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1937
Political philosopher Karl Popper takes up a lectureship in philosophy at Canterbury University College, Christchurch

How is it if in your research you strike a problem you are interested in – apart from applied work? Can you go to the boss and say: 'Dear boss, I want to work on this vague idea, I am in love with it. Can I leave my applied problem and work on this for some weeks or months?' The answer to this in New Zealand is always negative. But controllers of research should be able to trust their men, i.e. organised research has to be disorganised research in part.

Karl Popper, 1945

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Instructions to Authors

New Zealand Science Review provides a forum for the discussion of science policy. It covers science and technology in their broadest sense and their impacts on society and the environment, both favourable and adverse. It also covers science education, science planning, and freedom of information. It is aimed at all scientists and decision makers, and the interested public. Readability and absence of jargon are essential.

Manuscripts on the above topics are welcome, two copies of which should be sent to: The Editor, NZ Association of Scientists, PO Box 1874, Wellington, or e-mailed to allen.petrey@xtra.co.nz

As well as full papers, short contributions, reports on new developments and conferences, and reviews of books, all in the general areas of interest of the journal, are invited. The journal also accepts reviews of a general nature and research reports.

Full manuscripts (with author's name removed) will be evaluated and authors will be sent copies of the reviewer's comments and a decision on publication. Manuscripts should not normally have appeared in print elsewhere but already published results discussed in the different, special context of the journal will be considered. They should preferably not exceed 2500 words.

To facilitate anonymous review, author's names on manuscripts and any acknowledgement of assistance should be on a detachable cover page. Manuscripts should be accompanied by biographies of not more than 100 words on each author's personal history and current interests. Authors are also expected to supply a suitable passport-size photograph of themselves.

Manuscripts should be typed double-spaced with wide margins on one side of the page. Articles may be submitted in Word for PC, rich text format, or plain text, by e-mail, or on floppy disk or CD-R. Diagrams and photographs should be on separate files (preferably eps, tif, jpg, all at 300 dpi), not embedded in the text.

All tables and illustrations should be numbered separately – Tables 1, 2, 3, 4, etc., and Figures 1, 2, 3, 4, etc. – and be referred to in the text. Footnotes should be eliminated as far as possible. Diagrams and photographs will be printed in black and white, so symbols should be readily distinguishable without colour, and hatching should be used rather than block shading.

References should preferably be cited by the author–date (Harvard) system as described in the Lincoln University Press *Write Edit Print: Style Manual for Aotearoa New Zealand* (1997), which is also used as the standard for other editorial conventions. This system entails citing each author's surname and the year of publication in the text and an alphabetical listing of all author's cited at the end. Alternative systems may be acceptable provided that they are used accurately and consistently.

In this issue

Science has long been based on individual and institutional competition. The 1990s reforms of the sector in New Zealand led to the formation of the Crown research institutes (CRIs), which had responsibilities for specific economic or environmental sectors, independence and separate governance. The bulk of funding came via the Foundation for Research, Science and Technology, with often intense competition for resources. This was exacerbated by the openness of the investment processes to universities, research associations and other research providers. Since then, there have been various attempts to encourage interdisciplinary and collaborative programmes, manage overbidding and establish alternative models, such as outcome-based investments, but significant transaction costs in the competitive bidding processes remained.

In their article, *From competition to collaboration: Challenges for New Zealand science,* David Penman and colleagues provide some perspectives on the system from a review of a large-scale global collaborative programme in marine biodiversity, the Census of Marine Life. The authors highlight some of the lessons relevant to policy development and science management in New Zealand.

In *Nurturing genius: the childhood and youth of Kelvin and Maxwell*, John Lekner aquaints us with the remarkable similarities in the childhood and youth of William Thomson (Kelvin) and James Clerk Maxwell. Both were Scots, both lost their mothers at an early age, both had fathers who nurtured them intellectually and were ambitious for their careers. Arising from John's recent work on electrostatistics, his historical note describes Kelvin's and Maxwell's respective completion of the Cambridge Tripos examination, and describes some of their electrostatic researches.

The indefatigable David Penny has brought to our attention, in *Principles of Scientific Method*, a series of lectures delivered by philospher Karl Popper at the University of Otago in 1945 at the invitation of John Eccles. Eccles was at that time Professor of Physiology at Otago and, in 1963, winner of the Nobel Prize in Physiology and Medicine.

Popper's lectures give us a very good idea of his views on science. His primary theme is that the best and most effective science is characterised by people who test hypotheses, but who refuse to believe their own hypotheses. Read and enjoy!

Finally in this issue we pay tribute to Paul Callaghan GNZM, FRS, FRSNZ, who died 24 March 2011.

On Paul's passing, technology columnist Pat Pilcher said:

Sir Paul was New Zealand's only scientific rock star, but why was that?

Why is it that we seem to have a nearly inexhaustible supply of sports people to idolise, yet only one high profile scientist? I don't know about you, but that strikes me as being more than a little bit alarming.[1]

I think you'll agree.

Allen Petrey
Editor

[1] http://www.stuff.co.nz/technology/6637654/Sir-Pauls-legacy-invest-in-the-future

President's column

2012 stability or continued upheaval

Will 2012 be the year in which recent changes in the science and innovation system are allowed to bed in, or will we see further upheaval? The signs are not good. The Prime Minister announced on 15 March that the Ministry of Science and Innovation (MSI) will be subsumed into a new super-ministry on 1 July that includes Economic Development, Labour, and that odd bedfellow, Building and Housing.

Calls for fundamental change have continued from within and beyond the system. Some commentators bemoan a science system that fails to deliver the economic benefits it advertises. Others are alarmed at a perceived takeover of the sector and a marginalisation of science by an economic growth agenda. Will the science that underpins the long-term wellbeing of New Zealanders, including the environmental and health sciences, wither under the new Ministry? Even those that are comfortable with a greater focus on economic development will disagree on whether public investment should flow through the universities or through the CRIs. How will more fundamental science, such as that supported by the Marsden Fund, fare?

These concerns are not new. The difference today is that they are now debated at the highest levels of government. Sir Paul Callaghan's message of economic growth based on science and innovation has been absorbed by politicians of every stripe, and Sir Peter Gluckman's political dexterity has kept science and innovation in the mind's eye of government. The current leader of the opposition, David Shearer, has retained the science and technology portfolio he held before the election, a signal of his intent to make this a key part of his party's policies.

In such circumstances, change in our sector is probably inevitable. The spotlight is now firmly on us, and the expectations of performance, both internal and external, are now enormous. Yet if the sector is to deliver, change needs to occur as part of broader government strategy.

We need to acknowledge that the way science is practised is also changing. Big scientific problems require big teams these days and our current institutional arrangements, with their high transaction costs and researcher-scale accountabilities, are ill-suited to meet such challenges. Putting together large, multi-institutional teams to tackle complex problems remains depressingly difficult in the New Zealand environment.

It is also clear that scientists today require more specialised skills than they did a generation ago and many of these skills are now acquired post-PhD. One or more postdoctoral fellowships have become an important part of a modern scientist's training. It will be easy for a super-ministry to lose sight of its responsibility to ensure that the best and the brightest have sufficient opportunities to undertake postdoctoral fellowships.

Indeed, as I write there has yet to be any policy response to the issues raised in last year's open letter concerning the lack of postdoctoral opportunities in New Zealand. This affects science of all flavours. I know of one high-tech business that will only hire people with postdoctoral research experience because it needs to know that they will be able to hit the ground running. NZAS will be hosting a conference on 16 April to address the broader issue of career paths for early-career scientists. Confirmed speakers at the conference include the Hon. Stephen Joyce and David Shearer.

Reflecting on my own career path, I see that two of the tools I used to establish myself in the New Zealand science scene, the ISAT travel grants and the NZ Science and Technology postdoctoral fellowships, are no longer with us. These schemes were both vital to my success in changing fields upon returning to New Zealand. They also helped me learn the craft of writing grant applications.

What are the most important gaps in the support for emerging scientists as they struggle to find their place in the New Zealand science system? Has the Performance Based Research Fund reduced the number of opportunities for emerging scientists ahead of the 2012 assessment? It is not easy to find answers to these questions. MSI struggled last year to ascertain even the number of postdoctoral fellows that were employed in New Zealand.

This highlights perhaps the biggest challenge facing the sector, which I believe is a lack of openness and a consequent lack of self-awareness. MSI have laudably just opened a web portal (http://data.govt.nz/dataset/show/2376) that allows users to search by organisation or keyword for grants awarded over the last twenty years by FRST. This is a great start, but you will struggle to use it to find out who did the research or what the outcomes were. If you query the Ministry of Agriculture and Forestry over who has been awarded funding under the Primary Growth Partnership, as one of our members recently did, you will be rebuffed on the grounds that that such information is commercially sensitive.

New Zealand is always going to be a small player on the global science and technology scene, yet we make ourselves even smaller by taking a fragmented, opaque and often haphazard approach to doing science. If the new super-ministry can address this, I will be all for it.

Shaun Hendy
President, NZAS

From competition to collaboration: Challenges for New Zealand science

David Penman[1*], Andrew Pearce[2] and Missy Morton[3]
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Introduction

Science has long been based on a model of individual and institutional competition. The reforms of the sector in the 1990s led to the formation of the Crown research institutes (CRIs), which had responsibilities for specific economic or environmental sectors, independence and separate governance. The bulk of funding came via the Foundation for Research, Science and Technology, with often intense competition for resources. This was exacerbated by the openness of the investment processes to universities, research associations, and other research providers. Over the past decade there were various attempts to encourage interdisciplinary and collaborative programmes, manage overbidding and establish alternative models, such as outcome-based investments, but there were still significant transaction costs in the competitive bidding processes. Doubts remained as to whether the nation was maximising benefits.

A full analysis of the performance of the science system is beyond the scope of this article. However, we can provide some perspectives from a review of a large-scale global collaborative programme in marine biodiversity, the Census of Marine Life, and frame these within the context of how emerging policy settings for science in New Zealand may encourage more collaborative science. In 2010 the government initiated a process of reform of the sector, with an emphasis on expectations for performance of the CRIs. The reforms have given a greater proportion of funding decisions to the boards and management of the CRIs, based on more comprehensive and distinctive statements of core purpose. These statements give some national responsibilities for capability to specific CRIs, with expectations that collaborations will be developed across institutions and with end-users. This provides some challenges to the accepted system, to policy makers, and to the prevailing culture of science. Collaboration may be easy to say but hard to do.

Scientific research in New Zealand is dominated by significant government investments in the biological sciences, as befitting an economy with a base in biological enterprises. However, there is little experience in building large-scale international collaborations in the biological (including ecological) sciences. In contrast, the physical sciences, such as physics or astronomy, often require significant capital investments that can only be met by international collaboration. New Zealand's participation in the Australian Synchrotron facility and the bid for the Square Kilometre Array are but two of many examples. In biology we have more limited investment in global initiatives such as the Global Biodiversity Information Facility (GBIF). The Global Research Alliance for Agricultural Greenhouse Gases is also an emerging example of our leadership in a collaborative programme, which integrates biological and physical sciences to provide solutions for a key issue for the agricultural sector.

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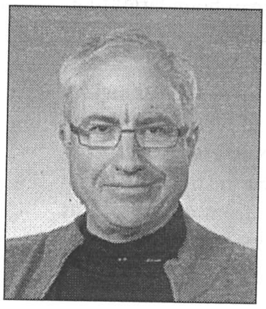
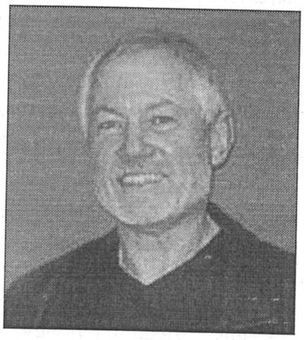


Professor David Penman has developed a strong interest in the development of large-scale collaborative science projects internationally, through his past chairmanship of the Global Biodiversity Information Facility, and nationally, through his leadership in the development and implementation of the Outcome Based Investments in biodiversity-related research at Landcare Research. David trained as an entomologist and has held senior roles in universities, Lincoln and Canterbury, and in senior management at Landcare Research.



Andrew Pearce has a long and distinguished association with the reforms of the science sector in New Zealand. From active research within the Forest Research Institute, he took on management roles, and leadership in the formation of the CRIs culminating in becoming the inaugural Chief Executive of Landcare Research. Dr Pearce has published widely on the structure and impacts of the reforms to science and is now an active director in a number of private and public sector organisations.

Missy Morton is Associate Professor of Education in the School of Educational Studies and Human Development in the University of Canterbury. Dr Morton's areas of research and teaching include qualitative research, cultural and social constructions of research and science, and evaluation studies.

We face challenges in moving from a competitive model towards greater collaboration, so we may be able to learn from how other large-scale collaborations have built new partnerships, capability, infrastructure and cultures. The authors of this article were commissioned by the Alfred P. Sloan Foundation (New York) to review the impact of the Census of Marine Life over their decade-long involvement and provide some lessons that might be relevant to other future collaborations in science. This article highlights some of the lessons of particular relevance to policy development and science management in New Zealand. The full report is available through Landcare Research (Penman *et al.* 2011).

The Census of Marine Life

The Census of Marine Life was conceived as a science discovery programme to address significant information gaps in our knowledge of the biodiversity of the oceans. In 2010 a decade-long \$US650 million programme was completed; this involved 2700 scientists from 80 nations and 640 institutions who spent 9000 days at sea on more than 540 expeditions, plus countless days in labs and archives. As one of the largest scientific collaborations ever conducted, the census produced over 3100 scientific papers and many thousands of other information products. The global community now has a legacy of a baseline of data on life in many of the ocean's realms that will shape policies and management of the oceans for decades to come.

The census pioneered a way to build scientific and community collaborations for the biological and ecological sciences. It was created with a simple and visionary goal: to understand the diversity, distribution and abundance of marine life.

The census emerged from a convergence of the need for information, largely expressed through the energy and advocacy of Dr Fred Grassle of Rutgers University in the United States, and the willing support of an initial investor in the idea, the Sloan Foundation (Ausubel 1997, 1999). The foundation provided funding to support initial workshops and proposal preparation, eventually culminating in a more than \$US75 million investment over ten years. The foundation then supported the governance and secretariat functions of the whole programme, administration of each project, development of core infrastructure for data sharing, synthesis of overall results, and outreach. Several key elements coalesced around the census, including recognition of an identifiable issue; a lack of response from traditional funding agencies in the United States; a research community which was fragmented and used to small projects shaped within existing funding constraints; a limited culture of collaboration and data sharing; and no recognised open-access data portal for information sharing, while at the same time increasing demands were being faced for more integrated management of the oceans.

We interviewed over 60 people from around the world, and views were also gained from participation in, and observation of, a number of census-related meetings and review of relevant documents. The review did not analyse the impact of the science; these impacts will continue to expand once the science moves into new projects, policy development and management of the oceans. Instead, the review focused on the lessons from processes such as governance, leadership, management, collaboration, globalisation, data management, synthesis, education and outreach, and future legacies. We were able to compare our findings with the perceptions of the census leadership which have been published elsewhere (Alexander *et al.* 2011).

Key lessons

Governance

The census developed at a time when our understanding of effective models for governing science was rudimentary. The census had no real defined governance structure, but functional relationships evolved despite limited documentation of roles and responsibilities. The Sloan Foundation as the key 'investor' ensured its interests were maintained through a strong link with the scientific steering committee (SSC), which provided review and support for the various projects making up the census. The SSC was a de facto governing board. A complex programme such as the census required more regular oversight than the SSC meetings (usually three per year), so the later development of an executive committee with more defined functions provided better support for the delivery of the census. This included a more formal consideration of risks, especially as the programme neared completion. Many science projects appear to have limited views on true end-points, so there were challenges to governance in getting participants in the census to deliver results by the end of 2010. There was also no successional plan or process at the governance level, so the census missed the opportunity to develop new leaders to take the project forward beyond 2010.

Our full report more comprehensively examines the principles and function of governance and compares the census with other initiatives. Governance arrangements for institutions are often well documented around lines of responsibility and accountability, and governors, through some form of board structure, take responsibility for approving strategy, approving plans to deliver the strategy, allocating resources, assessing and managing risks, measuring performance, and appointing and assessing leadership.

