein, he was not yet a serious contender for a Nobel Prize.² That, following a decades-long parallel career as a high-profile journalist, Krugman himself would eventually win the prize entails plenty of irony, but it does not seem to have affected his own credibility as an academic economist. Will the same happy fate await a Richard Dawkins or a Steve Jones? When they tire of going toe-to-toe with creationists and religionists, will anyone remember that they were also leading evolutionary biologists?

Quite clearly, factors affecting the public reputation of scientists may vary according to the characteristics of different scientific fields. They may also be affected by philosophical or ideological predilections in the populace and in the political elites. Quite clearly also, the forum in which a scientist pursues a parallel career as a public intellectual must be chosen with
utmost care, as must the parameters placed around what he or she chooses to comment upon in this role. There are many dangers for scientists who venture outside of science as such, not least that they can become separated from the science that gave them a public forum in the first place.

The Practice and Public Functions of Science in the Information Ether

My concluding argument is that the future public role of science and scientists in the information ether will be determined first and foremost not by how the ether is mobilized in the interests of science—that is, in a public information role—but by how it is incorporated into the practice of science as a career.

A former university colleague, a brilliant scientist who was once the recipient of a prestigious MacArthur “Genius” Fellowship, is fond of quipping that when you receive this award, your IQ automatically goes up 100 points, but when the award runs out, it falls mysteriously back to its original level. Like all really good quips, this one is both amusing and dreadfully close to reality, certainly in terms of what it implies for a career as an intellectual, public or otherwise. Prestigious awards, fellowships, and prizes sit at the pinnacle of a formal system of academic knowledge management that originated in the eighteenth century as a way to organize and assess scientific output in order that scientists could learn from one another and monitor the emergence of new knowledge. It still serves this function, but it has also evolved into a system for the formal evaluation and ranking of science, mainly for the purpose of allocating physical and financial resources.

In the system as it exists today, not only do the reputations of scientists and the quality of scientific institutions come under scrutiny, but increasingly even the validity or utility of scientific disciplines are ranked and compared. It is now routine for governments who fund universities to consider research in the natural, medical, engineering, and management sciences to have greater socio-economic value than history or the fine arts, even though there is no evidence that this is the case, and plenty that it is not (Fini, Lacetera, and Shane 2010; Stoneman 2010). This is reflected everywhere in the distribution of resources. In Canada, for example, less than 4 percent of the entire national research and infrastructure budget is allocated to the social sciences, arts, and humanities. The reason has nothing to do with
the social utility of these fields; it is entirely a product of the assumption that intellectual work acquires value mainly in the form of technology. The disciplines that seem to be most directly associated with this outcome are measured in this context, and the others are not.

Thus, what began as a system oriented toward the organization of learning has evolved into a system oriented mainly toward academic administration and the external assessment of the value of intellectual work. This evolution is significant because it signals a change in the reasons for making science nominally public and for how scientific reputations are built. This in turn has implications for how and why science is conducted, for the forms that scientific knowledge takes, and ultimately for its claims of value and validity in the public sphere.

This evolution was motivated by a real problem, first documented by de Solla Price (1961). Starting in the mid-1950s, public resources for science, which since the 1940s had escalated to unprecedented heights, had begun a steep decline, especially if compared to the corresponding increase in the production of PhDs. As de Solla Price predicted, these curves have never again moved in the same direction. Goldstein (1993) noted that, already by the 1990s, the likelihood that a new PhD would find a tenure track job in science had fallen from an average of about 10 in 15 in the mid-1960s to just 2 in 15 (fifteen PhD students being the average number supervised by the average US professor over a career). Likewise, the chances of being awarded significant research funding in a typical academic career had shrunk dramatically. Certainly when the increased costs of doing science are factored in, this resource decline has never been reversed.

Partly in response to these pressures, the ranking system has become extremely convoluted and complex. But it remains basically a grown-up version of a game played by children in any sandbox, namely, determining through various proxies—who works where, owns which car or lawn-mower, or plays which sport—just whose mom or dad is the biggest, the strongest, or, in this case, the smartest. In the academic sandbox, aside from awards and prizes, the smartest are identified mostly in terms of who is producing the ideas that are being incorporated into the work of other scientists, mainly as determined by the so-called impact factor of the journals in which they publish, reckoned in terms of average numbers of citations. In some fields, but to a much lesser extent, patent filings and company formation (the so-called translation factor) can also be taken into account.
The entire system is governed by an interlocking system of quality thresholds and hierarchies upon which the progress of a career in academic science is assessed. Thus, the system is more likely to regard the scientist who publishes regularly in *Science* or *Nature* to be figuratively “smarter” than the one who does not—to say nothing of the one who publishes trade books and in popular periodicals. Likewise, the professor from Harvard or Oxford must perforce be smarter than the one from Eastern Tennessee or Portsmouth.

