

TWENTY CHALLENGES FOR INNOVATION STUDIES

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by

Ben R. Martin
SPRU – Science and Technology Policy Research,
University of Sussex, UK Science and Technology Policy Research, The
Freeman Centre, Jubilee Building, University of Sussex, Brighton, BN1 9SL,
Email: B.Martin@sussex.ac.uk

and

Centre for Science and Policy (CSAP) and
Centre for Business Research, Judge Business School,
University of Cambridge, UK

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Abstract

With the field of innovation studies now half a century old, the occasion has been marked by several studies looking back to identify the main advances made over its lifetime. Starting from a list of 20 advances over the field's history, this discussion paper sets out 20 challenges for coming decades. At a conference in 1900, David Hilbert put forward a list of 23 unsolved mathematical problems that were to have a profound influence on the work of mathematicians during the 20th Century. The intention here is to prompt a debate within the innovation studies community on what are, or should be, the key challenges for us to take up, and more generally on what sort of field we aspire to be. It is argued that the empirical focus of our studies has not kept pace with the fast changing world and economy, especially the shift from manufacturing to services and the growing need for sustainability. Moreover, the very way we conceptualise, define, operationalise and analyse 'innovation' seems rooted in the past, leaving us less able to grapple with other less visible or 'dark' forms of innovation.

Keywords: innovation studies; science policy; challenges; achievements; dark innovation

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*‘WHO of us would not be glad to lift the veil behind which the future lies hidden; to cast a glance at the next advances of our science and at the secrets of its development during future centuries? What particular goals will there be toward which the leading mathematical spirits of coming generations will strive? What new methods and new facts in the wide and rich field of mathematical thought will the new centuries disclose?’
(David Hilbert, 1902)*

1. Introduction

At the International Congress of Mathematicians in August 1900, David Hilbert set out ten mathematical problems as challenges to the mathematics community. Subsequently increased to 23 (Hilbert, 1902), the problems included proving the Riemann and the continuum hypotheses. These challenges were to spur the efforts of mathematicians for decades to come. Can one likewise identify major challenges for innovation scholars to address over coming decades?¹ To do so will require taking ‘a risky walk on thin ice over deep waters’ (Lundvall, 2012, p.10), straying some way from my own area of expertise and drawing on ideas from a wide range of sources. The intention is not so much to come up with a definitive list but rather to stimulate debate among innovation scholars.² The challenges and the underlying arguments have therefore been couched in a forthright and often critical manner to wrest readers from taken-for-granted orthodoxies and cosy assumptions, and to encourage them to apply the same critical analysis we apply to others instead to ourselves.

Like Hilbert, we should seek to identify challenges that are ‘difficult in order to entice us, yet not completely inaccessible, lest [they] mock at our efforts’ (Hilbert, 1902, p.438). In certain respects, this effort to peer into the future in our field is more complicated than that which confronted Hilbert. In his view, mathematics ‘evolves from itself alone, often without appreciable influence from without’ (ibid., p.440). While this undoubtedly overstates the case, innovation studies is more subject to external influences, the unpredictability of which renders our task more complex. Furthermore, once a mathematical problem has been set, it is normally relatively straight forward to say when a solution or proof has been found, whereas in innovation studies there is often no such simple delineation of when, if at all, a particular challenge has been met. This points to the need to formulate the challenges in such a way that there is a clear target and preferably some way of assessing progress towards it. In addition, one boundary condition assumed here is that a challenge must be such

that overcoming it will result in benefits extending well beyond the field of innovation studies.

It should be stressed that many of these challenges are not ‘new’. In some cases, research is already well underway; in others, work is still at an early stage but the challenge has been identified by others. The main novelty in this paper resides not so much in identifying new challenges (although some are relatively new), but in bringing them together in a more systematic and comprehensive manner.

In order to peer into the future to identify the challenges, we first need to construct a robust viewing platform. Given the strong element of continuity and path-dependence in such matters, the foundations for this are perhaps best constructed from an analysis of the major achievements of previous decades. The paper therefore begins by summarising key advances over the lifetime of innovation studies (or science policy research, as it was often previously termed). This may then provide us with a base for the task of identifying the main challenges for innovation scholars to address over coming decades. The first eleven of these involve making a fundamental shift (‘from X to Y’) as the field attempts to keep pace with a fast changing world, while another four involve negotiating between certain intrinsic tensions and finding an optimum balance. The remaining five represent more general challenges for the field of innovation studies and its practitioners, revolving around the question of what sort of field we aspire to be, in particular whether we seek to become a more academic ‘discipline’. The concluding section summarises the issues arising from this discussion paper with a view to stimulating a fruitful debate among scholars of innovation about the future of the field.

2. The main achievements of innovation studies over previous decades

Let us first briefly define the focus of this discussion. The field upon which we are focussing comprises economic, management, organisational and policy studies of science, technology and innovation, with a view to providing useful inputs for decision-makers concerned with policies for, and the management of, science, technology and innovation.³ It was originally known as ‘science policy research’⁴, but more recently it has widely come to be termed ‘innovation studies’. Science policy research began to emerge in a recognisable form in the late 1950s, when there were just a handful of people interested in the subject. Now, there are several thousand researchers making up the innovation studies (IS) community (Fagerberg and Verspagen, 2009). In recent years, the rapid growth has been fuelled by an increase in the number of business schools, and

in the proportion of their faculty interested in innovation and related topics, in turn stimulated by the growing demand for a better understanding of the nature of the innovation process for management and policy purposes.

Previous work using data on highly cited publications sought to identify the most important contributions made in the field of science policy and innovation studies over the course of its history (Martin, 2012a).⁵ From this, a list of 20 major advances in understanding can be synthesised, as summarised in Table 1 below.

Table 1: Twenty advances in science policy

1	From individual entrepreneur to corporate innovators
2	From <i>laissez faire</i> to government intervention
3	From two factors of production to three
4	From single division to multidivisional effects
5	From technology adoption to innovation diffusion
6	From science push to demand pull?
7	From single factor to multi-factor explanations of innovation
8	From a static to a dynamic model of innovation
9	From the linear model to an interactive ‘chain-link’ model
10	From one innovation process to several sector-specific types
11	From neoclassical to evolutionary economics
12	From neoclassical to new growth theory
13	From the optimising firm to the resource-based view of the firm
14	From individual actors to systems of innovation
15	From market failure to system failure
16	From one to two ‘faces’ of R&D
17	From ‘Mode 1’ to ‘Mode 2’
18	From single technology to multi-technology firms
19	From national to multi-level systems of innovation
20	From closed to open innovation

Source: Martin (2010b), which explains what each of these advances involved and lists key references.