More challenging is how governance might work in collaborative contexts where projects cross a range of boundaries (e.g. institutional, disciplinary, national, etc.). Such projects will have their own governance structures and performance expectations, and the challenge is how to link those to wider expectations for benefits from large-scale collaboration, and what might be an effective model for governance given the sometimes overlapping expectations of the boards of participating institutions. Such projects often have complexities arising from areas such as financial resources, differences in capability and capital assets, policies on internet protocol and data sharing, political realities, and social and cultural differences.

There are differing expectations for governance and accountability and it is clear that there is no single model that is likely to meet the diversity of funding instruments, partnerships and stakeholder demands. In our view, there is no single 'right' model of governance – every set of governance arrangements contains compromises that reflect particular organisational circumstances, and often each compromise has to be balanced by another action to offset potential negative consequences. Thus, the design of effective governance needs to reflect a core set of governance principles rather than a rigid set of rules. From our review of governance of the census and comparisons with other initiatives, we contend that the design of governing structures should note the following key aspects:

- A 'cornerstone' investor is critical, and the willingness of the Sloan Foundation to commit a substantial sum for a decade underpinned the development of the census community.
- The 'cornerstone' investor should establish goals and expectations, including preferred governance models, performance measures and reporting processes.
- A substantial degree of autonomy and trust should be given to the programme director/executive director to enable rapid decisions about early investments to be made.
- A clear strategic plan should be developed early in programme planning to ensure progress towards achieving the goals, outcomes and impacts. Progress can be assessed and alterations made during the course of the programme.
- There needs to be clarity on the respective roles within governance groups, including decisions on representative, skills-based or mixed memberships.
- Risk-assessment and management is an important part of project direction and needs to be explicit.
- Leadership should be regularly assessed and reviewed to ensure new leaders are developed to support ongoing activities.

Leadership

Much leadership in science is individual, with the generation of ideas and hypotheses tested by experimentation or observation which then leads to peer-reviewed conclusions published in journals. Many scientific advances and societal benefits can be linked to this enduring process. However, occasionally some issues are so large and complex or require such a significant capital investment that they can only be addressed by a large collaborative initiative. The census had its inception in a visionary leader (Fred Grassle) who was able to convince a small group of colleagues of the need for such a project and find a like-minded individual (Jesse Ausubel of the Sloan Foundation), who saw the opportunity for the foundation to take a key role in bringing the census to fruition. This was not leadership that sought out problems to solve; it identified an issue that could not be addressed through conventional national funding mechanisms and could only be approached through a large-scale global collaborative endeavour.

We focus this article on public-good science, where the benefits of the research have wide societal outcomes and are not readily captured for direct private or commercial benefit. The traditional and linear view of science is that potential technologies emerge from basic research, and, with the assistance of institutional technology transfer and business development offices, new investors help to bring the ideas to commercialisation. Such a process recognises the role of the idea generator and his/her key role in the further development of the concept or product. However, it is now commonly accepted that the role of the 'inventor-scientist' should diminish as external investment increases towards 'product development'. Other professional managers and governors with different and wider business skills should then take increasingly significant leadership roles. The role of the 'inventor-scientist' (founder) becomes more one of a senior adviser, but with significant 'ownership' rights, which may, in turn, be diluted as more investors enter the project. We contend that this approach is equally valid in considering leadership of more public good-oriented projects.

The foundation was very clear that they would provide support (effectively as an 'angel investor') for a finite period to build the baseline in knowledge, the personal networks and the data infrastructure. Should the analysis of the results justify a positive business case, some new investor may take the census to the next phase. Scientists, as a rule, are not very good at such business decisions and disciplines. Comments from interviewees support the view that the SSC could have been more influential in recommending work to stop in some areas and enhancing investment in areas that promised a greater return – 'scientists are not very good at stopping things'. As a result, the census failed to generate a substantial and well-argued 'prospectus' on which to base a case for continuing some priority parts with new investors from 2010 onwards.

From our review, we contend that the following lessons are relevant to future collaborative projects:

- Apply the life-cycle model of 'inventor-scientist' followed by professional management and governance to the expected duration of the project, and form some initial views on the type of leadership that might be needed at different phases of the life cycle, and the approximate timing of any changes.
- Document roles and responsibilities for leadership at various levels and have processes in place for regular review and feedback.
- Consider term delineations, especially in advisory/leadership roles.
- Have a specific leadership development programme in place to develop the new echelon of leaders.
- Assign clear responsibility for completion of the initial phase of investment and for the preparation needed to obtain investment/investors for the next phase.
- Have a close understanding of the expectations of the lead investor.

Management

Large-scale collaborative science projects often have very complex management issues to deal with. Stakeholders want systems that are low-cost but enable their voices to be heard. The challenge is to have the right degree of support for the higher levels of leadership but ensure that issues raised by those who largely conduct the programme can be heard. It is almost universal that some form of secretariat provides management services, but the scope is very variable. In some cases it is merely administrative support, including planning and logistics for meetings; in others the secretariat does a substantial amount of the work.

The census established a secretariat based at the Center for Ocean Leadership in Washington, DC. This was independent from any research institution and provided access to politicians. The secretariat did not have full oversight of the financial status of the census, as the Sloan Foundation controlled its investments and the requirement for substantial leverage funding from participating institutions/countries to carry out much of the research meant that gaining a full understanding of the financial position of the census proved to be challenging. However, the secretariat did an outstanding job of project coordination and support; but the effective role of executive director was subsumed into the role of Jesse Ausubel as the representative of the Sloan Foundation. It was only in latter years that the executive committee began to provide some additional support to the interface between the management and expectations of the funders.

In designing a management structure for collaborative programmes, participants should consider the following:

- Design a programme management structure that has clear roles, responsibilities and accountabilities.
- Consider the use of collaborative information-sharing tools from the start of the project. Some uses can lead to closed teams, not shared systems.
- Managing risks is a key role of governance and management. The more complex the project, the greater the risks.
- Build an exit strategy to keep the community together. There is a risk participants may drift apart unless some secretariat functions can be sustained.

Data management

A critical innovation at the initiation of the census was the establishment of a means to share data. Grassle's promotion of the establishment of the Ocean Biogeographic Information System (OBIS) (Grassle & Stocks 1999) and the investment by the Sloan Foundation in establishing some core infrastructure was very forward-looking at the time. OBIS has been central to the delivery of primary data to a wide community, including researchers, policy makers and the wider public, and has been a crucial data portal for marine biodiversity data with links into GBIF.

Biologists and ecologists in many countries have been slow to recognise the value of data sharing. The census played a critical role in changing cultures among a community which had been resistant to making primary data more widely accessible. OBIS has become a key infrastructure project, but its future is not entirely secure and, while its move to come under the umbrella of the International Oceanographic Commission gives some institutional security, obtaining funds to maintain the infrastructure and build links to other organisations remains a challenge. These are issues which should receive more serious consideration as we examine how to make research data more widely available within the context of the open government and e-research policies.

Other issues relative to data management include:

- Having an explicit data-sharing policy at the outset of the programme, including standard protocols for metadata, data quality, intellectual property, etc. that meet best international practice.
- Ensuring that projects and individuals have specific expectations for data sharing and attribution, with appropriate sanctions; encouraging institutions to recognise data sharing as part of their individual reward systems.
- Considering having an advisory committee with specific responsibility for data management and ensuring the infrastructure is supported within an appropriate organisation.

Collaboration

Census participants who were interviewed were universal in their view that being involved in such a big programme enabled them to work across disciplines, institutions and countries in ways that were not previously possible. They built new research teams, and the funding available to support face-to-face meetings early in the formulation of ideas and the subsequent development of proposals was critical to working together. The groups built trust, with an ability to articulate some big goals and build ownership of a strategy to achieve them.

Collaboration in the census had no theoretical framework; instead, it was pragmatic and involved people who were willing to be engaged in a new sharing culture to achieve some challenging goals. Collaboration within projects led to innovative science, resulting in many publications in a wide range of journals. Questions were answered that would be beyond a more disciplinary and small-project approach. However, there were many other personal benefits from building a collaborative environment. Early-career scientists gained enormously from the census through building relationships with highly credible scientists and institutions. This has led to invitations to publish together and conduct joint research, while late-career scientists who had established their status were delighted to be able to put their work into a wider context and find a way to share data and ideas.

As previously outlined, building the census programme committed participants to data sharing. This was a significant challenge for scientists who have operated in a more competitive environment. The initial workshops were critical for developing a culture that shared data and ideas, and most census-aligned scientists have undergone a significant change in their culture and views towards the benefits of data sharing. This has not been without its challenges, such as institutional barriers towards internet protocol and data ownership, concerns about misuse of data, such as drawing unjustified conclusions, lack of recognition for data sharing, issues of data quality and coverage, etc.

The census built a new community which recognised the value of collaboration to address some big questions in biology and ecology. New technologies were deployed and some of these promise significant commercial opportunities, and, through OBIS, there is an infrastructure to support data sharing. The challenge is how to sustain the community, the technologies, and the infrastructure in any future initiative.

Delivering benefits

The census was conceived as a science discovery programme. A key driver was the development of the baseline of information of life in the oceans that might then be used for future policy development and management of marine resources. Providing information in a format relevant to policy and management was not an initial objective. As the census progressed and expanded in depth and breadth of coverage, the debate on potential relevance also grew.

Building links where the science becomes 'relevant' to a stakeholder or end-user can be challenging to some scientists. Many participants in the census were comfortable in doing the 'science we always wanted to do' but were more challenged when their results were being placed in a policy or management context. While the census did develop significant baselines of information on marine species, there are still many gaps. Policy makers cannot wait for the definitive science but must use current information and integrate this with other economic, environmental, social and cultural considerations.

However, the census had a simple message with clear goals. It was understood by funding agencies, institutions and researchers, and by stressing 'baselines, baselines and baselines' the basis for developing future policy and management options became possible. The census provided 'additionality' by bringing multiple funding sources together. It was held together by the innovative funding from the Sloan Foundation, which supported the development of trust and collaboration, built a culture of data sharing within a supporting infrastructure, and built a public profile and 'brand' by a very active outreach and education project. Our analysis provides the basis for programme design for any similar initiatives that might emerge. Such developments should include consideration of:

- Developing a governance structure that endorses an early investment strategy, supports proposals to potential funders with collaboration as a key objective, and supports some long-term planning for future legacies.
- Identifying a business model that will best facilitate programme delivery and ongoing support.
- Having a specific leadership development programme and successional processes.
- Having a globalisation and collaboration strategy that builds early links and capability with key countries, institutions and individuals.
- Seeking support for an independent secretariat to coordinate the programme.
- Having clear expectations for data sharing, attribution and storage.
- Building early links with potential end-users of the research.
- Identifying and supporting specific capability needs.

Conclusions

The Census of Marine Life challenged marine biologists and ecologists to find new ways of working together and it succeeded in building a new commuity which values collaboration and data sharing. A conventional process of competitive bidding would be unlikely to achieve such outcomes. Rather, it took the willingness of an investor (the Sloan Foundation) to facilitate the development of a culture committed to the sharing of data and the generation of widely accepted research questions, the development of compelling proposals, supporting secretariat services, and funding an outreach programme. The Foundation did not ask for these activities to be funded from existing individual or institutional resources. Instead, it provided funding on top of existing or proposed grants. This was very innovative and enabled a true competition for ideas rather than a competition between individuals and institutions.

The reforms to the CRIs in New Zealand, the emergence of core purpose statements and funding, and the merging of policy and investment processes within the Ministry of Science and Innovation provide the basis for some innovative development of large-scale collaborations, both nationally and internationally. There will be challenges, especially in bringing universities and other agencies with different funding streams and drivers into such programmes, but New Zealand does have opportunities in being able to embrace transdisciplinary approaches to research on key issues more readily than many other countries. It is essential that we provide funding over and above the core institutional resources if we are to develop effective collaborations.

Through the review of the Census of Marine Life we have identified some of the key issues relevant to any collaborative programme design, especially for governance, leadership and management. There is no one 'right' answer, but we contend that, with the right incentives, we can overcome any existing reticence to share data and ideas, especially in biology and ecology. This will require ongoing commitments to open access, especially to public-good data and research, to improved links to key end-user agencies, and to support of the key infrastructures to share data.

Finally, to quote Ian Poiner, chair of the scientific steering committee of the Census of Marine Life: 'The Census changed our views on how things could be done. We shared our problems and we shared our solutions.'

Acknowledgements

We thank all those participants in the census and external observers who so willingly gave of their time to provide comments. We are also indebted to the foresight of the Alfred P. Sloan Foundation which supported the review of the lessons learned from the decade of the census.

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Nurturing genius: the childhood and youth of Kelvin and Maxwell

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William Thomson and James Clerk Maxwell, nineteenth century natural philosophers, were friends and colleagues (Thomson was Maxwell's senior by seven years). This historical note gives a description of their early lives, with emphasis on the influence of their fathers and of Cambridge on their development.

Recent research on electrostatics got me into working contact with the early contributions of James Clerk Maxwell and William Thomson (later Baron Kelvin of Largs, and usually referred to as Kelvin). I read their biographies, and was struck by the remarkable similarities in their childhood and youth. Both were Scots, both lost their mothers at an early age, both had fathers who nurtured them intellectually and were ambitious for their career.

This note is mainly about William's and James' childhood and youth, and comes to a natural stop at their respective completions of the Cambridge Tripos examination. Only a brief catalogue of their later careers is given. Some of their electrostatic researches are discussed in my Author's Note at the end.

William Thomson, Lord Kelvin (1824–1907)

James Thomson, William's father, taught mathematics and geography at the Royal Belfast Academical Institution. William was born in Belfast. His mother Margaret (née Gardner) died in 1830 when William was six. His father became Professor of Mathematics at Glasgow in 1832, and the family of four boys and two girls moved there. An elder brother James (1822–1892, FRS) trained as an engineer, and became Professor of Engineering at Glasgow.

James Thomson senior was a man of wide interests, 'capable on emergency of teaching the University classes in classics'. His books cover an amazing range: *A treatise on arithmetic in theory and practice* went to seventy-two editions; other titles include *Introduction to modern geography*, *The romance of the heavens*, *Elements of plane and spherical geometry*, *Euclid's elements of geometry*, *Algebra*, and *Introduction to the differential and integral calculus*.[1, pp. 6, 7] And this from a farmer's son!

After Margaret died, the father taught James and William 'the use of the globes' and Latin [1, p. 6]. James and William were allowed to attend informally their father's lectures at the University. One of those present at the Junior Mathematics Class later recalled to Kelvin, 'As a mere child you startled the whole class, not one of whom could answer a certain question, by calling out: 'Do, papa, let me answer.' [4, p. 5] James and William matriculated at the University of Glasgow at ages 12 and 10, respectively, in October 1834. William '...carried off two prizes in the Humanity Class; this before he was eleven.' In the next session young William got prizes in Natural History and in Greek [1, pp. 8, 9]. And so on. Kelvin recalled (in 1907), 'A boy should have learned by the age of twelve to write his own language with accuracy and some elegance; he should have a reading knowledge of French, should be able to translate Latin and easy Greek authors, and should have some acquaintance with German. Having learned thus the meaning of words, a boy should study Logic'. In Natural Philosophy, under Professor Meikleham, William read *Mécanique analytique* of Lagrange and *Mécanique céleste* of Laplace [1, pp. 11, 12]. In 1839 he attended the Senior Natural Philosophy class taught by the professor of Astronomy, J.P. Nichol, who introduced William to Fourier's *Théorie analytique de la chaleur*. 'I asked Nichol if he thought I could read Fourier. He replied 'perhaps'. ... on the 1st May [1840] ... I took Fourier out of the University Library; and in a fortnight I had mastered it – gone right through it.' [1, p. 14]. William was fluent in French: in the summer of 1839 the family went to London, and then on to Paris, where the boys were left (in the charge of a trusted servant) for about two months to learn French. The father wished them to learn German also; for two months the whole family took lessons in German, and on 21 May 1840, Professor Thomson and his six children (William was 16, the youngest boy Robert was 11) left Glasgow for Liverpool, London, and then by steamer to Rotterdam. William's diary has the entry, 'Reached the bar at

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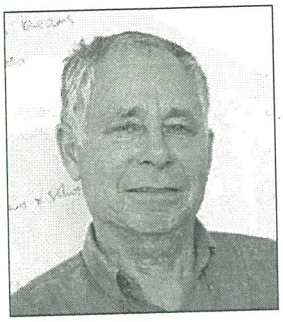

John Lekner was educated at Auckland Grammar, University of Auckland, and University of Chicago. He has taught at Cambridge , where he was Fellow and Tutor in Physics at Emmanuel College, and at Victoria University of Wellington. Professor Lekner has written one book, 'Theory of reflection of electromagnetic and particle waves' (1987), and about 130 papers, mainly in the fields of quantum mechanics, statistical physics, and electromagnetism.

the mouth of the Maas, near Brill, at about 4½ o'clock in the morning, where we had to lie till 10. The vessel rolled greatly from side to side, but the rolling was intermittent, as every two or three minutes it calmed down and then rose again with perfect regularity. This probably arose from two sets of waves of slightly different lengths coming in in the same direction from two different sources'. The family visited the Hague (the diary notes a visit to the Museum to see a stuffed mermaid!), Delft, Düsseldorf, Bonn, Cologne, Frankfurt am Main (where they stayed till 2 August), then on to Baden, from where the brothers James and William went on a walking tour of several days through the Black Forest. The family returned to Glasgow in early September. Certainly an educational trip, much to the credit of Professor Thomson. But young William did not spend all his time practising German: he had taken his Fourier with him, and surreptitiously read it in the cellar. 'When my father discovered it he was not very severe upon me'. [1, pp. 16–18]. A text by Kelland, *Theory of heat*, 1837, stated that the Fourier expansions were 'nearly all erroneous'. William found, while at Frankfurt, the cause of the misunderstanding. This resulted in his first publication *On Fourier's expansions of functions in trigonometrical series* [8, Vol. 1, pp. 1–9].

