Change in Universities, “Technology Transfer”, and the Commercial World: An Irreconcilable Clash of Cultures?

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“Knowledge itself is power” (Francis Bacon).

“How, in a modern technical civilization, can one prevent the separation of technical power from moral responsibility?” (Julius Lothar Meyer, Professor of Chemistry, 1876-1895, University of Tübingen; writing in 1870s).

“To limit [physiological] research, to points where we can now see the bearings in regard to health is ‘puerile’” (Charles Darwin; early 1870s).

Summary: *The world currently faces unprecedented economic challenges, whose solution will require far-reaching innovations, in which universities, government research institutes, and commercial industries large and small, must all work together. In the last twenty years, in the UK and New Zealand (countries on which this essay primarily focuses), a style of university administration has developed, with excesses of both managerialism and demands for accountability which prevent real innovation from emerging. In their research role, this has placed short-term aims above more fundamental long-term initiatives. University staff spend too much time and effort on tasks which detract from these more important goals. This has been demoralizing and insulting. It undermines trust within universities, the integrity of scientists, the public appreciation of science, and, increasingly, it undermines science itself.*

Practical implementation of basic research done in universities (often commercial, but not necessarily so) has almost always required a set of skills, experience and habits of thought quite different from those of the scientists from whom the basic ideas originate. Often practical applications originate with people who know the human face of real practical problems, rather than from the basic researchers. When fundamental research is turned to practical uses, it usually requires talents in addition to those of persons doing the fundamental research.

Much academic research can be criticized, because it is now dominated by "game-play", entirely internal to itself, serving a number of vested interests (not least the publishing industry). Its proper function, that of providing new understanding, from which practical solutions may emerge, is, to an ever-increasing extent, ignored. Such "game-play" is exaggerated by the Research Assessment Exercise, and its equivalent in other countries, but was going on long before those policies were instituted. For far too long, leaders in academia have turned a blind eye to this; and now academia pays the price. The historic tradition of science is thus now to a large extent subverted. Until university researchers can address such criticisms, they become an easy target for excessive government control. Since the world of academic science is now international, this will require rethinking many of the national and international ways in which science is organized. This is already beginning to occur, with the increasing importance of open access, internet-based journals, with less exacting (or even no) peer review. Not least, a correct balance (and interaction) between theory and experiment needs to be achieved, especially in biomedicine and biotechnology. This would enable progress both in basic understanding, and in its practical applications to proceed more quickly, more securely and more cheaply than at present. The era of fundamental physics between 1890 and 1940 is a superb example of such fruitful interplay at its pinnacle, from which many other disciplines should learn.

In relation to "technology transfer", the proper role of university research, especially in new fields like biotechnology, should mainly be to provide a large "well" of expertise, covering a very wide range of subjects, regardless of its commercial potential (which cannot be judged in advance). Technology transfer in biotechnology differs from that based on the physical sciences. In the former case, predictions for practical applications arising from basic knowledge are far less exact and certain than those in physics-based technology. This means that there must be much more effort in testing actual *usefulness, long after the* basic principles *have been formulated. This in turn has implications for the way biotechnology should be organized, requiring styles different from those found useful in technology based on the physical sciences.*

The practical development of basic science, which may require much larger investment than the basic research, and sometimes very big risks, requires fostering a culture of mutual respect, and regular communication at many levels, and on equal footing, between academia and the commercial world. Effective deployment of basic research in the form of practical applications is most likely to arise if such a climate of continual dialogue between academia and the *commercial world is achieved. This will require change in attitudes within academia; but it will also require increasing openness and transparency within the commercial world, and adoption within that world of some of the ethos traditionally associated with universities. This is already starting to occur. In UK such a climate of mutuality and regular interaction between academia and the commercial world has not developed very well over many generations, because of radically differing attitudes within the two worlds. Examples where it worked well were in Germany (1830-1880), a period when many of our modern university traditions developed, and in more recent times in USA (based on local - state-wide - rather than nation-wide interaction between universities and commerce).*

Large industrial enterprises can often be criticised, because of their focus on their own commercial success, negating what should be the real objectives of their industry (which wider society requires), and sometimes operating way beyond any democratic control. There should be the possibility of greater public influence on such industries, since in part they use taxpayers' money, or rely on previous basic research carried out in universities at taxpayers' expense.

To provide inspiration to young people about the values of science and the technological benefits to which it leads, and to warn against the moral failures of uncontrolled large-scale technology, education in universities, for both would-be scientists, and would-be business people, should include important background courses on the history of past successes of technology, as well as honest discussion of some of its past moral failures.

1. Introduction.

In present times all countries face unprecedented economic challenges. They arise partly from increased awareness of the need to use the planet's scarce resources in optimal fashion, and partly from the fact of climate change. It matters little whether one attributes climate change to expansion of human industries, or to other factors: Either in preventing it, or (more likely) in preparing for its inevitable consequences, there are big economic challenges ahead. It has been said that our present situation is the global equivalent of war, and requires a "wartime" reorientation of major industrial economies. There are, of course, differences: The challenges are more insidious in onset, and will unfold in a more protracted way. There is no human adversary, requiring security, secrecy, or espionage. However, the issues *are* now being taken seriously by some large multinational industries with a research base, as well as by governments. Francis Bacon wrote: *"He that will not apply new remedies must expect new evils; for time is the greatest innovator."* So, as in times of war, finding solutions to these issues will also need innovation. This will certainly involve government-funded research, the research role of universities, and the fostering of a generation of researchers open to collaboration rather than obsessed with competition. These topics are the main subject of this essay.

In the last twenty years unprecedented changes have also been occurring in universities world-wide, affecting their role in both teaching and research. Apart from specific policy initiatives, these changes have been driven by a number of independent changes in wider society. Their separate influences are not always consistent with one another, and vary from one country to another. The overall objective of the essay is to analyse the impact of these changes on the general ethos of universities, especially in relation to their research role, and the procedures for funding research and related policy issues. The specific aim is to understand better the inconsistencies and conflicts which have emerged as a result of these changes, relevant to the present times, the equivalent perhaps of wartime, though waged against the forces of nature rather than against a human adversary. In the end the aim of the essay is to propose tentative solutions. The main emphasis will be on universities in the United Kingdom and New Zealand, two countries where policy changes in the last twenty years have been rather similar, and with which the author is somewhat familiar. Other countries (France, Germany, U.S.A., etc) are mentioned to provide historical or contemporary background information.

After this introduction, the *second* section reviews briefly factors which have changed the face of universities in the last twenty years. It also describes some of the challenges these factors pose for the “traditional” ethos of universities, which, admittedly, was always more of an ideal than a reality. This section also touches on some of the inherent inconsistencies and conflicts arising during this period.

The *third* section goes in more detail into an important policy initiative related to university research, implemented during this period - the Research Assessment Exercise (henceforth RAE, in UK), or Performance-Based Research Funding (PBRF, in New Zealand). This section reviews the original intentions behind these policies, what they have actually achieved in a positive sense, their shortcomings, and many distortions, inconsistencies and conflicts to which they have also led. Some of these reflect head-on challenges to the fundamental core values supposed to be inherent in a university. Others have arisen fortuitously from interplay of the different agendas implicit, as each new influence bears upon universities.

The *fourth* section moves on to another important influence on university research, recently formulated in explicit policy documents, the desire for greater public benefit, to accrue from the investment in university research. This is closely associated with efforts to achieve more efficient “technology transfer”, from “pure” academic research to practical outcomes. These “benefits” and “outcomes” are often defined in commercial terms, especially wealth creation in successful businesses; but the object of this essay also covers “public good” in a wider sense, especially since the opening paragraph drew attention to global issues, which might best be resolved in a spirit of cooperation rather than competition. The original impetus behind these efforts to improve “technology transfer” has been largely independent of the RAE (or its equivalents in other countries), and in some respects these schemes seem to *prevent* effective “technology transfer”. In this essay the critique of the RAE/PBRF will be sharper than that of the basic principle of “technology transfer” policy initiatives, although the author has many criticisms of steps so far made to implement the latter in practice. To bring this section to life, a *fifth* section reviews past successes in “technology transfer”, in order to discern useful precedents from the past.

The *sixth* and *seventh* sections discuss some intrinsic shortcomings, respectively in academic science, and in research-based activity in the commercial world. Together these form a background to recent policy changes related to technology transfer. In part these changes may have been designed to correct these shortcomings, although the conclusions and implications for policy which I draw are not congruent with those currently being implemented. The *eighth* section pulls these threads together, asking whether there is an inherent conflict between the “traditional” ethos of universities, and the values of the commercial world whose encroachment within universities has increased during this period. A central issue here is the scope of legislation intended to protect intellectual property (patent law, etc). The *last* section follows through the argument, to propose tentative solutions to some of these problems. These are somewhat speculative, because the author is in no position to implement any such changes, and is not faced with the *real politik* faced by decision makers; nor, probably, has he much ability to influence policy formulation. Nevertheless the countries about whose policies the author writes *are* supposed to be democracies. The author therefore feels impelled to make his contribution to the debates surrounding policy formation.

2. The changing face of universities: challenges and conflicts.

Policy initiatives relating directly to funding of university research have to be set against a number of other changes, affecting many countries, and many areas of life other than universities.

(i) The “information technology revolution” has radically changed the range of employment opportunities in all developed countries. Computerization of many industrial processes has meant that manual tasks, the basis for employment of a large section of the working population in former times, have largely been automated. Employment in such work has shrunk considerably. To avoid high unemployment, it has been necessary for young people entering the labour market to acquire a completely different range of work-skills. At the same time, the new technology *requires* new skills, and not only those related to information technology. As a combined result of these major changes, the proportion of young people participating in higher education has vastly increased, compared to any previous generation. This applies not only to courses directly related to information technology, but much more broadly (since older sources of employment have disappeared). As a result we have seen the “massification” of higher education, with as many as 50% of a country’s young people undertaking some form of higher education.

The “IT revolution” is not the sole cause for increased participation in tertiary education: Rising prosperity made this an aspiration for more people in UK and New Zealand, just as it did a generation earlier in U.S.A. Overall, the simple fact of greatly increased numbers of students was likely to transform universities at the institutional level, turning them into large mass-market big-business and financial organizations, fundamentally altering their ethos. Logically, the increased participation might have been expected to lead to a shift within higher education in the balance between teaching and research, in favour of the former. What actually happened is more complex.

(ii) A second chapter of background history is the origin and eventual demise of much of the non-university sector of higher education, especially in technical education. In UK, two separate developments were involved: *First,* in 1956, ten Colleges of Advanced Technology (CATs) were set up, based on pre-existing technical colleges. Initially they were mainly funded direct from government. In 1965 they became full universities. With the University Grants Committee (UGC) as a buffer, they acquired greater autonomy. According to Scott (1995) the subsequent fortunes of these universities have been mixed, some being successful (Bath, Loughborough), others not so (Aston, Salford), although they may now be gaining in status. Scott (p. 46) comments “Pure scientific excellence and academic collegiality, the standards by which they were judged and found wanting in the 1970s and 1980s, are less persuasive politically in the 1990s. Their lack of inhibitions about collaborating with industry and business, and their tauter management structures inherited from further education have been transformed from liabilities into assets.”

Separately, in the 1960s, a system of polytechnics was set up. At their inception these were clearly “secondary”, of lower status than universities. They were accountable to, and funded by local authorities, whereas university finances were determined by the UGC, which reported direct to Treasury (until the 1960s, when the Department of Education and Science was established). Located in centres of industrial cities or in inner London, they were vocational in orientation, focusing on local needs, especially provision of skilled labour. They were thus primarily teaching institutions, and did not provide research to complement that of local industries. By the 1980s it was clear that they had mainly failed to achieve their objectives. By 1981, the proportion of the age group going to the polytechnics was only 8%, compared to 5% for the universities, less than had been envisaged at the planning stage, and by no means a system of mass higher eduction.

In UK, neither universities derived from CATs, nor the polytechnics led to successful interaction at the research level with commercial and industrial enterprises - the former because they aspired to the different ethos of the older universities, the latter because they were limited to a teaching role (and provision of skilled labour). Neither type of institution came to match MIT or CalTech. In both cases, the failure was (arguably) the result of a lack of proper definition in the mid-1960s of their role as prestigious technical institutions with both research and teaching roles, whether within or outside the university sector. This, it can be argued, held back technology transfer in that period.

With advancing prosperity in UK, and wider social trends, the élitism of the older universities was increasingly challenged. Institutionally, their privileged status (e.g. reporting direct to Treasury, rather than to a special government department) was replaced, incrementally, by greater government control, as the influence of the UGC waned. The ever-more direct government influence over universities (“nationalization” is the word used by Scott, 1995), was probably inevitable, and all governments in both UK and New Zealand, whatever their colour, bear responsibility. However, the challenge to élitism did not mean that the British populus was ready to adopt truly egalitarian social structures: Élitist instincts, deep-rooted in history, remained; but everyone now wanted to be among the élite! The same tension between élitism and egalitarianism, leading eventually to stifling beaurocracy developed in German universities between the wars (Lehrer, 2007). In UK, the paradox played out in subsequent developments. Eventually, in 1992, to improve the status of polytechnics, to cater for increased student numbers, and to give the impression that more young people were amongst the élite, these institutions were converted into new universities. This - the elimination of the “binary split” in higher education - produced, and continues to produce its “backlash” from older universities, jealous to guard their status, not least in the research field (see Section 3: *Research Assessment: Origins*). In addition, increase in tertiary student numbers now means that competition for the “good” jobs is just as fierce as ever. “Undergraduates fear that the Government's drive to get half of young people into university will make degrees worthless and leave them struggling to get a good job after graduation, an official report revealed.” (*Independent*, 10.04.08). From a completely different direction, Richard Lambert (now director-general of the Confederation of British Industry; see below under

"Lambert Review") warns that businesses had "very little interest at all" in government targets and fears that the quality of graduates is being traded for mere quantity (Times Higher, 5.06.2008).

(iii) A *third* shift, as a background to current debates, is the increasing prominence of the "market philosophy". It is difficult to separate this from the personality, background and political philosophy of a single individual, Margaret Thatcher, whose influence has been world-wide. While market philosophy was always more attractive to the British Conservative party than to the Labour party, the particular way in which it was developed under Thatcher in the 1980s, and especially its impact on health and education policies, had no precedents in either political party. So, in the Jarratt report (1985), Vice Chancellors were referred to as C.E.O.s, clearly implying a much greater emphasis placed on universities as financial, as well as educational institutions, with the bottom line of a balance sheet always in mind. Higher education was to become a marketable commodity, with an increasing proportion of its costs borne by students' fees, which also made up an increasing proportion of the institution's income. It was not a true market, but a pseudo-market, since students and/or institutions were still subsidized by government to a considerable extent, and, in many universities the "consumers" (i.e. students) were, in UK, still a highly selected group, a practice impossible in marketing (say) washing machines. Nevertheless, this change undermined what had been a key feature of British (and New Zealand) universities to that date (especially in Oxford and Cambridge, but also in other universities), the personal style of much small-group teaching.

The shift to fee-paying to finance much tertiary education was of course necessary for a more pragmatic reason: With the massive growth in student numbers, the cost of higher education was becoming a significant part of public expenditure, which should therefore be budgeted and accounted for by government in a systematic way, rather being regarded as a small "add-on", as in the days when UGC reported direct to Treasury. In this sense, it was rational for the "consumer" (i.e. the students) to pay a portion of the cost of their education, which was, after all, partly a "private good" as well as a "public good".

Market philosophy has also had an impact on research funding, but more recently. Much research is expensive, and funding agencies need to be able to justify the money "invested" in research. Without such justification, many research programs can be seen as persisting signs of an élitist role of established universities. Put crudely, these programs could be regarded as allowing privileged faculty members to pursue their "expensive hobbies", at tax-payers' expense. The justification for such "blue-skies" research programs was always that "in the long term the new knowledge will be valuable, in unforeseeable ways". The entirely fair rejoinder to this, in the words of John Maynard Keynes, is: "In the long term we are all dead", with the reminder that there are many urgent practical problems which need to be addressed by research in the here and now, rather than in the long term. As a result, research grant applications increasingly have needed to be supported by arguments of likely "outcomes", especially ones which are commercially marketable, and no longer just by arguments about the likely "outputs" (papers in prestigious journals). There is however a contradiction inherent in this impact of market philosophy on research: To get "value for money" in the research field means that the researcher can, to some extent "predict" the outcomes. However, semantically at least, research is a form of "search", where *unexpected* findings may be the most interesting, and potentially most useful outcomes. There is a severe paradox in the idea of "predicting" in advance "unexpected" outcomes of research, explored later in this essay.

(iv) The *fourth* historical trend encompasses several facets, the shift to greater involvement of managerial methods in universities, the increasing demands for "accountability" (in financial and other senses), and the ever-more-direct government influence over universities. The trend to managerialism applies to areas of professional life which in no sense can be regarded as mass-market industries, such as school teachers, physicians working in general practices, even police. However, in universities, their impact has forced complex yet pervasive changes. The increasing use of managerial methods in universities has several aspects, including its historical background, the overall concept of "management" as a distinct set of skills to be used across a wide variety of situations, as well as the details of techniques encompassed by this concept. These different aspects are not mutually consistent, and are dealt with in turn below.

One of the origins of this trend can be found in the writings of James Burnham (1905-1987). Educated at Princeton, and Balliol College Oxford, he was, before WWII a communist (actually a Trotskyite), but, in 1940, renounced this philosophy, and became a spoke-person for the neo-conservative agenda in U.S.A. His most influential work "The Managerial Revolution" (1941), was written before the USA entered the war, and was a great influence on George Orwell in "Animal Farm" and "1984". As a socio-political "theory" of history, similar to Marx's "Capital", it suggested that neither capitalism nor socialism would prevail. Rather a society would emerge dominated by the "managerial class", those who control, and increasingly, would come to own the means of production. As examples of this trend he cited both Nazi Germany (which in 1940 he expected to win), and the Soviet Union (which, as expected from an ex-Trotskyite, he did not regard as truly socialist). Germany, run on "managerial" lines was altogether more efficient than Britain and France, whose industrial organization was based on "laissez faire" and market principles. The new élite of the "managerial state", like all élites, would be entirely self-serving, and at heart not at all democratic. However, (as argued in a subsequent work, written in 1942, "The Machiavellians: Defenders of Freedom"), the interests of this new "ruling class" might be served better, if, unlike Nazi Germany and the Soviet Union, it retained some trappings of democracy, freedom of speech (etc).