Of these 20 advances, which have had an impact on management or policies for science, technology and innovation? There are around eight for which one could probably make a convincing case that they have had a significant impact on technology and innovation *management* in industry, including items 1, 3, 4, 7, 13, 16, 18 and 20. Which of them have had a major impact on science, technology and innovation *policy*? Here the list is rather shorter, perhaps incorporating items 1, 2, 13, 14 and 16 (if one includes related work on the ‘Triple Helix’ under this last heading).

It is not our task here to explore the possible explanations for this rather disappointing record in terms of impact on policy makers. Instead, I offer an intriguing observation on discussions with colleagues about the difficulty of making an impact on decision-makers in government, industry and elsewhere. For 30 years, innovation scholars have criticised ‘the linear model’ of innovation, repeatedly slaying it when it reappears in policies. Yet curiously many IS researchers still believe that such a model holds (or at least should hold) when it comes to their own work! They assume that, if they have a good idea, policy-makers should rush to exploit that idea through a new or improved policy. We evidently need a more sophisticated model of the interaction between policy research and policy making.⁶ This might look rather like our model of the innovation process following Kline and Rosenberg (1986) – in other words, not a linear model but a chain-linked interactive model (Martin, 2010b).

3. The challenges

It is difficult to be as precise in the formulation of the challenges confronting innovation studies as in mathematics. The first 11 are couched in similar terms to the advances or major shifts over the previous 50 years summarised in Table 1 – i.e. ‘from X to Y’. Four others involve negotiating between certain intrinsic tensions and finding some optimum balance. Lastly, there are five that represent more general challenges for the IS field and its researchers, giving a total of 20 challenges.

3.1 From visible innovation to ‘dark innovation’

According to ‘imprinting theory’, institutions often reflect the culture and times in which they were formed; their values, norms, ways of thinking and ways of acting (or ‘routines’) tend to become ‘imprinted’ and to live on (Stinchcombe, 1965). With innovation studies now over 50 years old, it is perhaps not surprising that ‘innovation’ is commonly conceptualised, defined and measured in terms of the dominant forms of innovation from several decades earlier – i.e.

primarily technology-based innovation (often high-tech innovation) for manufacturing, and generally involving prior R&D and patenting. This reflects the time when our notions about innovation were being developed. During the 1960s, manufacturing, along with agriculture and mining, still predominated. In that era, most innovations (i) were technology based, (ii) involved prior R&D, (iii) were developed by large companies, often on the basis of R&D conducted in their own labs, and (iv) frequently involved patenting. This encouraged IS pioneers to develop tools for ‘measuring’ innovative activity through indicators such as R&D funding, numbers of researchers, and patents. Today, however, such indicators may be ‘missing’ much innovative activity that is (i) incremental, (ii) not in the form of manufactured product innovations, (iii) involves little or no formal R&D, and (iv) is not patented.

Schumpeter, we should recall, used the term ‘innovation’ (or ‘new combinations’) quite broadly, identifying five types: (i) a new or improved product; (ii) a new or improved process (new, at least, to that particular sector, but not necessarily entirely ‘new’ to the world); (iii) the opening of a new market (again, ‘new’ for that sector in the country in question); (iv) the acquisition of a new source of raw materials or semi-manufactured goods (irrespective of whether that source already exists); and (v) an organisational change (in the firm or the sector) (Schumpeter, 1934, p.65; McCraw, 2007, p.73). There are several points to note about these categories. First, reflecting the times, they are couched largely in terms of manufacturing (products, processes, raw materials). Second, they are not restricted to product and process innovations – they include, for example, organisational innovations. And third, they are not confined to ‘new’ products, processes and so on – they can involve a significant improvement or merely be ‘new’ to the sector or country as opposed to new the world.⁷ Yet since innovation studies began to emerge 50 years ago, most empirical studies have tended to focus on product and process innovations rather than other types, and often more on radical than incremental innovations. Many other types of innovations have been ignored or are essentially ‘invisible’ in terms of conventional indicators – for example, innovations based on design, branding, software or other intangible investments rather than R&D.

At a 2012 OECD conference on ‘The new geography of innovation and the impact of the economic crisis’, participants were presented with the latest data on R&D, numbers of scientists, patents and so on.⁸ With the exception of the dramatic growth of China, there was not yet much evidence of the ‘changing geography’ of innovation. But presentations from officials in countries such as Vietnam and Argentina revealed a huge amount of innovative activity was going on ‘below the radar’ (Kaplinsky, 2011, p.194) – for instance, incremental

process innovations in factories of the BRICS and other developing economies, innovations that do not involve R&D nor work by ‘scientists’ nor patents, and hence are invisible.

Other examples can be found with regard to innovation in services. In financial services, there have been major innovations over the last two decades, especially credit derivatives (such as mortgage-backed securities, collateralised debt obligations and credit default swaps). The development of these involved substantial ‘research’,⁹ often conducted by former scientists (so-called ‘rocket scientists’), but it was almost invisible as far as innovation studies was concerned.¹⁰ Organisational innovations such as business reorganisation are likewise largely ‘invisible’, as are institutional innovations. The 2008 financial crisis was partly caused by changes in the regulatory framework,¹¹ in other words, by institutional innovations. Again, these are often ‘invisible’ to existing innovation measurement tools. And the same is true for many innovations involving profound social change,¹² such as those associated with Facebook or Twitter, or ‘grass-roots innovations’ in India (Gupta et al., 2003), or micro-finance (Morduch, 1999), or the innovative use of mobile phones by farmers in Africa to bypass corrupt middlemen and deal directly with markets (Bailard, 2010).¹³ All these are generally not captured in conventional innovation indicators.

There is an analogy here with astronomy and cosmology. Telescope observations reveal only a small proportion of the universe – the great majority lies unseen in the form of ‘dark matter’ or the even more mysterious ‘dark energy’. We know it’s there but we cannot measure it, at least with existing instruments. Likewise, we are dimly aware of the growing amount of innovative activity that is going on but it’s just not visible using existing measurement instruments – it is what might be termed ‘dark innovation’.¹⁴ The challenge to the next generation of IS researchers is to conceptualise, define, and come up with improved methods for measuring, analysing and understanding ‘dark innovation’.¹⁵ Instead of unthinkingly following the strategy of the drunk looking for his lost keys under the street lamp even though he knows he lost them somewhere else, we, too, need to put more effort into developing new ‘street lamps’ rather than forlornly searching under the existing ones illuminated by patents or R&D funding. Failing that, we are doomed to remain ignorant of just how much dark innovation is out there, whether it is increasing or not, and how it relates to improved economic performance.