In April 1841, William entered Peterhouse in Cambridge. (He had purposely avoided taking a degree at Glasgow, so as to be able to enter Cambridge as an undergraduate.) The choice of Peterhouse had much to do with the presence there of Dr William Hopkins, a geophysicist and famous as a Mathematics Tripos tutor. The Maths Tripos was an examination conducted (in Thomson's day) over six days, each with 5½ hours of hard writing, covering mathematics and the mathematical aspects of physics. To be placed high on the list, especially to be Senior Wrangler or Second Wrangler, was the making of a career. Hence the three years of intense preparation and tutoring. Young William, 17 when he entered Cambridge, was mature enough to realise the importance of the Tripos, and organise his life accordingly. He soon saw that there was a separation at Peterhouse into the classes of 'rowing men' and 'reading men'. 'All my friends are among the latter class, and I am gradually dropping acquaintance with the former ... even to know them is a very troublesome thing if you want to read, as they are always going about troubling people in their rooms'. (Letter to his father, 12 December 1841 [1, pp. 32–33].) However, together with another undergraduate, William bought a single sculling boat for £7. His father was surprised at not having been consulted, and urged William to 'Use *all economy* consistent with respectability. Be most circumspect about your conduct and about what acquaintance you form. You are young: take care you be not led to what is wrong. A false step now, or the acquiring of an improper habit or propensity, might ruin your life.' [1, p. 37]. William made good use of the boat, and rowed on the river Cam with another 'reading' man, G.W. Hemming of St. Johns, Senior Wrangler in 1844. His sister Elizabeth wrote on 27 February 1842 that 'papa' was reconciled to the purchase of the boat, much to the relief of William, who wrote to his father on 14 April 1842 that, 'The sculling is going on with great vigour, and is keeping me in excellent preservation. ... I find that I can read with much greater vigour than I could when I had no exercise but walking in the inexpressibly dull country round Cambridge'. [William was used to a more varied topography than the flat land surrounding Cambridge.]

During the summer vacation of 1842 the family were at Knock Castle (three miles from Largs, on the Firth of Clyde). There William wrote a paper *On the linear motion of heat* [8, pp. 10–15] in which he discusses solutions of the one-dimensional equation for the flow of heat, namely $\partial_t T = \partial_x^{\,2} T$, where $T(x,t)$ is the temperature, in the form:

$$T(x,t) = \frac{1}{\sqrt{\pi}} \int_{-\infty}^{\infty} d\alpha\, e^{-\alpha^2} f\left(x + 2\alpha\sqrt{t}\right), \qquad T(x,0) = f(x)$$

Another paper, *On the uniform motion of heat in homogeneous solid bodies, and its connection with the mathematical theory of electricity* [9, pp. 1–14] was written that summer. Not bad for an undergraduate of 18!

Back at Cambridge in October 1842, William began his training under the tutor Hopkins, with the aim focused on the Tripos examinations in the Senate House in January 1845. He won a mathematics prize of £5, which he proposed to spend on an *Illustrated Shakespeare*, but his father preferred him to buy Liouville's *Journal de Mathématiques*.

James Thomson's paternal care was ever focused on his son's long-term prospects: Dr Meikleham, the Professor of Natural Philosophy at Glasgow, was ill. If only he could last till William had completed the Tripos (and got the laurels of a Wrangler), William might succeed him – a natural wish for the father, to have his son join him as a professor at his University. On 9 April 1843, Professor Thomson writes to William that Dr Meikleham is better; he adds '...you must take care not only to *do* what is right, but to take equal care always to *appear* to do so. A certain [Professor of Moral Philosophy] here has of late been talking a good deal about the vice of the English Universities, and would no doubt be ready to make a handle of any report or gossip he might pick up.' [1, p. 53]. The next letter detailed the requirements of the chair of Natural Philosophy, which included skill in experiments. This he urges William to attain. William, ever cooperative, replies that in his spare time he is reading *Cours de Physique* by Lamé, 'which is an entirely experimental work'. James Thomson (4 May 1843) writes of the probable votes in an election of Dr Meikleham's successor, and adds 'Take care to give a *certain gentleman here* (who, as to private affairs, is more nearly omniscient than anyone I have known) no handle against you. Avoid boating parties of in any degree of a disorderly character ... as scarcely anything of the kind could take place, even at Cambridge, without him hearing of it.' [1, pp. 57, 58]. And William did avoid boating parties and any scandal, but he did row in the eights for Peterhouse, and won the single sculls [1, pp. 58–62]. He also played the cornet, and was one of the founding members of the Cambridge Musical Society.

The saga of the chair of Natural Philosophy continued, with Dr Meikleham becoming ill and recovering. On 20 April 1844 Professor Thomson urged William to 'Keep the matter in mind, therefore, and think on every way in which you might be able to get efficient testimonials ... Do not relax your preparation for your degree. I am always afraid some unknown or little heard of opponent may arise. Recollect, too, that you might be thrown back by illness, and that you ought therefore be *in advance* with your preparation. Above all, however, take care of your health.' William replied on the 22nd: 'I am very sorry to hear about Dr Meikleham's precarious state ... it is certainly very much to be wished that he should live till after the commencement of next session.'

Preparation for the Tripos was to continue during the long vacation, when Hopkins would go with a party of reading men to Cromer, Norfolk. William wished to go too, entailing extra expense for his supportive father, who agrees to the request. But soon William writes from Cromer (13 June 1844): ' My Dear Father – I have again to write to you on the same pleasant business that I had to write to you about so lately, which is to say that my money is again all gone.' (Details of his expenses follow.) [1, p. 80]. Later (12 October 1844), 'papa' sent his son the halves of bank notes for £100, noting that the three years' expenditure was now £774/6/7, and asked 'How is this to be accounted for? Have you lost money or been defrauded of it ...? ... you *must* exercise the *strictest* economy that shall be consistent with decency and comfort.' Lest the readers think 'papa' a cheapskate, let me remind them of inflation: the value of the pound has diminished by a factor of about 72 between 1844 and 2001 [10], so in present currency Dr Thomson's £774 is approximately £60,000.

The work of the 'reading party' entailed Dr Hopkins setting examination papers and discussing the students' answers with them. It went on for two months. After the reading party ended, Thomson and a fellow Scottish student 'took a boat and rowed out to sea, and intercepted the G. N. S. steamer *Trident*', which took them to Edinburgh! [1, p. 82] Railways were only just being established (the Edinburgh to Glasgow line opened in 1845), and travel was a major undertaking.

Let us fast-forward now to the ordeal of the Senate House examinations, set to begin on 1 January 1845. The 'Wrangler' contestants had trained like Olympic athletes for this six-day event. Nor was this the end, because the Smith's Prize (another week of examinations) followed soon after. And the results were: Parkinson of St. John's, Senior Wrangler, Thomson of Peterhouse, Second Wrangler. The disappointment of William's family and friends was mitigated by the fact that Thomson was judged clearly better in the two Smith's Prizes awards, Parkinson second.

Dr Thomson continued to advance his son's education (and the prospects of the Chair in Natural Philosophy at Glasgow) by funding a trip to Paris in early 1845. William went with introductions to Arago, Biot, Babinet, Cauchy and Liouville. He presented himself to Liouville, with whom he met often and became friends. He also met Sturm and Foucault, that is almost all of the living French scientists (Laplace, Legendre, Poisson, and Fresnel were no longer). Biot introduced him to Regnault, the professor of Natural Philosophy at the Collège de France, and researcher into the physics of heat engines. William worked with Regnault in his laboratory, met Liouville and Cauchy often, and in his spare time [1, p. 128], 'I have been reading Jacobi's *Nova Fundamenta* and Abel's 1st memoir on *Elliptic Functions*, but have been rather idle on the whole'. Indeed!

After four and a half months in Paris, William returned to Cambridge. At the British Association meeting he met Faraday. Soon after, he was elected Foundation Fellow of Peterhouse, this being worth about £200 per annum, with rooms in College. This post he held till his marriage in September 1852. In May 1846 the chair of Natural Philosophy at Glasgow became vacant by the death of Professor Meikleham. The timing was perfect. William and his father quickly gathered testimonials and information about other possible candidates. There were five other applicants. Among the testimonials supporting William Thomson were those from Arthur Cayley, George Boole, J.J. Sylvester, G.G. Stokes, M. Regnault, and M. Liouville. To the printed pamphlet of 28 pages containing the testimonials, given to the electors, Thomson added an appendix listing his published papers, twenty-six of them. William was 22 at the time of his appointment in October 1846, and kept the chair till his retirement in 1899.

Professor William Thomson, 1846

Our description of young William Thomson's nurture and development stops here. He was not just a mathematically gifted child – he had the great advantage of a highly intelligent and energetic father, dedicated to his son's advancement. In Cambridge, he had the support of the best tutor, working in possibly the best environment for mathematics and the natural sciences in Britain. In Paris, he met and worked with the foremost mathematicians and scientists of France. And he was sensible enough to make full advantage of these opportunities, through continuous and vigorous use of his exceptional brain.

James Clerk Maxwell (1831-1879)

James' father was born John Clerk, adding the name Maxwell upon inheriting the estate of Middlebie. He practised law in Edinburgh and seemed set on a quiet batchelorhood until he met and married Frances Cay. A child (Elizabeth) died in infancy, and James was born, when his mother was nearly forty, at 14 India Street, Edinburgh [11, pp. 2–3]. Frances was of a 'sanguine active temperament', and energised John to develop the estate of Middlebie and enlarge *Glenlair*, their home. John had a 'persistent practical interest in *all useful processes*'; he made a special last for shoes (square-toed) for himself and later for James, and planned the outbuildings of *Glenlair*, down to the working plans for the masons [11, pp. 7–9]. Even before he was three, little James likewise showed a practical interest in the world. A letter from Frances to her sister, Jane Cay, gives the picture: 'He is a very happy man ... has great work with doors, locks, keys, etc., and "Show me how it doos" is never out of his mouth. He also investigates the hidden course of streams and bell-wires ... he drags papa all over to show him the holes where the wires go through.' [11, p. 27]. Throughout his childhood the constant question was 'What's the go o' that? What does it do?' If not satisfied with an answer he would ask, 'But what's the *particular* go of it' [11, p. 28]. His great love was the outdoors, of streams and ponds and the frogs that inhabited them [11, pp. 33–34]. With his first cousin, Jemima Wedderburn, who was eight years older, he produced an animation of a tadpole wriggling from its egg and changing into a swimming frog [11, p. 37].

James was educated by his mother until she died of abdominal cancer when he was eight. After his mother's painful death in December 1839, Mr Maxwell hired a local lad to tutor James at home. 'The boy was reported slow at learning, and Miss Cay after a while discovered that the tutor was rough' [11, p. 41]. Just as well she did: his friend and biographer Lewis Campbell describes the 'roughness' (being hit on the head by a ruler, and having ears pulled till they bled), and the lifelong effect this had on James [11, p. 43].

So Mr Clerk Maxwell sent the boy of 10 to the Edinburgh Academy. He lived with his father's sister, Mrs Wedderburn, with occasional stays with his mother's sister, Miss Cay. His first day at school was tough: in his gray tweed jacket and square-toed shoes, he was a target for ridicule and worse. He returned home 'with his tunic in rags ... his neat frill [collar] rumpled and torn ...' [11, pp. 49–50]. His aunts made sure his dress conformed more to the norms, but his nickname 'Dafty' stuck with him. Places in class were allotted according to performance, and James was initially among the rowdy boys, who naturally made things worse for him. For the first two years or so, school was something to endure. Fortunately he had the warm refuge of his aunt's home at 31 Heriot Row, and its good library, plus the occasional visits of his father, when they would explore Edinburgh together. The love between father and son is clear in the letters reproduced in Lewis Campbell's biography. In a letter of 19 June 1844, addressed to 'My Dear Father', and signed 'Your most obt. servt. Jas. Alex. McMerkwell' (an anagram, decoded by numbers underneath), he remarks after news of swimming and other outings 'I have made a tetra hedron, a dodeca hedron and 2 more hedrons that I don't know the wright names for.' [11, p. 60]. Campbell notes that they had not yet begun geometry.

At school he excelled in Scripture, Biography, and English, and discovered that Latin and Greek were worth learning. At about this time, Lewis Campbell joined the school, and began a lifelong friendship. Lewis lived at 27 Heriot Row, and the two boys were continually together for about three years. 'We always walked home together, and the talk was incessant, chiefly on Maxwell's side. Some new train of ideas would generally begin just when we reached my mother's door. He would stand there holding the door handle, half in, half out ... till voices from within complained of the cold draught, and warned us that we must part.' [11, p. 68].

By July 1845 young James was coming into his own, with prizes for English and English Verse, and the Mathematical Medal. His father now 'became more assiduous than ever in his attendance at meetings of the Edinburgh Society of Arts and Royal Society, and took James with him repeatedly to both.' [11, p. 73]. A member of the Society of Arts, D.R. Hay, had written a book on *First principles of symmetrical beauty*; one of the problems in it was how to draw a perfect oval. James generalised the equation of an ellipse, $r_1 + r_2 = 2a$ (r_1 and r_2 are distances from the two focal points to a point on the ellipse, $2a$ is the length of the major axis), to curves which satisfy $mr_1 + nr_2 = \text{constant}$. With Mr Maxwell's skilled promotion of this work, the result was James' first paper, *On the description of oval curves* [12, pp. 1–3], which was communicated to the Royal Society of Edinburgh by Professor J.D. Forbes in 1846. Professor Forbes took Maxwell under his wing, and they became lifelong friends. As it happened, the curves were not new, having been described by Descartes, and their optical properties considered by Newton and Huygens, but Maxwell's practical construction by means of pins and string was new. And what illustrious company for a schoolboy of fifteen!

This paper and his other manuscripts on ovals can be found in the *Scientific letters and papers*, [14, pp. 35–67]. Maxwell was now launched into mathematical and scientific inquiry. His second published paper (1849) was *On the theory of rolling curves* [12, pp. 4–29], in which he already shows a mastery of plane differential geometry. Next, in 1850, came *On the equilibrium of elastic solids* [12, pp. 30–73], 'an astonishing achievement for a 19-year-old working almost entirely on his own. The mathematics went hand-in-glove with his experiments on polarised light ... He set out for the first time the general mathematical theory of photoelasticity...' [15, p. 32]. By this time, James was at Edinburgh University, which he had entered at seventeen. P.G. Tait, who was a school friend of Maxwell's and later a collaborator with Kelvin on their *Treatise on natural philosophy*, was one of James' chief associates at Edinburgh University, but stayed for only one session, going on to Peterhouse, Cambridge, in 1848.

