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Book Reviews

For Good Measure: The Making of Australia's Measurement System

Jan Todd

Crows Nest, NSW, Allen & Unwin, 2004, xiii + 290 pp., AU$45.00, ISBN 1 74114 363 2

This book is a detailed history of the evolution of Australia's measurement system, from its British origins to the opening of the National Measurement Institute.

I read Jan Todd's book as an economist interested in developing a deeper understanding of how measurement underpins innovation and other wealth-creating economic activities. This review will focus on what I think such a (generalist) reader can learn from the book. I am no expert on Australian economic history, nor a specialist in the history of measurement institutions, so I can't comment with authority on the book's contribution in those directions. But I found it a most impressive piece of scholarship and, even as quite a specialist history, a highly readable book.

The premise of Todd's book is that most people, including the politician and the economist, have little idea of the central role played by measurement in a modern economy. I entirely agree with Todd. It is a hidden part of the infrastructure that we take for granted, and which is largely unnoticed—unless it goes wrong.

There is one measurement instrument, however, which is well recognised to have had a huge economic impact. This is the mechanical clock. Karl Marx was one of the first to observe the importance of this:[1] 'The clock is the first automatic machine applied to practical purposes; the whole theory of *production and regular motion* was developed through it' (*his emphasis*). And Lewis Mumford famously concluded that: 'The clock, not the steam-engine, is the key-machine of the modern industrial age'.[2] Indeed, the economic contribution is well documented in several histories of the clock.[3] Indeed this is probably the best-understood economic contribution of any measurement instrument.

Todd (p. 243) identifies three quotes that together capture the economic role of measurement. The first, due to Lord Kelvin, is well known as one of the great maxims of science: 'When you can measure what you are speaking about … you know something about it; but when you cannot measure it … your knowledge is of a meagre and unsatisfactory kind'. In short, measurement is essential to scientific progress. The second is a slogan adopted by measurement agencies in Australia: 'If you can't measure it, you can't improve it'. In short, accurate measurement underpins

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efficiency and innovation. And the third is: 'Tested in one place, accepted in all'. Internationally comparable measurements and standards are essential to the growth of international trade.

These three quotations summarise very succinctly the place of measurement at the centre of scientific advance, innovation and trade.[4] Todd stresses these three principles throughout the book, noting that, 'innovation was demanding more diverse and more accurate measurements and creating a demand for a measurement infrastructure …' (p. 256). An interesting puzzle here for the economist, at least, is to decide what is driving what? Is it innovation that is driving the development of a measurement infrastructure? Or is it the development of this infrastructure that is enabling further innovation? The answer, I feel sure, is a bit of both, as captured in Figure 1 below.

Consider, first, the right hand side of the diagram. For sure, innovation drives the *demand* for further development in the measurement infrastructure. But equally, new developments in the measurement infrastructure *enable* the innovative producer to conceive new products and services—and if there is a demand for these, then measurement is enabling innovation. Note that this is not the same as arguing that all innovations result from 'technology push'. The measurement is not *pushing* anything. The company is pushed by a desire for competitive advantage. Measurement enables innovations, if the company wishes to pursue these. If the company can see scope for competitive advantage from these innovations, then it will exploit this source of competitive advantage.

However, there is more to the interactions, as the left-hand side of the diagram illustrates. Accurate measurement and common standards reduce transaction costs, and if transaction cost theory is right, this in turn leads to a change in organisational architectures, with ever greater use of the market as opposed to vertical integration. Companies specialise in particular components in the vertical chain, so there is much intra-industry trade.[5] Moreover, because specialist companies supply such components to a global market, they can achieve substantial economies of scale. Common measurements are even more important for trade than for

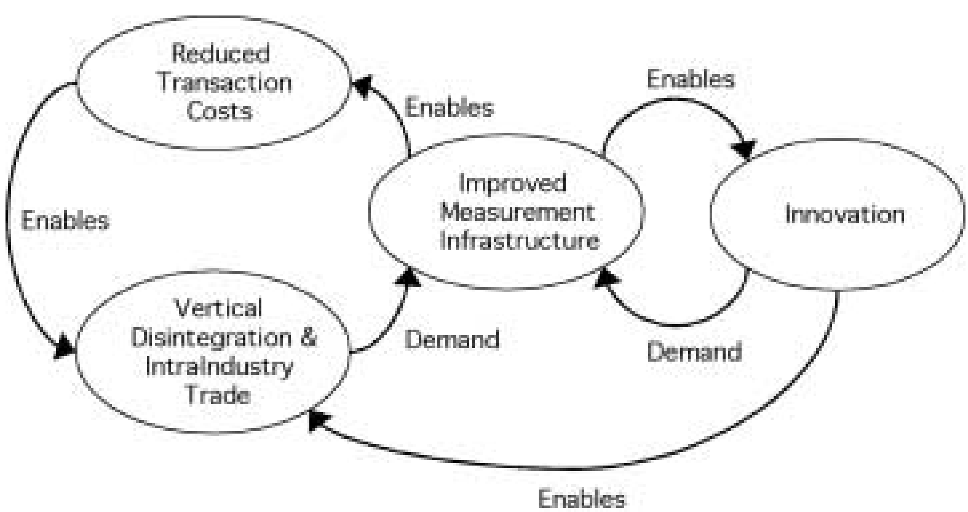

Figure 1. Simple model of interactions between measurement, innovation, vertical disintegration and intra-industry trade

internal processes, so this growth in trade increases the demand for measurement. And even this is not the end of the interactions, because some process innovations (especially those innovations that replace labour intensive processes with capital intensive processes) will increase economies of scale, and hence will reinforce the process of vertical disintegration described above. That in turn leads to a further increase in demand for measurement.