It is not hyperbole to trace the influence of Burnham's ideas on the encroaching managerialism in contemporary universities. Late in his life (1983), he received public recognition in the form of the presidential "Medal of Freedom", from Ronald Reagan, at a time when the neoconservative agenda was first emerging in USA, and a few years before managerial changes in universities started to appear in UK and New Zealand. His writings clearly indicate the separation of "management" from "labour", and all that implies. This was characteristic of big industry in UK a generation ago, leading to endless labour disputes. At that time such separation, and such disputes, were unheard of in universities, whose administration was generally made up of fellow academics, albeit with some special skills. This is not the case in universities nowadays, when it is easy to use the terms "management" and "labour" in the same sense as in big industry forty years ago, and where labour disputes *do* now sometimes occur. Admittedly, administrations in current universities *may* have ex-academics as their leaders, who see their role in part as defenders of older university traditions, with many inevitable compromises. Nevertheless the shift in administrative style has been enormous, with usually a decline of collegiality and the reciprocal, collegial relations between management and academic staff.

Apart from this historical background, there has emerged a trend which identifies "management" as a generic set of skills and "theory" (even regarded as a "science"[1]). From this viewpoint, all organizations are similar. A person with the requisite skills will find them sufficient to run a university, a regional health authority or an oil industry, regardless of his/her specific knowledge of the organization or enterprise being managed. Again, this is not hyperbole. In the author's own city a new appointee selected to run the regional health authority had a background in the oil industry; but major paradoxes arise from adopting such managerial assumptions within universities.

The "skills" of management are of several sorts, technical, personal, and conceptual, the former becoming less important, the latter more important as one moves from lower to higher levels of management. At the higher levels, decision making is guided by a distinct "management science" (otherwise known as "operations research"). This started off a century ago as "Taylorism", the principles behind optimising industrial production in the (then) new production-line industries. Nowadays, management science may involve computer or mathematical modeling, whose concern is the logistics, financing, game-theory etc, needed for success, especially in competitive commercial environments. Perhaps something like this may be necessary when universities become large financial corporations, competing with each other.

At the level of middle management, where personal skills are most important, I make a few points. A set of articles by G.M.Blair (1991-1993) on "Management skills", includes one on "Teams and Groups". This presents much sound advice on methods of developing group coherence, so that members of a team can work effectively and creatively together towards a defined goal, each person subjugating his or her individual interests in the interest of the group's objective. Amongst points made are recommendations on how to incorporate into the team's activities the individuals who tend to "stay silent", and how to get true participation from individuals whose natural inclination is to dominate any social group. While possibly sound in principal in the context of an industrial enterprise, these recommendations are, to say the least, impractical in the context of university research. This is because a fundamental tenet of academia has always emphasised that the actual person(s) responsible for a new finding, or a new idea, should be identified, and that specific individuals can take the credit (or, in the case of fraud, blame). Nowadays, university researchers are also encouraged to compete with one another, as individuals. University administrations may even make explicit suggestions on how staff can inflate their profile as *individuals*, rather than on how to reinforce group solidarity, in flat contradiction to recommended "Management skills" for development of effective teamwork. "Management skills" would then appear *not* to be generic, applicable here, there and everywhere in the same fashion. The recommendations are also in flat opposition to the competitive ethos encouraged by research assessment exercises (less for the "RAE" in UK, than for "PBRF" in New Zealand, since the former evaluates departments, the latter individuals). However, fostering effective teamwork certainly does not apply uniformly at all levels of big industrial concerns. At higher levels of management, where strategies for effective

[1] There is a scholarly journal with this title, concerned with "scientific research into the problems, interests and concerns of managers".

competition with commercial rivals loom large, the Machiavellian tactics of Burnham's "managerialism" are more likely to prevail.

It is also notable, that, in the articles by G.M.Blair, there are sections on "Time management", and "Delegation", but not on how higher levels of management can avoid overloading the lower levels. This is relevant to contemporary universities, where policies determined at high levels (e.g. government ministries) may place impossible demands on academics, in their multiple roles as teachers, researchers, administrators, and (apparently) entrepreneurs. So, for a university chief to say to departmental staff "You need to improve your time-management skills" really means, translating the 'newspeak', "You are not working hard enough".

Closely associated with the increase in managerial involvement in universities is the increased attention paid to "accountability", especially in the form of "quantitative performance indicators" (QPIs). "Accountability" is a general catch-all term, meaning little more than an extension of democratic responsibility from the arena of "government" (whose responsibility is to voters), to other areas of professional responsibility. Naturally enough, this extension, enforced by government, has been greatest in professions receiving greatest government support. In UK and New Zealand this includes both the health and tertiary education sectors, compared for instance with the legal profession. (In Australia, where health services receive less government support, the trend is also less.)

Use of QPIs to propel accountability, will be familiar, as an increasing stream of surveys, audits, forms requesting feedback, and other forms of assessment, which, in UK, extend even down to primary school level (see Onora O'Neill, 2002). In universities, this is associated with a proliferation of administrative jobs, with high-sounding titles such as "Quality Advancement Administrator". In the Soviet Union, under Stalin, such QPIs also flourished, as a means of optimising production (Nove, 1961). There seems to be no direct link from that history to present trends. However it is arguable that both arose from the same motive, as a lever to optimise performance, in situations where there is no natural "market", for instance where there are no "signals" from sales and profits, to guide performance. In the Soviet Union, by decree, there were no natural markets of any sort. The Soviet state was formed under the stresses of WWI and the subsequent civil war. In the 1920s, its economy was still run on wartime lines, and it never changed. Arguably, even now, nearly twenty years after the fall of the Soviet Union, its economy is still run on wartime lines. In modern Western countries, the health and education sectors are prime cases where there is no natural market, and so quantitative performance indicators have also flourished as part of the procedures for optimising performance. Such policies can be forced through most thoroughly where there is substantial state funding; and in this situation, public servants may insist on measuring performance through their own policies (rather than leaving it up to the profession itself). The invasion of QPIs into the tertiary education sector may also reflect the fact that university staff in UK and New Zealand have not been good advocates of their own importance, and have become an "easy target". In New Zealand this was true even 25 years ago, when (I remember) the prime Minister of the day, Rob Muldoon, made the casual but influential remark (no doubt, widely popular amongst the electorate) "there is a lot of fat in the universities". Since then such comments have been translated into explicit policies.

Use of QPIs to determine allocation of resources certainly *can* modify behaviour, which is its intention, but the modifications are not necessarily in the direction intended.

Clever people find ways of using the criteria to their own benefit, in ways not foreseen by those who initiate the policies. Nove (1961) gives abundant examples of this in the Soviet planned economy. Examples, as they apply to the RAE/PBRF, are discussed below.

(v) In *summary* these diverse influences have led to unprecedented challenges in universities, affecting their very nature, although their several influences are not always consistent with each other. At the very least, the “culture” of universities is undergoing a major shift. Many individual staff are disturbed, and this is not just innate conservatism: To have embarked upon a university career on the basis of one perceived set of “cultural standards”, and attitudes that go with them, and then to find that pressures from outside are enforcing a completely different set of standards (and sometimes a number of incompatible sets), *is* disturbing, at a personal level. However, if large scale changes *are* to occur, such changes are inevitable; and if present global economic challenges really do require a “wartime attitude”, arguably, we should be willing to make those changes. The fact that it is personally “disturbing” is irrelevant if the “wartime” metaphor is correct.

3. Origins and impact of “Research Assessment” policies.

In the last twenty years new policies regarding university research have developed independently on two fronts, one to assess and influence the *quality* of research, the other to influence and redirect the actual *objectives* or *direction* of research. We deal first with the former policies, and in Section 4, with those aimed at redirecting research.

Research Assessment: Origins.

In 1986, in the UK it was proposed that there be periodic nation-wide assessments of the quality of research in universities, the results being used to apportion government funding of research for each institution. Starting in 1986, the Research Assessment Exercise (RAE) was carried out, at three-yearly intervals. The latest round, scheduled for 2008, will be the last, with a substantial change in style scheduled for the future assessments (see below). Apart from background trends discussed above (expansion of student intake, the growth of “market philosophy” and managerialism, and the increasing demands for accountability), two further considerations favoured this particular initiative: One was to promote more and better quality research in universities. The other, especially after 1992, when polytechnics became universities, was to protect research funding for universities with an established research reputation from competition by the newly-formed universities. In effect this split government funding of universities into two portions, justified respectively by teaching requirements and resources needed to support research. So, when the new regime had been in place for some years, just four universities, out of a total of 170 Higher Education Institutions received around a quarter of all funds allocated according to RAE results. In recent years, according to Guardian Education (18.03.08), 84% of research funds from the Higher Education Funding Council for England go to just 25 institutions. As in the hey-day of the polytechnics, there was no suggestion that the new universities derived from polytechnics should take on, and be adequately funded for, a research role in technology, possibly in association with local industry.

The basis for the assessment in the RAE varied from one round to the next. In the 1986 round, it was based on complete publication lists, but subsequently it relied on four selected publications for each researcher, taken as his/her “output”, and included in the assessment of each department. Given this, at least in the natural sciences, the assessment was based on “impact”, actually computed as “impact factor” for each journal, itself based on comparisons between journals of the number of citations of papers in that journal in the previous two years. There are many other details to the RAE, two of which are relevant here: While the assessment was of departments and institutions, the output of only selected researchers was included, generally around 50% of university academic staff. In addition, in the natural sciences, book chapters might be included, but not monographs.

In 1993 Hong Kong implemented a modified version of the RAE, with assessment based on individuals rather than departments, and in the late 1990s several other countries showed interest in such schemes. Experience gained in UK and Hong Kong formed the background for developments in New Zealand, leading to “Performance-Based Research Funding” (PBRF), the first round of which occurred in 2002. The rationale for the policy in New Zealand was of several kinds: The funding of universities (per student) was widely seen as far from adequate. In early years after 2000, it was politically most acceptable to redress this deficiency in relation to the research rather than the teaching role of universities, especially if research could be linked to economic or social benefit. This shift could be “sold” to the public best, if it was linked with some kind of performance assessment of researchers. The policy *did* increase overall funding for universities with a research tradition. As in UK, it was also expected to protect the research capability of the older universities when many other tertiary institutions, without a research tradition had become able to grant degrees, and received government funding for doing so. As in Britain, there was tension between protection of research in older universities, and the demand to abolish élitism; and the solution provided by funding in accord with results from research assessment was an indirect (and suitably opaque) way to resolve the dilemma. A further reason for the PBRF initiative was to ensure accountability, including provision of evidence that teaching in universities, as stipulated in the Education Act of 1989, was linked to and informed by concurrent research. There were various other reasons for introducing PBRF, as described by Boston (2006), upon whose account the above comments are based. When it was introduced, as in the UK system, assessment was based on “impact factor” for selected publications, but, as in Hong Kong, assessed individual researchers rather than departments.

Research Assessment: Benefits and Critique

The positive and negative effects of the RAE/PBRF policy initiative have been widely debated. In its favour, Boston (2006) mentions (for the RAE) an improvement of quality of research (as measured), and of research management in universities, availability of better information for decision makers in government and tertiary institutions, as well as better public accountability. On the downside, he lists reduced resources for some disciplines and institutions (leading to curtailed research programs and low staff morale), the encouragement of “safe” or “traditional” rather than innovative or new research approaches, reduced focus on teaching (especially at undergraduate level), inefficiency due to competition between universities (including "poaching" of productive researchers), and the undervaluing of applied or practical aspects of research. PBRF has not been in operation long enough to evaluate its effects, but they are probably similar to those of the RAE in UK. On the basis of my own experience I offer the following comments, applying mainly to the natural sciences, some of my points being amplifications of those just made:

(i) Since the RAE/PBRF schemes evaluate impact of research within a short time-frame (only two years), they are limited to short-term impact, although often the papers with biggest impact may be digested slowly, with little immediate impact, their real impact being felt long after publication. My own first theoretical paper on psychosis, published in 1975, is only now becoming widely read and used as a basis for empirical work[2].

(ii) The emphasis on "impact factor" of journals is questionable for many reasons. "Impact" as computed, is an artificial objective, and not the same as real scientific progress. Even in its own terms, and in the short-term, assessment based on the "impact factor" of a journal does not assess the impact of a single paper, which is assessed better by the number of citations, or "hits" in an on-line publication, referring specifically to that paper. So, the RAE/PBRF policies lead, unintentionally, to accumulation of prestige for certain journals, and profits for their publishers. It is also a well documented fact from cognitive psychology that people in general (most scientists included) seek out, recall and interpret evidence that supports their prior beliefs, and are averse to that which challenges or disconfirms them. But "impact factor" is computed exactly on who seeks out and studies particular papers. Research assessment based on "impact" computations is thus biased towards the *status quo*, rather than towards innovation. "Innovation", in any sense, requires the investigator to stand aside from peer-group pressure (and therefore from the peer-review process, which is part of RAE/PBRF), and set his/her own standards. Other distortions of academic publication, especially in medicine, from unthinking reliance on "impact factor" figures, were discussed recently in BMJ (Brown, 2007)

(iii) There are serious distortions from regarding "anonymous peer review of papers", as the "gold-standard" to ensure quality. This *may* ensure standards of *methodological* rigour, but for real innovation at the level of *ideas* it is (again) usually biased towards the *status quo.* My own recent experiences in submitting theoretical papers to prestigious journals leads me to regard decisions based on anonymous peer review as often based on the fact that the journal has the power, and I don't, rather than any critical and detailed examination of my reasoning, literature cited etc. When the issues are complex, journals do not want to embark on lengthy debates, in deciding whether a paper should be published. (This has been referred to as "reviewer burnout"). I am not the only one to regard journal reviewing as the operation of a self-serving cartel (i.e. the panel of editors). In a recent conversation with a journalist in one of the quality newspapers in UK, my contact poured scorn on anonymous peer review in academic journals. For her, the best guarantee of the authenticity of a personal opinion was a signature at the foot of the piece. (I increasingly adopt this policy myself, when asked to review papers or grant applications from other researchers.) Anonymous peer review is increasingly coming to be challenged by open-access, open-reviewed, and usually internet-based journals. In

[2] On a visit I made to Cambridge University in January 2007, a full one-day symposium was based around this paper.

physics, I understand that the "ArXiv"[3] is now the first place for publication, regular journals increasingly being seen just as an afterthought. Of course the publications industry benefits greatly from emphasis on anonymous peer reviewed journals, and its role in the RAE/PBRF, because it guarantees a regular large income. I doubt if they want this to change; but that is another story.

(iv) The emphasis of RAE/PBRF, within the natural sciences, on high-impact factor journal articles rather than monographs has a serious detrimental effect on the long-term impact of scientific research, especially in development of complex and novel scientific theories. Long and complex scientific arguments, including in-depth scholarship are not possible in even the longest journal articles, yet some of the hardest scientific questions demand answers in this form. My recent work *"A neurodynamic theory of schizophrenia and related disorders"*, of 650 pages, cites approximately 5200 other sources, and is the result of over twenty years of scholarship. While it is unclear how much of the theory will stand the test of time, it is undoubtedly on a subject of great social importance, and of such enormous complexity, that it could never be addressed comprehensively in even the longest journal article, or in a book chapter in a multiple-author edited collection. In the years following the inception of RAE, university research output in terms of journal articles gradually increased, while that in monograph form fell[4]. In association with this, research scientists now seldom *read* monographs, and their skills in large-scale scholarship needed to write such works have declined. Compared with ten years ago, my impression from submitting book manuscripts to publishers is that research monographs have become greater publishing risks, from a financial point of view. My work on the theory of schizophrenia was submitted, without success, to eight international publishing houses, before I settled for an internet-based publishing organization. Ten years ago, that would not have been the case. As a form of communication, I conclude that scientific research monographs (at least those published by regular publishing houses) have become an "endangered species". The reason for including journal articles but not books, in the RAE/PBRF assessment in the natural sciences was probably not because papers were seen as intrinsically more important to science than books, but because their "impact", short-term, as it had to be, was easier to measure. As in many aspects of managerialism, decisions are taken, and policies implemented, not because one strategy or policy is intrinsically *better* than another (a judgment call which may be impossible to

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[3] This is an open access, web-based forum for physics and related fields, developed at Los Alamos National Laboratory. Rather than the usual refereeing procedure, there is a process of "moderation", whose role is limited to issues of formatting, topic, duplication, copyright, and excessive submission rate, and (I am told), rejection on scholarly content, only for most blatant breaches of standards. I am also told that this forum is becoming allied to prestigious regular journals in the field, who are coming "on side" with the new style.

[4] See for instance the following section from *Publisher's Association response to Joint Funding Bodies' Review of Research Assessment: A response from the Publishers Association to the Higher Education Funding Council for England, November 2002*: "Whilst the weight that journal articles give to RAE submission compared with books, book chapters and other 'outputs' is supposed to depend on the discipline and the views of the panel conducting the assessment, there seems to be a general view that publishing journal articles is the most effective way of getting a good rating in the RAE. The pressure to chalk up frequent publications (and the increased general pressures of academic life) is more conducive to journal papers, narrow research monographs, collections of essays and conference volumes than to major works of scholarship and synthesis, which generally require a longer gestation period. As a result, a larger proportion of the most ambitiously broad ranging and potentially influential major academic works now seem to originate in the USA or continental Europe."

make with certainty), but because the outcomes of one strategy or policy can more easily be *measured* than those of another. The ability to "measure outputs" becomes the "tail wagging the dog" of "actual outcomes".