Maxwell went to Cambridge also, but not till 1850. Campbell remarks [11, p. 114] '... it is perhaps to be regretted that he did not go to Cambridge at least one year earlier. His truly sociable spirit would have been less isolated, he would have gained more command over his own genius ...'. Eventually his father was persuaded, and James went to Peterhouse, but transferred to Trinity College to improve his chances of a fellowship. Maxwell's tutor in preparation for the Tripos was the same William Hopkins whom we had met earlier as William Thomson's tutor. Here is Hopkins' view of Maxwell, as recorded by a Cambridge contemporary: '... he is unquestionably the most extraordinary man [Hopkins] has met with in the whole range of his experience; ... it appears impossible for Maxwell to think incorrectly on physical subjects; that in his analysis, however, he is far more deficient; ... a great genius, with all its eccentricities ... one day he will shine as a light in physical science ...' [11, p. 133].

Unfortunately the letters James wrote as an undergraduate to his father from Cambridge are lost. His father's letters naturally seek his son's advancement: 'Have you called on Profs. Sedgwick at Trin., and Stokes at Pembroke? If not, you should do both. ... Provide yourself with cards.' [11, p. 150] James got a scholarship from Trinity College in April 1852. At the scholars' table he was in his element, with free debate on almost any topic. He was elected to the Select Essay Club, a discussion group of twelve students who were known as the Apostles. Maxwell's essays delivered to the Apostles (Chapter VIII of [11]) have titles such as *What is the nature of evidence* *of design*, which begins 'Design! The very word ... disturbs our quiet discussions about *how* things happen with restless questionings about the *why* of them all.' Another essay *Idiotic imps* is about pseudo-science (then called Dark Science), which Maxwell exposes and analyses. Yet another has the intriguing title, *Has everything beautiful in Art its original in Nature*? A serious late essay, from February 1856, is on analogies: *Are there real analogies in nature?* We need both data and theory to make sense of the world: 'The dimmed outlines of phenomenal things all merge ... unless we put on the focussing glass of theory and screw it up sometimes to one pitch of definition, and sometimes to another, so as to see down into different depths ...' In the same essay, Maxwell remarks on space and time: '... space has triple extension, but is the same in all directions, without behind or before, whereas time extends only back and forward, and always goes forward.' The arrow of time, which Maxwell's statistical physics was later to clarify!

In the midst of preparations for the Tripos exams, James took a few days of the 1854 Easter vacation, to stay at Birmingham with a friend. His father wrote [11, pp. 7, 168] 'View, if you can armourers, gunmaking and gunproving – swordmaking and proving – *Papier-mâchée* and japanning – silverplating by cementation and rolling – ditto, electrotype – Elkington's works – Brazier's works, by founding and by striking out dies – turning – spinning teapot bodies in white metal, etc – making buttons of sorts, steel pens, needles, pins and any sorts of small articles which are curiously done by subdivision of labour and by ingenious tools ... foundry works, engine-making ... If you have had enough of the town lots of Birmingham, you could vary the recreation by viewing Kenilworth, Warwick, Leamington, Stratford-on-Avon, or such like.' James began with the glassworks.

Maxwell now faced the trial of the Senate House examinations – in his year, five days of 5½ hours each. Ever solicitous and practical, his father wrote 'You will need to get muffettees for the Senate-Room. Take your plaid or rug to wrap round your feet and legs.' James was Second Wrangler, E.J. Routh of Peterhouse Senior Wrangler. They were declared equal as Smith's Prizemen.

Maxwell with his colour wheel, circa 1855

In October 1855, James Clerk Maxwell was elected Fellow of Trinity College. He had supported himself by taking private pupils, but this could now stop. Apart from teaching third-year hydrostatics and optics, he was free to do research. He was now 24. He left Cambridge in 1856 to take up the chair of Natural Philosophy at Aberdeen, then was Professor at King's College, London, from 1860 to 1865, when he resigned to live and work at *Glenlair*. After Kelvin and Helmholtz declined the offer, Maxwell became the first Cavendish Professor of Physics at Cambridge in 1871. He had but eight years to live. He died in 1879 of abdominal cancer, aged 48, at nearly the same age that his mother had died of the same type of cancer.

We are fortunate in having a warm and affectionate biography by his friend Lewis Campbell. Especially moving are his depictions of James' childhood and adolescence, and of his early death. We admire his works, and with this biography we can also love him.

Epilogue

William Thomson and James Clerk Maxwell both achieved greatness; it was certainly not thrust upon them. However, both were fortunate in their fathers, in more than their genetics. And their fathers were fortunate in them: in a letter anticipating James' 21st, Mr Maxwell says 'I trust you will be as discreet when Major as you have been while Minor', quoting Proverbs x.1 [A wise son maketh a glad father.] Both sons showed remarkable good will and cooperated fully with their fathers' guidance and instruction. This in contrast to much modern behaviour, and also to that of the musical genius Wolfgang Amadeus Mozart, who eventually rebelled against his father Leopold. Thomson and Maxwell senior never had to face Leopold's tragedy of having a cherished child spurn them.

In the addition to the wonderful love, instruction and support from their fathers, they each had the support of family, in Maxwell's case particularly the comfort of the Aunts. In the wider sphere, we should also note that Scotland had been important in the European enlightenment and that the rates of literacy were exceptionally high. William and James grew up in a culture with a strong work ethic and widespread respect for knowledge, a powerful combination.

Finally, they both had the great advantage of their Cambridge experience. This environment suited both, matured them, and gave them lifelong connections with some of the brightest minds then living.

Author's Note

Victoria University physicists Pablo Etchegoin and Eric Le Ru have refined surface-enhanced Raman scattering to such an extent that they are able to detect single molecules [16]. This remarkable feat is accomplished by using the enhancement of an external electric field (provided by an intense laser beam) in the gap between two close conducting particles. The simplest applicable model is that of two conducting spheres in a steady (DC) external field, which had been solved by Maxwell and others [17–19]. The solution is exact, and in the form of infinite series which converge rapidly when the sphere separation s is comparable to or larger than the radii of the spheres. However, the field enhancement is large when the sphere separation s is small compared to the sphere radii, and there the series converge more and more slowly as s decreases. This is precisely the physically interesting limit, that utilised by Pablo and Eric to such good effect. So we have the unhappy situation where an exact theory fails to deliver just where it is needed.

I got interested, and spent considerable time investigating the exact series, their integral equivalents and especially the logarithmic terms which appear at small s. What started as an exploration of field-enhancement in the limit of close approach of the two spheres [20a, d] grew to encompass the capacitance of two spheres (at the same potential, or with equal and opposite charges) [20b], and the polarisabilities (longitudinal and transverse) of a two-sphere system [20c]. In all cases, terms logarithmic in the sphere separation s appear in the formulae.

Maxwell had approached the problem from the other end: he obtained, for quantities related to the capacitance coefficients C_{aa}, C_{ab} and C_{bb} of two spheres of radii a and b and separation of centres c (with c and s related by $c = a + b + s$), expansions in reciprocal powers of c. There is the remarkable Section 146 of his *Treatise on Electricity and Magnetism* [17], in which he matches spherical harmonic expansions about the two sphere centres to obtain ℓ, m and n coefficients (defined below) as series in reciprocal powers of c. Section 146 is seven pages of formulae, in which the calculation is carried to the twenty-second reciprocal power of c! As is well-known, series expansions of this type get more complex the higher the order. Maxwell had no computing aids, not even a mechanical calculating machine. I checked all the coefficients in his formulae (using computer algebra, of course) and found all were correct. This attests to Maxwell's amazing ability to carry through very long and intricate calculations, but also raises the question: why did Maxwell do this enormous amount of work? His coefficients ℓ, m and n give the total electrostatic energy of the two spheres, carrying charges Q_a and Q_b, as

$$W = \tfrac{1}{2}\ell Q_a^2 + mQ_aQ_b + \tfrac{1}{2}nQ_b^2 \qquad (1)$$

The coefficients ℓ, m and n are related to the capacitance coefficients C_{aa}, C_{ab} and C_{bb}:

$$\ell = \frac{C_{bb}}{C_{aa}C_{bb} - C_{ab}^2}, \qquad m = \frac{-C_{ab}}{C_{aa}C_{bb} - C_{ab}^2}, \qquad n = \frac{C_{aa}}{C_{aa}C_{bb} - C_{ab}^2} \qquad (2)$$

The total energy expanded in reciprocal powers of the distance between sphere centres c begins [21]

$$W = \frac{Q_a^2}{2a} + \frac{Q_b^2}{2b} + \frac{Q_aQ_b}{c} - \frac{Q_a^2b^3 + Q_b^2a^3}{2c^4} - \frac{Q_a^2b^5 + Q_b^2a^5}{2c^6} + \dots \qquad (3)$$

The first two terms are the self-energies of the two charged spheres, the third is the Coulomb energy, the fourth and fifth are due to mutual polarisation of the two spheres. Maxwell had the information to give the energy up to terms of order c^{-22}, but he did not do that. Why not? And, why do all that work and give the results in his *Treatise*? My guess is that (i) Maxwell was looking for a pattern in the series, and hoped to sum them completely if he found the pattern; and (ii) he wanted to compare experimental results on the force between two charged spheres with theory, and needed all these terms to do so. There is no hint in Section 146 as to his reasons. Perhaps neither of (i) or (ii) came to fruition, but he wanted the results of his labours to be available to others.

Preceding Maxwell's work were the Kelvin papers of 1845 and 1853 [9]. William Thomson was 21 when the earlier of these was published. It deals with the force between an earthed sphere and a charged sphere, and uses the method of images that he invented. He obtained an infinite series for the force $F(c)$, in which successive numerators and denominators of terms in the series are related by recurrence relations. It is now easy to write down the complete expression for the energy [21]: if sphere a carries charge Q_a, and sphere b is earthed, the electrostatic energy, and the force between the spheres, are given by

$$W(c) = \frac{Q_a^2}{2C_{aa}(c)}, \qquad F(c) = -\partial_c W(c) \qquad (4)$$

So, if we know the capacitance coefficient C_{aa}, a simple differentiation will give us the force. Incidentally, the inverses of the relations (2) are

$$C_{aa} = \frac{n}{\ell n - m^2}, \qquad C_{ab} = \frac{-m}{\ell n - m^2}, \qquad C_{bb} = \frac{\ell}{\ell n - m^2} \qquad (5)$$

so the Maxwell coefficients ℓ, m and n could be used directly to give the force as

$$F(c) = -\tfrac{1}{2}Q_a^2\,\partial_c(\ell - m^2/n) \qquad (6)$$

The force is always attractive, as is to be expected since the charge induced on the earthed sphere b has opposite sign to Q_a. The force increases as the separation s between the spheres decreases, and in fact diverges as s tends to zero.

A more interesting but more difficult problem is that of the force between two charged spheres (Kelvin 1853 [9]). The Maxwell expansion in reciprocal powers of c fails at close approach, and in particular at contact, when the spheres are at a common potential. They share the charge; the force is clearly repulsive, whatever the sign of this charge. Again Kelvin used his method of images, and again obtained an infinite series for the force. For spheres of equal radii, in contact, his expression for the force is proportional to a double series,

$$\begin{array}{ccccccccccc}
\frac{1}{2^2} & - & \frac{1.2}{3^2} & + & \frac{1.3}{4^2} & - & \frac{1.4}{5^2} & + & \frac{1.5}{6^2} & - & \cdots \\
 & - & \frac{2.1}{3^2} & + & \frac{2.2}{4^2} & - & \frac{2.3}{5^2} & + & \frac{2.4}{6^2} & - & \cdots \\
 & & & & \frac{3.1}{4^2} & - & \frac{3.2}{5^2} & + & \frac{3.3}{6^2} & - & \cdots \\
 & & & & & - & \frac{4.1}{5^2} & + & \frac{4.2}{6^2} & - & \cdots \\
 & & & & & & & & \frac{5.1}{6^2} & - & \cdots
\end{array} \qquad (7)$$

Kelvin notes that adding by vertical columns gives diverging series, while adding by horizontal rows gives a convergent series, which he sums to $\frac{1}{6}(\ell n 2 - \frac{1}{4})$.

The evaluation of the double sum demonstrates young William's mathematical skill. He expresses the sums of the first, second and third rows respectively as

$$\int_0^1 d\theta \frac{\theta\,\ell n\frac{1}{\theta}}{(1+\theta)^2}, \qquad -2\int_0^1 d\theta \frac{\theta^2\,\ell n\frac{1}{\theta}}{(1+\theta)^2}, \qquad 3\int_0^1 d\theta \frac{\theta^3\,\ell n\frac{1}{\theta}}{(1+\theta)^2} \qquad (8)$$

[For those interested in the mathematics: set $\theta = e^{-x}$ to convert $\int_0^1 d\theta \frac{\theta \ell n \frac{1}{\theta}}{(1+\theta)^2}$ to the more familiar $\int_0^\infty dx \frac{x}{(e^x+1)^2}$; then expand in powers of e^{-x} to obtain the sum of the first row.] Noting that $(1+\theta)^2 = 1 - 2\theta + 3\theta^2 - \ldots$, William writes the sum of the row sums as the integral

$$\int_0^1 d\theta \frac{\theta \ell n \frac{1}{\theta}}{(1+\theta)^4} \qquad (9)$$

which he evaluates without further comment as

$$\frac{1}{6}\left[\frac{\ell n \frac{1}{\theta}}{(1+\theta)^3}(3\theta^2+\theta^3) + \ell n(1+\theta) - \frac{\theta}{(1+\theta)^2}\right]_0^1 = \frac{1}{6}\left(\ell n 2 - \frac{1}{4}\right) \qquad (10)$$

A reader who verifies each of these steps will appreciate what is involved, but perhaps not the difficulty of its formulation, and certainly not the complexity of the infinite sets of electrical image charges that it is based on.

Without further discussion, William takes the convergent result as correct! When I first saw this, I wondered how it was that the (mathematically extremely able) young Thomson could be ignorant of Riemann's theorem about conditionally convergent series, namely that they can be summed to any desired result by suitable re-arrangement of terms. The answer lay in chronology of course: Riemann (1826–1866) was a student at Göttingen under Gauss (with a spell at Berlin) from 1846 to 1849, and did not teach till 1854. His paper on the re-arrangement of series was completed in 1853, but not published until after his death in 1866.

In fact the Kelvin result is correct. I have obtained it directly from the properties of the capacitance coefficients, and have generalised the result to spheres of arbitrary radii, at arbitrary separation [21]. But young Thomson's choice of one result from the infinity of possible sums of that double series is the boldest move I have seen in theoretical physics.

P.S. From 1854 there was much correspondence between Maxwell and Thomson, who became friends. The Maxwell letters relevant to electromagnetism are reprinted in [22].

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John Eccles and Karl Popper at the University of Otago, 1945

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The following notes were made by Professor John Eccles of five lectures (and two informal talks) given by Karl Popper during a visit to the University of Otago in May 1945. The notes were written up, cyclostyled and distributed by John Eccles, who used the results of Popper's analysis of science in his own research, and who later shared a Nobel Prize in Physiology and Medicine. Thus, with a world-leading philosopher of science and an eventual Nobel Prize winner, we have in one set of notes an important part of the history of early high level research in New Zealand.

First the note taker, the Australian John (Carew) Eccles (1903–1997) was Professor of Physiology at Otago University from 1944 until he was recruited in 1952 as the foundation Professor of Physiology at the then new Australian National University in Canberra. In Canberra he continued his work on nerve conduction that was initiated at Otago and in 1963 was jointly awarded the Nobel Prize for Physiology and Medicine, particularly for finding that chemical signals transmitted the electrical impulse from one nerve to the next, thereby passing on the electrical signal. It was he who asked Karl Popper to deliver the lectures in Dunedin (Popper 1976, p120).