In that sense, I believe Todd may even understate the role of measurement: it is not just a passive *follower* of prior art in innovation; it is an *enabler*, which the innovative company may be able to exploit. However, Todd is right to say that competitive pressure and innovation do drive the demand for measurement capabilities, and at several points in the book she refers to a problem: can measurement capabilities keep up with the demand for measurement?

This is the first of four important and recurrent themes in the book. We often meet the idea that measurement systems are not able to keep up with demand on them, and this leads to a persistent pressure for organisational change—in the (possibly forlorn) expectation of productivity improvement. At times, I was led to draw an analogy to Thomas Malthus' pessimism about the ability of food supply to keep up with demand for food from an exponentially growing population.[6] Does this derived demand for measurement grow exponentially? As we saw in Figure 1, demand for measurement derives from innovation, from division of labour, vertical disintegration of production, and the growth of market transactions. Focusing on the left-hand side of my diagram, if the rate of division of labour proceeds at a constant (proportionate) rate, then the demand for measurement may indeed grow exponentially. If that is so, can the measurement infrastructure keep up? The answer must be, 'not indefinitely'.

The second recurrent theme in the book is the difficulty of finding Pareto-improving rearrangements in a measurement or standards system. Or, to translate from economic jargon to plain English, it is difficult to find organisational changes that benefit some users without damaging the interests of others. Why is this? It is a result of the very high degree of specialisation that is found in a measurement-intensive industry, and the importance of working with measurement technologies and standards that complement the competencies of the firm—if the firm is to remain competitive. Todd's discussion of metrication, and the views of its supporters and detractors, provides a striking example of this general point.

The third recurrent theme, related to the last, is the political sensitivity of some measurement issues. The hot petrol problem (pp. 215–6) is a striking example. The economics of measurement stresses that trade works best when traders have symmetric information. If one side has full knowledge about the items being traded, while the other has incomplete knowledge, then this generally leads to inequitable and inefficient outcomes. This problem can arise in petrol retailing because the value of petrol (to the buyer) depends on its mass, but petrol is sold by volume, and the ratio of mass to volume is not constant from day to day. The volume of a given mass of petrol depends on the temperature of the petrol, and the buyer does not routinely have this information on purchase. The issue became a sensitive one because many retailers thought that they were losing out from this information asymmetry. Many believed that the petrol deliveries they received were on average at a higher temperature than the ambient temperature in their tanks, and hence delivery volumes are artificially inflated.

The fourth recurrent theme is that the development of measurement institutions and technologies evolve in an interdependent way. Or, as we say in evolutionary economics, institutions and technologies *co-evolve*. The book describes the evolution of the Australian system from its fragmentary colonial origins, through co-operation to integration. The book describes how the end result is distinctively different from measurement systems in the UK and USA.

The book will be of interest to four types of reader. First and most obviously, it will be of value to historians of measurement and the institutions of measurement. It provides an important companion to studies of measurement history in the USA and UK, such as those by Lide, Linklater, and Pyatt.[7] Second, those evolutionary economists interested in the co-evolution of technology and supporting institutions will learn from this history. Third, the book will be of value to those interested in National Systems of Innovation—and the role of measurement and standards institutions as part of those national systems. Finally, the book will be of interest to those economists, like myself, whose main interest is to clarify their understanding of the economic role of measurement—especially its role in innovation, productivity, trade, globalisation and growth.

Notes and References

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Alan Turing—Life and Legacy of a Great Thinker

Christof Teuscher (Ed.)

Berlin, Springer-Verlag, 2004, xxviii + 542 pp. US$53, ISBN 3-540-20020-7 hbk

This festschrift volume celebrates the 90th anniversary of the birth of Alan Turing by bringing together a collection of 21 papers and a short play on topics as diverse and colourful as the work and life of the man himself. Turing's fundamental contributions to logic and computing kick started the modern computer era. However, he also made early and outstanding contributions to artificial intelligence, artificial neural networks, morphogenesis, cryptology and the philosophy of mind. The book includes papers on all these areas—much of the material stemming from talks given at the Turing Day conference held at the Federal Institute of Technology in Lausanne, Switzerland in 2002—an event I had the pleasure of attending.[1] Complementary material has been added, and contributors include luminaries such as Martin Davis, Daniel Dennett, Andrew Hodges, Douglas Hofstadter and Ray Kurzweil.

