

# Science and its Transactions Cost: The Emergence of Institutionalized Science

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Of the multiple springs of modern economic growth, the emergence of scientific communities capable of focussing and validating inventive effort in the production of non-proprietary information bearing on the constitution of the material world is probably the least well-understood.<sup>1</sup> That development is commonly dated to between 1600 and 1850, making it co-terminous with the fundamental scientific breakthroughs in mathematics and the physical sciences that laid the foundations for the second industrial revolution in the late nineteenth and early twentieth century. Thanks to several generations of historians of science, the intellectual history of those breakthroughs is fairly well understood. Probably the most important generalization to be derived from that history is the remarkable degree to which non-empirical notions concerning the necessary nature of reality affected the direction and interpretation of research.<sup>2</sup> In this respect, scientific invention differs fundamentally from technological invention, for which the the posing of problems and evaluation of solutions to them are more strongly influenced by considerations of practical utility.<sup>3</sup> Watt's invention of the separate condenser might have been inspired by insights drawn from Black's flawed caloric concept of heat; but the success of his steam engine ultimately depended on devising a workable disposition and regulation of valves controlling the flow of steam, air, and water. The connection between general systems of thought and scientific discovery thus highlights the intrinsically social character of scientific invention, in which research programmes and new findings are initially assessed on the basis of beliefs held within scientific communities.

David has recently drawn attention to the collective nature of science in his essay on the emergence of open science in the seventeenth and early eighteenth century.<sup>4</sup> He argues that prior to the development of institutions inducing swift public disclosure of

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<sup>1</sup> This statement was drafted before I had a chance to read Paul David's remarkable study 'The historical origins of "Open Science": an essay on patronage, reputation and common agency contracting in the Scientific Revolution,' *Capitalism and society* 3 (2008), 1-103. (Berkeley Electronic Press)

<sup>2</sup> Although references to the vast bibliography are superfluous here, I draw the reader's attention to the much overlooked work by John Theodore Merz, *A history of European thought in the nineteenth century*. Edinburgh and London (1907-1914), especially volumes I and II, and Ernst Mayer's *The growth of biological thought: diversity, evolution, and inheritance*. Cambridge MA: Belknap (1982).

<sup>3</sup> Vaucanson, named to the Académie des Sciences for his mechanical genius, complained to Trudaine in 1765: 'The intelligent public will understand that it is much easier to make meteorological observations, or to stage demonstrations with ice, magnets or electricity, than to invent and construct a good machine. In the one case it is only a matter of explaining as one likes certain known effects; in the other one must produce new effects. This is why the great majority direct themselves towards theory rather than practice.' Cited by Briggs, 'The Académie Royale des Sciences and the pursuit of utility,' *Past & Present* 131 (1991), 84.

<sup>4</sup> David, 'Historical origins'

new findings in order to make them available to other qualified scientists for validation (and reward), esoteric information was generally closely held as a proprietary good. That hermetic tradition, which in certain industrial crafts persisted into the nineteenth century (and survives today among acolytes of the political philosopher Leo Strauss), treated information as a ‘mystery’ to be guarded in the interest of maintaining market power and, in the imaginations of some thinkers, public order. That tradition broke down in ‘pure’ science, in the face of its increasing esoterism, particularly marked in the mathematical branches, which made it difficult for potential patrons of scientists to judge their quality (and more importantly their reputation) without drawing on the expertise of the scientists themselves. The critical historical element in this development was the interest of Renaissance princes in securing the services of reknowned scientists as a means of enhancing their prestige and (more hopefully) of securing some utilitarian benefit from their expertise. David argues that Open Science was a consequence of the growing advantage to individual scientists seeking patronage of having their discoveries warranted by a community of peers rather than withholding them for a doubtful private gain, and to the increased willingness of patrons to accept the peers’ judgment in determining who and how much to fund. The balance between disclosing and withholding information was a delicate one that remains difficult to sustain. Nevertheless, by the second quarter of the eighteenth century, a transnational social network based on reciprocal disclosure of discoveries and collective warranting of their merits was in place. Apart from the need to secure resources to fund research, the system functioned independently of the rest of society, elaborating and administering its rules of demonstration and rewards. That independence, which as late as the middle of the seventeenth century was far from secure, is perhaps its greatest achievement.<sup>5</sup>

The present paper deals with a different, though complementary issue. For scientific investigation of the material world to produce significant and sustaining effects on productivity, the work had to be carried out on a scale large enough to breach the high threshold of tested fact and theory required to support reliable generalizations covering phenomena characterized by high levels of complexity. This was especially true of what might be called the ‘taxonomic’ sciences like natural history, minerology and chemistry, where most early work necessarily consisted in identifying and classifying phenomenal types.<sup>6</sup> Between 1780 and 1850 the scientific enterprise experienced a massive expansion in the number of distinct scientific disciplines and persons working at high levels of specialization. The ‘scaling up’ of organized science, however, raised new problems of quality control and recruitment that the largely aristocratic and centralized institutions inherited from the seventeenth century were ill-suited to handle. As in the case of the emergence of ‘open science,’ resolution of these problems seems to have been an unintended by-product of other historical developments. We often think of science as

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<sup>5</sup> That it was not complete is demonstrated by the government’s banning of Diderot’s *Encyclopédie* in 1759 in response to the vigorous opposition of the religious authorities. Darwin’s self-censorship in delaying publication of his theory of natural selection for much the same reason is another example.

<sup>6</sup> It was not by chance that the earliest scientific breakthroughs in theoretical physics were in optics and celestial mechanics, where contextual variation in the objects of observation was comparatively slight. Harvey’s discovery of the circulation of blood is another example. Despite the complexity of the cardiovascular system, the structural relations between the heart, arteries and veins, and valves is the same across species, making it possible to draw generalizations from comparative anatomical investigations.

an expression of Western culture; but what it expresses is not so much a general attitude towards the physical world, as a contingent outcome of events and institutional developments specific to the late eighteenth and early nineteenth century. A review of that history reveals how unlikely that outcome was.

### *Science as a Distinct Social System*

Science has been defined by Ravetz as ‘the activity of investigating problems in the context of an abstract, technical discipline.’<sup>7</sup> To facilitate that investigation, scientists invent idiosyncratic languages in order to minimize ambiguity in transmitting and archiving information.<sup>8</sup> Conceptual work is largely dedicated to developing and refining such codes.<sup>9</sup> The resulting efficiency in communicating esoteric information, however, comes at the cost of heavy individual investment required to master the code, which creates a significant barrier to its diffusion to persons unable or unwilling to make the investment.<sup>10</sup> The specificity of scientific human capital has three major consequences for the organization of science. The first is to impede the ability of lay persons to assess the potential scientific significance of particular line of enquiry, and thus to articulate an effective demand price for their product.<sup>11</sup> The second is to situate the maintenance of standards and recruitment of new scientists within the community of those who have mastered the codes. The third consequence is the negative effect on recruitment of the cost and specificity of scientific human capital, for which there exist few alternative employment outside science.<sup>12</sup> Much work in science demands such high degrees of care and accuracy acquired as ‘tacit knowledge’ in learning by doing that it is hard to delegate even simple tasks to semi-skilled personnel.<sup>13</sup> All these factors encourage the social autonomy of the scientific enterprise, at the cost of making the financing of scientific research problematic and assimilation of new concepts potentially contentious.<sup>14</sup>

Problems of validation and recruitment posed by the cost and specificity of scientific human capital are compounded by the coordination problem created by the

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<sup>7</sup> Jerome R. Ravetz, *Scientific knowledge and its social problems*. Oxford (1971),

<sup>8</sup> Jacob Marschak, ‘Economics of inquiring, communicating, deciding,’ *AER Papers and Proceedings* (1968), 1-18; Kenneth J. Arrow, *The limits of organization*. New York (1974), 37-43.

<sup>9</sup> The history of mathematical notation provides numerous examples, e.g., the development of a standard notation for matrix algebra at the turn of the twentieth century. Much of the work of theoretical economics consists essentially in defining terms.

<sup>10</sup> The problem seems initially to have emerged in mathematics, where the development of algebra and the geometry of conic sections made the field increasingly inaccessible to mathematicians’ potential patrons. See David, ‘Open Science’, 40.

<sup>11</sup> Kenneth Arrow, ‘Economic welfare and the allocation of resources for invention,’ in *The rate and direction of inventive activity*. Princeton (1962), 609-25.

<sup>12</sup> The demand by financial institutions for ‘quants’ possessing advanced degrees in mathematical sciences is a spectacular exception. The case of pharmacy as an outlet for trained chemists in the early nineteenth century is reviewed below.

<sup>13</sup> Kollreuter’s field experiments in plant breeding were ruined because his workmen refused to follow his instructions to the letter. Cf. Robert C. Olby, *Origins of Mendelism*. New York (1966), 24.