(v) While RAE/PBRF policies do not directly affect grant funding of scientific research proposals, they have an indirect effect. Since theoretical research, which is not so glamorous from a publishing point of view, may be labour intensive, not easily delegated, but otherwise inexpensive, it has been sidelined, the emphasis shifting more and more (at least in biomedicine) to experimental projects; and these are increasingly expensive. Implicitly, success in the RAE/PBRF stakes depends increasingly on the scientist's success in raising money. (This becomes explicit in latest policy proposals from UK, as mentioned below.) In addition, the institutional evaluation of researchers in universities (for instance, for promotion) is increasingly influenced by the research grant money they bring in. The combined effect of competition in the RAE/PBRF stakes, and competition to bring grant money in to the universities produces many distortions of the research process, of the researchers themselves, and, increasingly, of science *per se*: "Ego inflation" may be encouraged by both granting agencies and universities. "Budget inflation" is also encouraged, because universities take a "top-slice" off research grants of 50% or more. The system of offering rewards and punishments is deployed explicitly to change behaviour. Sadly, it does exactly this, but at serious cost to the intrinsic motivations, and integrity of research scientists. Motives shift from the desire to obtain understanding, and to use it for public good; then to the number of papers (and their "quality"); then just to "grant money in", and with it, (no doubt) power, prestige and promotion. People of integrity may start to hate themselves for their apparent dishonesty in what they have to write to succeed in grant applications. They feel it is *insulting* (and bad for both morale and creativity) for experienced and skilled scientists to be treated like rats in a Skinner box. University administrations do claim to "value research", but, increasingly, this is not for the understanding brought about by research, or the practical benefits that research might bring, but because it helps the bottom line of their own balance sheet. The competition for RAE/PBRF ratings, and for grant monies also changes the direction of research: Under pressure from RAE/PBRF, researchers may change their field of research: to those which have high impact-factor journals; to pursuit of "glossy" projects (so the researcher, and his institution can claim "We did it first!"), or low-risk projects, which also have little chance of real benefit; or of even of the more expensive research, because it bring more funds into the university[5]. In the end, research by both the researchers themselves, and the administrations of their universities, is increasingly pursued for the wrong reasons. Winning in the RAE/PBRF and research grant stakes becomes the key motive, rather than achieving understanding of difficult and important questions, let alone realising practical benefits of research. The large research teams, and multi-author publications win; the isolated, independent-minded individual researcher loses. Pursuit of abstract ideas, including ideas with real possibility of practical applications, may cost little, needs much time, and (with the 50%+ top-slice), has little appeal to university administrations. But in the contemporary context, when we worry about optimal use of scarce resources, it is just *this* sort of research, and development of

[5] With regard to "budget inflation", I do not imply that this is by any means universal; but anecdotes I hear suggest it sometimes occurs on the initiative of either researchers themselves, or of research administrators.

the associated intellectual skills which is exactly what *should* be given more emphasis (see below).

(vi) The latest policy developments on research assessment, coming from UK have recently appeared in the form of a discussion document, on the Research Excellence Framework, scheduled to replace the RAE between 2010 and 2014. Although details remain to be finalised, assessment will still use QPIs, but, instead of "impact factor" of a journal, assessment will be based on bibliometric data (essentially, citations of each published article included in the assessment). In addition it will be based on measures of research income which a researcher (or institution) has attracted, and on number of research students. The focus on citations rather than impact of journals addresses one of the points made above, but leaves all others unanswered. The focus on research income will favour established research groups, rather than new developments, and, even more than at present, will turn leaders of each group from scientific work (their real expertise), to raising money and accounting for its use. As when any QPI is used to determine the distribution of substantial sums of money, it can be expected to have unintended, and adverse consequences. Covert agreements between researchers in different laboratories will arise to cite each other's work; and even more than at present, there will be a rush to recruit as many research students as possible, more than is good for the students, their supervisors, or the quality of their work.

(vii) Many university researchers (myself included) are dissatisfied with "ivory tower" research, and want to see practical application of their work. This may involve commercial application (and with it the desire to take out patents). It may involve non-commercial applications (say, in relation to public policy, public education etc), based on university research, and thought to be "for the public good". In my own work this included public education about the difficult subject of mental illness, which has probably been influential in New Zealand in reducing stigma and discrimination, but, at the time in 1999 when I resigned my position to go freelance, had received little recognition within the university. The environment created by the RAE/PBRF policies does not encourage realization of practical applications of university research (and was never designed to address those issues). It may actively mitigate against such applications, because the researcher's attention is focused elsewhere (not least on "quality of outputs" - as measured - rather than on applicability, which is not measured); because researchers may shift their field away from areas where practical spin-offs may arise; and because the atmosphere of openness, and free communication supposed to be part of the university ethos, and valuable for many aspects of "innovation" (whether in pure or applied science) has been compromised. This complex issue is discussed more extensively below.

"Academic Freedom" in the Contemporary University Environment

Many voices have been raised in the last fifteen years that changes occurring in universities are undermining the cherished concept of "academic freedom". Commonly there is reference to past times when things were much better, although, realistically, there never was such a "golden age". Nevertheless, the concept of academic freedom still has wide currency, not least because it was a key factor in bringing many staff (this author included) into university employment. How valid are these criticisms? Are they the result of the RAE/PBRF policies, or of these in combination with other changes discussed in Section 2?

Different traditions, and different definitions are given to the concept of “academic freedom” in different countries. In Germany, there is a long-established tradition that students can study whatever they want and at whichever university they choose. Surprisingly, this is largely the case also in most New Zealand Universities, except in the professional schools, which have selective entry, and (as of 2007), at Auckland University. Another important aspect of “academic freedom” is the freedom for staff, in their teaching role, to express whatever views they hold, without fear of censorship. This freedom, which is of more relevance in the humanities than in the natural sciences, is established in Germany, France, UK, and USA, although the exact definition of this freedom differs between these countries. These two aspects of academic freedom have little relevance here. Three other aspects of “academic freedom” *are* highly relevant.

One of these is “freedom of enquiry”. This includes freedom to question established views. This leads to controversy more often in the humanities than in the sciences, though it can be controversial in the sciences, for instance when views, backed by a staff member’s research, are expressed about public health policy. In the most-often cited abuses of such freedoms, university staff are sacked, pilloried, or otherwise punished, when they express views contrary to government views. This is mainly not the issue here; but there are other ways in which government policy can indirectly restrict freedom of enquiry. Another aspect of “freedom of enquiry”, is the freedom to research whatever the academic chooses (and to publish results, whether or not they are popular). Sometimes this freedom has been explicitly formulated. For instance, in UK, following the 1988 Education Act, the Senate of the University of Bath approved a code on Academic Freedom and Responsibilities, which included the following: “*Freedom:* To select one’s area of research, subject to constraints on the resources available; to publish subject to academic judgment. *Corresponding responsibility:* To maintain high standards of scholarship and to be responsive to reasoned discussion.” This freedom varies from institution to institution, and from department to department. Obviously when staff are appointed, they are chosen because of their potential contribution to the subject-area of that department. My own former head-of-department had a very broad perspective on this issue, and so, in an Anatomy department it was possible for me to do fundamental theoretical research on major mental illness. Nevertheless, in my last years of employment, when I was bringing no research grants in, external pressures meant I became overloaded with teaching to the extent that my research ambitions were becoming unattainable. Those external pressures were thus curtailing my “academic freedom”. Admittedly, in terms of the statement from Bath University, this was a restriction entirely due to the “resources available”. Nevertheless, the net result was that I resigned my position, to continue research in a freelance capacity. The same could happen for anyone whose research is costly in time, but not in the needs for financial support. This may apply mainly in the humanities departments.

Overall, the continuous pressure on academic staff can be seen as an insidious “trap” limiting freedom of enquiry: If you try to think long-term, you will be loaded with an excess of teaching, so that long-term research objectives become unattainable. The continual competitive pressure limits the freedom of a staff member to change the field of his/her enquiry. The pressure to recruit large numbers of research students means he or she may have little time for innovative thinking. The supervisor's responsibility to his/her research students means that he/she becomes drawn inextricably, and possibly very unwillingly, into the game of "winning" in the RAE/PBRF stakes, into the prevailing ethos that "research equals money", and all that entails (see above). The pressure to raise money may lead a principal investigator to collaborate with commercial enterprises (such as the tobacco industry, or some pharmaceutical giants) about whom serious ethical issues are raised. Because of the large financial backing needed to obtain past results, a principle investigator may lose objectivity, feeling under pressure to continue supporting past results, even when later events makes them questionable. Short of retirement, the only way to escape from this trap (for those who do not need expensive equipment, support staff, or laboratory space), may be to go freelance, as I did[6].

Another vital aspect of academic freedom, more a tradition than an explicit policy, is the ethos of "collegiality", that is an environment where open communication, and open debate, even of controversial issues can prevail. It can be argued persuasively that this ethos is vital for science *per se*, because the open, disinterested pursuit of knowledge is in turn essential for the objectivity and trustworthiness of scientific knowledge. How, for instance can the replicability of a new finding be ascertained, if it is not available for public discussion. Historically, the great strength of science is that its basic language, and its conclusions cross generations, and cross all national or cultural boundaries. This implies that environments where science can be pursued, evaluated and publicly shared, in a spirit of detached curiosity, as part of public knowledge should be protected. There are of course other considerations; but many observers now believe that that ethos is now severely compromised. Researchers are set up in competition with each other, to the great detriment of effective communication. But real progress in science is so difficult that we don't stand a chance unless we can collaborate and communicate freely; and, as argued below, the most important communication should be between dedicated theoreticians and dedicated experimentalist or applied scientists. For myself, and my own research, I do see the concept of collegiality as vital, but, if valid at all, it implies that my research should cross between generations, valid across all continents. I welcome communication from across the world; but that attitude is becoming harder to sustain for many researchers.

Many times in recent years I have tried to engage prestigious researchers in discussion about scientific topics, to be rebuffed because they are in the middle of a grants crisis. They are always in the middle of a grants crisis! This is partly because they are under continuous unrelenting pressure to perform (and these researchers have my sincere sympathy), but that is not the whole of the story. The decrease in open communication is also an indication of the excessively competitive aspects of the sociology of contemporary science. There is yet another interpretation: When rewards and punishments in any system are excessive - and it does not matter whether it is crack cocaine, or gambling or the research environment - the result is behaviour which we should call "addictive". Getting those rewards dominates the whole personality, to the exclusion of everything else, so that the original object of the exercise is totally forgotten. It is also, in my view, the biggest factor driving scientific fraud.

[6] I never had many research students to hold me back from this move, because I was not willing to take them on in theoretical projects, where they would inevitably be drawn into my own hard political battles of "experimental" versus "theoretical" research, ever-present as aspects of my own work.

The last aspect of the concept of academic freedom of relevance here is the role of universities as “critic and conscience of society”: The concept behind this high-sounding, somewhat quaint phrase was developed by Karl Jaspers (1923) in his work “The Idea of a University”, and is enshrined explicitly in the 1989 Education Act in New Zealand. The idea is much older, and, implicitly, is much more widespread in university traditions. Arguably, it is redundant in modern universities (reflecting its élitist history), especially in democratic countries like New Zealand where, in the Bill of Rights, freedom of speech is defined and guaranteed for *everyone* (to act as critic and conscience, according to their individual viewpoint). Similar legislation may soon be introduced in UK. Regardless of this, freedom to speak out *is* being compromised in Universities. I have heard of a case in a prestigious university, where an academic staff member, concerned about an issue of wide public concern, and of concern also for that university, was afraid to speak out, fearing for her job. I also hear that the academic staff who are most likely to exercise their freedom of speech are those in humanities departments, who are less likely to be financially tied to lucrative research grants than those in science departments[7]. In addition there is emerging in some universities “encouragement” from management (“Vice Chancellors” in their roles as CEOs), to adopt attitudes of “corporate loyalty” to their own institution (Gill, 2007). The Soviet Union could not have done it better. Perhaps soon it will be “Gibt deiner CEO dein ‘Jah!’” Where do principled “whistle blowers” fit into that scenario?[8]

Research Assessment: Summary: Contradictions and Conflicts

Increase in student numbers, increase of the accountability of academic staff (as teachers) to their students, as well as their role as university administrators, mitigate powerfully against both research quality, and its practical application. Observing just how hard university staff now have to work, I believe they are exploited. The managerial attitudes, if not the personal costs, are in some ways similar to those of the labour forces of nineteenth century capitalism[9]. Despite efforts to bring “market” philosophy into universities (with respect to both teaching, and via RAE/PBRF and the increasingly-competitive research funding, to research), there has actually been a great increase in state managerial control of universities. The stark (but to the author obvious) fact which has not been realised is that research, *sui generis*, holds promise not for the immediate future but for the longer-term. It may be possible to assess correctly the quality of work currently done; but it is frankly, impossible to assess it *potential for the future*, at the time

[7] Before resigning my position in 1999, I also was in a similar position, as an isolated theoretician, with little ties to the research grant system. Since then, with much looser ties to the institution, I am more certainly able to exercise my freedom to speak out on important public matters.

[8] The issue of international “collegiality” in the academic world reaches its sharpest focus when there are calls for an academic boycott of the state of Israel. However, for reasons outlined in the preceding paragraphs,those who press for such a boycott (in UK) might have more credibility if they had addressed issues of academic freedom in their own country in the last twenty years. Likewise, those in Israel who argue that “mixing science with politics and limiting academic freedom by boycotts is wrong" (Guardian, 8.5.08.), might also take notice of encroachments on academic freedom in their own country (as detailed on the website “CampusWatch” (http://www.campus-watch.org/article/id/1214).

[9] The Vice-Chancellor of Manchester University comments (Times Higher, 15.052008) that there is "something noble about the way the academic profession has fought to maintain academic standards over many years, tolerating increasing workloads and pressures ... But the truth is that this has been a gallant rearguard action. In the end gravely diminished per capita resourcing must tell on educational quality."

it is published. Any attempt to do so may use a measure which is objective in the sense of being *replicable*; but the *validity* of that measure is always questionable. Like all QPIs, used as a "lever" to optimise performance in operations where there is no natural market (or none in the short-term), its aim is explicitly to change behaviour, and this it does; but not only in the intended way, but in other unforeseen, and sometimes counterproductive ways. To get a more valid measure, one should wait fifty years. There are dangerous historical precedents for the idea that, because we claim to "understand" an historical process (in this case, the role of science in technological advance), then the time scale of that process can be collapsed by politicians who want solutions *now*. This fallacy lies hidden under current research funding policies.

The RAE/PBRF policies are one of several influences which have undermined the "traditional" ethos of universities. Lip service is paid to democratisation of universities, and doing away with élitism. Despite this, we have not really escaped from the élitism of the past, nor have we brought university research to be effectively deployed in solving real practical problems: In the new space-age academic supermarket created by RAE/PBRF, its "towers", like those of Tesco, while impersonal and stereotyped, are still constructed "mainly of ivory".

In the history reviewed above, of developments in the last forty years, there are several examples of the "tail wagging the dog". The desire to avoid the charge of élitism, was a major factor leading to the abolition of the "binary split", although élitism was surreptitiously reintroduced by other means, including introduction of RAE/PBRF policies. In turn this led to increased numbers of university academic staff, whose promotion depended substantially on research output. Because of the criteria used in RAE/PBRF for assessment, this research has tended to be mainly in pure, rather than applied areas. As a result, there is now a perception amongst politicians that too much public money is spent on research, without public benefit. Only 7% of applications to the Marsden Fund, in New Zealand (the major source of grants for "blue skies" research) are now successful. This has been taken as reflecting under-funding of basic science research; but it could also be taken as an indication that, with the expansion of the university sector, there are now too many university staff who see their research role in relation to pure rather than applied research[10]. (As pointed out early in this essay, logically speaking, the expansion of higher education might have been expected to increase the proportional expenditure on teaching *vs* research.) The expansion of pure research in universities (perhaps now excessive) has arisen in part because of the RAE/PBRF policies. They in turn were derived as a protective élitist reaction after abolition of the binary split in higher education, one of whose intentions was to eliminate élitism[11]. When the populist tail of this particular dog wagged, it had far-reaching consequences down the decades, never foreseen, in its innocence, even in its wildest day-dreams! The idea of expanding, and properly funding new universities with a defined role in technology research (perhaps in association with local industry), a role complementary to, different from, but with status similar to that of older universities, was not seriously

[10] Pure research which is experimental, like much applied research, is expensive, and the costs of such research have increased in recent years, without corresponding increase in funding. However, as argued later, the total costs of research would be reduced considerably if a better balance were to be achieved between theoretical and experimental or applied research.

[11] The ambivalence many of us feel about élitism was explicitly discussed in a recent piece in *Guardian Education*, (Tues March 18, 2008): "Should 'elite' cease to be a dirty word."

considered, until quite recently, in either UK or New Zealand. With that comment we move to the second major subject of this essay, to consider in detail the pressures to achieve greater practical outcomes from university research, in commercial or non-commercial fields.

4. Origins and impact of policies on “Technology Transfer”.

In Britain, and then in New Zealand, there has been increasing dismay, in recent decades, that other countries are better able to turn research done in their universities into commercial applications. While this concern has become a focus for explicit policies in recent years, the adverse comparison of UK with other countries is actually much older. In the author’s memory, it was a matter for concern in the 1960s and 1970s. According to the Lambert Review of Business-University Collaboration (2003, p.15) “the Paris exhibition of 1867 when Great Britain was awarded the palm of excellence in only ten of the ninety departments, was regarded as something of a national disaster.” In the late nineteenth century, the real comparison was made with Germany. Lehrer (2007) describes how, during the “golden age” of German universities (1830-1914), university researchers were regularly consulted by industry to solve problems. However, starting in about 1880, this fertile interaction was completely eroded by massive increase in student numbers, with little parallel increase in staff, increasing emphasis on academic qualifications (the “Lehrstuhl” system), which failed to accommodate specialized knowledge, as well as an increasingly autocratic “ivory tower” mentality adopted by professors (see also Lilge, 1948). Some of these changes in universities - increased student numbers, increasing emphasis on examination rather than education, possibly falling academic standards - are remarkably similar to those occurring in the UK and New Zealand in the last twenty years.