A New Kind of Machine

Given the present-day ubiquity of modern digital computers it is difficult to fully appreciate just how revolutionary and foresightful Turing's logical ideas were in the 1930s and 1940s. His idea of the universal Turing machine, dating from 1936, described a logical device capable of simulating the functions of any other computer. Turing's insight gave computation a formal definition and made possible the further revolution we now called software. At the time and for many years later, computers were purpose-built for specific calculating tasks, and so lacked such universal function. It came as a surprise to many early computing engineers that the functions of one computer could be changed to those of another simply by feeding the first a logical description of the second. In fact, a full 20 years after Turing's universal-machine idea, Howard Aiken, a computer pioneer from Harvard University, remarked in 1956:

> If it should ever turn out that the basic logics of a machine designed for the numerical solution of differential equations coincide with the logics of a machine intended to make bills for a department store, I would regard this as the most amazing coincidence that I have ever encountered.[2]

Such an outcome was amazing, but it was no coincidence—the fact reflected the deeper principles of the universal Turing machine.

Using early computing machines and while working for the British government, Turing was central to the breaking of the Enigma code, used by German forces for encrypting messages during World War II.[3] Papers in the book cover the code-breaking story in some detail—a feat it has been claimed shortened the war by a number of years. The early technological development involved in the code-breaking effort contributed to the construction of the world's first programmable digital computer at Manchester University, which in June 1948 ran its first programme.[4] Despite the subsequent exponential increase in the power and sophistication of computing technology, every computer in the world today remains logically equivalent to a Turing machine.

The Future and Post-humanity

However, this book is not a history book. Perhaps some of the most interesting papers contained in it are related to topics that Turing began, but which remain controversial and futuristic. For example, Turing's work on what he called 'intelligent machinery' caused surprise and alarm in its day, and has continued to foster fierce debate in the fields of computer science and philosophy ever since. When in 1948 Turing considered the idea of giving a machine wheels, arms and camera eyes so that it was mobile enough to find things out for itself in the world, some of his colleagues at the National Physical Laboratory in London exclaimed that 'Turing is going to infest the country side with a robot which will live on twigs and scrap iron!' Turing also proposed a test for intelligence in a machine, which is now famously known as the Turing Test—a topic taken up by Daniel Dennett's fascinating festschrift paper. In the Turing Test, a human judge asks any questions they please of two interlocutors, both of which remain hidden from view. The questions and the replies are typed out. One of the interlocutors is a human, the other is a machine or computer. If the judge cannot tell which is which, the computer is deemed to be intelligent. Despite its apparent simplicity no computer has yet come close to passing the Turing Test, and Dennett concludes that the difficulty of the test means that if a computer did pass it, it would be in every theoretically interesting sense, a thinking thing. Clearly computers are not built the way we are, and so passing the Turing Test will not stop philosophers asking questions such as: would a Test-passing machine *really* be intelligent or would it posses only *faux*-intelligence? Would such a machine *really* understand, or *really* have a consciousness like we humans do? If and when computers do pass the Turing Test, I suspect such questions will become moot. Humans have strong psychological biases to treat many things anthropomorphically.[5] A machine that conversed well enough to pass the Turing Test is certain to be irresistibly treated as another intelligence, regardless of its construction. Furthermore, Ray Kurzweil, argues in his festschrift paper that the exponential development of technology means that such an outcome will be only the quiet beginning. He claims that due to the law of accelerating returns, the amount of technological progress we will see in the next 100 years will be more like 20,000 years worth, and nothing like the previous century. Large quantitative changes can have significant qualitative effects—so we should not expect technology and computers to be analogues of what they have been in the past, but to be radically different in future. Kurzweil claims that such rapid development will eventually lead to a post-human future, where biology and technology will fuse, humans and machines will merge into one, and the rate of development will accelerate to a level causing a 'rupture in the fabric of human history'.

Seeds of Kurzweil's dramatic post-human ideas can be found in a number of aspects of Turing's original work. Christof Teuscher's paper takes up one of these, by exploring and significantly advancing the work that Turing carried out as early as 1948 on artificial neural networks. Turing claimed that these networks were the simplest model of the human cortex and was interested in building an 'electronic brain', which could be taught as one would teach an infant. In addition, before the term genetic algorithm was coined, Turing even proposed the use of what he called a 'genetical search' to configure his initially randomly-connected networks. Some of these proposals remain undeveloped to this day. To my knowledge, Christof Teuscher and I are the only ones to have conducted actual simulation experiments with Turing's networks. Copeland and Proudfoot have published discussion papers regarding them, but their work contains errors.[6] Thus this area represents a fruitful research opportunity for anyone willing to take it up.[7] Another aspect of Turing's work that may contain the seeds of a post-human future involves work in morphogenesis that Turing published in 1952—a paper which has since been called one of the most influential in the whole of theoretical biology.[8] Morphogenesis is the process of the development of structure in living things. Jonathan Swinton's festschrift paper takes up an unpublished aspect of Turing's morphogenesis work, namely that of Fibonacci phyllotaxis, or the particular structural arrangement of leaves and florets in certain plants. Turing became interested in the structure and development of living things through his desire to construct an artificial intelligence and likely intended to apply such natural organisational principles to his machines and artificial neural networks. Thus, Turing was interested in both the opposing design approaches that have become known today as top-down (writing code to yield specific desired functions) and bottom-up (training neural networks to approximate desired functions). It would be fascinating to know whether he intended to use them in combination, an approach I often call the middle-out design approach.