Throughout the rest of their lives Eccles and Popper remained close friends and associates, and later they produced a book entitled *The Self and its Brain* (Popper & Eccles 1977). Eccles was very supportive of the Popperian approach to science – with its emphasis on testing hypotheses; never giving in and simply believing a hypothesis. Indeed, as pointed out in his obituary of Eccles, John Scott (1999) comments that 'Late one night, in 1951, Eccles concluded from his own experiment that the central processes [of transmission of the stimulus between nerves] must also be chemical. He said calmly, "Lorente is right", and then immediately began to plan a new series of experiments.' This was before he moved to Canberra, and so means that the principal conclusion, and the new experiments that eventually led to the award of the Nobel Prize, came from early experiments done in good Popperian style in Dunedin.

Turning now to the author of the lectures, Karl (Raimund) Popper (1902–1994). He was initially from Vienna before becoming a refugee in England from the Nazis. He was employed in Christchurch at 'Canterbury University College' from 1938 until the end of 1945, when he was recruited by the University of London for the London School of Economics. Popper was initially fascinated by the rise of relativity theory and, as a philosopher of science, sought to understand how, and why, science was the most effective form of human knowledge. In other words, he took science very seriously, and sought to understand why and how science gave the best knowledge available to humans.

The notes are certainly detailed, and give a very good idea of Popper's ideas on science. His primary theme was that the best and most effective science is characterised by people who tested hypotheses, but who refused to believe their own hypotheses. He says of his work in New Zealand that he convinced himself of 'the immense historical importance of erroneous theories' – but only if they were subjected to new experiments and tests. During his time in Christchurch, Popper wrote *The Open Society and it Enemies* (Popper 1945). It was here that he extended his approach to criticise, first Plato (in Vol 1) and then in Vol 2, Plato's followers, Hegel (founder of Fascism, and therefore of Nazism) and Marx (founder of Marxism, and therefore of Communism). To Karl Popper, there was no absolute knowledge, either in science or in other areas of human activity. General philosophers never seem to have forgiven Popper for his criticism of aspects of Plato's philosophy, but to Popper, Plato was closely associated with the Tyrants who had tried to rule ancient Athens and who opposed democracy. As you will see in the notes, Popper does not see humans as ever having absolute knowledge – it is all testing of ideas (new and old), and forever learning.

Both Popper and Eccles were critical of the anti-research policy that characterised the New Zealand university authorities of the time; and both complained about the heavy teaching load. In Christchurch, Popper was told that 'any time spent on research was a theft from the working time as a lecturer for which I was being paid' (Popper 1976, p119). Of Eccles, it was said that 'because of the heavy teaching load, many of Eccles's crucial experiments took place at night and in the early morning' (Scott 1999). Both Eccles and Popper were among the 6 signatories of a statement in July 1945, advocating a much stronger role for research in New Zealand universities (see Allan 1945). Long live performance-based research funding!

Our copy of the notes came from the late John (Hans) Offenberger, himself also a refugee from Vienna and a student at Canterbury University College at the time Karl Popper was teaching there. John had been interred for several months in the Dachau concentration camp, but a formal entry into Britain allowed him to be sent there. From England, he was awarded an international scholarship for refugees organised by locals in Christchurch, and although technically a student, became a lifelong friend of Karl Popper. 'In the Popper tradition, he believed teachers and scientists should expand their knowledge through research lest they become seduced by rhetoric and dishonesty' (Dakin 1999; Kitchin 1999). The 'Offenberger Building' at the Massey University campus in Wellington is named in John's honour (http://www.massey.ac.nz/massey/about-massey/news/article.cfm?mnarticle_uuid=A2D776F6-E981-002E-2129-A29D3FA02F85).

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Principles of scientific method

Notes on Lectures by Dr K.R. Popper given at the University of Otago, 22–26 May 1945

Lecture 1. The hypothetico-deductive method

All science has a distinctive character, which may be summarised as follows:

1. All scientific statements retain their hypothetical character (hypotheticism). They are always hypotheses. Certainty is not, and cannot be, the aim of science.
2. Deductivism – the so-called inductive method is a kind of optical illusion. It looks like induction, but never is.
3. Testing of theories. Doctrine concerning the way we test our theories. (see Lecture 2).

Methodology of science

From John Stuart Mill onward, the problem has been approached by attempting to analyse: 'How is it that physics is so successful?', and applying the answers to backward sciences [sic], such as psychology and the social sciences, and to a lesser extent the biological sciences. Mill took over views from Bacon and others that the methods of science were fundamentally inductive (inductivism). Mill's law of causality is a generalisation from multifarious observations (see note below).

With the method of deduction one starts with original ideas of unclear origin, i.e. general hypotheses, and then tries to prove these hypotheses. The hypothesis is provisional. When established by some kind of proof, it becomes a theory. However, you never get beyond the stage of a hypothesis. The last word may be said on some scientific problem, but, if it is said, we cannot know it; hence, the whole distinction between theory and hypothesis breaks down, i.e. all theories are hypotheses and never more.

But the reverse is not the case. All hypotheses are not theoretical. Hypotheses are of two kinds – (a) general or universal, as in science (these could be called theories), (b) special or individual, e.g. a medical diagnosis.

To sum up: The aim of science is not certainty. It is a human effort and in consequence shares human imperfection.

Prejudices in the way of acceptance of hypotheticism

1. Mill's – 'If you don't get certainty in science, where do you get it?'. This is Science with a capital S, i.e. 'Science says …', of the popular conception. It is, however, adopting a magical attitude to science, just as is done with a medicine man, both ancient and modern!

   It is important to realise the significance of this attitude. Great scientists realise how little they know – the humility of the really great. There is no scientific knowledge in the general sense of the word 'knowledge'. We speak of knowledge in ordinary life as something we can be sure of. It is the higher standards that science applies which reduce 'scientific knowledge' to the hypothesis. The term 'body-of-scientific-knowledge' (for example, as in a textbook) is a misnomer – it is not a body and is not really knowledge.
2. The empirical prejudice – 'I believe only what is evidenced by my senses'.
3. Rationalistic prejudice – 'I believe only what can be proved to me'.

The last two together lead off Mill's point of view – which is inductivism. 'I believe only what can be proved on the basis of observation'. It is wrong to take them as a basis of scientific method. Before beginning to observe we must have a problem, i.e. a statement of a hypothetical character, otherwise the observations are uninteresting and unrelatable. One can, therefore, never isolate the observations as such, for then one has not the basis of the hypothesis on which they are superimposed. The rationalistic prejudice – 3 above – can be discarded as one can't prove anything scientifically.

The character of scientific method is rather that of situational logic, i.e. it resembles the character of the situation of a man dodging traffic – we are in a strange world, dodging in and out according to circumstances. An alternative analogy is that of a man finding his way through a forest in a dark night, pressing forward, bumping up against trees, moving round and past them to encounter more obstacles, etc.

Note

(Extracts from *The Poverty of Historicism III*, by K.R. Popper, 1936.)

Mill describes the law of causality as follows: 'An individual fact is said to be explained by pointing out its cause, that is, by stating the law or laws …… of which its production is an instance. Thus a conflagration is explained when it is proved to have arisen from a spark falling into a heap of combustibles …'

I suggest that to give a causal explanation of a certain specific event means deducing a statement describing this event from two kinds of premises, viz. from some universal laws, and from some singular or specific statements which we may call the specific initial conditions. For example, we can say that we have given a causal explanation of the breaking of a certain thread, if we find that this thread could carry a weight of only one pound, and that a weight of two pounds was put on it. If we analyse this causal explanation, we find that two different constituents are involved. (1) We assume some hypotheses of the character of universal laws of nature; in this case, perhaps: 'Whenever a certain thread undergoes a tension exceeding a certain minimum characteristic for that particular thread, it will break." (2) We assume some specific statements (the initial conditions) pertaining to the particular event in question; in this case, we may have two statements: 'For this thread, the characteristic minimum tension at which it is liable to break is equal to one pound weight' and, 'The weight put on this thread was a two pound weight'. Thus we have two different kinds of statements which together yield a complete causal explanation: (1) universal statements of the character of natural laws, and

(2) specific statements pertaining to the special case in question, the initial conditions.

Now, from the universal laws (1), we can deduce with the help of the initial conditions (2) the following specific statement (3): 'This thread will break'. This conclusion (3) we may also call a specific prognosis. The initial conditions (or more precisely, the situations described by them) are usually spoken of as the cause of the event described by the prognosis; so we say that the putting of a weight of two pounds on a thread capable of carrying only one pound was the cause of the breaking.

Such a causal explanation will be, of course, scientifically acceptable only if the universal laws are well tested and confirmed, and if we have also some independent evidence of the cause described by the initial conditions.

Before proceeding to analyse the causal explanation of regularities or laws, it may be remarked that several things emerge from our analysis of the explanation of singular events. One is that we can never speak of cause and effect in an absolute way, but that an event is a cause of another event – its effect – relative to some universal law. However, these universal laws are very often so trivial (as in our example) that as a rule we take them for granted, instead of making conscious use of them. A second point is that the use of a theory for predicting some specific event is just another aspect of its use for explaining such an event. And since we test a theory by comparing the events predicted with those actually observed, our analysis also shows how theories can be tested. Whether we use a theory for the purpose of explanation, of prediction, or of testing, depends on our interest, and on which statements we consider as given or unproblematic and which need testing, etc.

If we now compare our explanation of causal explanation with Mill's, we see that in Mill's discussion of the causal explanation of singular events, there is no clear distinction between (1) the universal laws and (2) the specific initial conditions. This is largely due to Mill's lack of clarity in his use of the term 'cause', by which he means sometimes singular events, and sometimes universal laws.

Lecture 2. Testing of theories

Essence of scientific method

One puts up a hypothesis, a guess, a leap into the unknown, and from this one deduces consequences and then tests these.

Mill thought that if these tests are to mean anything, they have to establish the hypothesis. But the fundamental procedure is the reverse – the test has to be an attempt to refute the hypothesis. One is, of course, happy if refutation is not done. We can call this view 'falsificationism', i.e. one adopts a hostile attitude to the hypothesis.

What is the procedure of deduction and of test? It is of the form: if A, then B follows.

As an example, take the law of gravity – if you have two bodies in space they give to each other forces which decrease as the square of their distances and increase as their masses. The temporal and spatial universality is the distinguishing character of such a theory, i.e. of A. How can we deduce consequences from this, that is, derive B from A? We must deduce an individual consequence, for we can only observe an individual thing, never anything universal. From a universal law alone, we can deduce nothing positively about an individual case. We must have also initial conditions, e.g. the position of planet and sun, etc., and then deduce movement. This deduction is called prognosis.

For example, the famous Aristotelian deduction – 'All men are mortal, Socrates is a man, therefore Socrates is mortal'.

In science this test by using the prognosis is only of value if we attempt in a most rigid way to falsify the prognosis. But we can never in this way verify a theory. For example, all men may not be mortal even though Socrates is mortal, and is a man. But if the theory is that all men are immortal, then the first dead man refutes the theory. This is, we can falsify a theory, but never prove it.

There is an analogy with a man finding his way through a wood in the dark. He must venture – do something. His method is trial and error, with emphasis on error. If you don't make mistakes, then you learn nothing.

The method of science is like a Darwinian method of selection. We produce theories and then eliminate them. The remainder are those not yet refuted – but not therefore true. We have many competing theories. We must dare to produce many – not just successful theories – and ruthlessly weed them out.

Two things are necessary.

1. Ideas – imagination in producing theories – bold speculation. In itself this has little to do with scientific method.
2. The real essence of science is the ruthless persecution of scientific theories, hounding them till we kill them. But, of course, it is a greater success if we have a theory that has stood up to a rigorous series of tests. The first scientific effort was the killing of a theory or a superstition.

Why is this hostile attitude a necessary complement to these bold leaps of the imagination in producing theories? It is practically always possible to save a theory from the fate of falsification if we want to, i.e. if it is a pet theory. We can, for example, make an *ad hoc* hypothesis which explains away the observation, or can say that we have made a false experiment – apparatus leaked, etc. If we adopt this attitude, then all testing becomes useless, i.e. if we do not accept falsification, then all testing is a farce; hence arises the necessity of attempting to force the falsification.

However, there is a partial withdrawal from this rigorous attitude. You don't need to throw the theory away. There may be something in it, some element perhaps of value. Theories usually are complex, and even though falsified, they may have some component of value. For example, the experiment of diffraction of light led to the refutation of the theory that light was just a stream of corpuscles, but again this theory reappears to some extent as the photon theory. This is an indication that the prejudice which the father of a theory has for the theory has a function – a theory is rarely so simple that it can be rejected wholly in one piece.

However, the people who produce a theory generally take the attitude that they wish to verify it. That is an easy attitude. The testing is left to others. Hence, the development of science is a social affair; as at least two people are necessary – one making theories, the other falsifying them.

A certain school has been questioning the objectivity of sciences. They say that, wherever the interests of the scientist are involved, they won't be objective. Social sciences, where class interests are involved, will not be objective. This is not so, they say, with physics. But this may be criticised, as no tie could be as strong as that which the father of a theory has for his offspring.

Scientific objectivity is fortunately something that does not depend on the objectivity of a scientist, but rather on the character of scientific method, that is, on the public nature both of the publication of a theory and of its attempted falsification.

An important point is the question of the *ad hoc* hypothesis which is introduced to avoid falsification of a theory. For example, accordingly to Newtonian theory, Mercury should behave differently in its orbit. But one could produce an *ad hoc* theory that the sun emits some resisting matter that produces the discrepancy, i.e. just make a special assumption. However, if one makes an addition to the *ad hoc* hypothesis that can be tested by other means, it becomes a hypothesis: for example, that light transmitted through from stars would be dimmed. If this is not found, one could produce a further *ad hoc* hypothesis saying why light is not dimmed, but this is very bad indeed. The better conclusion is that the first *ad hoc* hypothesis is falsified.

An *ad hoc* hypothesis has no other action but to explain the series of facts it was invented for. A proper hypothesis has other consequences which can be tested in order to attempt to falsify it. This is a new and different attack on the method of induction, for induction would only lead to *ad hoc* hypotheses, and these lead nowhere, i.e. process of induction is of no interest scientifically.

If we want to get anything to test we have to have a hypothesis that has a wider field than would be the case with an *ad hoc* hypothesis; and to be able to test is essential, or else we never move beyond the *ad hoc* hypothesis. The *ad hoc* hypothesis is always individual.

In order then that the testing of theories can occur, we have to have a theory that has some new implications, i.e. the scientist has to take risks, to say more than he knows, not as an assertion, but as a hypothesis. This is a direct contradiction of the inductive, rationalistic attitude, which only believes the evidence of the senses and what can be proved from this. This is contrary to the adventurous spirit of true science. It leads to the saying of nothing because it won't take any risks.

The true outlook of the scientist is to take risks and make the widest possible theory, then test it where there is believed to be its weakest point, where there is the greatest likelihood that it will break down.

The inductivist theory

The inductivist theory as described by Bacon (who is a bit overestimated as a thinker) is a kind of commonsense view of science. It went so far as to say that one should never make any unwarranted statements. But the true attitude is to make unwarranted statements. If no risks are taken, one remains silent, for that gives the only chance of making no mistakes.

Bacon described the scientific method as resembling the collection of grapes, then treading on them and squeezing out the juice, which is the essence, or what he called a scientific generalisation.

The whole attitude of inductivism is that nature does not lie, only you lie, hence you do nothing – just observe in your bucket-like mind and be careful you add nothing, for all our mistakes come from our misinterpretations.

The real method of science is the reverse – it is to risk hypotheses, which are not lies, as their hypothetical nature is recognised. The real hypothesis covers a wider field than it was originally invented for, e.g. Newton's laws not only explained Kepler's laws of planetary motion, but also covered falling apples. Further, Einstein's theory covered all this and unified the theory of gravity and inertia, but also covered further fields:

- deviation of light rays in gravitational field,
- spectral analysis of elements in the very strong gravitational field of a heavy star – a red shift,
- other spectral effects – Doppler effects,
- also certain deviations from other theories, e.g. Bohr's theory of the hydrogen atom by accounting for high velocity effects,
- the high velocity effects on the electron's properties,
- Mercury orbital anomaly also.

So, Einstein's theory covered a much wider field, hence there was much more opportunity for it to be falsified. The success of the theory in these very divergent fields is very impressive. The less a theory has the character of an *ad hoc* hypothesis, the better it is, for one can make more attempts to falsify it.