3.2 From innovation in manufacturing to innovation in services

In the early years of IS, manufacturing was still ‘king’ or at least one of the most important components of GDP in most economies. Now, however, in many advanced countries the manufacturing sector has fallen below 20% of GDP and is dwarfed by services. Yet empirical studies in IS¹⁶ still focus predominantly on manufacturing, as the data in Table 2 below reveal,¹⁷ although there has been a modest shift over the most recent decade.¹⁸ Like generals who continue to fight the last war or politicians in thrall to a long-dead economist, we seem to be devoting a disproportionate level of attention to yesterday’s problems. If innovation is fairly evenly spread across manufacturing and services (and there is no reason to think it is not), then the challenge for IS scholars is to distribute their empirical efforts more evenly in line with the large share of services in GDP.

Table 2. Manufacturing VS services – thematic focus of RP papers

	Number of papers		
	1981-90	1991-2000	2001-10
manufacturing	173	380	743
service sector	6	38	113
health service/hospital	18	34	110
financial services	5	21	67
leisure/sport	7	22	66

Source: Search on *Google Scholar* in August 2013 among *RP* papers – using as keywords ‘innovation AND manufacturing’ etc.

3.3 From ‘boy’s toys’ to ‘women’s liberation’

Many of those now most prominent in innovation studies established their reputations in the 1980s and ’90s. At that time, for economic and political reasons, the focus was on competition between the US, Europe and Japan, and on high-tech manufacturing – for example, electronics and IT, automobiles, and pharmaceuticals. It often required years of patient effort for an innovation researcher to build up a database on products, firms and innovations in a particular sector. Table 3 shows what sorts of sectors have been most popular as the empirical focus of studies published in *Research Policy*.

Table 3. Sector focus of *RP* papers

	Number of papers (up to 2012)
computer/PC	1280
car/automobile	546
television/TV/radio	466
camera/video	225
hard disk/disk drive	61
cell/mobile phone	62
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VS	
refrigerator/freezer	18
microwave oven	17
washing powder/detergent	10
washing machine/tumble drier	8
vacuum cleaner	3
domestic/toilet/kitchen/bathroom cleaner	0

Source: – Search on *Google Scholar* in August 2013 – using as keywords ‘innovation AND (computer OR PC)’ etc.

The table reveals a clear tendency to focus on what might be characterised as ‘boy’s toys’. This presumably reflects the fact that (i) a high proportion of researchers in the field are men; and (ii) those researchers are likely to focus their empirical work on an area they feel passionately enthusiastic about – i.e. objects that they find fascinating for one reason or another. Yet there are other innovations that have done as much if not more to improve human lives over the last 50 years, in particular those innovations that have freed women from the domestic drudgery of being ‘housewives’. As recently as the 1950s, washing, cleaning, preparing meals, shopping and so on represented a full-time job, inevitably one that fell on ‘housewives’, whereas now many of these tasks, with the help of various innovations, can be done in a matter of hours or even minutes.¹⁹

This focus on high-tech innovations and especially on innovations involving ‘boy’s toys’ may well have skewed our search for a better understanding of the innovative process with respect to methodological tools and indicators, concepts, analytical frameworks and models. Those that we have developed may consequently be less applicable to other forms of innovation. All too often,

more mundane (or ‘less sexy’) innovations tend to be ignored. For example, one innovation that has arguably done more than any other to foster globalisation and hence stimulate economic growth over last 50 years is the humble shipping container. Yet there appears not to be a single empirical study of this key innovation published in an IS journal.²⁰

The challenge for the next generation of IS researchers (one with a much improved gender balance extending into the higher reaches of the profession, one would hope) is to escape the lure of focusing on innovations in boy’s toys and to give equal treatment to often more mundane innovations²¹ that have done as much to improve the lot of humanity, for example, in terms of liberating women from household drudgery or the poor from grinding poverty.²²

3.4 From national and regional to global systems of innovation

The notion of a ‘national system of innovation’ (NSI) is one of the most important conceptual developments to emerge from IS. It shifted attention from the previous focus on individual innovation actors (e.g. firms, universities, public research labs) to the links and interactions between the various actors making up the national innovation system. Subsequent research has extended the concept to regional systems of innovation (or even to local systems or networks) and to sectoral systems of innovation, and some scholars have explored the interactions between those different systems.

From when the concept was first introduced (Freeman, 1987), it was clear that not all innovative activity is national in scope. However, at the time this focus was justified in terms of most R&D being nationally focused, with most companies (even multi-national ones) conducting R&D primarily in their home country. Since then, these assumptions have become progressively less true (Carlsson, 2006). The key players with regard to innovation are multi-national corporations, who increasingly operate on a global scale, not just in manufacturing, but more recently with regard to innovation and R&D as well. In so doing, they have begun to forge links between previously separate national systems of innovation (Narula & Zanfei, 2005). At the same time, the economic system has become more globalised and various mechanisms of global governance have emerged. Hence, we may now be seeing the emergence of *global* systems of innovation in some sectors (Christensen et al, 2005).

The challenge to IS researchers is to identify, map and analyse these global systems of innovation and their interactions with national and regional systems (Lundvall, 2007; Soete et al., 2010). This will surely yield important policy implications, just as the development of the NSI concept originally did, not least

as we are confronted by ever more urgent global challenges (economic, environmental, demographic, health, security, etc.) and attempt to respond to these.²³

3.5 From innovation for economic productivity to innovation for sustainability ('green innovation')

This and the next four challenges are all linked to the key point made by Stirling (2008) that innovation is not a process of following a single, pre-determined, linear path but it has an element of directionality: 'innovation is a vector, rather than just a scalar quantity' (ibid., p.263). Under different policies, innovation can take different directions, or assume different forms, or involve different processes, or bring in different actors and bodies of knowledge.

During the 1980s and 1990s, the political and economic agenda was dominated by concerns with economic competition, growth, wealth creation, productivity and economic efficiency, and with shifts in these between Europe, the US and Japan (and later the Asian 'tigers' and then China). Innovation was seen as key to achieving all these. Policies were shaped to stimulate such innovations. At that stage, there was relatively little concern with sustainability, exhaustion of finite resources or environmental impact. Consequently, the cognitive resources developed within IS were all oriented primarily to these types of innovation aimed at enhancing economic productivity. Table 4 shows that this was also reflected in the empirical focus of papers published in *Research Policy*.