Six Impossible Things Before Breakfast

The books weakest section is that on so-called 'hypercomputation' or super-Turing machines—these theoretical devices have no actual basis in Turing's original work. Turing machines define the extent of what is computable, and the set of computable functions can be shown to be a sub-set within the set of all functions. Therefore, functions exist which can not be computed by a Turing machine. Given such an apparent limitation of Turing machines, it is perhaps understandable that someone will attempt to define a super-Turing machine which can do more than a regular Turing machine—that is, take up what Martin Davis calls in this paper 'the impossible as a challenge'. It is important to understand that more than just a speed-up in the operation of a computer is needed to achieve more than a Turing machine. Faster and faster computers are produced all the time, but all remain simply faster Turing machines and therefore are restricted to the set of computable functions. All theoretical models of super-Turing machines that I am aware of obtain their ability to do more than Turing machines by adding some kind of infinite factor to the mix—either allowing the machine to run for infinite time, or allowing it to perform an infinite number of steps in finite time, or allowing it to have infinite precision variables. These models are all well and good if only considered as theoretical curiosities—a lot of strange and interesting things can happen when you include infinities in any system.[9] However, the 'hypercomputationalists' have begun claiming that such super-Turing machines are soon to be built *here* in our spatially and temporally finite universe, and that this is some new kind of revolution in computing. For super-Turing machines to be anything more then regular Turing machines it is critical that they retain their unapproximated infinite factors, but this is impossible in the world, short of a radical revision of most of the physical laws of the universe as we know it. In addition, the papers on super-Turing machines in the book contain inconsistencies and contradictions even within their own claims. For example, in the paper by Eugene Eberbach *et al.*, it is stated that:

> By contrast, modern computing systems process *infinite streams* of *dynamically generated* input requests.

This is nonsense for two reasons. First, as the life span of a modern computer, or even a large or persistent network of computers, is a very finite number of years, it is clearly false that it can 'process *infinite streams*' of input. Even the lifetime of the universe in which computers exist and the power supplies with which they need to run are finite. Second, if modern computers really were capable of processing *infinite streams* of input, then they would already be super-Turing machines, and so Eberbach's quest to develop such devices would be over before it began.

Conclusion

There is little doubt that Alan Turing was one of the most influential and important thinkers of the twentieth century. Computing technology has now been developing for over 50 years—according to Kurzweil, that time has seen about 32 *doublings* in computing power. If technological development continues at such an exponential rate we can expect to see some radical developments in the next decade or so. Will computers outstrip human intelligence, and when might it happen? Will we become more like computers, or will they become more like us? Ninety years on from the birth of Alan Turing such issues are more relevant and pressing than ever, and this book makes an excellent advanced introduction to the breadth of Turing's work.

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The Web of Science. The Scientific Correspondence of the Rev. W.B. Clarke, Australia's Pioneer Geologist, 2 vols

Ann Moyal

Melbourne, Australian Scholarly Publishing, 2003, 1,340 pp., AS$200, ISBN 174097042X

Correspondence has been a vital aspect of scientific advance since the scientific revolution of the seventeenth century. With the spread of printing, personal scientific correspondence provided the basis for scientific publication in journals such as the *Philosophical Transactions* of the Royal Society of London. In the nineteenth century, letters evolved into formal (and impersonal) scientific papers being written for increasing numbers of specialist scientific journals. Yet, this did not supersede personal scientific correspondence.

With the quickening pace of scientific discovery and the codification of new techniques and disciplines, the nineteenth century saw a massive expansion in the number of people (nearly all of them men) engaged in scientific activities and sharing their thoughts and skills on paper. The establishment of regular forums for direct interaction, such as the annual meetings of the British Association for the

Advancement of Science, only increased the scope for written communication. The spread of colonisation opened new fields for scientific endeavour and stimulated parallel studies in many disciplines. Networks of correspondence underpinned the elucidation and interpretation of scientific evidence in disparate parts of the world and provided fuel for argument between colonial and 'home' scientists over their relative fitness for providing those interpretations and the accompanying rewards of priority.

Indeed, between the introduction of regular pre-paid postal services—the 'penny post' began in England in 1840—and the advent of the telephone in the late 1870s, the letter was the sole means for long distance scientific correspondence. This is exactly the period in which the Rev. W.B. Clarke (1798–1878) built up an extensive web of scientific correspondents which spanned the globe from his base in Sydney.