<sup>14</sup> The classical statement of discontinuous assimilation of new scientific concepts is Thomas S. Kuhn, *The structure of scientific revolutions* Chicago: University of Chicago Press (1962).

absence of proprietary rights in scientific findings, which inhibits the use of markets to allocate scientific resources. Despite their absence, the practice of science is nevertheless highly decentralized, and today occupies several million persons in thousands of sub-disciplines at sites dispersed throughout the world. From an economist's perspective, perhaps the most intriguing attribute of that decentralization is how research activity seems to track patterns that mimic what one would expect from optimizing individuals responding to price signals: a swarming into 'hot' fields, and abandonment of lines of research experiencing diminishing returns to effort. It is hard to resist the conclusion that such patterns reflect the presence of a private property right in scientific output. Sociologists of science locate that right in the collective recognition of priority of discovery.<sup>15</sup> Paid and protected by citations, policed by editorial boards and referees, that right supplies an objective which goes far to explain the intensity and obsessiveness of scientific effort and the waves of creative activity attending major breakthroughs in scientific concepts and experimental technique.<sup>16</sup> As a means of encouraging rapid exploitation of scientific opportunities, the development of scientific property rights has interesting analogies (and differences) with the evolution of laws and customs regulating mining claims in early California that attempted to strike a balance between protecting the original claim and allowing newcomers to try their luck on abandoned ones.<sup>17</sup> As Adam Smith observes in *The Theory of Moral Sentiments*, a man's desire for approbation can lead him to undertake actions that together with similar actions taken by other men work like a 'invisible hand' to sustain civilized society.<sup>18</sup> Something similar sustains the economy of science.<sup>19</sup>

Independently of psychological rewards, the cash surrender value of rights in first discovery can be significant. In the first decades of the nineteenth century, Berthollet, Thénard, and Arago accumulated salaries equalling the pay of high French civil servants, and Gay-Lussac's income was truly princely.<sup>20</sup> Before 1850 German professors earned

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<sup>15</sup> For an insightful discussion of priority as a property right, see Robert K. Merton, 'The ambivalence of the scientist,' *Bulletin of the Johns Hopkins Hospital* 112 (1963), 77-97.

<sup>16</sup> The wave pattern of discovery is well documented. Given a reward structure that permits a successful discoverer to secure a permanent income stream in a context of free entry, such waves are inevitable consequences of conceptual and experimental breakthroughs that alter the probability of individual success. For evidence of the waves, see Joseph Ben-David and Awraham Zlopczower, 'Universities and academic systems in modern societies,' *European journal of sociology* 3 (1962), 45-84, and Joseph Ben-David, 'Scientific productivity and academic organization in nineteenth-century medicine,' *American sociological review* 25 (1960), 823-843. On waves as the consequence of optimizing responses to opportunity, see Yoram Barzel, 'Optimal timing of innovation,' *Review of economics and statistics* 50 (1968), 348-55.

<sup>17</sup> Karen Clay and Gavin Wright, 'Order without law? Property rights during the California gold rush,' *Explorations in economic history* 42 (2005), 155-183.

<sup>18</sup> Smith, *Theory of moral sentiments*. Page ref here.

<sup>19</sup> See David, 'Origins of "Open Science"', 71-74, for an elegant game-theoretic account of the incentives supporting the institutions awarding property rights in discovery.

<sup>20</sup> Gay-Lussac, who was probably the best paid-scientist in the early nineteenth century, held several teaching positions and memberships on government boards, including the post of chief assayer to the mint. His annual income came to 55,000 francs. Maurice Crosland, *Gay-Lussac. Scientist and bourgeois*. Cambridge (1978), 230. At the peak of his career Gay-Lussac's colleague Thenard earned 30,000 francs and was made a peer of the realm by the July Monarchy. Starting salaries for official positions were much lower, but a full professor at the *Institut* or the Museum of Natural History received 5,000 to 6,000 francs, less than a sub-prefect, but respectable all the same. Paul Gerbod, *La condition universitaire en France au*

500 to 700 thalers, which was roughly equivalent to the annual revenue of modest burghers in port cities, but a talented and ambitious scientist like Justus Liebig was able to increase his university pay from 300 to 3,000 florins and the government subsidy for his laboratory from 100 to 1,500 florins.<sup>21</sup> Material incentives, then, are also present in the economy of science. Yet instances of exceptional financial return from priority in discovery were winning tickets in a lottery that only a few scientific fields could support. Moreover, unlike in law, medicine and entertainment, cited by Adam Smith as examples where individuals' over-estimation their ability induce an excess supply of candidates hoping to strike it rich, scientific work did not initially support a stock of low-paid jobs that could support aspirants hoping to hit the jackpot. Scientific training was expensive and as noted above, its specificity made investing in it risky.

The emergence of institutions defining and enforcing property rights in scientific discovery, then, was a necessary but not sufficient condition for the emergence of scientific research on a broad and ever-expanding front. The development of an incentive system capable of supporting large-scale science proceeded in stages, the pieces being assembled piece by piece from existing elements of existing institutional arrangements. Resolution of the delicate informational problems posed by acts of doing, diffusing, and validating research on the frontier occurred in the first half of the nineteenth century, when scientific research came to be lodged in the university as part of the normal duties of professors, who recruited and trained new generations of scientists as a by-product of their teaching and research. Given sufficient funding, the enterprise was self-sustaining and able readily to colonize new areas of research as they opened up. It was decentralized, which in practice meant that in contrast to the earlier institutions of government-sponsored scientific academies, validation of establishment of priority in discovery were also decentralized. The evaluation of scientific output was in turn linked to recruitment and promotion within the academic establishment, providing further material inducement to undertake sustained research. By the third quarter of the nineteenth century, the scientific enterprise had acquired the institutional stability and independence from governmental and religious interference that would carry it into the second half of the twentieth century.<sup>22</sup> From an intellectual standpoint the Scientific Revolution takes its roots in the breakthroughs of the seventeenth century; from the institutional perspective, the Revolution belongs to the nineteenth.

That the scientific enterprise should have lodged in what down to the 1840s were still essentially medieval institutions dedicated to training civil servants, priests, high school teachers, and a narrow range of medical professionals could not have been predicted in 1750. Except for Eichorn's seminar in biblical criticism at Göttingen, there

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*xix<sup>e</sup> siècle*. Paris (1965); and Maurice Crosland, *The Society of Arcueil*. Cambridge (1967), 80, 133, 203, 231.

<sup>21</sup> Charles E. McClelland, *State, society and university in Germany, 1700-1914*. Cambridge (1980), 206-211; J. B. Morrell, 'The chemist breeders. The research schools of Liebig and Thomas Johnson, *Ambix* 19 (1972), 29-42.

<sup>22</sup> It is an important and as yet unresolved question whether this institutional framework for conducting science can survive the external pressures placed on it by the rising cost of research, which has led to calls for privatizing some of the product as a means of financing it, and the pressures placed upon the teaching establishment by demands for ever higher scientific production.

were no academic venues for training students in advanced research methods, and no formal procedures for certifying their successful completing of an independent research project. Such science as was then conducted took place in private libraries and laboratories, and in the a handful of specialized institutes like the Jardin du Roi in Paris. Scientific analysis of ancient and medieval documents found institutional homes at the Benedictine Abbey of Saint-Germain-des-Prés and among the Jesuit Bollandists in Antwerp, but while monastic life supported disinterested study and the training of young researchers, lines of enquiry were confined in practice to the technical work of editing and criticizing manuscripts. Mendel's experiments on the transmission of inheritable traits in the garden of the Augustinian abbey at Brunn is a notable exception, but as is well known, his revolutionary findings were ignored, possibly because he was so isolated from the scientific community.<sup>23</sup>

The one institution that possessed the capacity to provide advanced technical training was the craft guild, of which the medieval university was a distant cousin. But by the eighteenth century guilds were protectionist and conservative, guarding their secrets closely. But even had they been more open, the utilitarian focus and limited means of supporting apprentices inhibited any tendency to investigate and articulate the less appropriable general principles of their crafts.<sup>24</sup> Perhaps the greatest impediment to the development of science within the organizational structure provided by the guilds, however, was lack of employment opportunities for trained researchers unable to support themselves by scientific research once they graduated. This is, however, getting ahead of the story. We must first consider the difficulties that any system of organized science had to resolve to be productive of new and secure findings.

### *The Cognitive Constraint*

Considering the obstacles to its institutionalization, the scaling up of open science in the nineteenth century was highly improbable. As objects of exchange, scientific findings are characterized by their heterogeneity, uncertain reliability, and asymmetry in the distribution of information bearing on their validity and significance. These features reflect the extreme specialization of research scientists induced by scale economies of learning by doing on the research frontier. Behind that frontier, descriptive generalization and formal theory supply stable classes into which individual instances are fitted, interpreted, and understood; but across the no-man's land dividing the known from the unknown, unvarnished fact reassert its individuality. Initially ambiguous and indistinctly perceived, novel observations are hard to assimilate to known bodies of knowledge.

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<sup>23</sup> There is some evidence that his experiments were deliberately ignored.

<sup>24</sup> The instrument makers were a partial exception, as the work combined mathematics and craft skills of the highest level. Their achievement, however, was embodied in physical instruments that were custom built for discerning clientele with the ability and willingness to pay. For a major example, see Anita McConnell, *Jesse Ramsden (1735-1800): London's leading scientific instrument maker*. Aldershot (2007). Note from Briggs here.