In recent years the issue came to the fore as a result of the apparent beneficial effects of the Bayh-Dole Act, passed in the USA in 1980. This Act encourages universities to file patents on inventions resulting from research occurring in their own institute. In more detail (Bekkers et al, 2006) this act encouraged non-profit research organizations (such as universities) and small businesses to acquire title to inventions developed with public support, so the institute could own spin-offs enterprises, based on intellectual property developed within its walls. Universities may not then assign their ownership of inventions to third parties, except to their own Technology Transfer Offices. As a result, universities had a strong incentive to become actively involved in management of their “intellectual property” and the resulting spin-offs. In addition, each institute was obliged to have written agreements with its faculty requiring disclosure and assignment of inventions. The institute itself, rather than the individual faculty member, then became dominant in development of any spin-offs. The Bayh-Dole Act provided for royalty sharing from patents and licences between the institute and the inventor. In licensing under patent, preference was given to “small” businesses (<500 employees), if they had the capability to bring the invention to practical application. This nation-wide law soon showed its importance, in determining whether spin-off companies could be formed in the first place. Current belief is that technology transfer from universities creates little extra revenue for the university concerned, but may help to ensure that public funding of basic research and development of technology transfer offices may benefit the local or national economy as a whole (see Heher, 2006; Mazzoleni, 2006). Even the most successful US universities tend to generate only small amounts of money from their "third stream" activities, and most acknowledge that their reason for engaging in technology transfer is to serve the public good.

It has been held that this act greatly increased transfer of technology in USA from university research laboratories to new small industries (Bekkers et al, 2006), and therefore should be emulated in other countries, in order to improve their economic performance. However this inference can be questioned. In USA, the close relation between universities and industry long predated the Bayh-Dole act, depending largely on the fact that universities in USA were financially autonomous, with much funding obtained at state or local level, rather than federal level, though with federal support of research (see Mowery and Sampat, 2004). This produced an environment where research collaboration between universities and local industries grew organically, a relation probably similar to that in the "golden age" of German universities. The argument is developed in more detail by Goldfarb and Henrikson (2003) including a sharp contrast between the "bottom-up" approach to university/industry collaboration, typical of USA, which encourages organic growth of collaboration, and the "top-down" control, from central government policies, in Sweden, which does not favour commercial spin-off from university, though intended to do so. According to Nelson (2004) the arguments used to promote the Bayh-Dole act were concentrated on the pharmaceutical industry, where patent protection is an extremely important stimulus to research and development. In other industries, patents are relatively less important. The perception of industry in USA is that university research plays a smaller part in their commercial activities in physics and chemistry than it does in the biological sciences (Mowery and Sampat, 2004). This perception of industries may reflect short-sightedness on their part, because for physics and chemistry, the "lead time" between the basic research and its commercial application is much longer than is envisaged for biotechnology, so that, for research in physics and chemistry, industry recognizes the importance of "intermediate stages" rather than the really fundamental research.

With this precedent from USA as a background, the Lambert Review of Business-University Collaboration appeared in UK in December, 2003. In its "Foreword" the point is made that British business is not research-intensive, compared to that in other countries. In New Zealand a similar, but much stronger point can be made, because New Zealand's economy has been based on its primary products, rather than products derived from those dependent on R&D. In Britain, the "Executive summary" of the Lambert review points out that in 1981, the UK's total spending on R&D as a proportion of GDP was higher than that of any other member of the G7, with the exception of Germany. By 1999, it was lagging behind Germany, the US, France and Japan, and only just keeping pace with Canada. The UK's R&D intensity was higher than international averages in two broad areas - pharmaceuticals/biotechnology and aerospace/defence, but below average in all other important areas. The UK's business research base, the Lambert Review claimed, was both narrow and fragile, and heavily dependent on the investment of a dozen large companies mainly in pharmaceuticals and defence. It may be commented here that the relative strength of UK in aerospace and defence at this time was in large part the legacy of policies developed in the early 1960s, in the wake of Harold Wilson's "White heat of technological revolution" speech. This led to developments in "big industry” such as the Anglo-French *Concorde*, which proved to be a commercial disaster (though this was admitted very late). Moreover, despite the low research investment between 1981 and 1999, the Lambert review admits that “the country’s overall economic performance improved relative to that of other developed countries.” Thus, R&D spending does not necessarily translate into successful business, which can sometimes be achieved without R&D spending. The Lambert Review emphasised increasing R&D in small and medium-sized enterprises, rather than very large ones, and on local rather than nation-wide collaborative ventures. This emphasis may have been a reflection of a similar emphasis in the Bayh-Dole Act, or a response to the commercial failure of very big projects like *Concorde.*

Following this, in mid-2004 a document entitled “Science and Innovation Investment Framework” was published by the British Treasury. This worked out the policy implications of the Lambert Review in much detail. Unlike the RAE, which was concerned with “quality” of research, this document was more concerned with changing the *direction* of research. Within it, many objectives are prefaced with the phrase “In the next ten years, key outcomes could include . . ” but not actually specifying very detailed objectives. This acknowledges the undoubted truth, that detailed outcomes of research cannot be specified in advance, although the general area where they may be found can be guessed. However, spinning off from this document, it is now an almost universal policy in UK that applicants for research grants from the country’s research councils, must specify, in addition to the scientific details of their project, the nature of the “knowledge transfer” to accrue from the project. It is clear that this intended transfer is to commercial industry, rather than non-commercial practical benefits from research. In New Zealand, similar policies are now in operation, including requirement for grant applicants to specify the “Implementation pathway” (including plans for “achieving uptake”, “capturing benefits”, “knowledge exchange mechanisms”). Sometimes sources of funding are explicitly classified by the area where commercial benefit is to be obtained. Frequent reference is made to research “contracts” rather than “proposals”, implying that the outcome is to a degree known in advance. In both countries there *are* some funding opportunities for “pure”, “blue-skies” research, not directed to any particular practical end, but in both countries, the competition for these is so intense, that there is little chance of success for most potential applicants. Clearly government policy in both countries is giving a deliberate push to make university research directed to immediate practical outcomes, with intended impact on GDP. Practical outcomes of university research without commercial potential (such as this author’s work in the field of public education about mental illness) fit nowhere into this policy, although non-commercial spin-offs from university research are very considerable (Nelson, 2001).

It is noteworthy that in these policy developments the implication for researchers is “if you want research funding, you must redirect you focus in such-and-such a way.” A study from Venezuela (Gassol, 2007) suggests that if links between university and industry are introduced as a “turn-key” rather than as a developmental process, there will inevitably be resistance within universities, resulting from the “clash of cultures”. “In university systems, where academic freedom to search for truth and knowledge is considered the basis of its existence, relations with business can be perceived as a potential threat to this freedom, and can strengthen the accountable and accounting market-oriented view of universities and hence create an environment of strong opposition to it. In addition, if the linking activities are not introduced properly within the academic structure, executors of these activities will begin to form a new academic elite, divorced from the traditional academic society, creating cracks and conflicts in the system". From a Canadian study (Landry, 2007) it was reported that the best predictor across all science fields of effective technology transfer was the close relation between researcher and user, and the focus of the researcher being directed on the user's needs. From other studies it appears that fostering informal links between university and industry favours later collaboration (Ponomariov and Boardman, 2008), and the main role of technology transfer offices in universities is in linking scientists to external resource providers (O'Gorman et al, 2008). From the Canadian study, it also emerged that the percentage of time those researchers spent on university teaching (in this study averaging 31%) was a small but significant negative predictor of technology transfer. In New Zealand, teaching loads are higher (probably ~50%, but this ignores the fact that many researchers achieve their productivity by 'burning the midnight oil', with most of their regular working hours being spent on teaching and administration), so this negative impact is likely to be higher. From these studies it is clear that policy initiatives based on the implied threat "unless you do this you will not get our support" is unlikely to succeed. Indeed it smacks rather of the research environment depicted in Solzhenitsyn's "*The First circle*"[12]. In contrast, policies have a much better chance of success if they are based on the assumption that many university researchers actually *would* like to see practical benefits from their research, but need to be made aware of the practical issues on which their research has a bearing. This requires careful attention to "bridging the cultural gap" between the university and the commercial world, and fostering the subtle "chemistry of personal respect" between scientists with greatly differing backgrounds. It also requires an administrative framework favouring "translational" or "proof of concept" funding, encouragement of "new blood" rather than established research teams. In New Zealand, I hear that this framework is notably lacking.

In this context it is becoming clear that initiatives to encourage technology transfer are now occurring to a considerable extent due to changes in the *business* rather than the university sector, and regardless of government policies for university research. In the Lambert Review the point is made that the business world is now opening up to freer collaboration with universities, the businesses themselves increasingly finding that they cannot compete unless they recruit the skills of the best researchers working outside their industry, especially those in universities. There are certainly severe tensions between the cultures of universities and the commercial worlds, as described below, but there are also pressures which may increasingly bring the university ethos to the business world, rather than, inevitably, the business ethos to universities.

There are many other factors determining whether technology transfer from universities proves a success (although inevitably they are all subject to details of time and place, and the particular situation). One of these is the *size* of the commercial organization to which technology transfer is intended. As already mentioned the emphasis in the Lambert Review was on small-to-medium sized enterprises, and on local

[12] At several points in this essay, for instance in relation to use of "quantitative performance indicators", encouragement of expressions of "institutional loyalty", and again here, a comparison is made between current administrative styles in UK and NZ, and those of the former Soviet Union. This close similarity is clearly recognized by emigrés brought up in the Soviet Union, but now working within British academia (see http://stormbreaking.blogspot.com/)

rather than nation-wide collaboration. However, from Sweden Lööf and Broström (2008) conclude that industries improve their innovation as a result of university collaboration only for firms with >100 employees (which was only about one quarter of all firms surveyed). This would have been favoured by the Bayh-Dole Act in USA, for which "small businesses" were defined as those with >500 employees. From Australia, Marceau (2007) argues that in small countries where industry is patchy, state initiatives (but sophisticated ones) are important, because few other organizations are large enough to have an impact. From experience in South Africa Heher (2006) concludes that technology transfer offices may have advantages for the local or national economy, but these are seen best when a country already has a substantial and well-supported research base, and when viewed on the large scale, and the long-term. The Lambert Review also noted the recent trend for multinational corporations tend to relocate their R&D close to largest markets. Many of the above points have particular relevance for small countries like New Zealand, where a company with 100 employees would be considered quite a large one, and where multinational companies are likely to have branches concerned with marketing, but not their research base.

Another determinant of successful technology transfer is the nature of the transfer. In the Lambert Review (Executive summary, p.5) it is stated that "that there had been too much emphasis on developing university spinout companies, a good number of which did not prove to be sustainable, and not enough on licensing technology to industry". In the US where technology transfer is freer than in most places, although "spin-out" enterprises occur with relative ease, technology transfer is, in ~90% of cases, to established firms (presumably with a research base), not start-up firms (Behrens and O'Gray, 2001). In NZ this would be difficult, because there are few firms with a research base. Perhaps the emphasis referred to critically in the Lambert Review, on spinout companies rather than licensing, is a reflection of the relatively poor research base in UK to which transfer could be made, except in pharmaceutical and defence/aerospace. In USA, MIT is the most successful institution in technology transfer, and usually does this by licensing to industry (which may be exclusive or non-exclusive). This is very relevant to New Zealand, where there are few local industries with a research base, although, since New Zealand has many links world-wide, licensing with northern hemisphere research-based industries could still occur.

The nature of the technology also has a bearing on the success of technology transfer. For technology based on the physical sciences, very fundamental research may lie at the heart of new technology, with long "lead times", but given this, once development gets close to commercial production, the merits of the product can be predicted fairly well. The role of research scientists may have finished well before the time when the product is ready for commercialisation. In biotechnology, the "lead time" may be much shorter, but prediction of the merits of the product is much less exact. As a result there may need to be lengthy and expensive clinical trials, and research scientists may need to be in continual interplay with production departments, even at late stages. In pharmaceutical companies, late-stage clinical trials, and experience obtained even after release of a new product may need to go back to the basic scientists. This is all part of the lesser predictability of new technology in the biosciences compared with the physical sciences. This issue is developed further below. It implies that the relationships between basic science and product development, and potentially that between university researchers and industry need to be very different in biotechnology compared with technology based on physical systems. Germany, which has long been well ahead of UK in many aspects of the latter, lags far behind the UK in biotechnology. It has been suggested that this is because of the different organization needed in the two industries (Lehrer, 2007)[13].

There is a further implication here for large-scale planning of biotechnology transfer. Without empirical testing it is difficult to predict, on the basis of theory, whether a particular biotechnology application will work. To ask pure researchers to specify in a research grant application the means of practical implementation of their results does not fit this model. What would be better would be for industry-based researchers seeking commercial applications to have access to the "commons" of a wide range of "pure" knowledge, from academic research, of potentially-applicable knowledge. This leads to the idea that a quasi-evolutionary system should prevail in this area. In Darwinian evolutionary theory, (or in immunology, in the "clonal selection" theory) a wide number of variants should be available, with ways of selecting those very few which meet some requirement, and the possibility of rapid and massive amplification of the successful few. Likewise, in organizing a country's research capability, especially in New Zealand, where total resources are not vast, the implication is that it would be better to have a large number of small scientific groups, studying a wide variety of topics, rather than a small number of large groups. The universities, amongst other things, should be a "well" of basic research understanding, to be tapped, as and when necessary, at times when practical applications are emerging. The research policy operating at present in New Zealand has the opposite emphasis. Of course this system assumes that there is free collaboration between university and industry-based researchers. This does not yet apply in a comprehensive way, but may come to do so in the future.

Lastly, it is very clear that there is a need for recognition of the different attitudes, skills and experience needed as one moves from university research to commercial or non-commercial implementation of that research, and with this, an appropriate "division of labour". It is unrealistic to expect university researchers, unless they are exceptionally multitalented, to have skills at every level ranging from abstract theory, experimental testing of scientific principles, insight into avenues for practical exploitation, development of these insights, testing them in practice, and ultimately commerce and marketing; yet this seems to be the assumption made in many grant-application forms. As part of this division of labour, it is becoming recognized that industry is often unwilling to invest unless there is already "proof of concept", and this may be a substantial "bottle-neck". Some USA universities have therefore started "proof-of-concept centres". (These are really "sources of funding" rather than institutes with a physical location)(Gulbranson and Audretsch, 2008). They have not existed long enough to evaluate their success in accelerating commercialisation. From US experience it is also suggested that an important determinant of the success of a spin-off company based on university "intellectual property", once formed, is to attract an external professional (rather than the original inventor) as CEO (Gallaher and Rowe, 2006). Another basis for appropriate division of labour is the setting up of "Science and technology parks" as an interface between university and business enterprises (Sofouli and Vonortas, 2007). In a recent visit to UK, I drove past one such, for the biotechnology industry in Dundee. In any case,

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[13] It may also have a cultural/historical basis, in the strong anti-biotechnology tradition in modern Germany.

the idea of a single university researcher going all the way from pure research to commercialisation is challenged: There needs to be division of labour.

5. History of the relationship between experimental and theoretical research, and their practical applications.

One can get further insight into factors leading to effective practical implementation of the results of university (and other) research from the history of technology, especially in the last two hundred years. The relationship of this to the overall thrust of this essay is indirect, but nevertheless important as background material. The subject is divided into three main sections, dealing in turn with technology based on physical science, biotechnology, and technology within New Zealand. Only some major high points are covered, with details of the personal background of key figures.

Origins of Technology from the Physical Sciences

Even from earliest days (e.g. Archimedes' screw) practical applications of research were initiated by scientists. Galileo's mathematical skills allowed him to design marketable mechanical computation devices, including one for computing compound interest. In his last years he designed an improved pendulum clock, based on the scientific principles he had worked on earlier in his life, with an escapement mechanism. It was not realized in practice until a few years after his death.

More recently, the development of new technologies, at least for those based on physical science, falls roughly into three distinct patterns. In the *first*, early development of a new technology is entirely empirical, based on practical experience in technical matters, with little systematic scientific basis, let alone abstract theory. The history of development of steam power is a good example. Thomas Newcomen, an ironmonger, not a scientist, developed the first practical, but inefficient steam engine. In the 1760s, James Black (Professor of Chemistry in the University of Glasgow), developed the concept of "latent heat" (an early step in thermodynamic theory). In a workshop in the same university department, the young James Watt designed an improved steam engine, partly influenced by Black's work. Watt himself was the son of a shipwright, with only a single year's technical education in instrument-making. Development of the improved steam engine involved investment from a local iron works, the skills of a cannon-maker in North Wales, for accurate boring of the pistons, and an individual Act of the UK parliament, to get the patent. By the early 1800s it was realized that steam engines could become more efficient, smaller, and therefore useful for transport, if they worked at higher temperature and pressure. However, although systematic measurements were made to improve the conversion of heat to movement, there was no deeper research. The theory of principles determining the efficiency of steam engines lagged far behind their practical development[14].

The development of the internal combustion engine follows a similar course, depending on scientific principles known for centuries (the force produced by the gases

[14] Carnot in 1824 producing the first forerunner of the laws of thermodynamics which were not established until the later part of the nineteenth century.

resulting from chemical reactions, as in a bullet). Preliminary, but unsuccessful attempts to use this principle for driving vehicles went back two hundred years before Benz and Daimler produced a practically-successful internal combustion engine. Gottlieb Daimler himself had little higher education, had experience as a gun-smith, and travelled in Britain and France, before becoming involved with the engineering industry. As with the steam engine, the real breakthrough lay in engineering rather than in elucidation of new scientific principles.

The *second* pattern involves systematic use of scientific research, but without deep revelations at the theoretical level. This started in Germany in the early nineteenth century. Inspired by the educational pioneers Goethe and von Humboldt, Germany was the first country to develop research-led universities. These were not only in traditional academic disciplines: One university set up at this time in Karlsruhe, was established as a University of Technology, in 1825. Karl Benz, co-developer of the internal combustion engine, trained there as an engineer. In USA, Universities with similar origins, such as MIT, and CalTech appeared later. In UK, two institutions might be placed in the same class, the University of Manchester Institute of Science and Technology (UMIST), and Imperial College London, which originated by amalgamation of three smaller institutes, the most important being the School of Mines. It is not impossible for one institute to function both as a vibrant centre for "pure" intellectual activities, and as a focus for technological and commercial innovation. At MIT, after WWII, there was a period of major curricular reform, with the founding of various humanities faculties. In the 1960s MIT was the venue for the celebrated debates between B.F.Skinner and Noam Chomsky on behaviorism; yet MIT was, and still is the prime example of a university capable of successfully driving technology transfer.