William Branwhite Clarke was born in Constable's village of East Bergholt, Suffolk, the son of the local school master. Following his schooling Clarke went to Cambridge to take holy orders. His active mind drew him to literature and the classics, leading to the production of volumes of poetry and contributions to literary magazines. This habit of writing served him subsequently in his correspondence and his scientific journalism, and no doubt in the preparation of sermons.

By the 1820s geology was developing as a vigorous discipline in England. Clarke fell under the spell of geology in Cambridge guided by the mineralogist Edward Daniel Clarke and the newly appointed Woodwardian professor of geology, Adam Sedgwick. Both professors were ordained Anglican ministers so despite the wilder religious hostility to the claims of geology concerning the antiquity of the earth and processes of change, young Clarke had models for a liberal and harmonious view of the relations between religion and geology.

There is a breathless enthusiasm in the hand-written election certificate which Clarke took to London in 1826 to become a fellow of the Geological Society of London.[1] He had by then conducted several geological excursions, including visits to continental Europe, and over the following years produced numerous geological papers.

In the absence of connections and patronage, his career as an Anglican minister was a modest one. A growing family and considerations of health (as he later asserted) led him to emigrate to Sydney in 1839. He remained a practising minister throughout his career but undertook extensive geological fieldwork in New South Wales as opportunity arose, and conducted official mineralogical surveys for the government. He also contributed numerous articles on scientific topics, especially exploration and meteorology as well as geology, to the local press. He was long associated with the Australian Museum in Sydney, as secretary in the early 1840s and later as a trustee, and was a founder and active participant in the Royal Society of New South Wales. And amid everything else, he maintained a vigorous and wide-ranging correspondence. It is the scientific component of his correspondence that has now been published.

Personal letters provide key evidence for the elucidation of an intellectual life. This is reflected in nineteenth century biographies in the mode of 'life and letters'. In the twentieth century, there have been a number of major projects to publish the complete correspondence of intellectual figures in numerous volumes. These are often extended over lengthy periods. It was decided in 1901 to publish an edition of Leibniz's works, including letters, the first volume appearing in 1914. Despite the political upheavals of the century, volumes continued to appear into the 1990s.[2] The Charles Darwin Correspondence Project is a prominent example in the late twentieth century, having reached volume 13 in 2002.[3] The Internet offers an alternative to hardcopy publishing with the added advantage of scope for revision. The correspondence of the photographic pioneer Henry Fox Talbot has been prepared to high scholarly standards and published on the Web.[4]

The preparation of an edition of intellectual correspondence is a significant enterprise, usually undertaken with extensive institutional backing. It is therefore remarkable that Ann Moyal, as an independent scholar, has had the wherewithal and stamina to locate and transcribe the nearly 900 letters that make up this two-volume edition of W.B. Clarke's scientific correspondence. Dr Moyal has made a diverse and significant contribution to the unearthing and interpretation of Australia's scientific and technological history over some 40 years, Clarke having been with her in a sense for much of that period. Clarke featured prominently in Moyal's 1976 book *Scientists in Nineteenth Century Australia*.[5]

The first volume of *The Web of Science* provides an extensive biographical introduction which emphasises a number of themes that emerge from the correspondence. This is followed by 402 letters spanning the years 1836–63. In the second volume, the correspondence from 1864 to 1878 is followed by a geological table, a glossary, a list of manuscript sources, a very substantial 'scientific bibliography of W.B. Clarke', a bibliography of secondary sources, a register of the 895 letters giving their sources, and two indexes, one general and one of people. Pagination is continuous across the two volumes.

Clarke's outward letters are transcribed either from the repository of the recipient's correspondence or from drafts or copies preserved in Clarke's own papers. Very occasionally some of the letters were published in newspapers or periodicals. The majority of the letters are those sent to Clarke.

The task of transcribing such letters confronts several difficulties. The original manuscript can be fragile, torn or incomplete. The context of a letter may be obscure, particularly where only one side of an exchange is available. And then there is the handwriting! So often the letters end with an apology for the hasty scrawl, but the next mail was just about to go, when missing the boat (literally) could mean a delay of weeks. Clarke's handwriting was a trial to his correspondents. Adam Sedgwick in England, gouty and disgruntled, complained repeatedly of the difficulty of reading Clarke's letters:

> By the way [Sedgwick wrote in 1846 in reply to several letters from the previous year], it is no easy task to read them, & it will employ me the whole morning. I punish all my correspondents with my abominable scrawl: but you pay me back with interest: for your hieroglyphics are most formidable & I have given up on some of your letters in absolute despair of making them out completely (letter 70).

Moyal has dealt with such difficulties with occasional insertions of '[word obscured]'. For the most part these instances do little to interfere with an understanding of the discussion.[6]

In support of the letters Moyal has provided extensive notes at the conclusion of each. An often substantial biographical sketch is given the first time a correspondent writes to Clarke. Other notes provide diverse explanatory background on people, events or publications mentioned.