The early history of astronomy provides an example of that ambiguity. Astronomers initially found it difficult to replicate observations because their lenses were ground from glass of variable quality. Different telescopes ‘saw’ different things; even the same telescope might give different readings on different occasions. Astronomers sometimes shipped lenses to other astronomers to have their observations confirmed using the same equipment; but it was often hard to determine whether the reported object was something in the sky or an artifact of an irregularity in the glass. The problem occasioned bitter disputes, since the correspondent could claim that the initiating astronomer had seen something that was not there, while the initiator had a plausible claim that his correspondent was acting in bad faith.<sup>25</sup> Similar controversies disturbed the early history of microscopy. What Alexander Pope’s quipped about literary criticism was just as true of contemporary science:

‘Tis with our judgments as our watches, none  
Go just alike, yet each believes his own,<sup>26</sup>

The cognitive obstacles go beyond imperfect instrumentation, however. Observation and perception are not the same thing. What a scientist ‘sees’ is phenomenal, an electro-chemical reaction to light striking the retina. What he ‘perceives’ is that reaction simultaneously coded by neural circuits shaped by prior experience and theoretical constructs. As Baudelaire beautifully put it,

*La nature est un temple où de vivants piliers  
Laisserent parfois sortir de confuses paroles;  
L’homme y passe à travers des forêts de symboles  
Qui l’observent avec des regards familiers<sup>27</sup>*

As Ravetz observes,

In any real situation, there are too many subtle cues and too many partly relevant precedents for the knowledge of how to cope with novelty to be reduced to tables of experiences and inferences.<sup>28</sup>

Following a line of reasoning opened up by Wittgenstein, Hansen argues that observation is non-propositional.<sup>29</sup> When an observer ‘sees’ something, the immediate physical experience is organized (coded) by a context or *gestalt* supplied by his personal history, which in the case of a scientist includes his theoretical training and laboratory experience.<sup>30</sup> An observer sees the object ‘as’ something. That first sighting is

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<sup>25</sup> Maurice Dumas, *Les instruments scientifiques aux xvii<sup>e</sup> et xviii<sup>e</sup> siècles*. Paris (1953), 42.

<sup>26</sup> *An Essay on Criticism*.

<sup>27</sup> ‘Correspondances’, *Les fleurs du mal*.

<sup>28</sup> Ravetz, *Scientific knowledg*, 102.

<sup>29</sup> Norwood Russell Hanson, *Perception and discovery. An introduction to scientific inquiry*. San Francisco (1969).

<sup>30</sup> Hanson gives the example of a man who is ignorant of physics being introduced to a physics laboratory. He ‘experiences’ the equipment as variously shaped objects attached by wires and tubes to other objects,

idiosyncratic, as when a person sees a speck in the air as a duck rather than an airplane, or a star rather than an aberration in a telescopic lens. The problem is that while circumstances can suggest meaning to the observer; their idiosyncrasy can make it impossible for him to convey that perception in its raw state to others without further translation. The crucial cognitive step in science occurs when an ‘observation’ is asserted in the propositional logic of language, which makes the idiosyncratic first ‘sighting’ social.

The nature of perception as a *gestalt* experience explains why the process of discovery is so difficult to codify. The history of science is replete with examples of how difficult it is to establish a new fact, when that fact cannot be fitted easily into a formal frame of reference supporting a propositional description of it. Edgar Anderson relates how a distinguished plant breeder failed to identify a new strain of maize because his statistical techniques did not capture its identifying characteristics.

‘[T]he simple every day facts about the two strains were as follows. The two sets were of quite a different shape. One was broader and rounder at the base with a slight tendency to be wider in the middle, and it was nearly always shorter than the other. ... Having seen half a dozen of each kind, one could classify at least nine out of twenty specimens which had been gathered by an assistant and brought in without a label. ... Taxonomists are more like artists than art critics. They practice their trade and don’t discuss it. It was only by observing them trying to translate what they were doing ... that I made very much progress. It seemed to me that different kinds of things vary in fundamentally different kinds of ways.’<sup>31</sup>

Probably the greatest failure of scientific perception in the history of agricultural research stemmed from the inability of plant scientists to distinguish between saprophytic and parasitic fungi at the time of the great potato blight in the 1840s. Incontrovertible evidence revealed that copper sulphate dressings inhibited the rot, but contemporary scientists ignored it because they believed that the fungal infestation was an effect and not the cause of the ‘sickness.’<sup>32</sup> Something was seen, but not perceived. Part of the difficulty was noisy data, a common phenomenon on the research frontier. In South Wales the potato fields were largely free of the blight, because they were affected by fumes from a nearby copper smelter at Swansea. But although the fumes killed the lethal spores, they also killed everything else.<sup>33</sup> Noisy data were not the only excuse for

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and will describe them as such. For him to see them the way a trained physicist sees them require his knowing what they are ‘for,’ which implies his learning the physics. The novice and the physicist have the same retinal experience, but they ‘see’ different things.

<sup>31</sup> Edgar Anderson, *Plants, man and life*. Boston (1952), 179-80.

<sup>32</sup> T. A. O’Neill, ‘The scientific investigation of the potato crop in Ireland, 1845-1846.,’ *Irish historical studies* 5 (1846-47), 123-138. O’Neill claims that the discovery by Garrett Hughes, an Irish landowner, that steeping seed potatoes in the solution prevented the disease was dismissed by the scientific commission of inquiry into the causes of the blight because it didn’t make sense to them. Hughes’ finding was not a singleton. Other farmers made the same discovery.

<sup>33</sup> P. M. Austin Bourke, ‘The scientific investigation of the potato blight in 1845-46,’ *Irish historical studies* 13 (1962), 26-32.

scientific action, however. A committee appointed by the British government to collect scientific evidence on the causes of the blight commissioned no experiments. On any rational cost-benefit accounting the catastrophe warranted a huge investment.<sup>34</sup> A less momentous failure in perception was the Illinois Experiment Station's abandonment of a promising programme of inbreeding of superior strains of corn because the research director did not believe what his assistants had seen – that crossing pure strains yielded significantly improved hybrid varieties.<sup>35</sup> Neither of these and other failures were owing to lack of raw data. They reflect the difficulties of perception on the scientific frontier. As Hanson elegantly puts it, 'There is more to seeing than meets the eyeball.'<sup>36</sup>

Although uncertainty described above occurs most frequently in empirical work, it also plagues non-experimental sciences. Hagstrom reports a conversation with an eminent mathematician who confessed to him that

I have never had a paper that wasn't challenged. I've never published a paper without a serious mistake and I never intend to do so. A famous editor of a mathematical journal said the same thing as I have said about errors. Papers without serious mistakes are probably trivial. The work is too easy.'<sup>37</sup>

In fields like mathematics sheer complexity creates obstacles to understanding. When a Japanese mathematician 'solved' Poincaré's conjecture in a one hundred page proof, nobody believed it, even though no one could find an error it.<sup>38</sup>

The above examples illustrate the primary obstacle to decentralized science: the information costs imposed by the idiosyncrasy of novel findings. In considering why markets tend to underprovide scientific output, most economists emphasize the public goods nature of scientific information. From this perspective the main contractual problem is organizing the purchasers.<sup>39</sup> The cognitive problem raised above, however, is more fundamental. Collective goods can be collectively purchased by their joint consumers; but collective purchase of scientific information through, say, publically funded agricultural experiment stations, has no particular implication for the quality of the findings purchased. The history of 'commissioned' science in the eighteenth and nineteenth century is fraught with failures resulting from the inability of scientific patrons to distinguish good work from bad. None of the famous names in eighteenth-century invention appear in the list of prize-winners awarded by the Royal Society of Arts, which was expressly established to encourage invention.<sup>40</sup> Prizes were awarded for false

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<sup>34</sup> Mokyr estimates that that neglect cost between one and one and a half million excess deaths. Joel Mokyr, 'The deadly fungus: an econometric investigation into the short-run demographic impact of the Irish famine, 1846-19851,' *Research in population economics* 2 (1980), 237-77

<sup>35</sup> Richard A. Crabb, *The hybrid-corn makers. Prophets of Plenty*. New Brunswick (1947)

<sup>36</sup> Norwood Russell Hanson, *Patterns of Discovery*. Cambridge (1972), 7.

<sup>37</sup> Warren O. Hagstrom, *The scientific community*. New York and London (1965), 27.

<sup>38</sup> Hagstrom, *Loc. cit.*

<sup>39</sup> Arrow, 'Economic welfare.'

<sup>40</sup> Henry Trueman Wood, *A history of the Royal Society of Arts*. London (1913), 240-43. The French *Académie des Sciences* and later the *Institut* were also slow to credit important inventions, usually on the grounds that they were insufficiently original. Leblanc never received the prize set in 1783 for the method

solutions and withheld from inventors making promising but incomplete advances to the solution of a problem. In 1846 the Royal Agricultural Society awarded a prize to a man who claimed that the potato blight was caused by the unhealthiness of the plants, though any such finding could only be defended by a terribly flawed experimental design.<sup>41</sup>

Demand for information alone, then, was insufficient support for good scientific work. This is well brought out by the history of agricultural experimentation. There was obvious and strong demand for an agricultural science from at least the time Francis Bacon was steeping wheat seeds in Malmsey to see if the liquor inhibited smut, but the search for a way to increase crop yields was undirected by hypothesis. In light of the number of factors bearing on plant growth, it is hardly surprising that thousands of early 'trials' failed to turn up anything significant. Imprecise measurement and lack of controls limited the usefulness and impeded the replication of field and other experiments. Moreover, eighteenth-century sponsors of agricultural research were more interested in assessing costs of different methods cultivation than in uncovering biological processes determining plant and animal yields, and they treated experimental farms more as demonstration stations than as experiment stations. It was in that spirit that the Royal Dublin Society ordered John Wynn Baker to discontinue investigation into how row cultivation affected the yields of different varieties of wheat, and to get on with task of demonstrating the profitability of the 'new' system of husbandry propagandized by Arthur Young.<sup>42</sup> For his part, owing to lack of control plots, Young's undisciplined enthusiasm for the 'system' was reflected in the failure of his more than 2,000 field 'trials' to yield any useful information.<sup>43</sup>

It took persons trained in scientific method to appreciate the crucial importance of experimental controls. The need to design agricultural field experiments controlling for factors other than the one under investigation was not generally appreciated until the 1840s, when the appearance of costly concentrated fertilizers created a demand for accurate information on their effects. The improvement in experimental design was largely due to the incursion of scientists from organic chemistry recruited to test the effect of fertilizers on crops planted in different soil types and subject to different rotations. As Daubeny remarked in his address to the Royal Agricultural Society in 1841, any real addition to knowledge had to come from men trained in 'proper methods of experimenting' and not from laymen conducting trials in an 'unscientific manner.'<sup>44</sup> The founding of the first agricultural experiment stations in Germany was largely a response

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of obtaining salt from soda that is named after him. Philippe de Gérard's claim for the million franc prize offered by Napoleon for a machine to spin flax was rejected because his (successful) design was deemed too simple. Maurice Crosland, *The society of Arcueil*. Cambridge, MA (1967), 27, 31.