The fallacy common to the academy and the sandbox lies in the association by proxy of variables that can be observed and counted, like publications, awards, or patents, with those that cannot, like the quality or influence of an idea or of an intellectual. Worse yet, these proxies, although highly sophisticated technically, are far more dependent on the availability of data than on robust theories as to how the variables are associated (Basberg 1987).

In terms of the public perception of science, the system is practically invisible. However, some of its implications can be very direct. DeMaria (2003) cites an example, drawn from medical science, in which the efficient dissemination of new findings can be literally a matter of life or death. He notes how new knowledge can easily bypass the most relevant clinical constituencies simply because, for career advancement reasons, its authors chose to publish in journals that have the highest prestige value rather than in those that are regularly read by specialists in the most relevant fields of application.

Arguably, there has always been a close relationship between the actual content of science (the nature of what is being explored and discovered) and the “exposure” of science, referring not so much to how scientific theories and findings are explained to and understood by the broader public but to how scientists make it known to their peers what is scientific and what is not, what are significant findings and what are not, who are leading scientists and who are not. This more than any other factor supports claims for the special credibility and objectivity of academic science. However, the practice of ranking the output of scientists on the basis of published output is actually quite recent, dating only from the 1970s. Its original purpose was to mitigate the burgeoning resource crisis by providing “objective” proxies explicitly for the purposes of making more efficient resource disbursements (Martin and Irvine 1983).
Nevertheless, a declining resource base and an increasing workforce never yields happy outcomes, and, in this case, it creates multiple moral hazards. The peer-review principle may be useful in enabling scientists to learn from each other, but it acquires a very different dynamic when more and more peers are competing for shares of a dwindling resource pool. Imagine, for example, a bidding process for a commercial contract in which each bid is evaluated by those who have submitted competing bids.

The system seems less transparently flawed in the academic context because it retains a certain intrinsic meritocratic logic and also because, despite its flaws, it appears to work at some level. Thus, it is true that not all scientists who publish in *Nature* are really top tier, but it is also true that few genuinely top-tier scientists have not published in *Nature*. In terms of public perceptions, however, given that *Nature* is read almost exclusively by scientists themselves, the public logic of the system entails likely a higher assumption of credibility for a Harvard professor than for a local community college instructor, even though both may have published in *Nature*.

*Ranking Knowledge in the Ether*

Probably the most obvious effect of the ether upon the knowledge system is that it provides an extraordinarily broad potential for the development of new proxies and also for the construction of new ranking systems. The public implications of this potential can be observed simply by plugging the same search term into the Google general site and the Google Scholar site. Some of the same materials will come up on the first page, but most likely in a very different order and from a much wider range of sources. Although the comparison would not be scientific, given the way Google assigns listing priorities, it would be nonetheless a fairly good demonstration that the knowledge goals evident on the public site would be very different from those on Google Scholar. Perhaps with Google we have, for the first time, a means of comparing the knowledge priorities of the public and academic spheres.

Another effect is public accessibility to rankings that academics cannot themselves control. For example, any ambitious prospective university student can now consult online a half dozen or so statistically rigorous assessments of the quality of thousands of universities and academic departments around the world based upon such indicators as the average number of faculty publications and citations in top-ranked journals, success in attracting

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research funds, student-faculty ratios, or even Nobel and Field Prizes. These determinations may or may not yet affect how students select which universities to attend, but they are already exercising university presidents to add yet another metric to the quality regime to which academic staff is expected to contribute.

Moreover, these “unofficial” rankings often prioritize indicators that are not prioritized in normal faculty assessments. For example, one of the leading university-ranking schemes evaluates the impact of university departments according to the numbers of downloads of working papers and other ephemera, including reports and briefings intended for public consumption, on the assumption (quite correct) that these are often closer to the leading edge of science, and of higher general impact, than publications in journals, which may lag the research by years and are mostly incomprehensible for purposes of public dissemination.

A potentially positive factor is that many scientists now use the ether to supplement, if not to bypass, the official ranking regime by communicating directly to the public. Look at virtually any web site of any department in any university and you would have to conclude that any member of that department for whom there was no link to a personal blog, web page, or the like was trying to hide something. In this respect, scientists have come out of the closet. Or have they? Many science blogs are published under pseudonyms, thus breaking the characteristic reputational bond between the science and the scientist. Moreover, publicizing your own work is clearly not the same thing as being, for want of a better term, “acclaimed” as a public intellectual by some external court of opinion.