The original letters contain numerous sketches of fossil specimens and geological strata. These have been entirely omitted from the published edition. This was probably governed by considerations of cost but it is regrettable. The sketches would have enlivened the printed text and in some instances enabled the reader to make sense of relevant passages. There is, however, a stronger reason from regretting the omission. The sketches are an integral part of the process of communication of ideas. As John Macculloch remarked in the early nineteenth century, 'To the geologist, this art [of drawing] is invaluable, since there is much that words can never convey; while it prevents endless circumlocutions and details, which, even when given, leave much in obscurity and doubt'.[7] The sketches are part of the cognitive content of Clarke's correspondence and not merely an embellishment.

The publication of Clarke's scientific correspondence is likely to reveal more letters than Moyal has traced. The possibility of a second edition, perhaps on the Internet, would provide an opportunity to incorporate these letters and to amend various errors in proper names and scientific terms.

The letters reveal much about Clarke's geological interests and activities as one would expect, especially the long-running issue of the dating of the coal strata in New South Wales and matters relating to gold and Clarke's role in its discovery. Meteorology is another prominent theme, especially in the correspondence with the retired naval captain Phillip Parker King. As the recording of topography was important to geological surveying Clarke took great interest in the advent of the aneroid barometer as a more convenient means for determining altitude than the mercurial mountain barometer.[8] The correspondence shows several responses to the aneroid.

Rather than emphasising the content of Clarke's correspondence as such, however, it is perhaps of interest to readers of *Prometheus* to examine what the letters reveal about the processes of communication in the nineteenth century. That Clarke preserved so much of his correspondence shows that it was very important to him and reflected his sense of the significance of his scientific work. Although Clarke was not one of the major figures of Victorian science, he built a network which drew together such prominent scientists as Sedgwick, Murchison and Darwin as well as many minor figures and numerous others in between. The correspondence thus serves to show the complexity of scientific engagement among a great variety of people.

That Clarke was operating in a colony remote from the imperial centre gave him certain advantages in building such a network. Had he been in London so much of what was put on paper would have been said face to face at meetings of the Geological Society. Like Darwin in self-imposed exile in Downe, Clarke was reliant on correspondence for most of his scientific dialogue. While he did not have Darwin's scientific pre-eminence or focus of intellectual purpose, he did have a geographical and intellectual advantage for his work based in Sydney. His extensive geological knowledge was of considerable advantage to New South Wales and adjacent colonies, and the knowledge he gained from his own fieldwork and the collating of information from the other Australasian colonies made him a worthwhile correspondent for savants in Britain.

Although by the 1850s Clarke's prominence made him a target for people seeking advice, it is clear that he initiated many of the exchanges with scientifically minded men in other Australasian colonies and further afield as well, men such as Ronald Campbell Gunn and Charles Gould in Tasmania, Thomas Burr in South Australia, Frederick Barlee in Western Australia, and James Hector among others in New Zealand. Clarke drew local geological knowledge from such people while also providing advice and encouragement along with copies of his own publications. In the 1860s Victoria maintained a Geological Survey, the most vigorous geological campaign in the country, and Clarke engaged in an active correspondence with several of the scientific staff. When the survey was terminated—'ruthlessly swept away' says George Ulrich (letter 487)—in 1868, Clarke was instrumental in getting one of the Victorian geologists, Christopher D'Oyly Aplin appointed to a post in Queensland, as he had done earlier for Richard Daintree. Similarly he seems to have been instrumental in getting Charles Gould (the ornithologist's son) a geological post in Western Australia when funds dried up in Tasmania.

Clarke had a knack of developing warm friendships, even with much younger men such as Daintree and the Sydney University geologist Alexander Morrison Thomson. 'My dear Clarke', Daintree wrote in 1863, 'I think we might manage to drop the Mr for I shall have to write to you rather often for the future I'm afraid' (letter 378). Even with men he had not met, the correspondence provided opportunities for the exchange of fellow feeling with a sympathetic colleague. Henry Piddington in Calcutta bemoaned the lack of scientific support in 1848:

> The penalty one pays for all scientific research in India is to be thought 'fit for nothing else'. The road to preferment is to be a first rate tiger or hog hunter and to be grossly ignorant of the language, manners and customs of the people—to be notoriously in debt—& be the editor of a scurrilous newspaper (letter 98).

Such was the venting of feelings that this global network of correspondence made possible.

In order to keep informed in geological and other scientific developments Clarke needed to obtain journals and other publications. Rather than having them sent willy nilly through the post, Clarke had an agent in London who accumulated material to send as a parcel from time to time. This was the bookseller Richardson of 23 Cornhill who is mentioned over a long period of years. James M. Richardson was not only a prominent bookseller—he was chairman of a committee of booksellers seeking to maintain restrictive trade practices in the 1830s—but also a stockbroker.[9] There were presumably commercial advantages in provided such a shipping agency service but the published letters shed no light on Clarke's financial arrangements with Richardson.