<sup>41</sup> E. C. Large, *The advance of the fungi*. London (1938), 28.

<sup>42</sup> Arthur Young supported the Society on the grounds that the farm had been set up to demonstrate the new husbandry, not as a place where Baker could acquire knowledge and a scientific reputation at the Society's expense. George E. Fussell, 'John Wynn Baker: An "Improver" in eighteenth-century Ireland,' *Agricultural history* 5 (1931), 151-61.

<sup>43</sup> G. E. Fussell, 'The technique of early field experiments,' *Journal of the Royal Agricultural Society of England* 56 (1935), 82.

<sup>44</sup> Charles Daubeny, 'Lecture on the application of science to agriculture,' *Journal of the Royal Agricultural Society* 3 (1842), 137-38. See also E. M. Crowther, 'The technique of modern farm experiments,' *Ibid.* 97 (1936), 54-57.

by professional chemists to the failures of such non-professional experimenting.<sup>45</sup> The institutional innovation consisted in transferring not just the experimental technique, but the adapting the institutional form and ethos of organized science to agricultural research.<sup>46</sup> The form, if not the ethos, however, was of recent parentage.

### *The Identity Solution to the Cognitive Constraint*

Where products are non-standard and quality uncertain, decentralized supply often induces specialization by identity. This is most evident in the last redoubt of non-commercial supply, family provision of emotional support for children and aged relatives, where the person providing the service is in large measure the service.<sup>47</sup> Knowing the supplier lowers the cost of search on the buyer's side, and provides the supplier an incentive to maintain the quality of his product. Reducing 'acquisition cost' and providing incentives for high quality was especially critical to the early development of organized science, because the geographic dispersion of researchers, the highly specialized nature of their work and findings, and the slowness of communications meant that experiments could not be immediately verified. Yet, they could not be taken on faith, either. The French *Académie des Sciences* attempted to resolve the problem by centralizing the deposit of research findings in one place, where they could be discussed and evaluated by the members sitting as a committee of the whole. But despite its preference for 'collective' science, the *Académie* was forced to abandon the reception of anonymous contributions, because it was impossible to verify all experiments before the entire Company assembled at its premises in the *Bibliothèque du Roi*. Identifying the author of a research reports was accepted as a necessary, though inferior, guarantee of its quality.<sup>48</sup> Names mattered.

Non-anonymity in scientific work hardly surprising. Scientists publish their work under their own name because unassigned discovery supports no claim to priority, or at best supports a posthumous claim.<sup>49</sup> Scientists not only refuse to submit unsigned papers, but generally reject unsigned work. Identification encourages scientists to signal

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<sup>45</sup>Gustav Kuhn, 'Geschichtliches über die Landwirthschaftliche Versuchs-Station Mockern,' in *Die Landwirtschaftlichen Versuchs-Stationen*. Bd 22 (1877), 33; 48-55. Theodor Reuning, the Saxon official most directly responsible for the establishment of the first station at Mockern explicitly defended its scientific vocation, on the grounds that only the scientific method could achieve real results ('Das dieses nur sehr zu erreichen sein wird, liegt in der Nature der Aufgabe ...' Theodor Reuning, *Die Entwicklung der Sächsischen Landwirtschaft in den Jahren 1845-1854*. Dresden (1856), 40-52, 187.

<sup>46</sup>George Grantham, 'The Shifting locus of agricultural innovation in nineteenth century Europe: The case of the agricultural experiment stations,' in Gary Saxonhouse and Gavin Wright, (eds.) *Technique, spirit and form in the making of the modern economies: Essays in honor of William N. Parker. Research in economic history, Supplement 3* Greenwich, CT: JAI Press (1984). 191-214.

<sup>47</sup>Nancy Folbre, *The invisible heart. Economics and family values*. New York (2001).

<sup>48</sup>Roger Hahn, *The anatomy of a scientific institution. The Paris Academy of Sciences, 1666-1803*. Berkeley (1971).

<sup>49</sup>Mendel's case is the best known, but one of the saddest instances comes from the history of economics. Hermann Heinrich Gossen is widely recognized as the first discoverer of the relation between constrained optimization of an individual utility function and the market demand curve. He died a broken man when his book was ignored, and when his work was recognized by Jevons, his publisher pasted a new page on the *frontispiece* and marketed it as a second edition, adding insult to injury.

their quality by giving emblems of good performance, which include crediting the contributions of others, fully describing the data and methods of analysis, and cautiously interpreting results. These elements of scientific style give an assurance of reliability. Attaching one's name to a discovery also accumulates reputational capital. A scientist whose work has proved reliable and useful in the past will find it easier to get his work accepted than a novice without a track record. There is a certain economic logic to this, as the informational cost of using new work by a known scientist is lower than using that of one who is untested. As in other spheres of social life, science has its hierarchies.<sup>50</sup>

Scientific reputation, then, is a form of human capital. But exactly who is invested? The technical codes used to communicate specialized scientific work mean that only insiders can accurately judge the value of a particular research finding and thus create and validate a scientific reputation. The producers and immediate consumers of specialized research are thus mutually invested. The persons who 'recognize' priority must be as specialized in the particular field as the discoverer. It is in this sense that scientific communities resemble families: knowledge generated about scientific work of individuals is personal knowledge.

An economic explanation of the organization of scientific activity, therefore, would predict that it typically take the form of small groups of persons within which scientific communication is intense. The cost of acquiring and maintaining the stock of individual reputational information probably explains their small size. Members of what have been called 'invisible colleges' may be thought of as making person-specific investments in each other's work, acquiring in the process a specific capital that allows them to communicate with each other at low cost. Within the economy defined by that cost, individual scientists are linked by an implicit contract enforced through mutual monitoring of performance. The invisible college thus functions like a caste system.<sup>51</sup> Within the community, honest mistakes are not penalized unless they become habitual.<sup>52</sup> Sociologists of science report that scientists working outside established groups tend to be relatively unproductive, and that research tends to be repetitive and unoriginal in their

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<sup>50</sup> One of Hagstrom's informants told him that in the early stages of his career, he deliberately delayed publishing work related to that of a senior colleague. 'I waited until his appeared in print before submitting mine. He referred to my unpublished work. I was ready to publish first, but I deferred to his authority. His good will was more important to me than priority.' Hagstrom, *Scientific community*, 91.

<sup>51</sup> George Akerlof, 'The economics of caste.' Caste systems work by 'outcasting' persons who break the caste rules. The purchaser of a non-caste good is penalized along its supplier, lowering his income to the point where it is preferable for him to buy the caste product even though the non-caste good might be cheaper. Since the advantage of the caste system over autarky is that it is specialized, it follows that if the number of outcasts becomes large enough to secure the economies of specialized production, the caste can be broken. There are analogies to his process in the history of scientific revolutions.

<sup>52</sup> The historian J. H. Hexter observed in this connection that 'In every discipline the askers of easy questions come up regularly with easy answers, and the askeers of hard questions often fumble and miss. And in every discipline the practioners are rated not by a mere count of the number of uestions they answered correctly, but by a corporate judgement of how hard the question was, how much worth answering, and how good a try the practioner made in trying to answer it.' J. Hexter, *The history primer*. New York (1971).

absence.<sup>53</sup> The coagulation of scientific activity into small clots of specialists can thus be interpreted as a solution to the problem of assigning priority. Given the cognitive uncertainty attaching to truly new findings and the temptation to cheat, it is difficult to conceive a method of monitoring and rewarding good work in science that did not depend on review by qualified peers.<sup>54</sup>

The monitoring and evaluation of scientific output is related to the problem of creating incentives assuring continuous effort. For science to develop on a wide base, it could not continue to rest on a small number of wealthy persons supporting themselves in a life of research. The growth of organized science thus implied an institutional structure in which researchers are salaried. The problems of institutional design in this context are illuminated by the older literature on optimal labour contracts.<sup>55</sup> Wage structures that discriminate among workers based on performance induce employees to reveal their ability, but are only feasible if performance can be effectively monitored, and even where accurate monitoring is feasible, the high risk of failure in scientific research implies that even well-calibrated wage scales are unlikely to induce an optimal supply of potentially able scientists. It thus necessary to insure researchers against failure by paying a wage unrelated to their productivity. But any contract of this kind creates a moral hazard that once tenured, a worker will slack off. To overcome that risk, information about an individual's traits and likely productivity must be acquired through screening. The problems of institutional science resulting from the uncertainty of findings on the research frontier thus extend to recruiting a stable and productive labour force. These remarks illustrate the delicacy of the contractual problems engaged in establishing an institutional form for supporting a science that did not depend on the work of wealthy amateurs.