In Heidelberg, early in the nineteenth century, Liebig, a university-trained chemist, became professor, and set up the first research chemistry laboratory, coordinating activity of many researchers. As a result of his researches, he proposed practical developments for agriculture (nitrogenous fertilizers replacing dung). The modern product with brand name "Oxo" originated in one of his business concerns. Later, the German chemical industry originated in research aimed at invention of new dies, the first systematic use of university trained research scientists in industrial development. These developments were themselves based on earlier "pure research" in other German universities, not least Kekulé's discovery of the benzene ring[15].

This is not typical of British history. Michael Faraday, probably the best experimental scientist of the nineteenth century, whose pioneering experimental research led eventually to the production of electricity on an industrial scale, was himself the son of blacksmith, was self-educated, with no mathematical skills, and was held back in his early career by his socially-superior employer, Sir Humphrey Davy, until the latter's death. In his later life, Faraday was much involved in practical issues (investigating the causes of explosions in coal mines, production of optical quality glass, construction of light houses, pollution in the river Thames). However, "technology transfer" of his best

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[15] That tradition lives on in Germany. Many broad-ranging universities are still called "Technical Universities". I hear anecdotes of trainees from industry coming into German University chemistry laboratories, to learn new techniques. In a recent "ranking" of German Universities those with technical origins ranked amongst the best.

scientific work, on electricity (including design of the first dynamo in the 1820s) into industrial concerns was long delayed. Dynamos were first available commercially in the 1850s. Development of the electricity industry came only a generation later in UK (since gas was already well developed for lighting). In the 1850s, thirty years after his invention of the dynamo, he was able to remark, quite futuristically (about electricity) to William Gladstone, then Chancellor of the Exchequer "one day you may tax it". Faraday's own life was more like that of James Watt, from an earlier generation, than that of German scientists of the time, although his approach to experimental science was far more sophisticated than Watt's. In more recent history, my perception from education in UK in the 1950s and 1960s, is that from secondary school onwards, technical education has mainly been seen as of lower status than traditional academic education, although there are prestigious institutes with a technical origin, such as Imperial College and UMIST; and some smaller new universities are now establishing strong reputations in technical and applied fields of research.

The *third* pattern by which new technologies come into being, is based on new fundamental theoretical understanding. One key figure was Russian theoretical chemist, Dmitri Mendeleev. Born in Tobolsk, Siberia, and trained in Germany, he became professor of chemistry in St. Petersburg. He is best known for his version of the "periodic table of elements". Other chemists at the same time produced similar classifications of elements, but Mendeleev's was superior because he left "gaps", corresponding to elements, not yet known, whose existence, and some of whose properties, he predicted. Three of these were actually discovered within fifteen years of publishing his table. Mendeleev did not himself develop any practical applications of this work (although, separately, in his political/administrative role he encouraged development of the Russian coal mining and petroleum industries). However, two of the elements whose existence he correctly predicted - germanium, and gallium - eventually came to have important uses, not least as semiconductors.

Another key figure is James Clerk Maxwell. Coming from a wealthy family, and intellectually precocious, he did some experimental work, but was also a brilliant mathematician, able to use such a skill in several areas of physical theory. These included contributions to the kinetic theory of gases, but his most profound work was in developing concepts which unified the phenomena of light, electricity and magnetism, and led, in the early 1860s, to the wider concept of "electromagnetic radiation". This was the background from which Einstein developed the special theory of relativity. He died, aged 48, in 1879. Experimental verification of his theory of electromagnetic radiation, by Heinrich Hertz, a theoretician, with education in science and engineering, came in 1888, enabling many aspects of now-familiar modern technology, including radio, radar, television, to say nothing of mobile 'phones and satellite communication. The commercial exploitation of radio waves, in wireless telegraphy, starting in 1895, is attributed to the private Italian inventor, Guglielmo Marconi. He made no new scientific discoveries and was not the first to develop the technology. His success depended on the fact that he obtained backing from the Chief Electrical Engineer of the British Post Office, and then the British government.

In the field of nuclear physics, the role of abstract theory in realizing practical possibilities is even more remarkable. The development of nuclear physics between 1890 and 1940 was a superb example of the interplay between a rather small number of brilliant scientists engaged in essentially collaborative exploration of a new field, on budgets which, nowadays would make the proverbial shoestring seem rather extravagant. The fruitfulness of collaboration, especially that between experimentalists and theoreticians, not least that between Rutherford, the supremely-gifted experimentalist, and Neils Bohr, the expert theoretician, is an example to all other scientific disciplines. In realizing the practical possibilities in this chapter of scientific history, theoreticians clearly took the lead. A key figure in this was the Hungarian, Leo Szilard. Trained as an engineer in Hungary, and at the Technical Institute in Berlin, his work was mainly on the theory of nuclear processes. Amongst the patents he filed in Germany were those for a linear accelerator (1928), a cyclotron (1929) and an electron microscope (1931). The neutron was discovered in 1932 by James Chadwick, and within a year Szilard had realized the practical possibilities of a neutron chain reaction (patented by him in 1934). He foresaw the potential of this idea for production of nuclear power, and also its military possibilities. In 1936, as a refugee in Britain, he assigned the patent to the British Admiralty, to ensure it would remain secret. Another theoretician, Neils Bohr, predicted that an isotope of uranium (U-235) would sustain the chain reaction, proposed in principle by Szilard. This led to wartime development of the atom bomb, and, after the war, to the controlled use of nuclear fission in the nuclear power industry. Rutherford himself, in 1933 cast doubt on whether nuclear physics would ever have any practical applications. Hitler despised theoretical physics.

The insights leading to the development of programmable computers was also spear-headed by theoretical work. In the late 1930s, on the basis of most abstract work on the logical foundation of mathematics, Alan Turing conceived a machine which could implement all the consequences implicit in a given set of axioms. At first this was no more than a way of presenting a very abstract argument, but Turing, with a very literal turn of mind, foresaw how the abstraction could be turned into a real operating machine. Even at the initial stage he knew what would be needed in terms of programming skills (such as the specification, in the most minute detail, of every logical step needed for the program to work). The role of theory in some of these technological advances is summed up in the aphorism: *"There is nothing so practical as a good theory"*[16].

Biotechnology

Where does biotechnology fit into this scenario? Treatment of infectious disease is of central historical importance. This chapter of medical advance could begin until the germ theory of disease was accepted. This was a product of systematic research by Pasteur and others in the nineteenth century. Pasteur himself was not a physician, but a chemist, whose researches led him to the nascent discipline of bacteriology. His famous treatment by vaccination of a case rabies infection was preceded by other research, where the principle of vaccination, known informally for many years, was explicitly demonstrated in chicken cholera. Had his treatment for rabies failed, Pasteur faced prosecution.

A later success story was the discovery of penicillin, the first antibiotic, a substance emitted by the mould *Penicillin notatum*. Its bactericidal properties had been seen in the nineteenth century, but it was Alexander Fleming, a Scottish physician, working in

[16] Attributed to various sourcees: most recently to Kurt Lewin, to Immanuel Kant, James Clerk Maxwell, Albert Einstein, Leonid Brezhnev and others.

London, who in 1928 speculated that it might have therapeutic possibilities. However, he was sceptical of this idea, and never conducted tests of the agent in animals given lethal infections. Several circumstances came together ten years later to start production of penicillin on an industrial scale. In Germany in the mid-1930s, a much simpler molecule, prontosil, had been developed as treatment for bacterial infections. In 1939 Howard Florey, an Australian physiologist, and Ernst Chain, a German refugee with a background in the chemical industry, were inspired by the success of prontosil, and isolated the active agent from *Penicillin notatum*. Transferring research to USA, where more finance was available, the large scale manufacture of the agent assumed high priority in the next few years, given the requirements for wartime medicine.

Another chapter of early research success synthesised and defined the role of vitamins. Some of this research was closely related to local issues. Sheffield, in the early twentieth century, was an industrial city in England, noted for its urban deprivation, poor nutrition, lack of sunlight, and high prevalence of rickets. Initial work on vitamin D was carried out in the biochemistry department of Sheffield University.

Two other types of basic knowledge underlie much of the biotechnology industries, the pharmacological receptor types (and their effects) required for drug action, and, more recently, the nucleotide sequence data by which genes and their variants are specified. These data depend in turn on development of methods to identify receptor types, and the elucidation of the structure of DNA fifty years ago, both derived from advances in the physical sciences. The procedure, now standard in the pharmaceutical industry, of synthesizing large numbers of new compounds, and then screening them for the biological activity in a large number of systems was pioneered by Paul Janssen (founder of Janssen Pharmaceuticals). As the son of a Belgian general practitioner, he must have known from his earliest days of the primitive, and often dangerous nature of drug treatment in the 1940s and early 1950s. He had a medical education, but turned his back on academia, and acquired higher education in chemical and physical sciences. This was the foundation of his contribution to the pharmaceutical industry. As a result, innumerable agents with specific receptor affinities (beta-blockers for cardiovascular disease, antipsychotic drugs, muscle relaxants etc) were major innovations a generation ago. Methods to identify receptor-binding profiles of newly-synthesised molecules are now highly automated, depending on molecular modelling, or cell culture lines rather than whole organisms or organs.

The disorders for which pharmacological treatments are now sought are more complex than bacterial infections, or those for which treatment was sought in the 1960s and 1970s. For such “complex disorders”, there is no simple predictable relation between the action of an agent at a receptor, and the effects produced in intact organisms. Sometimes, as for the antipsychotic drugs, an agent’s therapeutic action is discovered by chance, and the requisite receptor-binding profile discovered later. In other cases (e.g. the development of SSRIs for depression, or the improvement of the earlier generation of antipsychotic drugs) there is a superficial theoretical basis for supposing a class of agent to have a therapeutic effect, but no proper disease theory. In such cases, apart from laboratory-based research, much effort is then needed in clinical testing of drugs, to discover their *actual* beneficial effects and possible harmful side effects. Arguably, in the last generation, the pharmaceutical industry has moved away from a secure basis in empirical science, towards a “try-it-and-see” approach, akin to early technological developments in engineering. This increases the commercial risk of drug development compared to previous times.

Clinical trials are expensive, in countries where there are rigorous regulations for quality control. The full cost of developing a new drug has been estimated at ~US$800 million, in 2001 (DiMasi et al, 2003), rising to US$1.2 Billion in 2006 (Profile 2008, Pharmaceutical Industry). As the nature of diseases targeted by new drugs shifts, the proportionate cost of developing a new drug has shifted from about 50% on preclinical research a generation ago to around 30% in recent years (DiMasi et al, 2003), with a corresponding increase in the cost of clinical trials. This increase is largely due to the increased *size* of clinical trials, perhaps a necessity, arising from the increased complexity of the diseases for which a treatment is sought (including, often, their long-term nature)[17]. The great cost of clinical trials, and the high level of commercial risk, now means that development of new drugs occurs mainly in large multinational companies. So, with spiralling costs of drug development, the pharmaceutical industry, in recent years, has put its emphasis mainly on "blockbuster drugs" (whose annual sales will exceed one billion US$ p.a.). One also suspects that, as drug development becomes more empirical rather than theory-based, with greater risks, and uncertainty (even when a drug has been launched), emphasis, greater than a generation ago is placed on marketing (now considered even before clinical testing [San Francisco Business Times, 1.12,2006]), patent protection of new products, and related legal matters. It is claimed that in 2001, the top nine US pharmaceutical companies spent 11% of sales on R&D, compared to 27% on marketing and administration (Chanana, 2006); and that, since 1995, the number of R&D jobs has remained static, while the number of marketing jobs has increased by 60% (AMSA, 2008). For average "blockbuster" drugs, earning 1-million US$ per annum, marketing expenditure has been estimated at ~$240 million (Triangle Business Journal, 24.02.2005). These vast sums have been questioned; and it is suggested that the risks are exaggerated by the industry, since most "new" drugs are modifications of older medicines, rather than new in a more radical sense (Anonymous, 2004). Moreover, effective, small-scale trials of drugs no longer under patent protection, can still be done with adequate ethical safeguards in less developed countries such as India, and China, for a tiny fraction of the cost in major developed countries. Since most of what is written on these issues, whatever their conclusions, is from parties with some vested, it is difficult to identify reliable figures. Nevertheless, the logic of the nature of diseases for which treatment is now sought supports the conclusion that costs and risks of drug development are very large and are truly escalating. Despite escalating costs the number of new chemical entities achieving sales of US$1 Billion p.a. has been falling in recent years (Grabowski, 2004) and other setbacks include legal challenges, political pressures, and competition from manufacturers of generic drugs (a topic discussed below).

Gene technology, used to develop treatment for human ailments, has not been under development long enough to know how the concept fares commercially. However, a gene variant has a relation to the effects of its expression in a whole organism which is even more complex, and less direct and predictable than that for a receptor-selective agent. Most *common* disorders with a genetic component are described as "complex disorders", the net result of many gene variants, so that a variant of a single gene often has only

[17] True pre-clinical costs may be greater, because industrial drug development makes use, free-of-charge, of knowledge gained in universities or elsewhere, funded by governments, and available in the public domain.

small, and unpredictable effects. We hear regular reports that, for this or that complex disorder, a number of genes have been found to increase the risk by 50% or so. That is not very impressive, when the increase is only from, say, 0.5% to 0.75%. High risk of those disorders involves unusual combinations of many gene variants, and we have barely started to understand gene interactions of the complexity required to make sense of the available data.

It may then be expected that the high cost of the clinical phase of drug development, typical of modern treatment for complex disorders, will apply also to gene-based therapeutics, perhaps even more than for conventional receptor-based drugs. As in any complex system (such as ecological ones), "chaotic" interactions are likely. Change of one variable has widespread effects, some of which may be compensatory, virtually impossible to predict, the overall change usually being less than anticipated on the basis of simple reasoning. In such cases, development of new treatments certainly do not equate to theory-driven "technology transfer" as in the electronics, nuclear or IT industries. So, drugs to treat complex disorders determined by multiple genes is usually aimed at alleviating symptoms, is discovered by chance, or empirically with only superficial theoretical understanding (as in the case of L-DOPA for Parkinson's disease), rather than by correcting root causes. The theory relating single-gene actions to actions in whole organisms, in relation to "complex" disorders, is (almost) "terra incognita".

A further point is that genetic disorders for which there *is* a simple relation to a single gene are generally quite *rare* in most human populations. Effective gene therapy might be possible in principle for such disorders, but the relative rarity of the disorders may mean that the new treatment are not commercially viable, because the eventual profits are not enough to justify the research outlay. Despite incorrigible optimism, gene technology is fraught with serious risks of expensive failure. Analogy with technology transfer in the physical sciences and engineering do not hold true for bio-technology. If one takes note of the fallacies so commonly made, especially the assumptions that biological systems are as simple as physical ones, and that all genetic influences are simple categorical ones such as described by Mendel, it was predictable that commercial failure, rather than success would be the rule. This has actually happened: The majority of biotechnology companies formed fifteen years ago have not survived. Biotechnology has been characterised as "one of the biggest money-losing industries in the history of mankind," according to Arthur D. Levinson (CEO of Genentech). He estimated that the biotech industry as a whole lost nearly $100 billion since Genentech, the industry pioneer, opened its doors in 1976. Only 54 of 342 publicly-traded American biotech companies were profitable in 2006, according to Ernst & Young." (Sunday New York Times, 11.02.2007; Canadian Econoview 12.02.2007).

This scenario provides a rationale for greater investment by the pharmaceutical and biotechnology industries in the theory needed to obtain better fundamental understanding of complex disorders, including proper disease theories, not least in psychiatry. Likewise in the UK government document "Science and Innovation Investment Framework", emphasis for future research is placed on finding treatments that address root causes rather than just symptoms, and how to "embed" genomics into systems biology. My own work on schizophrenia gives me insight here: Many investigators look for a simple explanation and a simple solution, generally based on models of disease occurring elsewhere in the body, or in animals. Sadly, such simple models do not work: What is needed is disease models based on very deep knowledge of normal brain mechanisms, these models inevitably being very complex. To develop such complex theories involves a little-developed, almost unprecedented style of theoretical work - combining "functional genomics", and "integrative physiology", as applied to disease processes. My theory of schizophrenia may fall in the latter category, and does make modest predictions for strategies of drug treatment. Such work involves lengthy programs of library-based study. It is quite cheap, compared to other aspects of drug development (though not easily rushed), and might be worth investment by pharmaceutical companies. Nevertheless the intractable problem remains, that in complex systems, simple changes do not produce simple predictable outcomes.

Many biological researchers, buoyed by infinite optimism, think they can find simple solutions to such problems, an attitude encouraged by both granting agencies (who should know better) and their political masters (who should be better advised). The saying is worth repeating: "To every complex problem, there is a simple solution, *and it's wrong!*" The author knows of no treatments for such complex disorders derived as predictions from full theoretical understanding. Many proven therapies for such disorders are discovered accidentally, their mechanistic bases remaining unresolved (such as clozapine, an antipsychotic drug with properties different from, and superior to any other; or, more simply, sodium bicarbonate as immediate treatment for first-degree burns). The same may be true of many herbal remedies, as used traditionally by indigenous peoples (hence the interest of pharmaceutical companies). Francis Bacon's comment -*"Nature, to be commanded, must be obeyed"*- may have its sharpest application in biotechnology.