Other figures also played a personal role in the maintenance of the extended networks of communication. In 1846 Adam Sedgwick advised Clarke that if he had a packing case to send—presumably containing fossils or other specimens—it should be addressed care of the porter of the Geological Society in London. Sedgwick had a running account with the porter who would settle the fees. Without such a person on the spot such shipments could languish interminably:

> If any delay occurs for want of an agent [Sedgwick advised] the boxes are put in a kind of wharf lumber room & forgotten. I remember being in this way kept out of a box of specimens from Madeira for two or three years, tho' I applied for it again and again personally & by agents: and at last it only came to me on the return to England of the friend who had sent it. He routed it out by a personal application to the Captain who had brought it to England (letter 70).

Given these difficulties it is remarkable how readily Clarke and others were willing to lend books and specimens to people in remote locations. It was often the case though that parcels were entrusted to friends sailing between the ports of the correspondents.

Among those habitually at sea, the officers of the Royal Navy had a special role in linking the scattered intellectual elite of the British Empire. On arriving in a colonial port the naval officers were a source of news from other parts of the empire as well as information on scientific matters such as curious meteorological phenomena encountered on the voyage. And the resident scientists provided intellectual stimulus to punctuate the isolation of the years at sea. While naval officers were at times prevailed upon to courier parcels they could also look to the residents for support. Frederick Evans, master of HMS *Acheron* stationed in New Zealand, wrote to Clarke in 1850. He had heard that HMS *Rattlesnake* was to return home and thought he might be able to swap with *Rattlesnake*'s master as his absence from England 'has caused much affliction in my little domestic circle'. It was a delicate matter and Evans wished Clarke to talk to Owen Stanley, Captain of the *Rattlesnake*: 'I am sure you will pardon my apparent liberty in soliciting your good offices on a matter so foreign to your ordinary occupation' (letter 114).

In addition to the sketches which form a part of the original letters, other illustrations accompany them. When in May 1845 Clarke asked Sedgwick for a spare copy of his portrait 'that Australians may take cognizance of your visage!', Sedgwick grumbled that 'I have had at least 20 similar applications, & I am beginning to be tired of them for the print is not my property … I am compelled (rather against my will) to go to a shop & pay for it'. Eventually Sedgwick sent the 'lithograph of my own Phis.' which Clarke acknowledged in June 1847: 'You are in an Australian wood frame over my drawing room mantle-piece and very much you are admired' (letters 61, 70, 75, 82).

In 1870 though, it is a very different matter, when de Koninck adds a post script: 'May I be so bold as to request you to send me a photograph of yourself'. He would send one of himself shortly (letter 525). In fact there are frequent references to photographs from the early 1860s. In 1861 Clarke wrote to Darwin enclosing a stereoscopic view of an area under discussion. The photograph 'pleased me much' Darwin replied, 'for it has vividly recalled to my mind the views from the Blue Mountains' (letters 322, 330). How Clarke obtained his photographs is not always clear but some of them came from Daintree who is better remembered today as a photographer than as a geologist. 'Any photographs you have of mine, or may wish to have are entirely at your service for any purpose you please', Daintree assured Clarke in 1863 (letter 377).

Publication was of course an essential part of communicating science and much of Clarke's correspondence touches on publication in various ways. Reports and papers are sent, advice on the costs of figuring specimens given. Clarke was an inveterate contributor of articles and papers in addition to the reports he prepared for the government. He sent papers to the Geological Society in London but he published extensively in Australia also. The 'Sydney Herald [is] our only scientific publication in N.S.W.' he informed Sedgwick in 1842 (letter 33). Clarke contributed numerous articles, reviews, editorials and letters to the *Sydney Morning Herald* between 1841 and 1874, covering a broad range of scientific subjects.

In the 1840s the principal scientific periodical in Australia was the *Tasmanian Journal of Natural Science*, founded while Sir John Franklin was governor. Clarke contributed to this but found it difficult to obtain in Sydney. 'It occurs to me also to request whether it would not be admirable', Clarke suggested to the editor Ronald Campbell Gunn, 'to have some agent, say Mr Colman, Booksellers or Mr Ford both of George St. Sydney, from whom the numbers could be procured'. But as Gunn replied 'A fair number of all the early Nos were sent to a Bookseller in Sydney for sale, but no acct sales or proceeds were ever received so that I now propose to forward Nos to all applicants direct' (letters 71, 76). Here we see the fragility of scientific infrastructure in colonial Australia. Quite a number of scientific journals were short lived but major newspapers were enduring.

As an increasingly prominent expert, Clarke became a target for people seeking advice on matters of potential economic gain. Hopeful correspondents sent him mineral specimens to identify. He was fortunate in being able to pass them on to Dr Thomson for analysis but it was clearly an unwanted burden: 'If you can give me your opinion before Monday', Clarke wrote to Thomson in 1868, 'it will save me some letter writing, as persons who send such matters for examination write & write if they don't get answers *by return of post*' (letter 478). This was a common problem for scientific figures. Sedgwick had grumbled to Clarke a decade earlier:

> Sometimes for many days together, I literally do no work but what is employed in answering the questions of my correspondents—men who have no claim upon me, who can give me no real information; who know absolutely nothing of geology; yet who smother me with questions which they do not know the drift of: cannot or will not understand my answers when I do my best to answer them; and yet are mightily offended if I remain silent (letter 230).