### *The Evolution of Scientific Contracts*<sup>56</sup>

We can now examine the evolutionary path of the institutional forms regulating recruitment, rewards, and mutual surveillance of scientists in the eighteenth and early nineteenth century. The above discussion suggests that the key elements of effective scientific organization are peer review, and a reward system controlled by persons capable of assessing scientific performance. The hypothesis advanced below is that an institutional form capable of resolving these issues and providing a means by which talented young researchers could signal their ability did not emerge full-blown, but came together like the pieces of a puzzle in a sequence of events largely determined by the difficulty of securing necessarily implicit contracting arrangements. An economic

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<sup>53</sup> Diana Crane, *Invisible colleges. Diffusion of knowledge in scientific communities*. Chicago and London (1972).

<sup>54</sup> Attempts to design algorithms to assess scientific work are defeated by its heterogeneity. An attempt to 'grade' scientific output using 'objective' criteria based on the novelty of findings, concepts, and hypotheses yielded 36 concepts and 6 hypotheses for a two page paper.. For a discussion of this point see Ben-Ami Lipetz, *The measurement of efficiency of scientific research*. Carlisle, MA (1965).

<sup>55</sup> For example, Joseph Stiglitz, 'Incentives, risk and information: notes towards a theory of hierarchy,' *Bell journal of economics* 6 (1975), 552-579; James A. Mirlees, 'the optimal structure of incentives and authority within an organization,' *Bell journal* 7 (1976), 105-31.

<sup>56</sup> The ideas presented in this section were worked out before I read David's 'Orgins of "Open Science."' .

approach to this question would posit that the contracts having the highest net value would be the most likely to have been invented first. Yet, before the middle decades of the nineteenth century, by which time the basic institutional form assumed by science was in place, the market demand for scientific output was limited. It is thus plausible to look for explanations of the institutionalization on the supply side, in changes affecting the cost of organizing the work, rather than changes in its perceived value.<sup>57</sup>

Of the solutions, peer review was the first to be achieved, because it depended on little more than the natural desire to publish one's discoveries to the world. The critical barrier was the cost of communication.<sup>58</sup> Recruiting scientists from beyond the ranks of the leisured curious was more difficult, because it required finding a way of identifying and encouraging young scientists without giving them so much security that they ceased to be productive. Most difficult of all was developing pay and employment structures rewarding scientific performance in rough proportion to scientific contribution. Because the reward system required effective monitoring, the review system had to be in place first. The reward and signalling system had to be in place to before it was possible recruit new generations of scientists from non-wealthy backgrounds. When all the pieces were in place a science capable of expanding and employing large numbers of men and women in specialized pursuits could emerge. There was nothing, however, to ensure that solutions should be found just when the social benefit exceeded their cost. Just because something was socially desirable did not mean it was inevitable.

### *Peer Review*

Students of science and invention agree that the instant of discovery is one of intense emotional excitement, when nature seems to speak directly to the discoverer. But the moment is personal. As Adam Smith observes, we do not experience another person's tooth-ache. The desire to publish findings is largely motivated by the need to have one's vision validated by the approval of persons competent to judge. Darwin confessed in his *Autobiography* that

'I believe that I can say with truth that in after years, though I cared in the highest degree for the approbation of such men as Lyell and Hooker, who were my friends, I did not care much about the general public. I do not mean to say that a favourable review or a large sale of my books did not please me greatly, but the pleasure was a fleeting one, and I am sure that I have never turned one inch out of my course to gain fame.'<sup>59</sup>

The audience is the peer group. Scientific peer groups emerged as soon as there were men possessed of the means and leisure to investigate nature, and the desire to publish their findings to others. Their appearance was a natural consequence of scale economies

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<sup>57</sup> As always, there are exceptions. The usefulness of geology in locating mineral deposits was recognized by the 1830s, which induced significant public support for training and employing geologists..

<sup>58</sup> For a persuasive alternative view that focuses on incentives that emerged from problems associated with the patronage system, see David, 'Historical origins.'

<sup>59</sup> Cited in Hans Selye, *From dream to discovery*. New York (1964), 13-14.

in acquiring knowledge on the research frontier. As the number of persons interested and engaged in science increased, such groups emerged wherever the cost of communicating findings was not prohibitive.<sup>60</sup>

The initial institutional solution was to place the validation of scientific findings in an authorized company of scientists sitting as judges. This centralization of the function reflected a view that since science is a collective enterprise, assignment of priority in discovery ought to be carried out collectively. It was in that spirit that seventeenth-century scientific societies published findings under their collective name, on the grounds that after prolonged discussion, it was often difficult (as it still is) to disentangle individual contributions to a particular scientific result. An assembly of persons having broadly similar interests in a subject, however, does not guarantee effective judgment. Samuel Goodenough, an authority on seaweeds, complained of the Society for Promoting Natural History that

‘[T]he present Society goes on in the usual way of having a fossil or a plant go round the table; nothing is or can be said about it. It is referred to a committee to reconsider: the committee call it by some name and send it back to the society. The society desires the committee to reconsider it. In the meantime nothing is done; indeed it does not appear to me that any of them can do anything.’<sup>61</sup>

In France the system broke down when more and more experiments came to be conducted off the Academy’s premises.<sup>62</sup> Even after anonymity of contributions was abandoned, however, members of the *Académie* were forbidden from attaching their name to new work until it had been cleared by the entire company. Specialization by identity thus came about in part because of the physical impossibility of concentrating specialists and conducting experiments in one place. But decentralizing verification required an efficient medium of scientific communication.

### *Letters and Periodicals*

Prior to the late eighteenth-century the cost of publishing scientific journals for the small audiences that could effectively use them was too high for them to function as a medium for diffusing and evaluating scientific findings. With minor exceptions, there does not seem to have been any significant decline in the cost of publishing periodicals between 1600 and 1800. The presses and methods technique of printing hardly changed, while the cost of paper, which was the most expensive variable input, remained high.<sup>63</sup>

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<sup>60</sup> The attentive reader will note that this account differs fundamentally from that advanced by David, who emphasizes material incentives to cooperate in disclosing and judging scientific work. My argument draws on Smith’s premise of self-expression and the desire for approbation from peers as the psychological motivation to cooperate..

<sup>61</sup> Cited in Harold R. Fletcher, *The story of the Royal Horticultural Society, 1804-1848*. Oxford (1969), 26.

<sup>62</sup> Roger Hahn, *The anatomy of a scientific institution. The Paris Academy of Sciences, 1666-1803*. Berkeley (1971), 24-29.

<sup>63</sup> For an extensive collection data on periodical publishing costs in Germany in the eighteenth and early nineteenth century, see Joachim Kirchner, *Das deutsche Zeitschriftswesen*, Bd. 2. Weisbaden (1958-1962), 430-472, and Walter Krieg, *Materielen zu einer Entwicklungsgeschichte der Bucherpreise*. Wien (1953).

Scientific publications were costly. In the 1790s the *Annales de Chimie*, which was subsidized by the French State, cost 12 francs at a time when the daily wage was less than one.<sup>64</sup> The cost of engraving publishing in descriptive sciences like natural history prohibitive. In 1817 Royal Horticultural Society spent more than £3,000 to print its *Transactions*, which sold at over twice a English workingman's weekly wage.<sup>65</sup>

Journals, then, were not the preferred method of communicating scientific findings too brief to be written up as a book. Instead, scientists relied on personal correspondence, often formally written up.<sup>66</sup> Circulating in manuscript form, scientific letters served to announce and discuss new findings. By the middle of the seventeenth century their volume had grown to a point that warranted the emergence of clearing houses, a task performed by *Père Mersenne* and *Ismaël Boulliau* in Paris and *Henry Oldenburg* in London.<sup>67</sup> Among the advantages of correspondence was freedom from the pervasive censorship to which printed material was then subject. The volume of correspondence, however, soon became immense, though no one seems to have matched the output of the Italian humanist *Fabrizio Peregrino*, who is said to have had more than 500 correspondents and once wrote 42 letters in a single day.<sup>68</sup> These networks of scientific correspondence did not originate in the scientific community, however. Epistolary diffusion of insights and findings to selected correspondents goes back at least to *Petrarch*. Early scientists thus had no need to invent a new means of communication. They borrowed the technique from the international literary community, and well into the seventeenth century shared the same networks of correspondence.

The French *Académie des Sciences* tried to short-circuit these private and uncontrollable networks by centralizing the communication of scientific findings in its transactions, but publication was so slow the material was dated by the time it appeared in print. Personal correspondence thus remained the preferred method for establishing priority in discovery into the early eighteenth century.<sup>69</sup> The first 'learned' journals thus played a modest role in sifting and diffusing scientific discoveries, and used more as digest to be browsed by persons preparing to attend a salon than serious venues for research. As the editor of the *Journal des Sçavans*, the official organ of the *Académie des Inscriptions*, complained

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<sup>64</sup> Virginia E. Yagello, 'The early history of the chemical periodical,' *Journal of chemical education* 45 (1968), 426.