History of Technology within New Zealand

The history of technology in New Zealand is worth reviewing briefly here. It depended essentially on the country's distinctive social history. Founded in 1840 as a new "post-enlightenment" modern state, the task of the new settlers was to construct a viable economy and way of life, starting from little except the land and the ability of its people. Great emphasis was placed on their strength and energy, and their ingenuity in adapting knowledge from overseas to local conditions, which were often rather basic. The attitude of endless ingenuity and improvisation of these pioneers is referred to even today as the "number eight fencing wire" mentality. Ernest Rutherford, New Zealand's greatest scientist was from such a background, his father being a wheelwright. In the early days, there was an astonishing spirit of innovation, when the whole country had fewer than 1 million inhabitants, and the south island (as large as England), where much of it started, only ~100,000. Some technologies were adapted from overseas, very soon after their first use elsewhere. These included engineering skills developed in the 1860s in Dunedin for the Otago gold rush, refrigerated holds in ships, devised in the 1870s and used since 1882, and hydroelectricity (the Waipori scheme developed by local investors in Dunedin, opened in 1904). None of this had a systematic research base within New Zealand, this being introduced into the meat industry only in 1955. In this respect, New Zealand followed UK traditions, where much technology arose from the skills and inventiveness of practical people, rather than from systematic industrial research, as in Germany.

The railways deserve special mention. The first railways were built in 1863 (23 years after start of modern New Zealand), the main rail network being built in the 1870s, when the total population of the country was only 250,000. Financially this was a much bigger gamble than the “Think Big” policy promoted in the late 1970s, under Prime Minister Muldoon, the equivalent of 2/3 of a year’s salary for the whole working population. Because of the terrain, and the lack of a large labour force for making extensive cuttings and embankments, railways were narrow gauge, with many tight curves. The locomotives initially came from the UK and USA, but were ill-suited for local conditions. In 1887, New Zealand started to make its own locomotives, an industry which continued until very recently. As with James Watt’s steam engines, it depended on experience and engineering skills rather than “science”. Other early inventors worked on flight, matching in some respects the Wright brothers.

Agriculture, the biggest industry, has the longest history of systematic applicable research. An early spin-off from the dairy industry started as a general store in Wellington in the earliest days of modern New Zealand, and led to J Nathan and Co, forming, in 1904, the first factory, world-wide, for production of milk powder (called Glaxo). The “research side” of this was not sophisticated, involving no more than getting the right balance between protein and cream to suit a baby’s immature digestive system. The business soon developed commercial links with UK, where the first feeding trials of Glaxo were carried out in 1906. Since then, expansion of the firm depended largely on marketing strategies within UK, and elsewhere, its commercial success being boosted by nutritional requirements for soldiers during WWI. The first scientist was employed in UK, in 1919, to supervise quality control, leading to early research in the UK on manufacture of vitamins. This industry eventually became part of the multinational company for health products, Glaxo-Smith-Kline (see: Edgar Jones: 1994).

Most systematic agricultural research in New Zealand in the first half of the twentieth century was started by, or partly supported by government, because no local industries were big enough to have their own research base. The area of New Zealand technology whose development owed most to systematic research was grasslands technology, developed from ~1910 onwards, involving development of new strains of grass and clover. The use of clover reduced the need for nitrogenous fertilizers, and is unique to the New Zealand pastoral industry. This research was soon linked to the new idea of “aerial top-dressing”, also first developed in New Zealand in the 1920s-1940s, and in routine use since then. The combination of these two techniques allowed pasture growth (in the North Island) to continue for most months of the year. The first specialist research institute (Dairy Research Institute) was set up in 1927, funded jointly by government and industry, following advice from a UK advisor. In the 1950s selective breeding of cattle was initiated to increase milk production. None of this research was initiated in universities; rather, its development in research institutes sometimes formed the nucleus around which universities were formed later. Until the last generation, most talented scientists needed to move overseas to pursue research in international contexts (Rutherford, Wilkins, McDiarmid).

Conclusions

A number of *conclusions* can be drawn from this brief but varied historical review of technology transfer. The impetus for technological advance has usually arisen because, first, there is a perceived problem (in former times, the pressing need for more efficient transport, or the ever-present danger of infectious disease), to which solutions are sought. Sometimes it is a local problem; sometimes it was special challenges, such as the needs of early settlers in New Zealand, or the unique challenges in wartime, which led to new technology, both in engineering and medicine. Hardly ever do scientists, starting from findings in the "pure research" field, then ask "how can I apply this commercially?" and then go on to develop profitable industries. When it *does* it may need artificial "creation of a market" (see below). To use technology transfer explicitly to boost a country's macroeconomic performance has little precedent, since data allowing such international comparisons of performance have not been available until the last generation[18].

Often, technological advance has developed completely separate from academia, from people, inventors, and the like, with experience, skill and talent in solving practical problems within their experience. So, in mechanical industries, it was usually engineers, rather than physicists who first realized possibilities for commercial development, as in the case of James Watt, and the Daimler/Benz collaboration. Even in theory-driven technology, this is often the case: Heinrich Hertz, and Leo Szilard were both engineer-trained theoreticians. Peter Kapitsa (one of the more practically-oriented researchers at the Cavendish Physics laboratory in Rutherford's day) was an engineer, whose work led to developments dependent on production of high magnetic fields, and very low temperatures. The special role of people whose formative experience is of practical issues often also holds true in developments in biotechnology and medicine. In New Zealand, the joint sponsorship of agricultural research by the government and the agricultural industry led to development of technology tailored to real needs of that industry. In medical research, there is often a critical interplay between clinicians who see in greatest detail the nature of a clinical problem (and its human dimensions), and the basic scientists who know most about the biological or biotechnological requirements which can solve those problems. Pasteur was primarily a scientist. The full development of what he found was realized by physicians, and scientists in other fields. Very occasionally, as in the case of Paul Janssen the same person is both the fundamental scientist, and the practical man, aware simultaneously of practical issues, scientific possibilities, and commercial realities. For my own theoretical work on schizophrenia, any practical spin-off is in the future, and I cannot foresee what it will be. In so far as it is relevant, I had the (dubious) benefit of being both a neuroscientist, and the recipient of effective treatment for such a disorder - giving me both strong motivation and close insight.

There are few examples where "blue skies" university research leads directly to commercial application, developed and marketed by the initial researcher. Almost always there is an intermediary with quite different experience and skills, an engineer rather than physicist, a businessman with marketing skills, rather than a scientist, or a doctor rather than a laboratory scientist. Often new developments come about by serendipitous combination of circumstances, or of people with very different skills and experiences (as in the industrial production of penicillin). The deeper and more abstract the theory, the less likely is the theoretician to be the one who actually develops practical applications. James Clerk Maxwell would never have guessed the practical uses to which the concept of electromagnetic radiation would so soon be put, nor would J.J.Thomson, discoverer of the electron in 1890s have guessed that electrons would be used for a new type of

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[18] I refer to the OECD statistics; but the OECD Convention of 1960 stressed that its aim was cooperation, rather than the use of its statistics as a lever for competitive advantage.

microscope, nor when James Chadwick discovered the neutron in 1932, would he have guessed that control of neutron fluxes would, within little over ten years, have led to the atom bomb, and later to nuclear power plants. Initial scientific insights can occur on a small scale, without big finance, or large scientific teams, but subsequent development needs the finance of a big organization, either a big industry, or government sponsorship. In short, technology transfer always needs collaboration of people with different skills, attitudes and "habits of thought".

We should also remember that transfer is and always was as much from commercial technology to science, as it was from science to commercial technology. Pioneering research on nuclear physics could not develop without a secure supply of electricity. The start of the discipline of pathology in the late nineteenth century relied on availability of special dies for histology, products of the new German chemicals industry. More recently, advances in medicine depended on sophisticated technology (CT, and MRI scanning, radiolabels, EEG, computer analysis of data etc), impossible without industries producing the relevant equipment or special materials.

6. How experimental science lost its way intellectually.

My comments here are based mainly on knowledge of neuroscience and related areas of biological psychiatry. They may apply more widely to biomedical research, perhaps to research within the humanities, but possibly less in the physical sciences. Readers can check whether my comments apply in the areas with which they are familiar.

Historically the critical step which brought science into existence in the seventeenth century, with pioneers such as Galileo and Newton, was that for the first time - with the possible exception of Archimedes and some Arabic scientists - sophisticated theoretical reasoning was combined with systematic empirical observation. The combination of well-reasoned theories with experiments is at the heart of science. Very often, this results in what can be called "cross-level explanations" - of phenomena at a high level in terms of data or hypotheses at a lower level. I say "hypotheses" because, very often - for instance in the case of Dalton's atomic hypothesis, or Gregor Mendel's hypothetical genetic factors - there were no techniques to verify directly the critical assumptions; so support for those assumptions was indirect, based mainly on what they would explain at the higher level. There are some examples of this in biology. Charles Darwin was well aware of the essential relationship between empirical observation and theory, when he said "No-one can be a good observer if he is not an active theorizer". Perhaps the most influential modern example of this interplay in biology was the way in which the double helix structure of DNA found its use in accounting for replication of genetic material, and, in a wider sense, the large-scale facts of Mendelian genetics.

Mainly however, in recent years, bio-medical research in the areas I know has pursued an increasingly-doctrinaire empiricist course, with less and less reference to theory, or to understanding coming from combining the two. This has involved piling up of mountains of empirical data, not guided by any preliminary theoretical notions, and with no subsequent attempt to integrate or assimilate it within the framework of theoretical constructs. I can illustrate this from my own work. My recently-completed book on the theory of schizophrenia cites about 5200 other sources. I thought it wise to cite such a large number to make sure that my conclusions could not be challenged from areas I had failed to cover, and were not based on a biased perusal of previous work. However, it is my estimate that, had the last generation of research consisted of a better balance between theory and experiment, this reference list could have been half the size, the book itself half the size, the overall expense of previous research on which the book was based perhaps one quarter of what was actually spent; and in the end my book might have been produced sooner.

One special area, covered rather briefly in this book, where, in my view, there has been an enormous excess in research expenditure, with little tangible benefit, is in the molecular genetics of schizophrenia. A few hopeful publications appeared around the year 1990, followed by an explosion of "preliminary" results, and then, regular reports of "failures to replicate". From comments made above, about the complexity of the relation between single genes and the actual phenotype in common complex disorders with a heritable tendency, it was entirely predictable that no simple relations would be found. However the research appears to have been pursued by scientists with no historical perspective, blind to the fallacies outlined above. A colossal waste of resources could have been avoided. This critique can also be applied to much of the biotechnology industry, where no expense was spared, but most companies failed to deliver.

Looking more widely at biological psychiatry, the objective of most research seems to have been to generate data, and high-impact papers, but not understanding. Despite intense research activity in this area in the last generation no true "disease theories", explaining symptoms (and other high-level - psychological - findings) in terms of fundamental postulates, have arisen. Generally the research is driven by the desire to exploit the available techniques, rather than to develop explanatory ideas. "Explanations" (so-called) usually rely on constructs derived from psychology, whose conceptual validity is often insecure, rather than on true cross-level explanations bridging between the psychological or behavioural level, and the biology of neurons in the brain. Since the symptoms and other things to be explained are also at the psychological level, there is a tendency for the reasoning to be circular. Sophisticated statistics are used, but, often apparently, *post hoc*, as a way of presenting results, rather than to test hypotheses explicitly stated in the initial design. (In principle this *should* be the standard objective of statistical tests, except when they used in a purely descriptive way). Rather, the statistics are used because sophisticated statistical packages are freely available, often purchased in association with the equipment used. So researchers come to think that statistical parameters (often "mapped" over the surface of the skull) represent real results. True theory development is better based on real physical measurements rather than statistical parameters, the latter being important only when one has definite hypotheses to test. Statistical flaws abound in presentation of results of MRI investigations, both for structural and functional MRI. Other serious fallacies apply to the inferences about neural activity from functional MRI studies, due to the complex relation between neural activity *per se*, and the blood flow or other parameters actually detected in fMRI. Usually there is the brave, but naive hope that a simple solution can be found to immensely complex issues. The latter would require much deeper knowledge of basic neuroscience than is possessed by most researchers in this field.

There are many areas of research where a cheaper approach, of "library-based theoretical research", leading to explicit predictions, combined with *carefully-selected* experiments to test these predictions, would probably bear fruit sooner in terms of real understanding, were it acceptable to research administrators. In my own field (the science of the central nervous system, and related areas of neurology and psychiatry), this could encompass many psychiatric disorders, and some neurological disorders. Here there is urgent need for the fuzzy concepts of disease with which clinicians must now work to be clarified and sharpened. There is also the large and complex subject of sleep, on which profound questions are unanswered, related to many common human problems. There has been negligible progress in this area in the last generation, probably because it is based on the currently dominant but rigid empiricist approach.

In more basic areas of neuroscience, my critique has a different emphasis. Many research papers are impressive in terms of the high intelligence in their execution and technical sophistication, and sometimes with an incisive, if small-scale input from theory. When I try to understand fundamental brain functions, such papers are undoubtedly important, each of them being small, and hard-won steps, necessary for gaining better understanding of brain function. Nevertheless, when I read such papers, increasingly I ask “what were the real motives of the researchers themselves?” I cannot convince myself that this was to provide the understanding which I myself seek. This could be done more cheaply and quickly in other ways, especially if there were a better balance between empirical data-gathering and the scholarship/theory-generation, together with the predictions it can generate. Rather the motive appears to be to go on “playing the game”, to try and “win” in this game, as determined by criteria defined entirely internal to academia worldwide, and the many smaller enclosed academic communities of which it is comprised. “Winning” in this game involves always searching for, and then magnifying out of proportion, the weaknesses or errors in other people’s research, rather than looking for how their results, if replicated can combine with other data to increase understanding. The instinct of most scientists is thus to defend or attack, with other researchers always seen as rivals rather than respected participants in a joint adventure. This attitude spreads like an infection. I also know how to play the game. The experience of having good ideas stolen, without acknowledgement, is not quickly forgotten, and leads me to modify my behaviour. Some of the papers, so impressive in one sense, appear to invite the reader to judge the paper, by itself, as evidence of its own merit (like a work of art), rather than in the enormous context of other related scientific findings. Apart from occasional mavericks like me who read widely, many papers are read only by the small isolated communities to which the papers has immediate relevance. Above all, the motivation appears to be to raise as much research grant money as possible, rather than to understanding anything.

My ambivalence in relation to research, whether basic, or targeted towards specific practical problems, applies now even to academic libraries. I have had enormous regard for good academic libraries. Nevertheless, in the fields in which I read, increasingly, I now see them as a repository not of “knowledge”, but of people’s research careers. They *do* contain information which could be used for extensive, large-scale, and important theory-construction; but are now severely under-utilized for this proper purpose.

The way in which science in these areas has lost its way, is, in my view, an effect of the directions favoured by administrators in former times, which started long before the introduction of RAE/PBRF. Some university researchers (perhaps those most unnerved by recent changes) may actually have gone into universities to get away from practical matters (and perhaps even from involvement in their moral implications). Altogether, over many decades there has been a gradual perversion of the historical traditions of science. The aim has been to produce "high quality", and then "high impact" papers, rather than real understanding. Research has become a game, increasingly detached from real goals of providing understanding, a game pursued according to rules internal to the world of "ivory tower" science, incomprehensible outside this esoteric world. Few contemporary neuroscientists (let alone researchers in biological psychiatry) appear to know what an explanation *is* any more. The scientific publishing industry, and manufacturers of scientific equipment are partners in this game. Where the real "market", providing evaluation and corrective feedback often operates only over the long-term (generations even), it is substituted by short-term QPIs. These do succeed in changing researchers' behaviour, not always in the intended way, but at the same time science loses its direction. Real science is subverted.

Under pressure from granting agencies, to justify results to "stakeholders" as "tangible benefits", we hear glossy salesmanship, usually with qualifications that it will be another ten years before the latest finding will translate into practical spin-offs. No-one captured this aspect of modern research better than Jonathan Swift, nearly 300 years ago, in "Gulliver's travels": One of the "researchers" at the Academy at Lagado

> "had been eight years upon a project for extracting sun-beams out of cucumbers . . .He did not doubt that, in eight years more, he should be able to supply the governor's gardens with sunshine at a reasonable rate. . . .I made him a small present, for my lord had furnished me with money on purpose; because he knew their practice of begging from all who go to see them."

Current research administrators and government ministers are probably aware of such criticism, though perhaps not with my own emphasis on "imbalance between experiment and theory" in many disciplines. *This perception, I suspect, is a major force driving present national policies compelling researchers to show more tangible benefit from the research they do.* I do not object to this trend in principle; but I feel that, had the criticism of current academic research been based on better and fuller analysis, different policies might have emerged, to which I could give more whole-hearted support.

7. How big commercial concerns lost their way ethically.

Regardless of *commercial* development of basic research, once that research is used for *any* applications, the door is opened to ethical dilemmas about potential uses. "Technology transfer" in some areas (public health, such as screening or public health education; road safety campaigns etc), though informed by solid research, requires complex decision-making at political levels, in its actual implementation. In the physical sciences, the sharpest ethical dilemmas tend to be few but extremely severe, as in military uses of physics technology. In biology, there were dire warnings a generation ago of the forthcoming "biological time-bomb" (Gordon Rattray Taylor, 1968). We now live in the age when we have to deal with the fall-out from that time-bomb. The issues are individually by no means as overwhelming as those with which physics had to deal with sixty years ago; but they are exceedingly complex, numerous, and expanding.

Collectively they are completely transforming our view of human nature, with profound consequences, not least in sharpening the debate on the relation between science, religion and human values, forcing fundamental rethinking on all sides.

Early in 2008 I attended a conference on schizophrenia and bipolar disorder at Montreux in Switzerland. At an opening cocktail party I got to know a representative from one of the pharmaceutical companies, whom I got on well with; and he probably realised that I knew quite a lot about schizophrenia, from an unusual perspective. On returning to New Zealand, I received an e-mail from him asking if I was interested in collaborating with the company in educational matters about mental illness. I already had a good deal of experience of this in New Zealand. I regard it as an intelligent strategy for pharmaceutical companies making antipsychotic drugs to become involved in such public education. I wrote back saying that in principle I would like to collaborate in this way. However I added some of my critique of current public-domain research on schizophrenia, and then also said that I could be critical of pharmaceutical companies. I mentioned no specific company or product. There were two specific areas where I was critical: (i) Some antipsychotic drugs, even some of the new-generation products, are, apart from their antipsychotic effects, quite sedative, and have other serious side effects. However, there has been very little research effort to define the lowest effective dose of any antipsychotic drugs, both generally, or in individual patients; and, as result, it was my impression that some patients were severely, and unnecessarily sedated, by their being prescribed doses far larger than needed. (ii) Some of the older antipsychotic medications, now being abandoned, may have had useful properties, very helpful for some patients, but these properties were never properly defined, in publicly available literature. The antipsychotic drugs have received much uninformed public criticism over the years; but my two points were not uninformed. I guess they went right to the heart of the psychopharmaceutical industry, both scientifically, and commercially, and that was the real problem for my correspondent. (Commercially, because the companies want their product to be prescribed in large rather than small doses; and they are not interested in older products, no longer under patent. In both cases, the companies want to maximise profits.) These were both fair questions to raise. Later I received a message asking if my response could be passed on to others in the company for discussion, and I replied to say that that would be appropriate. Since then, I have received no further communication; but that in itself is a clear answer. The conclusion I draw from this little story is that my questions went too near the bone, and they did not welcome discussion on these issues.