Table 4. Productivity vs. Sustainability – thematic focus of RP papers

	Numbers of papers		
	1981-90	1991-2000	2001-10
productivity	144	281	712
sustainability	0	13	172

Source: Search on *Google Scholar* in August 2013 – using as keywords 'innovation AND productivity' etc.

The 1990s saw increasing concern with environmental damage, the using up of scarce resources, and global warming. This led a few IS scholars (e.g. Kemp & Soete, 1992; Freeman, 1996), mainly in Europe and especially the Netherlands, to become more interested in innovation for sustainability. They often drew upon inputs from Science and Technology Studies (STS), one of the few occasions where this has occurred. It resulted in work on such issues as regime

shifts, niche formation and socio-technical transitions by authors like René Kemp, Arie Rip, Johan Schot and Frank Geels. Initially, such work was regarded as rather ‘flaky’ by some in IS, although it is now beginning to have a significant impact (e.g. Geels, 2002). Nevertheless, there is still much more to be done before we complete the transition to environmentally sustainable or ‘green’ innovation (Leach et al., 2012).

3.6 From innovation for economic growth to innovation for sustainable development²⁴

Despite the achievements of recent decades in removing hundreds of millions in China and elsewhere from poverty and shifting them into an urban, even middle-class, life-style, the world is still afflicted by a stark polarisation between rich and poor. We will not rehearse the arguments here, nor what challenges these pose for the IS community – they are set out eloquently elsewhere – for example, in Lundvall (2012, 2013a & b – see in particular his ideas on linking innovation systems research to development economics) and Perez (2013). Yet even after all the sterling work by the GLOBELICS network in recent years, there is clearly still a long way for go. The challenge for IS scholars is to respond to the pressing world need for more equitable development, ensuring that we have the conceptual, methodological and analytical tools needed to facilitate this shift to innovation for sustainable development through appropriate policies.

3.7 From risky innovation to socially responsible innovation²⁵

Science, technology and innovation have undoubtedly been a major force for good in improving economic and social conditions and contributing to human progress, not least in helping to extend our life expectancy.²⁶ However, they have also brought a number of risks and unintended consequences,²⁷ whether in terms of damage to the environment, less desirable working conditions or other adverse effects on the quality of life. Over the last 50 years, concern with risks has brought about fierce debates over such issues as nuclear energy, insecticides and chloroflourocarbons, and more recently global warming and GM crops. Some have even argued that modern technology has been accompanied by an increase in the overall level of risk, giving rise to ‘the risk society’ (Beck, 1992). Giddens (1999) in particular discusses the increase in the level and pervasiveness of ‘manufactured risk’ – i.e. ‘risk created by the very progression of human development, especially by the progression of science and technology’ (ibid., p.4) with their unknown and often unpredictable consequences. However, an alternative interpretation of the public’s evident

growing concern with risk is that a combination of increased scientific knowledge, improved communication, a more questioning media, a legacy of previous botched attempts by government or industry to suppress open discussion, and a less deferential attitude to authority (whether political or scientific) has increased the sensitivity of the public's 'antennae' for detecting possible risks and hence resulted in more concern about them.

The assessment of potential adverse consequences of technology and innovation underpinned previous work in IS on technology assessment and appropriate technology. This is another area where there has been a substantial contribution from the STS community, with work on risk and the wider social impact of technology pointing to the need for a more open and participative approach to decision-making to enable all stakeholders to have their say. The research been carried forward under such labels as constructive technology assessment, the public understanding of science, the ethical, legal and social implications of research (ELSI), and the precautionary principle, as well as through mechanisms such as consensus conferences or citizen juries and other approaches for 'opening up' decision-making processes (Stirling, 2008 & 2012). It has given rise to a call for 'responsible innovation' (e.g. Hellström, 2003). Although some have begun to respond to this challenge (e.g. Owen & Goldberg, 2010; von Schomberg, 2011), there is still much to do in coming decades.

3. 8. From innovation for wealth creation to innovation for well-being (or from 'more is better' to 'enough is enough')

For several centuries, human 'progress' has been seen essentially in terms of 'more is better', or perhaps more broadly in terms of 'more, bigger and faster is better': more money, more possessions, even more choices²⁸; bigger houses, bigger TVs; faster cars, computers and internet connections; and so on.²⁹ The political agenda has been driven largely by economic growth. In democratic countries, this is what politicians assume they must deliver if they are to be elected, as aptly summed up by Bill Clinton: 'It's the economy, stupid'. We have all become victims of the tyranny of GDP, assuming that more wealth and more 'stuff' will result in improved well-being. And that was probably true for most of human history.

However, research on well-being suggests that for subjective wellbeing, at least, this assumption may be only true up to a particular point, a certain level of income – the so-called 'Easterlin paradox' (Easterlin, 1974).³⁰ Moreover, the world cannot sustain a population likely to plateau at around 9-10 billion, all with US living standards – it would require perhaps half a dozen worlds to

sustain such a lifestyle. Therefore, the political and economic agenda and, more fundamentally, our very notion of progress all need to change to reflect this. We likewise should shift the focus of our empirical work from innovation for wealth to innovation for well-being (see Table 5 below).

Table 5. Wealth VS Happiness – thematic focus of RP papers

	Number of papers		
	1981-90	1991-2000	2001-10
wealth/profit	126	251	634
happiness/well(-)being	9	17	74

Source: Search on *Google Scholar* in August 2013 among *RP* papers – using as keywords ‘innovation AND (wealth OR profit)’ etc.

Such a transformation in our concept of progress and in societal goals requires fundamentally new policies (Coyle, 2011), and these, in turn, require the development of appropriate empirical methods, indicators, analytical approaches and conceptual frameworks. Work on such issues has been begun by a few (see e.g. Stiglitz et al., 2010), but IS scholars will need to build on these foundations if the shift to innovation for well-being is to be achieved.

3.9 From ‘winner take all’ to ‘fairness for all’?

To some such as Lundvall (2012, p.4), ‘polarisation and growing inequality [seem to be] inherent in the globalising learning economy’.³¹ One apparent consequence of globalisation is an increasing incidence of the ‘winner take all’ phenomenon (Frank and Cook, 1995), in which one organisation (or individual) benefits from an innovation to a far greater extent than competitors with only marginally inferior products. This can be seen most obviously in the IT sector, where there is a long history beginning with IBM, followed by Microsoft, Intel, and Oracle, and more recently by Apple, Google and Facebook. The process has often resulted in the creation of billionaires among the founders and principal stockholders of these firms. It is somewhat paradoxical that, as innovation has become more collective and ‘open’, the rewards have become increasingly individualised (Mazzucato, 2013; Lazonick & Mazzucato, 2013). Moreover, the phenomenon is not confined to IT, being evident in other sectors such as pharmaceuticals, where a slightly better product becomes the next ‘blockbuster’ drug, generating sales and profits far in excess of those for the next best drug.