<sup>65</sup> Fletcher, *Royal horticultural society*,

<sup>66</sup> Leibniz is said to have composed a treatise in philosophy in a letter to a German princess. David A. Kronick, *A history of scientific and technical periodicals. The origins and development of the scientific and technological press, 1665-1795*. New York (1962), 50-55.

<sup>67</sup> A list of such letters is inventoried in Robert Hatch, ed. *The collection Boulliau (BN FF. 13019-13059). An inventory*. Philadelphia (1982).

<sup>68</sup> Robert A. Hatch, 'Peregrino as a correspondent: the Republic of letters and the Geography of ideas,' in *Science unbound. Geography, space, discipline*. Umea (1998),

<sup>69</sup> Michael Hunter, ed. *Archives of the scientific revolution. The formation and exchange of ideas in seventeenth-century Europe*. Woodbridge (1998).

‘Journals have been invented for the relief of those either too indolent or too occupied to read whole books. It is a means of satisfying curiosity and becoming learned with little trouble.’<sup>70</sup>

If the transactions of scientific academies did not perform the function of screening scientific production, the same was true of the non-official periodicals which first appeared in Germany during the first half of the eighteenth century. Published by booksellers, they aimed at a wide readership and were therefore printed in large runs.<sup>71</sup> Not surprisingly their mortality rate was high. Of the 167 learned German periodicals founded before 1716, 36 percent died in the first year, and only 5 percent survived five years.<sup>72</sup> The high mortality persisted through the first two thirds of the century, despite or perhaps because of the growth in the number of periodicals published. The main reason was their unspecialized readership. At a time when the number of competent specialists in individual scientific fields probably did not exceed two or three dozen persons, periodicals published for profit were poor vehicles for disseminating original scientific findings. Unlike the *Transactions* of officially sponsored scientific societies, contributions to the early scientific journals were not refereed beyond the minimal level needed to avoid censorship. Always short of material, editors accepted or translated what they could get their hands on.<sup>73</sup> German periodicals are perhaps unique in that the states subsidized publication for the benefit of their *gelehrten*, and circulation was mainly local. Although they served a valuable function in republishing foreign work and popularizing science among educated people, they could not serve as true scientific journals, which demanded editing by scientists.<sup>74</sup>

The first professional journal to accept original scientific findings date to the 1770s, with the Abbé Rozier’s *Observation sur le physique* (1771), and the series of chemical journals initiated in 1778 by Lorenz von Crell, who had studied under Black at Glasgow.<sup>75</sup> They were nevertheless edited by amateurs. The title of one of Crell’s periodicals gives a good idea of the audience to whom it was intended: *Chemische annalen für die Freunde der Naturlehre, Arzneigelehrtheit, Haushaltkunst und Manufacturen*.<sup>76</sup> The first scientific journal controlled by professional scientists was the *Annales de chimie* (1790), whose editorial board included Fourcroy, Lavoisier, Guyton de Morveau, and Berthollet.<sup>77</sup> The *Annales* was a special case precipitated by the Revolution. Elsewhere, the editorship of learned journals tended to stay in the hands of

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<sup>70</sup> Sherman B. Barnes, ‘The scientific journal, 1665-1730,’ *Scientific monthly* 38 (1934), 258.

<sup>71</sup> The historian of scientific journalism in Germany claims that the absolute minimum run was 500 volumes, and that owing to high fixed cost of distributing them few were less than 1000. Joachim Kirchner, *Die Grundlagen des deutschen Zeitschriftswesen mit einer gesamtbibliographie der deutschen Zeitschriftswesen bis zuer Jahre 1790*. Leipzig (1928-31), 54.

<sup>72</sup> Kirchner, *Die Grundlage*, 37.

<sup>73</sup> Sherman B. Barnes, ‘The editing of early learned journals,’ *Osiris* 1 (1936), 167-69

<sup>74</sup> Kirchner, *Das deutsche Zeitschriften*, 74-75.

<sup>75</sup> John L. Thornton and R. I. J. Tully, *Scientific books, libraries and collectors. A study of bibliography and the book trade in relation to science*. 3<sup>rd</sup> rev. ed. London (1971), 273-84.

<sup>76</sup> Kronick, *Scientific and technical journals*,

<sup>77</sup> Marcel Delepine, ‘Les *Annales de Chimie* de leur fondation à la 173<sup>e</sup> année de parution,’ *Annales de chimie* 13 série 7 (1962), 1.

interested amateurs. It was only in the second quarter of the nineteenth century that editorial control passed irrevocably to professional scientists.

The passage was not peaceful. Justus Liebig's transformation of a sleepy north German pharmaceutical periodical into the leading chemical journal of its day was little short of a coup. Periodicals reporting pharmaceutical and medical findings had appeared in the 1780s to keep pharmacists and physicians abreast of the latest developments in chemistry. They were essentially newsletters published by professional associations featuring news of interest to pharmacists and describing new drugs and methods of preparing them. Liebig, who 'for the sake of the money involved' agreed to co-edit the *Magazin für Pharmazie und die dahin einschlagenden Wissenschaften* installed the new regime by subjecting all chemical findings to criticism and as far as possible to verification. He renamed the journal the *Annalen der Chemie*. The innovation led to disputes with his coeditor, who quit the enterprise to found his own journal. In the first five years of his editorship, Liebig edited the *Annalen* with the help of pharmacists teaching in other universities or academies, who in effect served as referees. In 1837 he acquired total control of the journal and secured the services of Dumas and Thomas Graham, making it the most powerfully edited scientific journal of its time.<sup>78</sup> Despite this, the *Annalen's* distribution was uneven. As late as 1841 his German colleague Robert Bunsen was unaware of it.<sup>79</sup>

Although peer review continued to function on the basis of letters and books circulating among scholars and scientists, down to the early nineteenth century its supreme institutional expression remained the scientific academy, where material rewards, status and the opportunity for intense discussion with experts supplied a powerful mechanism for screening and publicizing new findings. The effectiveness of that institutional arrangement depended on the quality of the members, and between the last quarter of the eighteenth century and the first two decades of the nineteenth the French *Académie* and its successor the *Institut* assembled a company of scientific geniuses that has rarely if ever been equalled. Writing to Lakanel in the summer of 1793, Lavoisier pleaded with the member of the Convention to save the *Académie des Sciences* to reserve subsidized positions for professional scientists. The *belles lettres*, he wrote, can survive on reading and personal experience. 'Il n'en est pas de même dans les sciences: la plupart ne peuvent être cultivée avec succès par les individus isolés. Il faut une réunion d'effort.'<sup>80</sup> When qualified men assemble to hear and criticize new work, 'il en résulte une véritable sanction sans laquelle elles inspiraient moins de confiance.'<sup>81</sup> The signature of the Secretary of the First Class of the *Institut*, which was the successor to the *Académie*, constituted the seal of authentication for major discoveries; on occasion the *Institut* sat as a court to determine local priority.<sup>82</sup>

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<sup>78</sup> Kirchner, *Das Zeitschriftswesen*, Bd 1, 231-37, and Bd 2, 40-41; and H. S. van Klooster, 'The story of Liebig's *Annalen der Chemie*,' *Journal of chemical education* 34 (1957), 27-30.

<sup>79</sup> Klooster, *Story of Annalen der Chemie*, 28.

<sup>80</sup> 'It is not the same in the sciences, of which the majority cannot be prosecuted successfully by isolated individuals. They require united effort.'

<sup>81</sup> Lavoisier, *Oeuvres*. Vol. 4, Paris (1864-93), 616-17, 619.

<sup>82</sup> On June 21, 1813, the *Institut* sat in secret session to consider Arago's protest to Biot's claim of priority. Crosland, *Society of Arcueil*, 334.

Nevertheless, there were limits to what could be achieved through centralization. Already by the first decade of the nineteenth century the scientists were complaining that meetings had ceased to be scientifically useful because members lacked the specialized knowledge needed to judge and criticize topics presented to them, and because ‘real’ work was being conducted and published elsewhere. A committee composed of Fourcroy, LaPlace, Cuvier, Lacépede, and Legendre observed that

‘today so much of science is so specialized that it cannot enter into a general education, and the most highly educated man does not understand what is said unless he has devoted himself to that particular subject.’<sup>83</sup>

The substitution of periodicals for correspondence networks and scientific academies as the locus of validation of scientific work raised other barriers to the assessment and diffusion of scientific information, however. I have already noted Bunsen’s ignorance of Liebig’s *Annalen*. The Cambridge botanist James Henslow complained in 1841 to the Royal Agriculture that it was impossible to keep up with continental work on diseases in wheat because the University library didn’t carry the relevant books and journals.<sup>84</sup> Jacob Schleiden, one of the cofounders of the cell theory, was ignorant of the microscopic work by mycologists on spores, and thought that they were diseased cells. Liebig thought blights were caused by bad sap.<sup>85</sup> As scientific journalism became more specialized and more professional, the quality of the work improved, but its audience narrowed.