Broadening the issue, one can refer to the recent saga of the “selective serotonin reuptake inhibitors” (SSRIs) as antidepressant drugs, claimed in a recent review paper from the Psychology Department at Hull University, to have at best minimal beneficial effects. It is clear to me that this bland conclusion is far too simple, and that there are also issues at work in this story concerning professional rivalry in the UK between clinical psychologists and psychiatrists. However, the important point is that the recent paper had used “freedom of information” legislation in USA to access *all* information available to the FDA in USA, some of which had not hitherto been publicly available. As a result, the authors reached conclusions different from those based on public-domain information. A similar issue has recently emerged with regard to side effects of painkiller drugs, involving another multinational pharmaceutical company (Guardian, 16th April, 2008). I have heard of other attempts, using other means, to force pharmaceutical companies to reveal all the data from their clinical trials.

With regard to antipsychotic drugs I have further reason to suspect that important information is kept hidden. Thirty years ago, with a few later papers providing more detail, it was suggested that some patients for whom these medicines were prescribed became progressively less and less responsive to their beneficial effects, and, in the worst cases, acquired a form of psychosis unresponsive to standard drugs. Considering the great potential importance of this suggestion, it has been the subject of remarkably little subsequent research. Nevertheless, in private conversations, I hear from psychiatrists that they do recognize the pattern of events described in these few papers. In addition, some antipsychotic agents are distinctive in that, if they are withdrawn suddenly, severe psychosis can emerge very quickly, a pattern of events not seen with most antipsychotic drugs. Again, it is clear that many psychiatrists know of this pattern, and with which drugs it occurs; but there is very little published on the subject, even on basic facts, let alone on underlying mechanisms. This leads me to ask: “Do pharmaceutical companies have data on such questions which they will not release?” I do not know; but it might be expected that they would have investigated such important matters.

At present, I do not read papers on clinical trials of new drugs, because I know the picture presented is skewed. I know other university scientists who say the same thing. I *do* make predictions about possible new antipsychotic agents, but that is based on looking at preclinical data, such as receptor binding profiles (which are less likely to be biased) informed by wide theoretical understanding, and sometimes, from speaking with people who have actually received certain medications. I suspect there *is* valuable information, known, but kept secret within the industry, which would help me. Data protection by the pharmaceutical industry may be holding back drug development, or new uses for older drugs, no longer under patent protection.

I conclude that there *are* issues of real public concern about hiding of important data, and of biased presentation and use of research information, to obtain commercial gain. There are other indications of the “opaque face” of the pharmaceutical industry: I may be invited to expensive conferences run by such companies; but I do not get a chance to discuss real science with their key researchers. “Participation” of employees of pharmaceutical companies in research meetings is very different from that of scientists from academia, steering well clear of issues such as I raised above, but projecting only the corporate profile. To be blunt, the most solid realities for these industries now often appear not to be science-based, nor (arguably) even focused on patients’ well-being. If it were so, why do these companies place so much emphasis on marketing products under patent, and abandon good products no longer under patent? Patients’ well-being comes second to the primary motive of commercial success (and with it, the probable increase in expenses on administration, marketing and legal issues, compared to that on clinical trials, and even less on the scientific basis of these trials).

In a wider sphere, the commercial world puts much effort into “creating markets”. This is not in itself objectionable, in areas where there are fundamental unmet needs, which no-one ever dreamt could be met. I think of such revolutionary developments as the personal computer, and satellite communication, making world-wide immediate communication an everyday reality via e-mails and the internet. Many other aspects of modern life, now thought indispensable, at some stage depended on creation of markets;

but sometimes there appear to be ethical problems in “creating markets”. In the pharmaceutical industry, it is a common charge that many psychiatric “conditions” for which the industry provides drug treatments are arbitrary constructs, with no proper underlying disease theory, where the industry makes a killing out of newly “defined” conditions. I offer only a few comments: The term “depression” is likely to encompass a variety of quite different problems, for none of which is there at present an adequate disease theory, all currently then being lumped together under one heading, for which, supposedly a single treatment (perhaps pharmacological) will suffice. Likewise, in cardiovascular medicine, some experts argue that “statins” should be prescribed universally as prophylactics to prevent arterial disease, despite the fact that most people are at low risk. Opponents argue that this is medicine used “to treat statistics rather than patients”. Of course there are enormous commercial issues involved in these examples. The wide sales of some of the gadgetry of the electronics industry is also more a product of clever marketing than any felt need prior to the marketing. This is of course a subjective judgement; but perhaps we need a change of attitude, to limit consumption of such items, and to develop a mentality appropriate to times when optimal use of scarce resources becomes a high priority. Omnipresent and powerful advertising strategies for commercial products could be scaled down. Perhaps we would then have more fulfilled lives, freed from addiction to “retail therapy”.

In twentieth century history, the area where the sharpest ethical dilemmas for research scientists emerged was in development of military technology. Sometimes, as in the case of radar, wartime developments came to be a vital part of civilian technology. The same might be said of the development of the internet, initially designed as a possible means of communication, when, in the aftermath of nuclear war, much everyday infrastructure might have collapsed. In the most serious military development, that of nuclear weapons, what had been “ivory tower science” in the 1920s and 1930s, became, by the late 1940s and 1950s a searing moral crisis for some of the scientists involved. Notable figures in the development of nuclear weapons - Einstein, Szilard, Joseph Rotblat (founder of the Pugwash movement), and (in the Soviet Union) Andrei Sakharov - became committed activists against nuclear proliferation. Military/industrial styles of research were carried over into the post-war period, and, sixty years later, may still be a major aspect of many government’s policies for big industry.

This brings me to very big issues which should not be overlooked. Evidence just considered suggests that many big industries have no real accountability except to their shareholders, provided that laws of the land are not breached (and sometimes even that is irrelevant). In big industrialized countries major industries also often receive substantial state financial contributions. When big industry comes under state control, that may also put it beyond democratic control. Just as in big commercial undertakings with a large research base, defence industries in major industrial countries escape democratic control. It can be argued that, even in “peacetime”, large nations need to maintain defence industries, and that involves maintenance of tight security; but that does not necessitate making the defence industry into a big money-earner based on international export sales. The borderline between commercial and “defence” industries, supposedly having quite different objectives, becomes blurred. The biggest nuclear industry in the world is EDF in France, which produces 80% of the country’s electric power by nuclear technology. France also has the largest nuclear armament capability (apart from USA and Russia, where it was developed in a quite different historical context). What is the relation between the civil and defence arms of this state-run industry? Does the latter have any form of democratic sanction in France? The UK state-financed armaments industry involves large-scale apparently corrupt dealing with countries which are far from democratic. Does that have democratic sanction in UK? The exact nature of the nuclear accident at Winscale/Sellafield in UK in the late 1950s was always kept under wraps (presumably because civil and defence uses of nuclear technology were intermingled). When really big industries come under state control, they may even lose their access to prudent advice about *commercial* prospects. The Anglo-French Concorde, recently abandoned, was a major outcome of state-sponsored big industry. Commercially it was a disaster. How much of the present style of administration of big business was forged in wartime conditions of secrecy, and never relaxed? Might it not be prudent, in matters of such national importance that secrecy must prevail, that they be kept separate from regular commerce, and regular export opportunities. The latter might then operate in a more open, ethical manner.

I finish this section with the words of Julius Lothar Meyer. He is best known for producing, a few months after Mendeleev's publication of the Periodic Table of elements, a similar classification (but without the "gaps" indicating elements "yet to be found"). Educated in a technical university in Germany, he became Professor of Chemistry in the University of Tübingen in 1876. In the 1870s he asked the sharp question, one of those which heads this essay: "How, in a modern technical civilization, can one prevent the separation of technical power from moral responsibility?" (Lilge, 1948). Considering the dark clouds which brought disaster to Germany, and many other countries over the next seventy years, the prescience of these words is truly astonishing.

8. The Ethos of the University and that of the Commercial World: Today, is there fundamental incompatibility? Can rational compromise be reached?

In earlier sections of this essay, mention was made of the increasing demands for accountability in universities, and of the traditions in academia of transparency, free communication and collegiality (sometimes now threatened). However, accountability, transparency and free communication are no more than the values of a representative democracy. Perhaps even so is collegiality, in the sense that in an effective democracy the validity of the democratic system is agreed by all, as a priority which overrides all specific areas of *dis*agreement. Of course universities are no paradise, and never were; neither for that matter is a democracy. University politics, as any democratic politics, is, and always was very tough, as so powerfully demonstrated in C.P.Snow's novel "The Masters". Democracy has been defined succinctly as "a way of taking big decisions without shooting people". Despite the tough realities of both university life and national democracies, *ideals* for universities or democracies should not be abandoned just because they often do not conform to reality.

We might define the "commercial ethos", as being based on competition between enterprises, and with it a reluctance to share the knowledge base on which commercial success may depend. It is commonly assumed that, in present times, this commercial ethos will naturally prevail over the university/democratic ethos, simply because the former has far more financial muscle than the latter. From data on the USA, for 1994, it is known that, in centres for university/industry research collaboration, the centre permits agreements in which the company is given the right to block information flow in 35% of cases, and to delay publication in 50% (Behrens and Gray, 2001). Such compromises may be more characteristic of North America, as the down-side of the organic way in which university/industry collaboration has grown there, compared to UK and Europe.

However cracks are appearing in the seemingly monolithic facade of big industry. Scandals in US and Canadian universities due to pharmaceutical company sponsorship of university research with inevitable severe limits on academic freedom are widely known, and are taking their toll. Sometimes the ideals of universities and democracies *are* approximated also in the commercial world, and there is evidence that the philosophy of corporate global responsibility is increasing (Mullerat, 2005). Many large multinationals are said to be leading the way (compared with national governments) on big issues such as "carbon neutrality" and climate change. Within the pharmaceutical industry, there are increasing demands for mandatory registration of *all* clinical trials before they are started, with *full* reporting after they are completed, whatever the results. The ethical considerations surrounding subjects' participation in clinical trials may lead to legal enforcement of the requirement that pharmaceutical companies freely share all results. The pharmaceutical industry thus faces demands for democratisation. The university ethos is starting to pervade big industry.

Pharmaceutical companies now often realize that their business is so high-risk, and the stakes so high, that they should collaborate on more equal terms with academia. In the future it may be academics who "hold the aces", and who can determine, from positions of strength, the nature of the contracts they sign with pharmaceutical companies. As a small indication of this "wind of change", at a recent small conference in which I took part, the invitee from an international pharmaceutical company was a real participant in the conference proceedings, capable of free participation in the debate, and of voicing criticism of his own company, rather than giving a stereotyped projection of the corporate profile. I welcome such participation with open arms!

However, with the best will in the world, there *are* clear differences between the traditional ethos of universities and that of commercial enterprises. The "incentive structure" in academia, based, as it is, on recognition and citation of a researcher's work by others within the relevant academic community, is largely irrelevant to that in the world of commerce, and may sometimes be in opposition to it. More crucially, the ethos of universities is based on the universality of knowledge, which crosses between generations and spans all continents; the latter, at least in part, is based on competition between different enterprises, between different countries, and even between generations (since future generations sometimes pay the price for commercial success in the present generation). This *is* fundamentally opposed to the open, disinterested pursuit of knowledge, which, in turn, is essential for the objectivity and trustworthiness of scientific knowledge. The great strength of science (historically) is that its basic language, and its conclusions cross generations and all national or cultural boundaries. This seems to imply that "pure" science, pursued without clear practical aims, publicly shared, evaluated, as part of the public "knowledge base" should come first; its commercial application, where some degree of competitiveness is undoubtedly also needed, is then secondary. There are few if any major technological developments where commercial ethos (as defined above), and with it secrecy of its knowledge base led from the beginning. Leaders of industry recognize this: The director of Philips Research Laboratories - Hendrik Casimir - was quoted as saying “I think there is hardly any example of twentieth century innovation which is not indebted [...] to basic scientific thought” (Moriarty, 2008). Many of the debates within academia, about “blue skies” vs “targetted” research, and the shifting emphases of the corresponding policies, occur as much within big industries like Philips as they do in universities.

A central issue for the interface between commercial and academic worlds concerns intellectual property (patenting and other form of legal protection). In principle, “intellectual property” refers to legal protection for invention, brands, designs and creative works in the form of patents, copyright, designs and trademarks. This concept, in principle, applies to “technology transfer” but not to “knowledge transfer”. So, “patent law”, “trade secrets”, and “intellectual property” might include (in addition to inventions, designs, and trademarks and artistic creations) music, literature, techniques, processes, symbols, names, images, circuit designs, databases, even plant breeds or varieties, but not “knowledge” itself. *In principle*, this is nearly the same as the distinction between “science” and “technology”, or between “knowledge” and “practical inventions”.

It *can* be argued, in a general way, that science and technology are *in*separable. *First*, as mentioned above, science depends as much on advance in industrial-scale technology as does technology on science. *Second,* much (perhaps most) research in universities is not establishing new fundaemental scientific principles, but “filling in gaps”, “exploring all implications of ‘new paradigms’”, or “working out possible combinations” of what is known. Here, “basic research”, and “practical implications” come close together conceptually. Such research could contribute equally to new applications, or to providing the springboard for elucidation of new fundamental principles (for instance by exploring inconsistencies in current understanding). Admittedly, in these cases, the frontier between university-style research and commercial application *can* be drawn: University research, except where a clear commercial goal is in mind is conducted in a spirit of free enquiry, detached from commercial pressures. However, although this is distinction is important in principle, in practice it becomes increasingly blurred.

In the biotechnology industry (where new developments include discovery of new DNA sequences), the “knowledge gained” (assumed to be non-patentable) is very closely related to the technique essential for gaining that knowledge (which is patentable). In modern high-throughput DNA sequencing machines, the knowledge provided is so closely linked to the commercial equipment used, that they become virtually inseparable. Whereas in former times, private investment was directed to producing usable *products*, now, with biology as the dominant science, and biotechnology as a major form of technology, that investment is directed to produce potentially usable *information* (i.e. DNA sequences), in effect “knowledge”. This gives a quite new slant to patent law, and creates unprecedented challenges for the distinction between knowledge *per se* and *application* of knowledge. Rather than Marshal McLuhan’s phrase “the medium is the message”, we can now almost say “technique is knowledge”. Pure and applied science, and, implicitly, the respective ethos of commerce and university start to merge.

Some commentators hold that these changes in what is patentable are having adverse effects on both the processes of science, and on its effective implementation as technology. Increasingly scientists find obstacles to their use of information in “pure” research, arising from ever-more-complex combinations of patents, use of which requires obtaining permission (and sometimes paying large fees). Heller and Eisenberg (1998) explain that there is a recent trend of patenting more and more steps along the research path. Each such "check point" slows the pace of research. The trend to make patentable what previously was thought of as "basic science" raises the costs for other scientists wanting to use such insights, which may be exactly the patenter's objective. Patent law may thus be slowing innovation. The spread of patent law to include aspects of "knowledge", as well as methods of *applying* knowledge is thus seen as a potentially dangerous trend.

What is at stake here is the concept of scientific knowledge as a "commons", a shared resource upon which every researcher can draw freely, just as, before the industrial revolution in Britain, the "commons" was land on which every small-holder could freely graze their livestock. In the case of land usage, various "enclosures acts", introduced over the period of industrial development, transferred land usage rights to just a single owner. This might be seen as equivalent to patenting new information, in our own times. However, the analogy with older patterns of land usage is not an accurate one: One of the justifications for the "enclosures acts" was to improve efficiency of agricultural production, by economies of scale, an objective which these acts probably did achieve (though at great social cost). However, "economies of scale" do not apply to the knowledge economy, or where innovation is concerned. What is required in this case is a process whereby the insight of a single individual can be recognized, be subject to a process of competition for applications, and then, for the winners, to great amplification and commercialisation. The commercial production may need a large enterprise, but the initiator is often a single individual. This suggests that as many people as possible should have access to the information "commons".

In defence of the increasing scope of patent law, the counter-argument is made that patents are necessary for research. Without them, scientists would keep secret all their discoveries for fear that colleagues and rivals would steal their ideas. This is a valid point. In early history of applicable mathematics, especially in France, mathematicians would guard their "patent" method of computation, as if it were a "trade secret". Without patenting, it is also argued, there would also be little incentive for large-scale investment from the private sector, because the same product could be manufactured on a large scale (but without research investment), by companies other than those from whose research it originated. This is especially important in the pharmaceutical industry, where initial research costs far outweigh those for production of a generic copy by manufacturers who "free-ride" on the backs of those who took the risks in the early development.

Which of these two arguments has actually prevailed is an empirical matter, on which there have been research studies. Results indicate differences between different fields of technology. Mansfield (1994) reports that, in an average industry, only 15% of products would not have been developed had there been no patent protection. However, for the pharmaceutical industry the figure is about 60%. In that industry, patents are filed before clinical testing of a new product starts, so half or more of the (usually) 20 year period of patent protection may have elapsed before the drug is on the market (see Sauer and Sauer, 2007), a situation quite different from that in most other industries. As a result of this, various forms of "data protection", acquired as an "add-on" to an existing patent, become more important than an actual patent. There may also be “add-ons” to the period of patent protection, by specifying a new use for a drug, when its patent is near expiry date.