Among the insidious effects of the ‘winner take all’ phenomenon has been to foster a wider belief that extreme wealth for a few individuals is a necessary facet of free-market capitalism – that CEOs should be paid hundreds of times the average salary of their staff, or that top bankers need multi-million pound bonuses as an annual right if they are not to become demotivated.³² All this despite the fact that there is little evidence that performance-related pay enhances performance; indeed, meta-reviews of its effect indicate that it actually undermines intrinsic motivation (e.g. Deci et al., 1999; Fehr & Falk, 2002).

Innovation studies is certainly not ‘to blame’ for the ‘winner take all’ phenomenon, but to what extent is it complicit in this? By contributing to an improved understanding of the innovation process, to more effective innovation policies, and to improved management of technology and innovation, IS has presumably helped to some extent in the development of innovations that triumph in the gladiatorial combat in which the winner takes all.³³ If so, can we simply sit back and maintain that the consequences are ‘not our fault’ – that *how* the knowledge, skill and tools we developed are used is nothing to do with us, just as some atomic physicists argued that the development of the atom bomb had nothing to do with them? Or do we, like doctors, have a higher moral responsibility for ensuring that we ‘do no harm’?

It is not clear whether IS scholars will ultimately have anything significant to contribute to reducing the ‘winner take all’ phenomenon and ameliorating its economic and social consequences, although work by Lazonick and Mazzucato (2013) represents a promising start. Yet surely we have a duty at least to explore whether we can say something about how firms and others might generate innovations that, rather than turning a few fortunate individuals into billionaires, instead result in greater ‘fairness for all’? Lundvall (2013b) suggests that IS perhaps needs to adopt a more critical perspective, while Perez (2013) stresses the need to consider innovation systems and policies that are ‘not only for the rich’. To achieve this, innovation studies may have to forge closer links with STS, a community with a stronger tradition of dealing with issues of fairness and moral responsibility.

3.10 From government as fixer of failures to the entrepreneurial state³⁴

Under *laissez faire* neo-liberalism and the ‘Washington consensus’, government has come to be seen as playing a restricted and largely passive role. Its task is to ensure the macro-economic climate is conducive for free-market capitalism to operate without let or hindrance, and then to ‘get out of the way’. The cheerleaders for neo-liberalism, rather like those for the previous economic policy ‘mania’ of monetarism, have been drawn mainly from prominent mainstream

economists at top universities. Central to the neo-liberalism ideology is an almost religious belief in ‘efficient markets’, with Nobel prizes being awarded for those claiming to prove that government intervention results in inefficiency or other forms of harm (Lundvall, 2012). The contrast between the public and private sector is always drawn in unflattering terms – the former is portrayed as lumbering, bureaucratic and inefficient, while the latter is invariably characterised as nimble, efficient and above all ‘entrepreneurial’ (Mazzucato, 2013). The government role in liberal market economies³⁵ such as the US and UK is viewed as largely confined to fixing ‘market failures’, such as those encountered in the areas of defence, health, education and research (and more recently banks).

But as the 19th Century clergyman remarked: ‘Why should the devil have all the best tunes?’ The above caricature grossly underplays the entrepreneurial role of the state with regard to many of the crucial innovations of the last 50 years, as innovation scholars have convincingly demonstrated. The list includes pharmaceuticals (benefiting from the huge scale of NIH funding in the United States), airliners (for many years based on earlier military planes), microchips (funded by DOD and NASA), PCs and Apple (funded by the SBIR programme), Internet (DARPA), the World-Wide Web (CERN), cell phones (DOD and the Finnish government innovation fund SITRA), Google (for which NSF funded the original algorithm) and GPS (the NAVSTAR satellite program) (Mazzucato, 2013). Yes, there is also a depressing list of government failures such as nuclear fusion, supersonic transport and synthetic fuels. Yet surely it is unrealistic to assume that *all* government policies will be successful. In the case of research, we do not assume nor indeed expect all research to be successful. And in the case of entrepreneurial initiatives, we know the vast majority will fail. Surely similar considerations of fallibility should apply with regard to our expectations concerning government policies? If governments do not take risks in their policies, they may not have failures but they won’t have any great successes either. In short, we need to change our conception of government from a fixer of failures to ‘the entrepreneurial state’ (Mazzucato, 2013).

The government role with regard to the early stage development of future innovations has often been underplayed for political or ideological reasons, especially in the US, where DOD and DARPA, NIH, NSF and SBIR all form part of what can be regarded as an informal technology policy or indeed an industrial policy (Etzkowitz & Gulbrandsen, 1999) – the ‘invisible hand’ of the state, if you like. In years to come, the government role in developing biotech and nanotech may well be seen as having been equally crucial, even if the main benefits are still some way off. Today’s pressing need for green technology and green innovation to address the problem of climate change will not be solved by

‘the market’ nor by taxation nor even by ‘nudging’, such is the power of established vested interests and the path-dependent nature of the trajectories pursued over previous decades. This points ineluctably to the need for the state to play a more entrepreneurial role – in other words, government acting not just as a coordinator or as a fixer of market failures, but as a strategist, lead investor and risk-taker until technology has reached a sufficiently mature stage where venture capital and industry are willing to take over (Mazzucato, 2013). Recognising this, Bakhshi et al. (2011) argue that there needs to be a fundamental shift to a more experimental approach to innovation policy, outlining the experimental processes that would need to be embedded in publicly supported innovative activity to reduce the uncertainty that entrepreneurs face with regard to opportunities and constraints.

3.11 From faith-based policy (and policy-based evidence) to evidence-based policy?³⁶

The driving philosophy of the founders and pioneers of IS was premised on the assumption that science, technology and innovation (STI) are fundamental to economic and social progress, but that one needs effective policies (and effective management strategies) to ensure the potential benefits are actually achieved. It was further assumed that STI policy research could provide data, methods, analytical tools, conceptual frameworks and perhaps eventually theories that would help ensure better policies, and that the resulting evidence-based policies would, in turn, lead to greater benefits for humanity.