How can this be applied to solve the controversy [over cosmological methodology] in *Nature*,[1] between Milne & Eddington and Dingle? It began with an attack by Dingle in the name of science (empirical science) – Dingle is an empiricist. He says that they are introducing speculative and philosophical methods into science. Eddington says, in fact, that he will squeeze nature into a system of pigeon-holes, i.e. because science is deductive, it cannot be falsified.

This view is not new. It was invented by Poincaré, who says in application to Euclidean geometry that we use it because it is simplest. No tests can ever refute it, as it always can interpret

[1] *Editor's note: The debate on cosmological methodology was still topical at the time of Popper's lecture, after a special issue of Nature on the subject had been published in 1937. The Stanford Encyclopedia of Philosophy on 'Cosmology: methodological debates in the 1930s and 1940s' (http://plato.stanford.edu/entries/cosmology-30s/) suggests that Milne was the successful protagonist and his hypothetico-deductive methology was subsequently developed by Bondi, with inspiration from Popper, into the 'perfect cosmological principle' – a steady-state universe – which was falsifiable and 20 years later abandoned by Bondi when evidence of an expanding universe turned up.*

facts in terms of the system and, of course, the system is always true. Poincaré extended this to physics, e.g. to the principle of the conservation of energy.

Deductivists are right in saying that science is our making, but wrong in saying that we can't throw it away if we find it contrary to nature. The difference between a conventionalist like Eddington and an empiricist like Dingle is that the conventionalist is not ready to be falsified and the empiricist is ready to be falsified. The attitude is the difference between one who looks to science as having the last word, and the scientist who is ready to be falsified.

Lecture 3. Objectivity and measurement

The thrill and adventure of science are exemplified by relativity theory.

Development of any science proceeds from less general to more general, i.e. in an inductive direction. For example, close to the beginning of science the relativity theory could not stand. The same is even true for the Newtonian theory. However, this inductive direction of development is an optical illusion.

The whole pattern is a series of leaps out and then a return to the observational data. The deductive method consists of leaps into the unknown, and this is not a rationally justifiable step in science. While it is true of every type of thought, in science there is testing which eliminates those leaps inconsistent with observational data. The language of empirical science is characterised by the readiness to be falsified. A non-empirical science is a system of tautologies.

The further spread of data is covered by additional leaps. That is to say, sweeps into the deductive direction with wider and wider spreads comes from the very nature of science. Merely to leap out to a hypothesis covering observations at hand is just to develop an *ad hoc* hypothesis. The truly scientific hypothesis covers more than the available observations; it is a leap in the dark, and so gives scope for falsification.

The principle of falsifiability is an attitude, not a logical position.

How do we start research?

According to Bertrand Russell in *The Scientific Outlook* [1931], 'the particular facts, A, B, C, D, etc., suggest as probable a certain general law, of which, if it is true, they are all instances. Another set of facts suggests another general law, and so on. All these general laws suggest, by induction, a law of a higher order of generality of which, if it is true, they are instances.'

This is really a method of *ad hoc* hypotheses. One cannot really start scientifically in this way, for no tests are possible.

How then can we start? We can start by observations, but then we don't know what to observe. But what to observe is most important for science. A high degree of exclusiveness is vital in this respect, e.g. what one observes now in the room is of no scientific value whatsoever, and never will have any value. The other idea is that we start with hypotheses. This too is impossible, as one wouldn't know what hypothesis to suggest. That is, both observationalism and hypothesism are alone quite impossible. You must not look at the problem in this abstract way. You have to realise that one starts science in an already formed situation.

Today every scientist begins his research career by being put on a problem, or by himself seeing something unusual in some scientific story and then finding a problem and investigating it.

Thus one can go backwards and backwards, but one will say: 'What about the first scientist?' The answer is that this raises no difficulty, for historically science comes from something not science, i.e. there is an origin of science from superstitions or fairy tales. And still science retains this character in its leaps into the dark, but the particular new character of science is the testing for falsifications. That is, science really begins with the first falsification of a superstition.

Take the particular case of early science of 5th century BC in Athens, where there was some medical science. You have also some superstitions, e.g. Herodotus says snakes grow on trees. So too, if we don't accept the falsification idea, we have many examples even now of such false observations. You see too easily that which you wish to see.

The real beginning of physical science in a narrow modern sense can be dated back to a falsification in the 5th century BC. Before that there was just speculation about the world, e.g. what the world was made of, but without any attempt to say why these statements were made. They were just dogmatic statements. Then Parmenides developed his ideas as a deductive theory – a chain of inferences from a fundamental assumption

What is can be
That which is not cannot be
Nothing cannot be
There can be no void, i.e. no empty space
The world is packed full
There can be no motion

What we think we observe is just delusion – a world of dreams i.e. in clash between reason and observation, reason is supreme. That is still true. For example, if you see a magician taking a rabbit out of a hat, obviously the observation is wrong.

Both Parmenides and Democritus identified being with fullness. Thus we have the Democritan idea of little atoms (beings with fullness) and empty space, i.e. it preserved as much as possible of Parmenides with these atoms moving about in empty space.

This Greek atomic view was different from our atomic theory. The nothing or void has played an increasingly larger role, as we have at present little but nothingness. Lucretius was in the tradition of atoms and void, like Locke, and Bertrand Russell. With this goes, curiously enough, hedonism, which was added by Epicurus. Russell and others don't realise this, which is indicative of how scientific tradition is often handed on unconsciously.

There is a close connection between the old atomism and the modern atomism, the connection being through Descartes. This Greek atomic theory was a physical theory of the first order. Compression, rarefaction, condensation, were explained in terms of models – usually with a type of casual observation, but it still was a Greek physics of a high standard.

Hence the answer of question: 'How does one start in science?' You pick up a problem. There are two possibilities now: (i) go to a Professor, (ii) read in the literature and find inspiration there. 'What can I do about a problem?' (i) hypotheses, (ii) observations.

You have to get familiar with the whole background of the problem, not only with the technical side. Good research has always to consist of thinking about what you are doing, attempting always to develop hypotheses, and seeing how they fit and how they can be tested.

Problem of objectivity of research

The above statement makes science dependent on a historical situation. The Hegelian school of thought emphasises rightly that all scientific thought is relative. That is, it is dependent on a certain time, and relative to a certain background. It is a sad accident that Einstein's theory is called the relativity theory and so used to support this general idea, but it could as well be called the absolutist theory, for velocity of light is absolute not relative to some system of coordinates as heretofore thought.

The relativity of science, i.e. its dependence upon a certain time, is a trivial matter and not a deep truth. For example, you can say that we have the 16th century and the 18th century views, but no absolute truth. But this is not true, for even if you can't get at truth, at least one can make definite decisions. The standards in science do not change, and there is progress in one direction. We abolish a theory not because it is no longer true, but because it was never true; for example, that the Newtonian theory is not true. We can thus make progress, and can find a new theory that covers a wider field than the older, and so on. Hence there is no relativity in science. We can ask questions and get yes or no answers. One can, of course, have a kind of relative truth; just the best theory at this time, but this does not mean any relativity of truth. It is just how far we have gone till today. The acceptance of a theory has a time index, but not because of relativity of truth.

The Hegelian school in the form of the Marxist school has also another relativity, not only of time, but also a social relativity. That is, it is not only the time, but also the society you live in, or even the class you live in that determines your science. Hence the doctrine that there is a proletarian science and truth, and a bourgeois science and truth.

This is a serious attack on the unity of mankind, which opens the way to the worst evils. For example, it was put into action by the Nazis with their German truth, etc. Hence would follow the complete breakup of the unity of mankind.

Further, people say that this relativity is not so serious in physics, but is serious in sociology, where there is so much class interest involved. That there, there are two kinds of truth that will never meet.

This is a complete misunderstanding of the whole objectivity of science. Science is a social or corporate enterprise because of its origins and methods. This has been missed by a whole school of sociologists of knowledge (Mannheim). They have missed the social character of science. The mistake was to find the objectivity of the individual scientist in physics, but not in the social sciences. That is, the sociologists of knowledge miss the whole point that science is made objective solely by the social nature of scientific method and criticism. This delusion is due to the dilettantism of these people who have never seen a physicist in their lives. They miss the whole friendly-hostile view of scientists for each other, which is the nature of their cooperation. This depends on the basis of a common medium of language and rationality. Once you break this, you will really destroy science and the whole of civilisation with it.

Science is essentially a public thing: hence the tremendous importance of libraries, where science is for all to look at it. That publicity gives the objectivity to science. Objectivity does not mean that the result is objectively true, but that it is open for discussion by everybody and so it is objective in method.

Measurement in science

A numerical theory is easier to test than a qualitative theory. The moment a theory can predict mathematical values, and is combined with a theory of precision of measurement, it becomes a better theory in that it gives much more opportunity for falsification. Qualitative predictions cannot be refuted so easily..

Two problems result:

1. Degrees of testability – the more *a priori* the falseness, the better it can be tested. Hence we want the smallest number of parameters in equations, e.g. if we have enough, we can fit it to any number of observations, and hence it is no longer falsifiable.
2. The smaller the number of parameters, the more universal the theory, and the more testable it is. A higher degree of generality is thus important as inductivists state, but it also means that one can test with a greater degree of precision, e.g. Einstein's theory as against Newton's. A theory of higher universality must have greater or at least equal precision, i.e. the same or a smaller number of parameters than the theory of lower universality. The direction of progress is towards greater generality, greater precision (i.e. greater testability, fewer parameters).

Even in a qualitative field one may have the same point of going beyond testability, if one adds enough complications.

Lecture 4. Probability

Application of probability to story of induction

Consideration of probability is necessary to finish up the case against induction.

If we say that a certain hypothesis is probable, we may mean:

- I think this hypothesis good (a subjective judgement), or I hope this hypothesis may not be falsified pretty soon;
- probability in a more technical sense, i.e. something which follows laws of mathematical probability theory. For example, the probability of an event happening plus probability of an event not happening – 1 (probability of an event happening is never greater than one), e.g. dice throwing.

Right back to Hume we find the idea that inductively one cannot derive a law, but just a high probability of a law, i.e. it seems that one can have a watering down of law. For example, we could determine say 90 per cent of all men drink tea, Socrates is a man, therefore a 90 per cent conclusion that Socrates drinks tea. This is the simplest way to show that there is a *prima facie* view that by inductivism we could establish the probability of a hypothesis.

However, this method does not play a role in practical scientific method. For example, a scientist never says: 'This hypothesis is 50% probable' (unless he is spoilt by philosophy); he just says: 'I think this hypothesis is a good one'. Hence one takes a slightly suspicious view of the probability story.

But there are more serous objections:

Probability depends on a situation not found in induction, i.e. on a statistical sentence – not on a series of observations – hence it is no wonder that the conclusion contains a probability. A sentence containing probability cannot be derived if you don't put probability in, i.e. you never get in logic something in conclusion that you didn't put in premises. Let us say then, not a definite numerical probability, say merely that it is probable that all men are mortal. But still one can't say it is even probable, as one can't investigate the majority of men. Similarly, one cannot say that it is probable, as one is thereby getting out of the logical system more than one put into it, i.e. putting in something one did not know about. Thus the situation leads to an infinite regress: 'It is probable that….., it is probable that it is probable…., and so on. Thus the attempt to introduce the word probable in no way is allowable. This is a negative approach.

The positive aspect: Question: 'How far has a hypothesis stood up to tests?' Does the answer to this give the probability of a hypothesis? The better test is the one from investigation, and it therefore gives the better hypothesis. We should therefore substitute for probability, the degree of confirmation possible. The hypothesis which has the higher degree of 'potential confirmation', i.e. testability, is initially the more improbable hypothesis, for it gives the greater opportunity for falsification. Similarly, after test the hypothesis that is the better confirmed is the more sweeping one, or the one having the higher precision in its predictions. In other words, the one with the higher degree of falsifiability is the one eventually with the higher degree of confirmation. Thus the strange situation is always that the better testable the hypothesis is, the least probable it is initially, i.e. the goodness of a hypothesis is utterly different from its probability; or, expressed in another way, the more precise the prediction the greater the degree of improbability. For example, it is a more probable hypothesis that asserts one will throw a dice and score 2 to 5, than that which asserts one will throw 4.

Thus we come to the result that the people who speak of the probability of science went contrary to the whole spirit of science. To be more probable you have to be more vague, but what we want in science is not probability but precision; the degree of confirmation is what is of value to science.

How is it that this probability of hypothesis has become so important? With both Hume and Locke it had the same vague use as 'I hope', 'I like', etc., but later people use probability more definitely, e.g. the school of atomic physicists.

There are two kinds of law: (i) statistical for the whole of a population, (ii) laws valid for every individual in a population. These latter laws are of causal character, e.g. the laws of falling bodies.

The statistical laws have become very important in physics, and one can speak of probability phenomena in physics. So people have mixed up a hypothesis about a probability with the probability of a hypothesis. These are obviously entirely contrary, but the confusion has been made and defended for a long time, and has not finally be given up with a good grace and clearly.

Final problem relating to probability

What about a probability hypothesis? Can it easily be falsified? The logical position is that if one makes a statistical hypothesis, how can one falsify it? It implies auxiliary hypotheses that can always be used to back out from a falsification. Hence the problem is, how is this reconcilable with the view that opportunity for falsification is essential if we are to preserve the scientific character of a statistical hypothesis?

However, if we proceed always to invent auxiliary hypotheses, then actually we are not scientists. For example, in tossing heads and tails we have the hypothesis that we get half of each. This can never be disproved by going on long enough and scoring heads only, etc., for one can always say that if we go on still longer it will come all right. As scientists we have to say that in disproof we must go only to a certain extent. Schrodinger recently said that the world would wind itself up again (contrary to the 2nd law of thermodynamics), but he was then speaking metaphysically, not scientifically, for he disregarded this limitation to a series which is essential if its scientific character is to be retained.

Teaching of science

Our system is based on the passive view of science, i.e. 'the bucket theory' of the mind. This theory holds that our minds are passive receptacles like buckets into which information is poured through the various orifices provided by the sense organs. It implies that the mind is passive in learning, and entirely neglects the essential part of learning, namely knowledge in action. This 'bucket theory' is so widespread that it overwhelms teaching entirely and threatens even the organisation of research. In spite of the psychologists' view that we are teaching better, we have still the idea that the more hours you teach the better, i.e. our system is based on the 'bucket theory' of the mind.

The proper method is that everything depends on the degree of activity and not on passivity, i.e. on an active enquiry by students posing problems and looking for answers. We would get close to this if we just cut down intake, for we are always answering questions that are not asked. In fact we overwhelm pupils so much that questions are not asked. In a way the educational system is based on natural selection – only those with first class minds and bodies survive complete damage.

Especially in the secondary school, and still more so at the university level, they are taught to become intellectually dishonest. Here there is the simple belief in the theory that scientific method consists in careful observation and then the derivation of the law; that is, in generalisation. But the actual result is not obtained in this way, for the theoretical conclusion is suggested by the teacher or book and so the process is dishonest. If it were honest, we should get no result – a series of observations, remaining just a series of observations. What can one do?

- Pose problems: Say, 'How could that be solved?' Then say that the following theories have been suggested, and then students could suggest experiments. For example, the phlogiston theory could be shown first before the modern theory of combustion, then experiments that falsified the phlogiston theory, i.e. the historical method.
- One could also show the thrill of science: As long as one feeds results one can get no enthusiasm – that only comes when one shows the human element in it – men erring and quarrelling.

There is a distinction between the arts and science faculties in our universities. The arts faculties, miserable as they are, are more human than the science faculties. But science should become the most human of activities. The great adventure of the search for truth is one of the most fundamental moral activities of man – the search for truth without the lure of knowing if one ever gets there.

Lecture 5. Organisation of science

Application of probability

The statistical hypothesis about probability asserts so little that it cannot be falsified. One could, however, falsify the assertion that the probability of 'tails' is one by just tossing one 'head'; but not if a lower probability is asserted. One could construct mathematical models for all strange sequences, e.g. a regular series 100, 100, 100, 1….. for millions of times, and then suddenly it goes malignant and alters type. Could have a mathematical law covering this behaviour.