Scientific journalism nevertheless benefitted in the 1820s from the decline in the cost of printing resulting from the invention of lithography and the steam press. This was especially important for natural history, where graphical description is crucial to taxonomic identification. Before the fall in cost, such books were so dear they were accessible only to gentleman scholars.<sup>86</sup> Yet on the whole, declining printing cost played a modest role in the development of decentralized peer review rooted in the editorial boards of scientific journals. The primary force driving that development was simple increase in the number of practitioners and the proliferation of specialized scientific languages. Given the innate psychological need for approbation by scientific peers, simple growth in the numbers of scientists (and the number of sciences) seems to have been sufficient once scientists gained control of the publishing apparatus.

### *Solving The Problem of Recruitment*

While networks of corresponding scientists reduced the transaction cost of monitoring scientific output, they did little to encourage individual investment in the long-lived risk-prone human capital producing it. Given the specificity of that capital, prospective investors needed assurance of a steady and profitable return. For their part,

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<sup>83</sup> Crosland, *Society of Arcueil*, 159-60.

<sup>84</sup> J. S. Henslow, ‘Report on the diseases of wheat,’ *Journal of the Royal Agricultural Society* 2 (1841), 3.

<sup>85</sup> C. K. Parris, *A chronicle of plant pathology*. Starkville, MO (1968), 34-38.

<sup>86</sup> David E. Allen, *The naturalist in Britain*. London (1976), 96-99.

the purchasers of the services of that capital needed assurance of its productivity. The recruitment of scientists thus raised signalling problems on both sides of the market for specialized scientific skill. In the absence of institutions producing reliable signals of stable employment in scientific careers, investment in scientific human capital would have been restricted to the well-off or the exceptionally dedicated. Darwin is a good example of the former; the co-discoverer of the principle of natural selection, Alfred Wallace, is an example of the latter.<sup>87</sup> The emergence of a large scientific community needed a broader base.

Scientific academies were not well suited to that role. As elite institutions whose entry was awarded as an honour, membership was generally poorly rewarded – stipends in eighteenth-century France were at best two-thirds of a comfortable middle-class income. Moreover, a scientist entered the Academy only after he had made his reputation. As a means of recruiting and supporting young talent, then, Academies established to promote science were a catch-22.<sup>88</sup> In the course of the eighteenth and early nineteenth century the pool of scientific talent was deepened in two ways. The first drew on the traditional method of patronage, which exploited person-specific information. The second resulted from growing demand for skills that involved formal scientific training, which created a derived demand for science teachers and a joint supply of potential scientists. When the two combined in the *École polytechnique* in the first two decades of the nineteenth century, powerful mechanisms for screening and supporting young scientists were brought into play.

As a mechanism for recruiting scientists the traditional system of aristocratic patronage of promising young men was an effective means of acquiring information about their talent and giving them a loose expectation based on future performance. The drawback was that patronage depended in significant measure on chance encounter, was limited to relatively small numbers of recruits, and was spread across the whole range of literary and scientific fields. Crosland's history of the Society of Arcueil illustrates the changing patterns of patronage in the late eighteenth century. When Berthollet arrived in Paris in 1772, he secured an introduction to the Duke of Orléans, who in turn recommended him to Madame de Montesson as her personal physician, a post giving him enough income and time to conduct chemical research in his own laboratory. His contemporary Fourcroy was the son of an apothecary in the Duke's household. Vicq-d'Azyr, inventor of comparative anatomy, owed his ascent to the friendship of Condorcet and Turgot. In the decades preceding the Revolution, however, the source of patronage shifted to prominent scientists. Vauquelin got his entry through a letter of introduction from Fourcroy; Thénard got admitted to Fourcroy's laboratory through the intercession of Fourcroy's sisters. In the 1790s and early 1800s Arago, Poisson, and Gay-Lussac were recruited by competition among already highly selected students at the *École polytechnique* for minor teaching positions, where they distinguished themselves before their illustrious teachers. Posted to Beauvais to teach mathematics in a *lycée*, Biot

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<sup>87</sup> Michael Shermer, *In Darwin's shadow: the life and science of Alfred Russel Wallace*. Oxford: Oxford University Press (2002).

<sup>88</sup> R. Hahn, 'Scientific careers in eighteenth-century France,' in M. P. Crosland, ed., *The emergence of science in western Europe*. London (1975), 131.

secured Laplace's patronage by offering to correct the proofs of the great man's *Mécanique céleste*.<sup>89</sup>

Probably the most prominent patron at the turn of the nineteenth century was Alexander von Humboldt, who identified the talent of the young Justus Liebig and secured his entry to Gay-Lussac's laboratory. He then secured the Hessian government to create a post of extraordinary professor for him at Giessen. Ten years later, he met and encouraged the young Boussingault, one of the discoverers of the nitrogen cycle, and was off to Bogota for ten years to set up a school of engineering and study volcanos.<sup>90</sup> Sir Joseph Banks, whose spidery web of correspondence touched almost every branch of early nineteenth-century natural history, encouraged the timorous Thomas Knight to publish his research on plant physiology.<sup>91</sup> Scientific patronage, then, reproduced some of the hierarchical elements of the Academies, but in a looser and more effective way.

The ability to control the allocation of positions was the critical element of scientific patronage. Crosland documents how the creation of subordinate positions in teaching and research establishments in France provided promising young men with material means to finance their research. There were also private subsidies. Berthollet's opening his laboratory facilities at his estate in Arcueil on the outskirts of Paris to promising chemists is perhaps the best example. The crucial feature of these positions was that they remained in the giving of professional scientists with the ability to judge prospective scientific talent. The number of apprenticeships, however, was small, limiting the number of qualified persons who could be so accommodated.<sup>92</sup> The growth of scientific specialization needed a broader institutional foundation. In this respect the high degree of centralization in Paris, which had done so much to advance the prosecution of physical and natural sciences in the late eighteenth and early nineteenth century, was to prove less effective than the diffuse university system which grew up in Germany after the Reformation and the Thirty Years War to supply principalities with civil servants, priests, and medical specialists.

Whereas the Paris assistantships were explicitly allocated to recruit and support talented young scientists, by a tradition inherited from medieval times universities in Germany permitted anyone possessing an appropriate degree to teach privately for fees. In mid-nineteenth-century Germany, the position of *privat-dozent*, which was unpaid but

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<sup>89</sup> Crosland, *Society of Arcueil*, 77, 125-127, 254-55.

<sup>90</sup> Humboldt wrote Boussingault in November 1825, 'Avec les talents que la nature vous a donnés, avec une activité dans exemple, vous vous placerez parmi les hommes supérieurs qui ont illustrés votre patrie; il ne s'agit chez vous que d'avoir toujours une forte volonté. C'est comme cela que j'ai deviné d'avance ... Gay-Lussac et Arago. Je ne me tromperai pas plus sur vous que ne me suis trompé sur eux.' Cited by Louis Leouzon, *Agronomes et élèves*. Paris (1905), 321.

<sup>91</sup> Knight was apparently so shy and retiring that it was only through Banks' wheedling that he was persuaded to publish his research. He wrote to Banks in 1799, 'If I have become a troublesome scribbler to you, I must claim your pardon on the ground that you have made me such; for without the attention I have been honoured with from you, I am certain that I should never (in print) have scribbled at all. Fletcher, *Royal horticultural society*, 63-64. Knight's extremely long and verbose correspondence with Banks can be found in W. R. Dawson, ed., *The Banks Letters*. London (1958). Banks' life is chronicled in Patric O'Brian, *Joseph Banks. A Life*. London (1987).

<sup>92</sup> Crosland, 'Development of a professional career in science.'

gave access to the university library and laboratory facilities, played role similar to that of assistantships in the *Grandes Écoles*. *Dozents* typically taught small specialized courses, the fee-rich general classes being monopolized by full professors. The difference between the *dozents* and the French assistants was that the number of *dozents* was not restricted by the institution's funding. A *dozent* capable of attracting students could pay his own way. The problem of quality control, however, remained. In the course of the eighteenth century, titled professors, concerned that the growing popularity of tutorials given by *dozents* would eat into their fees, began to impose new examinations and the *habilitationschrift* as conditions for acceding to the *dozentur*. Although the initial purpose was protectionist, its unintended consequence was to transform the *dozentur* from a modest self-employment into a specialized occupation geared to research in expectation of an ultimate appointment to the professoriat.<sup>93</sup> All this lay in the future.

The biggest boost to expanding the basin of potential scientists came from growth in demand for science-based skills. The expansion of scientific education in the first quarter of the nineteenth century reflected growing demand for a few well-defined specialties, such as military engineering, pharmacy, and medicine that required basic and in some instances advanced instruction in the relevant sciences. Joint supply of skills possessing employment outside the research community reduced the risks attached to specialized scientific education, encouraging more young people to undertake it. The ability to earn a living in pharmacy provided an insurance policy for someone investing in advanced education in chemistry and plant physiology. The demand for scientific professionals thus shifted the derived demand for science teachers, providing a stable means of support that stimulated greater investment in advanced scientific skills. This effect was reinforced by certification examinations imposed by the state for physicians, pharmacists, and teachers in state-financed *gymnasias*. The need to prepare exams covering scientific material created a derived demand for courses and instructors, especially in Germany, where the exams governing eligibility to teach covered a sufficiently wide scientific area to generate a steady clientele for instructors in elementary science courses.<sup>94</sup> This development created positive feedbacks, as larger classes lowered the average cost of instruction, opening the possibility of a scientific career to a wider stratum of the population. An increasing proportion of scientists after 1830 came from the lower middle classes, for whom scientific achievement provided one of the few contemporary vehicles for social advancement..