Over the last 30-40 years, there has certainly been some progress with regard to providing relevant data, methods, conceptual frameworks and so on, as summarised above (see also Martin, 2012a). Some of the advances in IS have had an evident impact on policy, although as noted earlier, that impact has been rather occasional, limited and accidental.³⁷ Those attempting to provide systematic evidence in favour of a particular policy option have often found that policy-makers resist their overtures, being already politically wedded to a particular policy (i.e. they are intent on pursuing an ideology- or faith-based policy), and only willing to take on board evidence supporting that prior position (i.e. they seek policy-based evidence) rather than evidence which might point towards a rather different policy (i.e. evidence-based policy). Thus far, we have little evidence that all our efforts have resulted in substantially better policies. And as for whether those policies have resulted in the world becoming a better place, the evidence ‘locker’ is rather bare!

Providing such evidence and encouraging a shift to evidence-based policy represents another crucial challenge to IS researchers.³⁸ However, it will not be

easy. In the world of medicine, efforts to ensure ‘evidence-based medicine’ are reasonably well developed (although a worryingly high proportion of medical care is still not fully evidence-based³⁹). To achieve this requires randomised control (‘double blind’) trials in which neither patient nor physician is aware whether the new drug or a placebo is being administered. It is not clear how this methodological ‘gold standard’ for ensuring evidence-based initiatives can be transferred to the world of policy.⁴⁰ It would imply trials in which the ‘real’ policy and a ‘placebo’ policy were administered by different policy-makers ignorant of which they were each actually administering, and with the public likewise in blissful ignorance as to which they were receiving. However, experiences over recent years in universities with baffling initiatives coming down from on high sometimes suggest that this experiment may have already begun!

The next four challenges to be considered are rather different in character from the previous eleven in that they each involve negotiating between certain intrinsic tensions (for example, between intellectual property and open source) and finding some optimum balance.

3.12 Balancing the intrinsic tensions between intellectual property and open source

From knowledge accumulated over several decades about the nature of the innovative process, we know that, while a particular approach or policy may work in one sector and in certain circumstances, elsewhere it may be less effective, or indeed the opposite policy may even be more effective; and in yet other sectors or circumstances, some judicious balancing of those competing approaches may be required.

One example where such balancing is called for concerns protecting intellectual property through patenting and the like, on the one hand, and adopting an open source approach, on the other. In a sector such as pharmaceuticals, the case for patenting to protect new drugs – and hence to provide pharmaceutical companies with the necessary incentive to invest the hundreds of millions of dollars required to develop a new drug – is strong. In contrast, in sectors such as software, an ‘open source’ approach appears to be more effective in stimulating innovations. However, in many sectors some balance is required between protecting intellectual property through patenting, copyright and other means, and an open source approach. The task for IS researchers is to clearly specify precisely what balance between the two is required in different sectors and under particular circumstances.

3.13 Balancing the intrinsic tensions between exploration and exploitation

One area where rather more is known about balancing two competing alternatives is with regard to the extent to which an organisation should focus on the exploitation of existing knowledge or on the exploration of new knowledge. Since March (1991) first examined the relationship between the two and argued that ‘maintaining an appropriate balance between [them] is a primary factor in system survival and prosperity’ (ibid., p.71), there have been a number of studies exploring the ‘ambidexterity hypothesis’ (e.g. He and Wong, 2004) and the ‘ambidextrous organisation’ (e.g. O’Reilly & Tushman, 2004). However, there is further important research to be done here by IS researchers. What are the respective pros and cons of exploration and exploitation? Under what conditions is each the more appropriate? What is the optimum balance for individual sectors or firms, and what are the factors affecting that balance? This is linked closely with the next challenge.

3.14 Balancing the intrinsic tensions between closed and open innovation

One of the most highly cited contributions from IS over the last 10 years is Henry Chesbrough’s 2003 book on *Open Innovation*, which argues that ‘we are witnessing a ‘paradigm shift’ in how companies commercialize industrial knowledge ... [from] the old paradigm Closed Innovation ... [in which] [c]ompanies must generate their own ideas and then develop them ... on their own ... [to] Open Innovation ... a paradigm that assumes firms can and should use external ideas as well as internal ideas ... as the firms look to advance their technology.’ (Chesbrough, 2003, pp.xx & xxiv). While one can debate whether open innovation is indeed a new phenomenon (much innovation in the 19th Century was based on an open rather than a closed model – see e.g. Mendonca, 2012), the book has stimulated considerable debate as to *how* open an organisation should be – in other words, to what extent can it rely on external sources of knowledge? Unfortunately, some in industry have seen open innovation as a justification for slashing internal R&D, assuming that this can be simply outsourced. But if large numbers of organisations simultaneously adopt an aggressively open approach towards innovation, will there be adequate R&D conducted ‘elsewhere’ upon which they can all draw? Nor is it clear where the exact boundary lies between ‘legitimate’ reliance on an open innovation approach, and unfairly ‘free-riding’ on the efforts of others.

Furthermore, we have known since Cohen and Levinthal’s (1989 & 1990) work on ‘the two faces of R&D’ and on ‘absorptive capacity’ that firms need to conduct a certain level of R&D if they are to be in a position to identify and successfully exploit knowledge developed externally.⁴¹ The challenge for IS

researchers now is to explore what is the appropriate balance between internal and external sources for specific sectors and firms, and the factors that affect that balance in different circumstances (West, 2003).⁴²

3.15 Balancing the intrinsic tensions between competition and cooperation

Linked to the previous topic is the issue of when an organisation should compete with others and when it should cooperate. Most will need to pursue a strategy based on a combination of the two ('co-opetition', as it is rather inelegantly called – see Nalebuff & Brandenburger, 1997, and Gnyawali & Park, 2011), but the exact balance will undoubtedly depend on a range of factors including the sector and competitors, the state or maturity of the technology involved, whether radical or incremental innovations are being sought, and so on. Further research is needed to obtain a more detailed understanding of what is the most appropriate balance in different cases, and the factors affecting it.

3.16. Pricking academic bubbles

As Perez (2002) has eloquently described, economic history has been punctuated by periods of unbridled optimism or 'irrational exuberance', giving rise to a rapidly expanding 'bubble' that at some point inevitably bursts with disastrous financial consequences. Examples include speculation in exotic tulips in early 17th Century Holland and in South American trading company shares in 18th Century Britain ('the South Sea bubble', apparently the first such use of the term 'bubble' in this connection), the canal building 'mania' at the end of the 18th century and the railway mania in 19th Century industrialising Britain, and the US stock market bubble in the 1920s. Nor do we seem to have learnt from these mistakes, despite the efforts of eminent economists such as Galbraith (1954) and Bernanke (1983) – witness the Dotcom bubble of the late 1990s, and the sub-prime mortgages and associated financial derivatives giving rise to the financial feeding frenzy in the early years of the 21st Century.