Here we see the gentleman's dilemma in the 'moral economies of exchange'. For scientists like Clarke and Sedgwick, correspondents could be a valuable source of specimens or information. More generally there was a role in aiding economic development, at least for Clarke. For every 'useful' unsolicited correspondent there were probably several who were merely a burden. As gentlemen the scientists were obliged to respond. Anne Secord has analysed the rituals of exchange between artisans and gentlemen in scientific correspondence in early nineteenth-century Britain.[10] In the more fluid social conditions of colonial Australia one might expect less deferential approaches to the gentleman expert. Either way it is clear that the public recognition which such public experts strove for also brought them an unwanted claim on their time.

The natural sciences have on occasion been stigmatised as glorified stamp collecting. However unfair this may be it is perhaps not surprising that Clarke's correspondence provides some early examples of interest in stamp collecting. The Victorian age was an age of things, and stamps by their size, variety and design readily lent themselves to a culture of collecting.[11] John Edward Gray, keeper of zoology at the British Museum, claimed to have begun collecting stamps on the very day the Penny Black was first issued in 1840.

Stamps reflect the rapidly expanding patterns of postal communication in the mid-nineteenth century and became a way of displaying national character and identity. The first mention of stamps in Clarke's correspondence comes in a letter from the Belgian palaeontologist Laurent de Koninck who had undertaken to describe some of Clarke's palaeozoic fossils. This represents an expansion beyond the English-speaking sphere of Clarke's previous correspondence and perhaps he had been encouraged to send some stamps to open the exchange of letters:

> I thank you most warmly for the postage stamps that you have had the kindness to send me. Since you tell me that you have procured some for the children of my learned colleague [James Dwight] Dana [at Yale], I believe that I will give both him and you pleasure by enclosing in my letter some postage stamps which were sent to me by one of my little protégés who hopes to receive in exchange some of the numerous special stamps which are current in the U.S.A. and which have not been defaced there (letter 409).

This was written in February 1864, the year in which Georges Herpin coined the term *philatélie* in Paris.

These various matters reflecting the nature of communication in the nineteenth century, its processes, difficulties and character, are largely independent of the reasons why many readers would turn to the correspondence of a scientifically active Anglican minister in colonial Australia, but they illustrate the way in which the publication of original documents can reveal aspects of the past that evade the purposes of writers of historical monographs. *The Web of Science* is a pleasure to read as much for its incidentals as for its scientific dialogues.

Notes and References

1. Such certificates were usually printed forms with the name of the candidate and those of the proposers written in. Clarke's certificate (No. 669) was written by Clarke's Cambridge mentor J.S. Henslow, then professor of mineralogy. The late John Thackray kindly arranged for me to inspect archival material in the Geological Society of London in 1992.
2. Michael Hunter and Malcolm De Mowbray, 'The editor in the republic of letters', *British Journal for the History of* Science, 30, 1997, pp. 221–5.
3. The Darwin Correspondence Project began in 1974. Volume 14, covering the year 1866, will be published shortly, bringing the project almost to the half-way point.
4. *The Correspondence of William Henry Fox Talbot*, comprising nearly 10,000 letters, was prepared for publication under the direction of Larry J. Schaaf and is hosted by the University of Glasgow at: ⟨www.foxtalbot.arts.gla.ac.uk⟩. The Darwin Correspondence Project also has a website: ⟨http://www.lib.cam.ac.uk/Departments/Darwin/⟩. All the letters from the first seven volumes are being prepared for publication online, and presumably all Darwin's correspondence will eventually be available in this way.
5. Ann Mozley Moyal (ed. eith Introductions) *Scientists in Nineteenth Century Australia: A Documentary History*, Cassell Australia, Melbourne, 1976.
6. Clarke notes in 1870 (letter 541) 'I am writing with a split skewer called a pen'. As metal nibs had come into widespread use in the 1840s it is not clear what innovation this represented, nor whether the 'split skewer' improved Clarke's writing!
7. John Macculloch, *A System of Geology*, London, 1831, vol. 2, p. 482.
8. See Julian Holland, 'Australian exploration and the introduction of the aneroid barometer', *Bulletin of the Scientific Instrument Society*, No. 61, June 1999, pp. 24–6.
9. James J. Barnes, *Free Trade in Books*, Oxford, 1964, p. 7 ff.
10. Anne Secord, 'Corresponding interests: artisans and gentlemen in nineteenth-century natural history', *British Journal for the History of Science*, 27, 1994, pp. 383–408.
11. Asa Briggs, *Victorian Things*, London, 1990 devotes an entire chapter to 'Stamps—Used and Unused'.

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