This illustrates that a merely statistical hypothesis can never be falsified. It is not scientific, but is metaphysical. It becomes scientific only when we adopt an attitude towards it in order to falsify it. One must use (as in physics) a statistical hypothesis in order to deduce physical effects from it, which can in their turn be tested. Let us take the statistical theory of light. There is a statistical bombardment of photons. Luminosity is just a measurement of the probability of the hits of photons on that point, hence one can deduce effects of relations of distance from source to brightness, or angular relationship, etc. Since these light hits are irregular there is always the probability that the photons will miss the area for a time, etc. The rule in the conversion of a statistical law into a physical law is to convert it into the production of mass effects, and test these effects without the possibility of retreating back into statistical law. If you don't do this, you can explain anything and therefore nothing.

Physicists assert that in any part of the universe there will eventuate a tepid death with temperature uniform and movements uniform. They cannot, however, assert this for the whole universe as they don't know the number of particles in the universe.

Schrödinger's view rests on the idea that, as we have infinite time, the universe will run up in time. He writes that this can be predicted with mathematical certainty. This is true enough, but though this assertion on the surface may be physically correct, it cannot be tested; thus it is metaphysical. The point is not that you cannot wait so long, but that Schrödinger forgets that statistical theory has to be used for prediction of scientific facts and no further. If we accept his view, everything will happen – the world runs up and down in all times and to all degrees. It is a random process. We cannot now, therefore, know where we are – anywhere at any time we may be going up or down. Actually at the back of Schrödinger's view is the conclusion that with mathematical certainty we can predict anything, and moreover with mathematical certainty we cannot predict anything.

Hence one realises the importance of the falsification principle in keeping science to science, and to stop it running away to wild speculation.

Organisation of science

There is a saying of Mussolini: 'Live dangerously'. It is a mean saying, as you always do it at other people's expense. You get into a dangerous situation and rescue parties have the danger too, etc. It is not a good maxim for social life.

There is one field where we can live dangerously, however, and that is in science. If science were a quest for certainty, then we should keep quiet. In science, in living dangerously one need have no scruples. If you make a dangerous hypothesis, then others get a kick out of kicking you; hence it is most exciting for everybody. Children likewise spiritually live dangerously until they get to school.

This leads to an understanding of science which is different from what the inductionists think. James Jeans argued that it is quite wrong to think that science is a revolutionary hypothesis – rather it grows like a library. This idea of growth is false if you look at a practical library. Books are taken from the shelves and put in the cellar at about the same rate as new ones appear. If the scientific library is thus changing, the history of science library grows.

The decisive feature is that science continuously lives in revolutions. I do not believe in social revolutions, for violence and irrationality are predominant there; but in science, revolution is essential to its continued existence; hence we have the colossal liberating influence of science.

Plato had the idea of us living in a cave, etc. with the shadows of reality on the cave wall. We never see reality. He thinks that a few mortals are blessed to see reality, and these can become the spiritual and political dictators of those who don't know. The actual situation in science is similar in part, but also it is totally different. Jeans' view is that the activity of science slowly unchains us and allows us to see more and more of reality. But this may be criticised for we don't know where the light comes from. Our cave is much darker than Plato's ever was. We have no sure direction of light. Our cave is such that by bumping with our heads we can push one of the walls back and the spark so formed illuminates a little; we then bump in another direction, and so on. Some are crushed in the process, but more room is obtained, and so on. There is never any certainty that the whole cave will not tumble down.

Biologically it can be said that science is one of the ways in which man adjusts himself to the world. J.B.S. Haldane and others with eugenicist ideas do not understand the function of thought. Instead of mutations biologically occurring, e.g. longer fingers, we use a pair of pliers, i.e. we develop something outside, by thought. This is a new kind of adaptation to environment, not by changing oneself, not by growing more clever. For example, cave-men may have been as intelligent as Einstein. I see no prospect of eugenics success, but little danger that it will be happen.

Before attempting to eugenise man, one should first understand the nature of science. Science is not just adaptation to environment, for instincts are also this. Spiritual liberation is the main achievement of science, not the adaptation to the environment, for no one would wish to change to an ideally adapted bundle of instincts. This argument tells heavily against the pragmatists, for adaptation by instincts can be amazingly efficient.

A group of pragmatists in London have put forward a movement for planning in science on the basis of the following ideas:

- We cannot tolerate haphazard scientific research – we want more evolution. They are neurotic Darwinists – not only more evolution of science, but faster and faster evolution.
- Science is haphazard as is everything in capitalist society. Therefore we have to organise science for efficiency.
- They insist as pragmatists that this planning should be fundamentally the urging on of applied science. They speak disparagingly of pure science. Their only purpose is evolution for the sake of evolution.

The group contains some influential people. *Nature* is perhaps 65 per cent under the influence of the group. Bernal is a good scientist, and also some other good scientists are in the group.

This method is fundamentally part of the 'bucket theory' of the mind. You can organise research in this way provided that it is a more or less highly skilled technical activity, if you like. The work done is proportional to the time occupied, e.g. 20 hours is twice 10 hours. There is something like this in highly skilled labour, but it is not so with science, where one is working all the time, thinking or sleeping or waking.

In criticism it can be said that there is no obvious way from observation to theory. No way but becoming one with the subject – living with it. It is an attitude which is one of the most personal things in the world. You can organise marriage, but not love. It is just as personal with science. It is an intensely emotional attitude. Science is very largely an emotional affair, but of course it is also rational. The driest mathematical paper ever written is packed with emotion. Emotions are a private affair, and not really science, but cut them off and science stops. Hence pure research cannot be organised.

Real organisation of science

How is it if in your research you strike a problem you are interested in – apart from applied work? Can you go to the boss and say: 'Dear boss, I want to work on this vague idea, I am in love with it. Can I leave my applied problem and work on this for some weeks or months?' The answer to this in New Zealand is always negative. But controllers of research should be able to trust their men, i.e. organised research has to be disorganised research in part.

Of course, it is no use if your man just wants to leave applied work because he is bored with it. Moreover, every man who is doing moderately good research will have the conscience to consider practical points and forego his problem temporarily.

But the whole relationship is important. Does the paymaster say that we only pay you to work for New Zealand agriculture? We don't pay you to make your mark in science, etc.

This freedom is even not uneconomical, because actually one gets more value for money this way. The Kodak laboratories are run in just this way. They give complete freedom and pay also very well. In a way, of course, research must be organised, because one cannot earn money with one's science.

The necessity of this kind of freedom is the direct consequence of the one step in the scientific method – the making of hypotheses. This is not a rational procedure.

Lecture 6. Principle of indeterminacy*

(Notes on an informal talk to a group of physicists)

The question of indeterminacy is the most sensational [sic] in quantum theory. It really is one of the adventures of the mind.

Werner Heisenberg found that deductively his equations led to a formula: that the consequence of lack of precise prediction leads to a statistical character, and that this statistical character is what is ultimately needed to explain the eigen-states of the atom. Erwin Schrödinger showed this with wave mechanics. Heisenberg said that as a consequence there are very few causal laws for atoms, which is, he said, a refutation of the principle of causality.

Although I also don't believe in a general principle of causality, I do not think Heisenberg is right. A statistical conclusion is derived from a statistical premise – a probability conclusion is derived from a probability premise. A simple consequence of this is that some statistics has to be put in, in order to get statistical laws.

The answer to Heisenberg is to discover from which assumption his formula is deduced. Heisenberg said that if one observes a particle, the observation interferes with the particle, i.e. measurement disturbs. Neils Bohr says that his complementarity principle is involved here [i.e. although the velocity and position of an atomic particle cannot be accurately measured simultaneously, the measurements are complementary in giving a complete description of the behaviour of the particle]. He used a 'screen and spring' picture of measurement [to screen out individual particles and measure the force of their impact, see below]. We also have the same situation with energy and time.

Heisenberg is in a different position from Bohr, as he says one interferes with particle in the measurement. He assumes a causal principle and then says that, by using that principle, one reaches a situation where causality doesn't work.

I consider that the whole thing is derived from statistical assumptions. Both the Bohr interpretation and the Schrödinger wave equation imply that the density or wave amplitude is really the probability that the particle will be at a particular place. My view is that you could not get a super-pure case, i.e. a bundle of particles without a wide range of momenta or locations. However, this limitation to accuracy doesn't obtain for a single particle and therefore you cannot exclude the possibility of a more accurate measurement.

The result of the subsequent discussion was that neither Heisenberg or Bohr raised convincing objections. But Victor Weisskopf said: 'if you can measure the particle to a higher degree of accuracy then you should be able to make an apparatus for measuring a super-pure case, i.e. a contradiction exists between the assumption in making this apparatus and the hypothesis of the quantum theory.

I do not think the Heisenberg assumption of disturbing the particle by measurement is more than a vague popular idea. The present situation is that, from indirect arguments, it is clear that one cannot indeed measure both complementary magnitudes beyond a certain degree of accuracy. From the point of view of measurement this is a consequence of the non-existence of a super-pure case, not only in the scientific sense, but also in the ideal fictitious sense.

The Heisenberg disturbance theory has been developed in some respects. If you attempt measurement, you disturb the particle, and you have a new situation. In the jargon in the Heisenberg–Bohr school the essential word is 'smear'. An additional theory is that, only if one makes an experiment does one force the electron to show its flag and say where in the smear it really is: the particle has not momentum and has not position, but you only force it to show its position by measurement.

Einstein and Schrödinger on the other side now say that something is not yet clarified, as the Heisenberg view does not work in quantum mechanics.

Heisenberg and Bohr always show that the principle of indeterminacy works, but they assume more and are rather dogmatic.

Assume that we have a particle, A, and want to measure it. We collide it with a particle, B, and get something like a Compton effect [transfer of energy between particles]. Einstein said that after the systems A and B separate, there is no interaction at all. By measuring either the position or the momentum of B you can measure either the position or the momentum of A. Thus you have a choice after the event, i.e. nobody any longer interferes with A, and so the Heisenberg and Bohr interference idea cannot work.

Schrödinger shows that in all quantum mechanical situations you have just this sort of situation.

Bohr answered by showing with his 'screen and spring' that, if you measure the impulse of B you have to have screen loose for B, and hence also loose for A. Hence Bohr now transfers the blame for indeterminacy away from the electron smear to a vagueness (or subjectivity) of the coordinate system, i.e. the measurement rather than the properties of elementary physical particles. If the coordinate systems are thus smeared, it fits in excellently with Bohr's fundamental ideas of complementarity. This is the present position [*c.* 1934, in a topical debate among leading atomic physicists].

* *See Popper, K.R. 1934.* Logik der Forschung *p. 181. Berlin, Springer, for a full account.*

Lecture 7. Atomic theory and biology

(Notes on an informal talk to a group in the Medical School)

Analogy between organisms and atoms

The individuality problem is common to the organism and the atom.

- The stability of the individual is one of the most fundamental problems in biology. In the atom you have the same quality, stability; but also the property that this stability can be disturbed, e.g. by a light stimulus. The stability is only relatively disturbed by a not too great stimulus, but it can be killed with too great a stimulus. The fundamental idea of atomic theory is that there are a number of possible states that the atom can take, i.e. there is a discreteness of orbits close to the nucleus, but further from the nucleus the discreteness becomes diminished and disappears. The problem of [atomic] individuality is the problem of stability. Similarly, it is likely that the problem of an organism's individuality is the problem of the stability of the organism.
- Probability is very closely connected with the stability problem of the fundamental atom. Experimenting on individual atoms is sometimes possible, but in a large number of experiments on populations of atoms one is just investigating total behaviour. One is also forced in biology into the statistical method because, although one may be able to produce controlled extra-organismal conditions, one can't be sure of intra-organismal conditions, and so the statistical approach is essential for biological investigation. The intra-organismal conditions are dependent on the adaptation of the organism, and hence on its life history.

Counter-arguments

- The atomic analogy is progressively lost as you ascend the molecular scale. The simple molecule is like an atom. The larger the molecule the less it is like an atom. The statistical character of the atom is lost if you come to heavy molecules, i.e. the atomic statistical character disappears with molecules having hundreds of atoms, and this is still more so with the simplest virus, i.e. the analogy is superficial and cannot be carried through rigorously.
- The various stable states of the atom – eigen-states – may exist in the organism analogously, but it is extremely unlikely that they are closely related to atomic states. Thus the discrete character of the organism may have something to do with stable states, but it is unlikely that it is in any way connected with the stable states of atoms.
- As you move further out from the nucleus of the atom, you have a merging of the discrete characteristic orbits into the outer continuous state, i.e. at is gets larger it merges into classical physics. There is apparently nothing analogous to this with the organism.

In accordance with the laws of atomic physics one can never get a series of elementary particles in identical states, i.e. with the same spatio-temporal coordinates. The nearest one can get to a pure state is illustrated by a monochromatic light beam of infinite width. Here there is a statistical distribution of photons. You cannot get a monochromatic parallel beam through a hole because of diffraction.

There may be a real parallelism here with populations of biological individuals, for example with a series of individuals not sexually reproduced, where you could have a pure type. We have no idea whether processes in which biologists are interested are essentially of atomic dimensions. Are they instead of molecular character only?

Effect of X-rays and γ-rays on living tissue

- 'Hit theory': How do γ-rays work on living cells? Is there a valid comparison of the inside of an atom to the inside of some atoms of the living cell? Does the hit inside the atom of a cell lead to a change in the whole cell? This is the 'hit theory', that is, it resembles the Bohr concept [of complementarity, see below].
- Another theory is that all rays are ionising rays (electromagnetic waves), and hence they produce electrical and therefore chemical changes.

  There is no decision between these two theories.

An alternative possibility is quasi-hits; for example, there are susceptible spots where ionisation happens to be of great consequence, e.g. in chromosomes, but it looks like hits, for a high degree of localisation is essential for action.

It would be possible by investigating the statistical character of effects to distinguish between these hypotheses, because one should find a different character in the statistics. One would give a simple statistical relationship (ionisation), the other two superimposed statistical processes. It is likely that both happen. There is no doubt that ionisation happens particularly with mutation, but is seems also that hits happen.

Bohr has interesting ideas on the subject of atomic theory and biology. He operates with the principle of complementarity. There is also the principle of correspondence. The complementarity principle is that you can't have your cake and eat it. Complementarity means for Bohr not only an analogy with complementary angles, but also an exclusiveness. You can get one answer or another, but not both. The reason is that the questions are determined from macro-experience. In the micro-world we are asking too much; just as for example, in the optical plane of a microscope you see only a narrow optical layer, not the great depth of your macro-field of vision.

The Heisenberg principle of indeterminacy is one example of the principle of complementarity. Bohr even says that we get the application of this principle of indeterminacy of the macro-world, but he would agree that peculiar problems are created there. He even applies it also to linguistic problems; for example, it is impossible at the same time to use a word and discuss its meaning, but logic has an alternative explanation. Similarly, the principle of complementarity holds for power and knowledge.

In biology, Bohr is worried by the relationship of the vitalism-versus-mechanism controversy to the free will problem. He sees free will as a relationship between them, and thinks it may be an example of the complementarity principle.

Bohr is not a vitalist methodologically, for he is not interested in the problem of what is just physics and chemistry and what is not. He says that there is something unique about life and something unsatisfactory in considering organisms as just physical and chemical machines. It is possible that this unsatisfactory character will never be removed, as something always escapes us.

If we would like to give a complete description of a machine (how it works) we would have to break it up and put it together again. This would kill an organism, and hence complete knowledge of it is impossible; hence a part escapes us, and hence we can only describe an organism as a machine. I find this idea interesting, but would criticise it in the following respects.

- There is not good evidence that one must kill an organism to find out how it works, e.g. this is not so with investigation with X-rays, or with our electrical investigations, etc. Thus it may not be necessary to kill in order fully to investigate, i.e. the organisms may be so robust that they will stand full investigation.
- I question whether disturbance of an organism on the one hand and of an atom on the other is of the same significance. There may be a disturbance of a cell on quite a crude level.
- If Bohr is right, is it impossible to produce life artificially in a test-tube? The question of production of life is independent of vitalism and mechanism. The origin problem is independent of the functional problem, i.e. we may do things that appear to us miraculous. For example, Beethoven writes a symphony, but he can't explain how it acts on one, i.e. the musical appreciation.