“Generic drugs” (copies of previously-patented drug, no longer under patent), at present account for only a small fraction of total sales in US (Kirking et al, 2001). Producers of generic drugs have sometimes succeeded in challenging existing patents, the public thereby benefiting by reducing the price of available drugs. However, it is feared that new US laws easing regulation on generics will threaten research-based companies (Cimons, 2003). The public may thus lose in the long-term, by limiting profits of research-intensive pharmaceutical companies, which might be used for developing new drugs. One might suggest that an international convention is required, akin to payment for copying under copyright law, so that producers of generic drugs make proper payment to those who did the research. Such agreements are already being reached between research-based pharmaceutical companies, and manufacturers of generic drugs (Kirking et al, 2001). Another solution is to negotiate a collective agreement between research-based pharmaceutical companies, so that clinical testing is done on a world-wide scale, with both risks and revenue shared. Such “joint ventures” are not unknown in the oil industry, another very large, but high-risk industry.

Historical circumstances in Scandinavia permitted a controlled study of the effects of change in patent law (Valentin and Jensen, 2007). Prior to January 2000, Denmark and Sweden had similar patent laws, encouraging university researchers to give patents to companies, in exchange for funding and publication rights. After that date, Denmark introduced a law like the Bayh-Dole Act in USA. In Denmark the change led to a reduction, not seen in Sweden, in domestic academic inventions, with a small increase in patents filed by Danish universities. The authors suggest that the effect was due mainly to companies pulling back from supporting the very risky exploratory stage of early drug development in universities, because, with the new law, the universities would hold the patent. The older law was better for the pharmaceutical industry, but the authors thought that the new law in Denmark might have had a positive effect in other areas of technology. The principle may be that when development of new technology is high-risk (as it is in much biotechnology, especially in early stages of drug development), the risk should be taken by big enterprises. This may be a large commercial business or a state-run enterprise, rather than a single university. Increasingly it may become a coalition between the two, with commercial enterprises recruiting, and/or supporting researchers from universities, but not exposing the university alone to risks of failure.

Two recent studies on intellectual property issues have shown that patenting has had an adverse effect, but not a large one on the sharing of information between researchers (Blumenthal et al, 1997; Campbell et al, 2002). Anecdotes (such as one recounted by Martinelli et al, [2008]; and I have heard others) indicate that there is a problem here, of people working in the same university department, on same topic, who do not talk to each other, because they have signed different contracts. However, Martinelli et al (2008) conclude that generally, at Sussex University, “external links do not undermine cooperation within faculty. Of course, reluctance to share information or ideas does not need encouragement from such external contracts: it often occurs anyway, and I hear that it also occurs within a big multinational company, just as it does within academia. The effect of patent law appears to be greater when it comes to sharing specialist materials than “knowledge” (Walsh et al, 2005). In some areas, such as molecular diagnostics of hereditary conditions, the issue turns out to be a severe constraint, which has deterred a lot of not-for-profit hospitals from developing some tests (Merz et al, 2002; Cho et al, 2003). The patents in these cases can restrict tests for a particular mutant gene sequence, a clear case of patenting “knowledge” as well as the “use of knowledge”.

Nelson (2004) argues that the traditional distinction between science and technology (“knowledge” vs “practical inventions”) *is* still important for patent law. Much research, while providing “pure” knowledge, has its direction set because, in the relevant “general area”, it is thought that there are practical outcomes to be found (what he calls “Pasteur’s quadrant”, referring to Pasteur expectation that in the “general area” of his research, practical outcomes were to be found). In that area, just as in “pure research”, a scientific “commons” *is* needed, because the particular way in which knowledge comes to be applied is unpredictable, often dependent on serendipity. An exception to this may be the use of DNA sequences in diagnostics of defined hereditary conditions, where the relation between knowledge and its application can be stated in very exact terms. However, this serious situation seems to apply mainly in the USA, where, arguably, it is just another argument for introducing socialized medicine. For most biotechnology, including its use in developing new treatments, the process is probably still very chancy, with success often depending on an element of luck. Technological advance is then best assured by encouraging an “evolutionary” process, based on a wide commons of knowledge, even if that knowledge has been obtained with a particular end in mind. As one gets closer to a commercial application, the larger the “repertoire” of ideas available to be exploited at short notice, the better the chance of eventual success. Nelson suggests that patenting by universities is not objectionable in itself, but *exclusive* licensing of a patent to a single commercial enterprise is so. Also use of patented materials should not be prohibited by the universities who hold the patent, provided that the new users of the material do not themselves take out patents on resulting applicable knowledge.

If such an “evolutionary” system can be arranged, it should not be modelled too closely on Darwinian concepts of “survival of the fittest”. Since success is, to a degree, a matter of luck, there needs to be protection for those who lose in a particular part of the competition, because their talents may emerge as successful on another occasion.

Overall, the recent challenges in patent law to the traditional distinction between “knowledge” and “application of knowledge” probably *do* mean that, philosophically, the definition of what is, and what is not patentable can no longer be made (and sustained) in a precise way. Nevertheless, that does not mean that the doors are open to file a patent on *any* knowledge. Legal systems evolve as circumstances change. Continual adjustment of patent law may be needed from time to time, just as there is continual back-and-forth adjustment on issues like nationalized versus private control of major industries, free labour market versus unionised labour etc. Nelson believes that the move to allow “knowledge” to be patented has now gone too far, and some counter-move, returning to the idea of the scientific “commons” is now needed.

Broadening the debate beyond the issue of intellectual property, a cultural gap seems to have developed between the university ethos and that of the world of big business. It was not there at the time of James Watt, Liebig, or Daimler. Perhaps it is an enduring legacy of 150 years of oppositional politics between management and labour, or of the way major industries were administered during world wars. In the contemporary scene we cannot afford to leave this issue unaddressed, to move just along old tracks. Pressure is increasing to resolve enormous economic problems which loom, the growing crises of climate change, and the increasing need to optimise use of the world's scarce resources. We have to develop smarter, more intelligent and cheaper ways of developing, selecting and using technology, above all, ways more responsive to world needs. This is also part of another broad issue, of how global industries can be made accountable, as world citizens, just as are citizen and organizations operating within individual democracies.

Sometimes (times of war), there are research issue of great national importance, whose practical impact is inevitably on an industrial scale, and which need to be developed in secret, thus completely contravening usual codes of academic freedom. Here I do not mean issues whose ethical impact will be forever controversial (such as the Manhattan project), but others, which, though developed in wartime, came later to be essential aspects of peacetime technology (such as radar, or penicillin).

More relevant to our own times, there are issue of similar urgent national and international interest, requiring specialized research, which will succeed best if conducted in a spirit of free enquiry. On the small scale, one could cite research in the nineteenth century Britain in the School of Mines, one of the components out of which Imperial College was formed; or the Safety in Mines Research Institute in my home city, Sheffield. At the international level, issues like combating or adjusting to climate change, will require technology to be developed and applied on an unprecedented scale, dwarfing even current large multinational industries. This too will have best chance of success if researched and developed in a spirit of open enquiry. Here, as in instances already discussed, the university ethos needs to pervade the commercial world. Historically, the best precedent we might have is the cross-disciplinary research in many fields including, nuclear physics, meteorology, radiation biology, and the complex international diplomacy, leading to the nuclear test ban treaty in the late 1960s.

9. Conclusions

One theme which pervades much debate about current funding policies for university research is the saying "He who pays the piper calls the tune". So, when universities are large, with mass student intake, and are funded substantially by the state, it may then be impossible for public servants to justify to voters the expenditure on universities, unless there is tangible return for the investment. This justification is readily available as regards the teaching function of universities; but for research, such a tangible return is more difficult to identify. Certainly there can be economic benefits in the short-term by greater emphasis on technology transfer to established or newly formed commercial businesses. However, it is argued here, that the most influential practical outcomes of research accrue on a much longer scale than this. So, for research, we "pay the piper now, so that future generations can play a tune we will never hear." Surely we can give assent to that degree of faith! Effective research also generally requires academic freedom, without a dominant influence from immediate commercial pressures (although this does not mean that academic freedom guarantees effectiveness of research). So, the argument goes, if you want real academic freedom, if you want to pursue research with potential long-term but unforeseen practical benefits (rather than short-term aims), in other words, if you want to "call your own tune", you will have to provide the finance yourself. This may appear to be an argument for privately-funded universities, a change which is apparently already beginning in the UK (*Independent*, 29.05.2008). To put the argument another way, it could be argued, with regard to the great changes which have occurred in publicly-funded universities in the last generation, that "there is no alternative". That may be true, but if it is true, then there is another undoubted truth: Inevitably some vital aspects of research will increasingly come to be done by mavericks such as myself, working freelance. That often was the case in the nineteenth century, when pioneering research was done by people of independent wealth (Charles Darwin and James Clerk Maxwell to name but two).

This argument for private funding is unduly cynical: The traditional values of a university *can* be promoted to the voting public, in terms similar to those used to support democracy. Independence of universities can be seen as an extension of the "separation of powers", in the theory of democracy. The encroachment of commercial values into universities is not inevitable, not an indication (as sometimes implied about the west's "victory" in the Cold War) of a superior value system in the cynical sense of a "Darwinian struggle for survival". That encroachment reflects failure of past generations of academic leaders to understand and advocate for its cherished values. In some areas of science it reflects loss of direction of research, increasingly playing a game divorced from its proper social context, not least in losing sight of the goal to provide understanding. This plays into the hands of those who advocate for total dominance of the commercial ethos. The encroachment of that ethos also reflects failure by governments over many years to either understand properly the shortcomings of much "ivory tower" research, or to advocate for a more truly egalitarian and productive system. However, a more straightforward, and honest relation did exist once, and can exist again, between the endeavours of university researchers, and those who might develop them into practical applications or economic benefit. This is possible, and need not be dominated by either the artificialities of the publications rat-race in academia, or the questionable ethics arising from cut-throat commercialism in big industry. To achieve the correct synergy requires changes on both sides: If the worlds of academia and commerce do not "hang together", they will certainly, in due course, "hang separately".

Some aspects of the changed university scene are irreversible - the changing nature of employment, the increased participation in higher education. They should be accepted as "givens", to some extent, at least. Other aspects of the changes are *not* immutable: the managerialism, the excess of beaurocracy and demands for accountability, and the lack of trust of skilled professionals. These are insulting and demoralizing to people of talent and good will within universities. They should be vigorously challenged. It is now being realized, at least in the UK, that *they* are the problem, not part of the solution (see: *Independent*, 2.06.2008, Letters section).

What conclusions or recommendations arise from the discussion presented here? I list below some which occur to me. The essay is focused on UK and New Zealand. Some of my points apply in both countries. Some apply mainly to New Zealand, by virtue of its history, geographical location and size.

The first six recommendations are about education

(a) A form of education, neglected at present, is on history of science, of technology development, and the interplay between the two. At present, it is my perception that history of science is better known in humanities than science faculties, to the disadvantage of the latter. It should receive increased emphasis. It would give future exponents of technology transfer a perspective vital in helping them to “stand above” immediate pressures, to refuse to “play the game” characterising so much of contemporary academia, to “think outside the box”, and to turn dreams into reality. It should be a regular part of science education, starting in secondary schools, leading forwards into universities. Major additional changes in academia world-wide are also need to reduce the tendency to “game-play”, and bring greater integrity to research.

(b) With respect to Lothar Meyer’s penetrating question (“How, in a modern technical civilization, can one prevent the separation of technical power from moral responsibility?”) part of the answer is again, that education, in both sciences and humanities, should include history. It should deal not only with the history of scientific and technological success, but also of its times of moral failure. The country where, in my view, this lesson has been assimilated most comprehensively, is that where the question was originally posed, namely (now modern) Germany. People of that country know, from their country’s own history, that to follow orders, or succumb to peer-group pressures is no excuse in international legal tribunals.

(c) Change in education should apply not only to would-be science and technology researchers, but also to future entrepreneurs, and others going into business, or for adult students seconded from industry for further education. This should increase awareness of the history of technological development, of the advantages of the “open” university ethos, and of free flow of information, at many stages of research and development. It should also be quite honest about of the darker side of the corporate world, when it presumes to stand above the democratic process, or collaborates with anti-democratic government regimes.

(d) The status of technical education could be enhanced, starting in secondary schools, but within a broader educational framework, cognisant of the ethical dimension to practical developments.

(e) For people educated in both science and business disciplines, there should also be greater emphasis on the fruitfulness, historically, and potentially today, of correct interplay between ideas and practice, or (equivalently), between theory, experiment and practical applications (and not just on rigid empiricism).

(f) In science classes at universities, there should be increase in courses dealing equally with scientific principles and practical problems. In engineering schools, this no doubt happens already, but could happen to greater extent in health-related science courses. There *has* been recent growth of “health science” courses, perhaps an expression of the same motive, but this could go much further: It could include classes where people with immediate knowledge of practical problems discuss them with students in “pure” science. In the health field, this could mean bringing patients and their families, not only into medical classes, but also into bioscience classes, or classes in biomedical technology/engineering. It could extend from the educational to the research level, and not necessarily dominated by the medical profession.

The other eight points are about research, both within universities, in industry, and at the interface between the two. Several of my points expand Shakespeare’s line: “No man is but an island, entire unto himself”. This applies not only to individuals (men and women), but to organizations, including even prestigious universities, and the largest multinationals. “Reciprocality” of relations between individuals, and organizations with a common interest should be of paramount importance if today’s challenges are to be met.

(g) It is necessary to ensure that both long- and short-term objectives are addressed. Unlike the RAE/PBRF philosophy, there need not be a focus just on short-term impact. There is no incompatibility in principle between long- and short-term research, although at present there may be one. An effective big industry works at both levels at the same time. The same could occur for university research with regard to commercial possibilities. There should be room for research concerned with short-term tactical goals, as well as that concerned with long-term fundamental goals, including large-scale in-depth scholarship, whose application may be twenty years away. One might even consider two tracks for university staff, one whose pay is larger, but who are employed on short-term contracts, the other less well paid in the short-term, but with secure long-term tenure compensated well if long-term aims are brought to fruition.

(h) Within universities, can research quality be assessed? Should it be, and if so, how? Should government seek to influence the “direction” of research? Strictly, “quality” *can* be assessed, but “potential” - which is probably more important than what is apparent immediately, - *cannot*. Mention was made above of the concept of “Pasteur’s quadrant”, the “general area” holding promise for useful outcomes. However, for the vast issues which now face us - climate change, greenhouse gas emissions, and new sources of energy, especially for transport - there are no such guidelines. “The sky is the limit”. We cannot really predict from which direction, if any, the best solutions will come - from new GM bacteria, geothermal, carbon capture, nuclear fusion etc. Therefore, especially within universities, we need to give support at a low level to as wide a variety of approaches as possible, sometimes suspending judgement on imponderable questions about likely future impact, rather than promoting fierce competition, so that the weakest “go to the wall”. This is especially important for improving the chances of successful technology transfer in biotechnology, where prediction of likely outcomes is less exact. What is needed is a wide range of basic science expertise, to be drawn on at short notice, during various stages leading to the commercial product. Support of basic research, and “targeted basic research” (especially in New Zealand) should be of a large number of small teams, not a small number of larger ones (as at present).

(i) Better interaction between academia and industry could be encouraged by sponsoring joint fellowships for university staff, to be seconded to industry (and vice versa), jointly funded by government and industry. Likewise, researchers from industry could be invited to address advanced science classes, and university teachers to present regularly at industrial training forums. In New Zealand, this might involve financing overseas visits, or, if overseas travel becomes prohibitively expensive, via tele-conference facilities. This already happens within academia, and could be extended, by negotiating alliances with research-based industries in the northern hemisphere. Since the economy of large multinationals is comparable in size to that of a small country like New Zealand, negotiation of such collaboration with industry is probably best done at governmental level, with advice from leaders of the science community.

The intellectual climate so created would be one where academics negotiating contracts with industry are on equal footing with their potential partners, not afraid sometimes to argue from a position of strength.

(j) Joint ventures at the interface between research and technology, including educational programs, could be negotiated with less developed countries with a rapidly developing industrial base, especially India and China (but keeping a close eye on ethical guidelines, as understood in New Zealand and UK.

(k) To ensure that adequate ethical/moral standards are not compromised, there should be transparency at all levels, until one gets so close to commercial production and marketing that the knowledge base is quite specific to the product. In drug development this requirement for transparency should cover clinical trials as well as the preclinical research base. The pressure coming on pharmaceutical companies to register all clinical trials before they are carried out, and to report them fully is already part of this transparency. However, for this to be accepted, and so that research-based drug companies still have the finance to continue drug development, manufacturers of generic drug should pay a fair share of the research costs.

(l) Non-commercial research applications, and blue skies research, should be discussed in the same forums as commercial applications, and not just in ethics committees. If business enterprises are to work well with university researchers, business needs to adopt some of the ethos of universities, rather than the other way round. Industry would then be better respected by both public and university researchers. It is also a reasonable demand, since, just as university research gets grants from government, so often does industry, in UK and other countries with a large research based.

(m) The role of "ethics committees" is regarded now by many researchers as overbearing and counterproductive, and (as "gatekeepers"), going beyond their brief. With deployment of the above strategies (see especially points [b], [c] and [k]), the role of ethics committees could be defined more clearly, and probably reduced (not abandoned, however). In any case, much commercial activity is not scrutinized by ethics committees, or is covered with quite different standards (market "research", military research). With "transparency" and "openness" as the normal style, transgression of ethical norms is likely to be safeguarded as effectively as it currently is by over-zealous ethical committees.

(j) In New Zealand, we cannot hope to compete with regard to very big science projects, though we could contribute in a substantial way, if international collaboration is called for. Despite its small size, the place where New Zealand scores is in holding firm to what I call "the currency of personal values", of the value of individual persons, their hopes for their own lives, their expectations from scientific research (their own and that of others), and their contributions as individuals, with their personal efforts, to research. Dare I say it, in these terms, at the level of such values, we "punch well above our weight". We do have important contributions to make. But, just as we now have an independent foreign policy (born of hard experience), we also now need an independent policy for research, science and technology.

In a (very slightly) facetious tone, I finish quoting Albert Einstein: *"Only two things are infinite, the universe, and human stupidity, and I'm not sure about the first"*.

10. References

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