Moreover, scientists do not seem to be immune to such herd instincts. In physics, thousands of 'string theorists' have devoted their professional lives to a theory for which there is as yet no direct scientific evidence (Smolin, 2006). A few years ago, among scientists and social scientists there was a sudden dramatic upsurge of research on 'chaos' and then 'complexity', although the subsequent outcome in terms of verifiable predictions has been disappointing. At first sight, it might seem puzzling that researchers, as rational and intelligent individuals, should be just as vulnerable to being swept along on a wave of unrestrained optimism. However, closer inspection of the psychological makeup

of the researcher suggests an explanation. Scientists and other researchers are (with a few exceptions) not in it for the money. What drives them to devote their lives (often at some sacrifice to their families as well as in terms of income) is a passionate belief that what they are studying is important – indeed, that the subject of their research is *more* important than that of other researchers. To justify the long hours devoted to exploring the subtleties of string theory or complexity, they must first convince themselves that they are on the right track to some fundamental new advance in knowledge – hence, their strong if not over-riding self-belief. They must also be successful in the competition for scarce research funds, which may encourage them to raise expectations to unrealistic levels through expounding dramatic visions of what benefits their research may yield. Once those unrealistic expectations are dashed, funding may dry up until researchers come up with a new vision, giving rise to a further hype-disappointment cycle (Brown, 2003; Konrad, 2006; Rip, 2006; Verbong et al., 2008).

Do we in the IS community sometimes fall prey to similar manias or bubbles? With the benefit of historical hindsight, can one identify areas or topics where perhaps rather too much attention was given? At the risk of offending some but in an effort to stimulate debate on this matter, let me offer some suggestions for discussion. Has too much attention been given in the past to total factor productivity and ‘the residual’, or in the 1980s to Japanese production processes (e.g. total quality management, just-in-time, and ‘lean production’)? Have we on occasions been guilty of contributing to the hype over biotechnology or other advanced technologies? Or exaggerating the potential benefits of clusters or networks, or of university-industry links, technology-transfer offices and science parks, or of the innovative and employment-generating potential of SMEs (in particular, start-ups or ‘gazelles’)⁴³, thereby contributing to ill-considered policies and initiatives? More fundamentally (and more provocatively), are we possibly in danger of adopting too uncritical an attitude towards Schumpeter (‘we’re all Schumpeterians now’) or even systems of innovation?

The challenge to younger (and even some older) innovation scholars is to maintain the ability to stand dispassionately to one side and decide if a particular line of research is in danger of becoming a fad or whether it still represents the most promising line of enquiry. In short, we need a few ‘contrarians’ willing to risk ridicule by suggesting the new emperor has no clothes!

3.17 Avoiding disciplinary sclerosis

During its first two decades, the emerging field of innovation studies was populated by ‘immigrants’ from other disciplines (economics, sociology, management studies etc.). It thus became intrinsically interdisciplinary⁴⁴ in nature – an intellectual ‘melting pot’ characterised by diversity and an eclectic borrowing of cognitive resources from others (Fagerberg et al., 2012a; Martin, 2012a). The research was primarily explorative in nature and to a large extent qualitative, with case-studies featuring prominently (Nelson, 2012, p.37). It was also driven primarily by policy issues (Lundvall, 2012 & 2013a), not least those arising from the cold war tensions between the US and the USSR, and subsequently by the growing economic competition between the US, Europe and Japan.

Over time, innovation studies has matured as a research field (Morlacchi and Martin, 2009; Martin, 2012a). Particularly in Europe and Asia, there are a number of dedicated centres of research on innovation. Innovation studies now trains a large proportion of its own PhD students rather than recruiting them from neighbouring disciplines. It has its own journals and conferences. It has developed more rigorous methodologies, generally quantitative in nature (and increasingly involving quite sophisticated econometric analysis). In short, it is beginning to exhibit certain disciplinary characteristics (Martin, 2012a), perhaps even approaching a Kuhnian transformation (Steinmueller, 2013).

Yet while becoming more discipline-like is a testament to the field’s growing academic standing and self-confidence, it also has certain consequences or aspects that should give pause for thought. One is the increasing homogeneity⁴⁵ in terms of the researchers (most now with a PhD in the field, giving rise to a possible danger of intellectual ‘inbreeding’), the studies they carry out (an increasing proportion of which are quantitative, even econometric), and the papers they publish (a growing proportion following a fairly standard form⁴⁶). Peer review, particularly for leading journals, can now give ‘non-conventional’ studies a rough ride, damning them for a lack of theory or hypotheses even when the exploratory nature of the paper’s theme may make that undesirable or unrealistic.

Furthermore, with the emergence of a possible proto-paradigm in the form of what Dosi et al. (2006) have labelled the ‘Stanford-Yale-Sussex synthesis’, there are signs that the field of IS may be becoming more theory-driven and less policy-driven (Martin, 2012a), or more akin to ‘normal science’ in Kuhn’s terms (Kuhn, 1962) and less adventurous (Steinmueller, 2013). Indeed, one

possible scenario is that innovation studies may end up as little more than a subfield of management, having shed its societal focus and policy orientation.

At different stages, similar concerns have been raised about neighbouring fields. In management studies, there have been periodic debates about the appropriate balance between theory and practice, with Hambrick (2007), for example, asking whether that field's growing devotion to theory represented 'too much of a good thing'. The case of economics is also instructive and offers a salutary warning. In former times, it spanned a heterogeneous mix of researchers and subjects, in which neo-classical economists rubbed shoulders with political economists, institutional economists, development economists, industrial economists, labour economists, Marxist economists, economic historians and others. But over time, neo-classical economics with its seeming fetishisation of econometric models⁴⁷ and equations⁴⁸ has increasingly come to dominate (Lundvall, 2012).⁴⁹ Just as the native red squirrel has been steadily driven out of the British Isles over the course of the 20th Century by the larger, more aggressive, North American grey squirrel, so the neo-classical economic 'grey squirrel' has been driving out other varieties of economic 'red squirrel' from the groves of academe.⁵⁰

With regard to the field of innovation studies, now is the time for a debate as to what sort of field we want to be in the longer term. More specifically, do we want to become a more academic and homogeneous discipline, or a field that continues to respond to the challenges encountered by decision-makers in government, industry and elsewhere in society with regard to policies for and the management of innovation, a field incorporating a broad and heterogeneous mix of research activities, a field that continues to embrace non-mainstream IS work (for example, by economic and business historians) as well as engaging in fruitful intercourse with social scientists from neighbouring disciplines and fields (such as STS)? Putting it bluntly, do we want to continue to operate as an interdisciplinary 'mongrel' of lower academic status rather than a high-status disciplinary 'pedigree'? Resolving this issue represents another challenge for the next generation of IS leaders to address.