These obvious connections help to explain the sequence of institutionalization of the individual sciences. On the basis of observational facility alone, one would expect the science of economic entomology and plant pathology to have developed before agricultural chemistry. Chemistry, however, was a required course of study for aspiring pharmacists, and by the nineteenth century was already being exploited by manufacturers who encouraged its study. Except for geologists for whom demand had economic support in mineral exploration, the sciences of natural history had few commercial uses. There were few job opportunities for mycologists and entomologists, though the rate of

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<sup>93</sup> Alexander Busch, *Die Geschichte der Privatdozenten*. Stuttgart (1959), 7-21.

<sup>94</sup> Turner, 'Growth of professional research,' 140; and Friedrich Paulsen, *Die deutschen Universitäten und das Universitätsstudium* (1902).

publication accelerated there as elsewhere.<sup>95</sup> Entomologists were generally poor men who gained their living from a grab bag of poorly paid employments.<sup>96</sup> Mycology, too, was slighted and relegated to amateurs. Only botany with its intimate link to the preparation of pharmaceutical simples had a strong base in institutionalized science.

### *Hierarchies and Selection*

New openings were not in and of themselves enough to sustain the expansion and proliferation of scientific work, because teaching positions were neither originally nor inherently research positions. Professors published books, but most of the books were lecture notes, and although nomination to the highest scientific honours demanded original contributions, a satisfactory academic career could then as now be quietly pursued as long as the teacher stayed on the good side of his colleagues and the authorities. Decentralized science conducted on a large scale required stronger incentives and stronger measures to ensure quality and productivity. One means of encouraging sustained effort was the institution of ranks or grades, ascension the passage through which exposed a scientist to repeated screening. In France the *Institut* was so organized, but once a member was admitted he could also progress through the ranks by longevity. In Germany the professorial hierarchy was well defined and strictly maintained, with two professorial ranks plus the untenured *privatdozenten*. Above the local hierarchy stood another hierarchy of institutions, with the major metropolitan universities in Berlin, Munich and Leipzig attracting better students, more funding for research laboratories, and possessing greater prestige than the smaller provincial institutions. By the 1820s Germany, then, with its professorial hierarchy and multiplicity of institutions possessed an institutional basis for continuous screening and promotion of scientific talent.

The critical element, however, was scientific control over research positions, and the imposition of scientific criteria for appointments and promotions. Although their role as a repository of information and as the place of instruction in advanced studies made educational institutions likely venues for scientific research, the development of large-scale university-based science was by no means inevitable. One can imagine the scientific enterprise supported by sinecures, as had been the case in the seventeenth and eighteenth century, and to some degree remained the case in early nineteenth-century England. The lack of original research in universities before 1800 and its thinness through much of the nineteenth century in local academies and technical schools is a warning against a too easy identification of higher learning with advanced scientific research.<sup>97</sup> In the eighteenth century, the greatest scientific achievements in universities outside philosophy were in the textual criticism of biblical sources, which inspired the first 'graduate seminars' in which students participated in the research of their professors. The classical seminar at Göttingen served as a model for Liebig's laboratory at Giessen at

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<sup>95</sup> S. L. Tuxen, 'Entomology systematizes and describes' in Ray Smith, Thomas Mittler, and Carroll N. Smith, eds. *History of entomology*. Palo Alto (1973), 95-117.

<sup>96</sup> L. O. Howard, *A history of applied entomology*. Washington (1930), 203-204; Carl H. Lindroth, 'Systematics between Fabricius and Darwin, 1800-1859,' *History of entomology*. Palo Alto (1973), 117-54.

<sup>97</sup> McClelland, *State, society and university in Germany*,

the end of the 1820s, which established the completion of an original piece of laboratory research as the criterion for a scientific Ph.D.

Turner's account of the rise of university-based science in early nineteenth-century Germany indicates that the critical events involved the seizure of academic appointments and promotions by peer groups constituting the then existing scientific community.<sup>98</sup> Paradoxically, the critical event handing control of appointments in scientific education to scientists was the Prussian government's decision to remove responsibility for appointments and promotions from local professors and confide it to a larger community of scholars. Down to the end of the Napoleonic Wars, German universities were parochial institutions whose internal affairs were managed somewhat like those of a private club. Although excellence did not go unrewarded, what mattered was collegiality and what mattered even more was good connections. Although they were in part funded by the state, like other sovereignties comprising the Holy Roman Empire, the universities were legally bound to a superior entity, but in practice quasi-independent; and since the universities, like the states of which they formed a part, were divided along religious lines, the main item of contemporary contention before the rise of atheism was off the table. Professors could thus engage in research, or spend their time teaching and writing works of scientific popularization. It didn't matter, because success in research yielded no differential reward, and indolence no penalty.

The break came with the Carlsbad Decrees in 1819. issued at Metternich's command in response to the murder of a conservative writer by a deranged student. The decree enjoined the states of the German Confederation to dissolve student associations (*burschenschaften*) and imposing censorship on university teachers. The Decrees thus effectively put the German universities under trusteeship, ordering the member states to appoint curators to vet classes and public lectures in order to ascertain and encourage the political reliability of instructors. As the bureaucrats extended their control over appointments, however, they found it useful to shield themselves against the charge of interfering in purely 'academic affairs' by drawing on the expertise of outsiders, thereby removing the power to appoint and promote from local professors who might be swayed by personal and parochial allegiances to hire political undesirables.<sup>99</sup> Subject to respecting the political constraint, it was inevitable that once the system was in place, academic scientists would impose scientific judgments on the respective merits of different candidates. The vetting process also provided a means of placig their students in academic positions. Thus, the decision to draw on expert opinion led to experts determining appointments.

With this, the last element of the institutional form that could support the expansion of science through the second half of the twentieth century fell into place. By the middle of the nineteenth century the central elements of scientific organization were all present: employment in universities and research institutions provided incomes

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<sup>98</sup> R. Steven Turner, 'The growth of professorial research in Prussia – causes and context,' *Historical studies in the physical sciences* 3 (1971), 137-82.

<sup>99</sup> McClelland, *State, society and university*; Turner, 'The growth of professional research in Prussia.' Marx was a victim of that policy.

independent of results, thus providing insurance in what was otherwise an impossibly risky enterprise; networks of peer review overcame the moral hazard associated with that insurance; while a loose hierarchy of positions allocated by the scientists themselves provided a material incentive to do good science. Much of course remained to be done. Probably the most important institutional innovation within the University was Justus Liebig's teaching laboratory at Giessen, which served as a hothouse for training research chemists until his students were able to establish daughter institutions first in Germany and later throughout the world.

That the institutionalization of science in the universities was far from inevitable can be inferred from what happened in France, where control of the University, which was comprised of all secondary school teachers licensed by the state, was in the hands of a Grand Master who controlled their advancement. Jules Simon writes of his experience as a teacher under the autocratic regime of Victor Cousin, noting that the Master personally presided over the *agrégation* (the competitive examination for teaching positions), determined where the *aggrégés* taught, and managed their careers. Because university regulations guaranteed tenure, Cousin simply left chairs open and had them taught by *chargés de cours*, who performed all the duties of a full professor but received only that part of the salary that Cousin decided to give them. Their position was precarious, as any deviation from Cousin's philosophical party line could result in exile to the provinces.<sup>100</sup> Despite the size and excellence of the French scientific establishment concentrated in Paris and perhaps because of it, the provincial universities in France never achieved the scientific prominence of provincial universities in Germany. Within the narrow confines of the *Grandes Écoles* and the great Parisian research establishment at Jussieu, the selection of scientists was rigorous and effective. But the system could not expand beyond those limits.

### *Conclusion*

By the middle of the nineteenth century a system of decentralized science much like the one we know today was in place. One sign of its robustness was the speed at which agricultural experiment stations transformed themselves into quasi-academic associations with tight links to the universities. The first station was founded in 1850 in Mockern, Saxony. By the early 1860s Germany had more than a dozen of them, and in 1862 they had formed an association supporting two 'academic' journals and holding annual meetings at which researchers presented their research. All this in the science of fertilizer analysis and bovine respiration. Successful researchers moved to larger research stations and eventually obtained professorships in universities. The incentive system perhaps played against purely practical demands for farmers for information on optimal feeding formulas for dairy cattle, but it is hard to see what other kind of organization would have sustained the research that produced the discovery that bacteria living in symbiosis with the roots of legumes were the primary source of nitrogen in the soil.

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<sup>100</sup> 'Au moindre écart, il pouvait être envoyé de Paris à Carpentras.' Cited in Antoine Prost, *L'enseignement en France, 1800-1967*. Paris (1968), xx

The emergence of this system reflected the intersection of several distinct strands of historical development that might have crossed in ways resulting in distinctly different outcomes. Institutionalized science of the type that emerged in the middle of the seventeenth century would undoubtedly have survived, along with the aristocratic science that sustained geology and natural history. But what is one to make of the less prestigious branches of enquiry that required much rote work of the kind eventually carried out by research assistants in provincial universities, or the mind-numbing experiments recorded in the papers of nineteenth-century agricultural experiment stations? The institutions of higher learning supplied an exceptionally rich soil in which science could take root and flourish. They created small societies in which ambition and success could be rewarded at relatively low cost to the outside world. That life, which in many ways (though not all) was idyllic, may be coming to an end. It was good while it lasted.