Ultimately, one is left with doubts as to whether the new media offer a way out of some of the dilemmas faced by science and scientists in the public arena or whether they merely reinforce them by substituting different methods of surveillance and enforcement of professional norms within the scientific professions. For example, what would be the consequences should an anonymous blogging scientist be “outed,” especially if what is published informally contradicts what has been published formally or draws out more direct and controversial public implications? Perhaps the more serious questions, though, concern the reasons that scientists might feel the need to circumvent or subvert the formal institution of science in the first place.
Knowledge Ranking and the Elevation of Scientists as Public Intellectuals

What seems sure is that Einstein’s experience exemplifies a peculiar and perhaps obsolete process whereby scientific reputations can be transformed by opportunity and circumstance into reputations for general sagacity about issues of the day. Indeed, it is remarkable, and perhaps even salutary, that as Einstein’s significance as an active contributor to science waned, the frequency of his public comments about a greater variety of other issues increased. However, it must be recognized that by the standards of the information ether, Einstein also had little competition for public recognition and did not really have to work at the task of becoming a public intellectual. He did not have to maintain a website, or a blog, write a popular book, host a TV series, or even to climb the academic career ladder. In fact, throughout most of his career, he never held a mainstream academic position with normal duties and expectations. Moreover, he never had to contest his scientific credibility when making comments about matters outside of science. By today’s standards, he had it easy.

Of course, scientists of Einstein’s generation worked within a very different paradigm. How this evolved exemplifies the structural problem of how the knowledge-centered ethos of science can conflict with the information-centered ethos of the ether. Faced with the sheer volume of knowledge production in the past fifty years, scientists have inevitably become more specialized, and discoveries and insights have become more incremental. Accordingly, it has become more typical for scientists to cast the public issues of the day in narrower scientific contexts and to propose basically technical solutions. These may be valid solutions, but only in exceptional circumstances do they grab the public imagination and propel a scientist into a public role.

As Smolin (2007) notes, since the 1950s the theoretical sciences in particular have become largely a world of faceless expertise and arcane scholasticism rather than of expansive personalities and viscerally appealing theories like relativity or the Big Bang. Such observations are by no means unique to the natural sciences. In reflecting upon his tenure as editor-in-chief of the American Economic Review, one of the signature journals in economics, Clower (1989, 27) observed regarding new submissions: “What was remarkable was the absolute dullness, the lack of any kind of new idea. . . . Close to a thousand papers a year—and I swear that the profession would be better off if most of them hadn’t been written, and certainly if
most hadn’t been published.” Comments such as these raise questions as to whether science is somehow compromising its own credibility in the public mind by its own hand. The effect of new media in this endeavour may not even matter.

Jumping to such a conclusion, however, merely ignores the fact that knowledge can now be disseminated more efficiently than it can be produced and that scientists are under ever-increasing pressure to produce simply in order to advance their careers. Coupled with specialization, these pressures inevitably lead away from the radical conceptual breakthroughs characteristic of the early twentieth century and toward a dialectic of tiny increments.

**Conclusion**

Science will be a viable platform for the public intellectual not just to the extent that it can be made *intelligible* to the public—which is not always possible—but to the extent that its outcome or content has genuine social, economic and political significance that is *evident* to the public irrespective of how well they understand it as science.

This task will be easier in some fields than in others. The obvious utility of antibiotics can completely mask the much less obvious utility of high energy physics, even though it is actually not that difficult for any reasonably educated citizen to comprehend its utility, once explained. The real danger is that the dynamics of science as a profession may lead to a failure of explanation and of public engagement. This danger is the direct product of a disconnect between how knowledge is produced and how scientific careers are built. The ether already plays a huge role in reinforcing this disconnect. The question is whether the scientific professions can also mobilize it to thwart this result.

**Notes**

1. The remark was attributed to a conversation at Princeton University in 1946, later documented by Greenville Clark in the *New York Times* (22 April 1955).
2. Krugman also neglected to explain fully that, under the rules, Nobel Prizes in the natural sciences are not awarded for theory, which is
Hawking’s strong suit, until and unless that theory has been demonstrated empirically in some way. Einstein’s theory had to wait over a decade for the demonstration that secured him the prize. The Nobel Prize for economics, a recent addition, requires no such demonstration.

References


Establishing the Public Legitimacy and Value of Scientific Knowledge