How would this individuality problem appear if Bohr is right? We should be unable to formulate a set of laws of how an organism functions. What if we now have a set of procedures? Whenever A exists, we find B (where B is an actual living organism). In other words, if we can really produce a living organism systematically, would vitalism be defeated? If Bohr's point of view is right, we would have a statistical population distribution curve, with perhaps always a time variable.

Bohr's views on indeterminacy and free will were developed by Pascal Jordan, his pupil – the Bohr amplifier theory of indeterminism. Bohr said that, according to the principle of complementarity you can't will and observe at the same time, but free will would imply that you actually could.

Jordan says that, if complementarity is involved, it must be due to some atomic indeterminacy, i.e. the will is indeterminate in the same way as the atom is indeterminate. In fact, it may have the same basis as atomic indeterminacy with the 'hit theory' – the electron jumping to new orbits. In other words, he thinks that big changes could be produced if only you have amplifiers, hence the name, the amplifier theory.

Personally, I consider that this is an outrageous theory : partly on account of the great size of a body relative to atomic magnitudes, but also because free will is too vague, and too diverse for amplifier theory, which would give a statistical population distribution curve for behaviour.[1]

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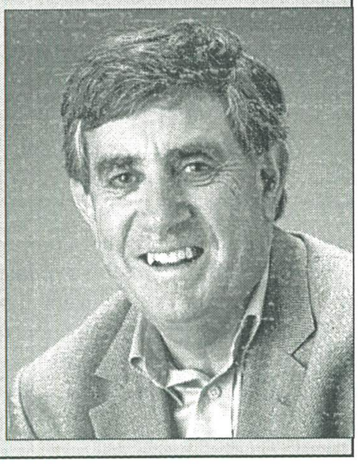
[1] Editor's note: In 1977, in the first Darwin Lecture, Natural Selection and the Emergence of Mind (http://www.informationphilosopher.com/solutions/philosophers/popper/), Popper said:

> *The selection of a kind of behaviour out of a randomly offered repertoire may be an act of indeterminism; and in discussing indeterminism I have often regretfully pointed out that quantum indeterminacy does not seem to help us; for the amplification of something like, say, radioactive disintegration processes would not lead to human action or even animal action, but only to random movements.*
>
> *This is now the leading two-stage model of free will.*
>
> *I have changed my mind on this issue. A choice process may be a selection process, and the selection may be from some repertoire of random events, without being random in its turn. This seems to me to offer a promising solution to one of our most vexing problems ... .*

Obituary

Sir Paul Callaghan (1947–2012)



Sir Paul Callaghan, GNZM, FRS, FRSNZ, was arguably the greatest scientist ever to ply his trade in New Zealand. He led the world in his chosen field of science. He led a team of almost three hundred scientists who changed the way New Zealanders do science. He led the thinking behind the science and innovation policies that are embraced today by the major parties in New Zealand politics.

Paul was born in Whanganui in 1947 and often attributed his interest and aptitude for science to the adventurous, free-wheeling childhood he was able to enjoy there. He did not come from a wealthy family, and he was always grateful for the opportunities afforded to him through the New Zealand public education system. This no doubt helped cement Paul's strong sense of social justice and compassion for the less fortunate.

He studied physics at Victoria University of Wellington before winning a Commonwealth Scholarship to Oxford University to study for a Doctor of Philosophy in the Clarendon Laboratory. At Oxford, Paul was introduced to the phenomenon of nuclear magnetic resonance (NMR), which he used to study atoms implanted in crystals that had been cooled to milli-Kelvin temperatures. Paul's first scientific article on 'Nuclear magnetic resonance of Sb^{124} and long-lived Sb^{120} oriented in Fe' appeared in 1972 in *Physics Letters B*.

Paul returned to New Zealand in 1974 with a freshly minted DPhil to take up a lectureship at Massey University in Palmerston North. He soon formed a partnership with chemist Ken Jolley and a JEOL FX-60 spectrometer that enabled him to strike out in a new direction: the use of the NMR effect to study the properties of complex liquids and materials at the molecular scale. This was the field in which Paul would become pre-eminent.

The sensitivity of the NMR effect to the strength of an applied magnetic field allows the use of magnetic field gradients to encode a spatial signature on the atomic nuclei in a sample. The decay of this spatial correlation over time can be measured, providing information about the movements of molecules within the sample. By developing several clever variants on this basic technique, and then designing and building the necessary hardware, Paul's team was able to non-destructively image the structure of soft materials under strain or shear. This mastery of technique and technology allowed Paul's team to be the first in the world to image the internal structure of a microporous material and the first to observe of the flow profile of a complex polymeric liquid during shear banding.

At Massey, Paul's natural talents for leadership soon began to shape his career. In 1984 he was made Professor of Physics and took over as head of the new physics department, a position which he held for more than a decade. This role involved many new responsibilities and Paul soon found he was busier than ever. Looking back on those years, Paul would often remark that the busier he became, the more success he had. This era saw a step change in his research productivity and impact, culminating in his first book, *Principles of Nuclear Magnetic Resonance Microscopy*, published in 1994.

Paul remained an active and energetic lecturer throughout this period. One of us (SCH) was lucky enough to have been taught by Paul as an undergraduate at Massey in the early 1990s and well remembers the panache and clarity of exposition that Paul brought to his lecturing. His sharpness of mind and his deep grasp of the subject matter made an impression on all those he taught.

In 2001, Paul was given the opportunity to return to his alma mater, taking up the Alan MacDiarmid Chair of Physical Sciences at Victoria University of Wellington. This was a great coup for Victoria, which had been struggling to maintain critical mass in its physics faculty in the EFTS (equivalent full-time student) era of university funding. The following year, he helped establish the multi-institutional MacDiarmid Institute for Advanced Materials and Nanotechnology, becoming its founding director.

With the MacDiarmid Institute, Paul hit on a new way of doing science in New Zealand. Having shown how a Kiwi scientist could do world-beating science from a lab in New Zealand, Paul now set out to build an institute of world-beating scientists. Slicing through the institutional barriers that had fragmented the science community in previous decades, he assembled a team of the best materials scientists from around the country. Within a few years, Paul had forged a truly national collaboration of scientists that was competing with the MITs and the Cornells. Many other research institutes and organisations in New Zealand have now followed Paul's model and there is evidence that this has lifted the performance of New Zealand science across the board.

International success opened up many opportunities for Paul. After he became the first scientist outside of Europe to win the AMPERE Prize for magnetic resonance in 2004, Paul was interviewed by Kim Hill on National Radio's *Saturday Morning* show about the science that had put him on the world stage. National Radio immediately realised that it had uncovered a sparkling new talent. Over the next three years, Paul and Kim discussed a diverse range of topics in science, from fatty foods to string theory to antibiotics. Paul became New Zealand's first celebrity scientist.

With the support of the communications staff at the Royal Society of New Zealand, Paul took science communication to a new level. From traditional forms of outreach, such as lecture tours, through to science classes for people in leadership roles in business and the media, Paul was tireless in his efforts to showcase the importance of science to the public. Anyone who was lucky enough to attend a Paul Callaghan talk will have a vivid recollection of his ability to captivate an audience with an unmatched eloquence and flare for storytelling.

At around the same time, Paul's career took yet another turn when he and several of his students and colleagues founded a company called Magritek. In order to take their imaging systems to the Antarctic, Paul and his team developed a portable NMR imaging system that utilised the Earth's magnetic field to control the NMR effect. Realising the value that could come from being able to perform NMR imaging outside the laboratory, Paul and his team started Magritek to commercialise this technology. Today, Magritek exports millions of dollars' worth of NMR instruments for use in teaching and as analytic tools for a number of industries.

This confluence of his new interest in the commercialisation of science and his growing role as a public figure in New Zealand now presented him with another intellectual challenge. Why had New Zealand's prosperity fallen behind that of the rest of the developed world over the preceding decades? Paul's response came in his book, *Wool to Weta: Transforming New Zealand's Culture and Economy*, where he outlined a powerful vision for New Zealand. Aspects of this are now embedded in the policies of all our major political parties.

Paul wrote a number of other books for the general public, including *As Far as We Know: Conversations about Science, Life and the Universe*, based on his interviews with Kim Hill. Of these, he regarded *Are Angels OK?*, which came out of a collaboration between physicists and artists, as the most important of his public works. Paul thought that this project in particular had broken the mould for scientists in New Zealand. Scientists had been unshackled from their laboratories.

Paul loved New Zealand and all things Kiwi with a passion. He was immensely proud of New Zealand's multicultural heritage and particularly valued the place of Maoritanga in contemporary New Zealand society. He prized New Zealand's unique landscape, flora and fauna, and played an active role as a patron of the mainland island, Zealandia, in Wellington. In recent years, reflecting on how he had to use Skype to read his grandchildren in the UK their bedtime stories, he became particularly concerned with what he termed the 'Kiwi diaspora'. He became determined to reverse the outflow of talented young people from New Zealand and make the country 'a place where talent wants to live'.

Paul's exceptional achievements brought him many accolades. For his scientific advances, he was elected as a Fellow of the Royal Society in 2001. In 2005, he received the Rutherford Medal, New Zealand's top science honour, and in 2010, he received the Gunther Laukien Prize and the Prime Minister's Science Prize (together with his team at Magritek). For his achievements as a leader, he was appointed a Principal Companion of the New Zealand Order of Merit in 2005, awarded the 2007 Blake Medal, and named as the Kiwibank New Zealander of the Year in 2011.

Paul faced his battle with cancer with no less determination than he had shown in other spheres of his life. His descriptions of his journey through the health system and the people he met along the way, which appeared in his blog and occasionally the media, were infused with his characteristic humanity. Paul thoroughly researched his cancer and the treatments available, and as his options dwindled, he was prepared to test less credible alternatives such as high-dose vitamin C. These he eventually rejected as his prognosis worsened. He worked as hard as ever throughout his illness, completing yet another monograph, *Translational Dynamics and Magnetic Resonance*, in 2011.

From our own perspective, it has been an honour and a privilege to have worked with such a formidable scientist and human being. It is quite likely that neither of us would have remained in science in New Zealand were it not for the opportunities and support Paul lent us at critical moments in our careers. There are many other New Zealanders, young and old, and from all walks of life, who are similarly in his debt.

Paul passed away at home on Saturday, 24 March 2012, surrounded by his family. He will be mourned by all those whom he inspired, motivated and moulded during a career that was cut tragically short. We will all miss him greatly.

Shaun Hendy and Kathryn McGrath

Press release

New Zealand Association of Scientists, !5 March 2012

Super-ministry not good for health or the environment

The Government's plans* to merge the Ministries of economic development (MED) and Science and Innovation (MSI), the Department of Labour (DOL), and the Department of Building and Housing (DBH), demonstrate a lack of vision around environmental sustainability and human health and well-being, according to the New Zealand Association of Scientists.

'Such a merger shows a desire for science in New Zealand to focus on the short-term bottom line' said Professor Shaun Hendy, President of NZAS. 'But it makes no sense in terms of environmental science for environmental sustainability or in terms of health science to improve the wellbeing of New Zealanders' said Professor Hendy.

'We know that more scientific research is needed to grow industry, manufacturing and exports. But large components of the science system are concerned with the broader view, such as environmental and health science research, areas that do not often deliver an immediate payoff but which can be immensely valuable over longer time frames' Professor Hendy remarked.

The recent report of the McGuinness Institute on the government science system in New Zealand (http://www.mcguinnessinstitute.org/) also highlights the need for a long-term vision for science.

If the newly-formed MSI is to be subsumed into MED, then environmental science management should become the purview of the Ministry for the Environment, and health research should be managed by the Ministry of Health, with suitable allocation of funding, according to the NZAS.

An explicit focus on science purely for economic growth would only further destabilise an already splintered New Zealand science sector. The creation of MSI has had some positives for New Zealand science, but it has not been in place for long enough to establish a coherent strategy for the sector. 'Further change such as this is likely to add more uncertainty to funding structures and to science career paths, especially for younger scientists', said Professor Hendy.

NZAS is hosting a conference on 16 April to address the issue of career paths for early-career scientists (http://www.scientists.org.nz/event/2012/2012-nzas-conference). Confirmed speakers include the Minister for Science and Innovation, the Hon. Stephen Joyce, and the leader of the Labour Party and Labour spokesperson on Science and Innovation, David Shearer.

The New Zealand Association of Scientists (www.scientists.org.nz) is a nationwide association of practising research scientists spanning the universities, technical institutes, Crown research institutes, government departments, industry, museums, other science institutions, and independent researchers.

** Mr Key announced [15 March 2012] Cabinet has agreed in principle to establish a single, dedicated, business-facing government department. The new Ministry of Business, Innovation and Employment will integrate the functions of: the Ministry of Economic Development; the Department of Labour; the Ministry of Science and Innovation; and the Department of Building and Housing. This follows similar moves in Australia and the United Kingdom. 'This new department will help to drive the Government's priority of building a more productive and competitive economy', Mr Key said. 'Our intention is to create the Ministry on 1 July this year. It is also our intention for current employees of the four departments to move across to the new Ministry on 1 July, and for there to be changes at the senior leadership team level'.*

NZAS 2012 Conference

Do Emerging Scientists have a Future in New Zealand?

Rutherford House, Victoria University of Wellington, 23 Lambton Quay, Wellington

9.00 am to 5.30 pm, Monday 16 April 2012

The conference is targeted at emerging scientists, their existing and potential employers, future emerging scientists, policy makers and politicians.

Session one: The State of the Nation; Government, Universities and CRIs

- ***Hon. Steven Joyce***, Minister for Economic Development, Minister of Science & Innovation, Minister for Tertiary Education, Skills and Employment, Associate Minister of Finance
- ***David Shearer***, Leader of the Labour Party, Spokesperson for Science & Innovation
- The whole world in your hands – are you prepared to make New Zealand a place for your talents?. *Prof. Richard Blaikie, Deputy Vice Chancellor (Research and Enterprise), University of Otago*
- A 30-year perspective on career opportunities for emerging scientists in the current CRI environment. *Dr Kelvin Berryman, Manager, Natural Hazards Research Platform, GNS Science*

Session two: Policy, Statistics and Fellowships

- Post-study outcomes of PhD graduates in the Natural and Physical Sciences – what do the statistics show? *Dr Warren Smart, Ministry of Education*
- Trapped in the postdoctoral void. *Dr Melanie Massaro, University of Canterbury*
- The role of emerging scientists in New Zealand. *Dr Prue Williams, Ministry of Science and Innovation*
- The Royal Society of New Zealand and the Rutherford Discovery Fellowships. *Dr Mark Stagg, Royal Society of New Zealand*

Session three: The Emerging Scientists and their Research Mentors

- Training and retaining emerging scientist talent – the importance of networks, mentoring and stakeholder involvement. *Cosmin Laslau, PhD student, University of Auckland*
- A university-based perspective on early career pathways in science. *Dr Rob McKay, Antarctic Research Centre, Victoria University of Wellington*
- Taking the non-traditional route: leaving academia. *Dr Andrew Preston, Publons*
- Cutting the strings of academic convention: scientific research in geographically challenged locations. *Laura Green, PhD student, Victoria University of Wellington*
- The optimism of youth: insights from an emerging social scientist. *Dr Wendy Saunders, GNS Science*
- Stratus: a voice, guide and ambassador for emerging scientists. *Dr Debbie Hay, Senior Lecturer, University of Auckland*
- Don't worry, they always need scientists! *Dr Richard Furneaux, IRL*

Session four: The Industry Perspective

- Science and business converging. *Phil O'Reilly, Chief Executive, Business NZ*
- Chaos theory, getting out of bed and the emerging scientist. *Hans van der Vorn, Managing Director, Izon Science*
- Why PhD graduates are vital to the future growth of our industry, *Dr Peter Surman, Manager, Research and Development, Douglas Pharmaceuticals*

Session five: Panel Discussion

Drinks & nibbles

Contact: Prof. Kate McGrath (Kate.McGrath@vuw.ac.nz);
Dr Justin Hodgkiss (Justin.Hodgkiss@vuw.ac.nz)

Registration fees (including GST):

NZAS members

- Student and postdoc: $40
- Full registration: $120

Non-NZAS members

- Student and postdoc: $70
- Full registration: $150