3.18. Identifying the causes of the current economic crisis

The economic crisis now confronting us is arguably the most serious since the 1930s. Just as that earlier crash spawned a huge literature on its causes, so we need to understand the causes of the current crisis. Earlier, we noted the often calamitous contributions of the economics profession to the crisis (see also Dosi, 2011 & 2013). Innovations had a major part in this. Financial innovations such as sub-prime mortgages, collateralised debt obligations and credit default

swaps all played a central role in creating the crisis, giving rise to a process of ‘destructive creation’ (Soete, 2013). Each of these particular innovations may have been created with honourable intentions. Sub-prime mortgages were introduced, partly in response to pressures from politicians, to enable the less fortunate to get a foot on the housing ladder. Collateralised debt obligations (CDOs) and credit default swaps were set up in order to reduce and even apparently remove any risk (through some magical form of financial ‘origami’ that few pretended to understand but which somehow converted junk-grade mortgages into ‘triple A’-rated CDOs to sell on to others⁵¹). However, the former were quickly exploited by avaricious mortgage brokers on performance-related pay, while the latter rapidly spiralled out of control into a trillion dollar form of casino banking, in which the players, not surprisingly, became addicted to gambling with other people’s money encouraged by enormous bonuses reflecting their winnings but unfortunately not their losses (Crotty, 2009).

Here, it is not so much that IS researchers contributed to these financial innovations; rather, it is that we almost completely failed to provide any analysis and understanding of them, and hence were unable to offer any warnings. With a few honourable exceptions (e.g. participants in the recent FINNOV project⁵²), the IS community has been strangely silent about the slew of financial innovations witnessed since the ‘big bang’ and the liberalisation of banking. Even sociologists and anthropologists have had rather more to say (e.g. Beunza & Stark, 2004; Mackenzie, 2006; Tett, 2009).

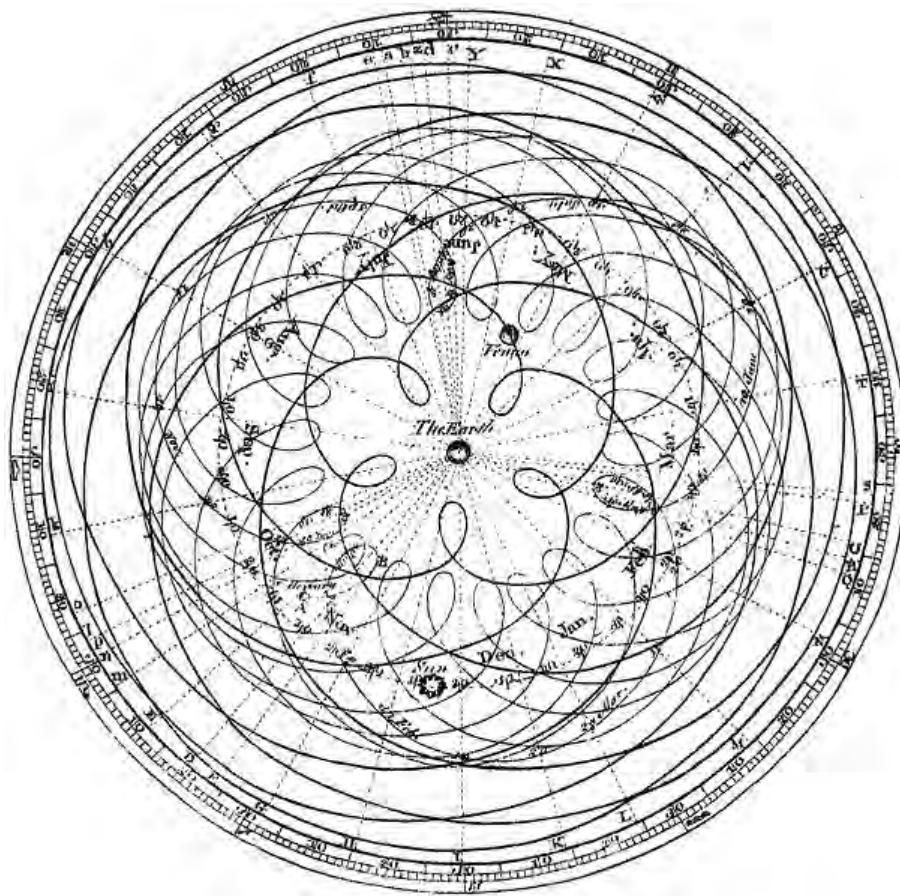
What might explain this ‘curious incident of the dog that failed to bark’? Partly, it may reflect the continuing fascination of IS researchers with innovation in manufacturing and with high-tech industry.⁵³ Related to this is the current lack of data on innovative activity in financial services, and the large amount of time and effort that would be needed to collect these, starting virtually from scratch. There is also probably a problem of access, in that banks tend to be less welcoming to academic researchers than industrial firms. In addition, many IS researchers are perhaps put off or even intimidated by the technical complexity of financial services and products such as derivatives. The challenge to younger IS researchers is to overcome all these hurdles, and to provide us with an understanding of the role played by financial innovations in contributing to the current economic crisis, and the lessons we can draw in order to minimise the risk of such an event happening again in the future.

3.19. Helping to generate a new paradigm for economics⁵⁴ – from Ptolemaic economics to ???

In the wake of the 2007-8 financial crisis, Krugman (2009) offered a particularly devastating critique of economics entitled ‘How did economists get it so wrong?’ Lundvall (2012, p.11) likewise is in little doubt that ‘the economics profession and the policy advice given by economists has a major responsibility for the current crisis’,⁵⁵ with some becoming paid-up members of ‘the financial industrial complex’. Nor does mainstream economics appear to have any credible ideas for getting us out of the mess.⁵⁶ As Krugman (2009) notes, economists first ‘have to face up to the inconvenient reality that financial markets fall far short of perfection, that they are subject to extraordinary delusions and the madness of crowds’.⁵⁷ Lundvall (2012, p.11) argues that ‘The current crisis is similar to the one in 1930s ... in the sense that there is a strong need for a paradigm shift in economics. This is why we need to discuss how innovation scholars with roots in economics can contribute to such a paradigm shift.’⁵⁸ Others such as contributors to the 2010 Special Issue of *CE/Sifo Economic Studies* on ‘What’s Wrong with Modern Macroeconomics?’ (e.g. Kirman, 2010) and contributors to the book edited by Kolb (2010) on *Lessons from the Financial Crisis*, as well as researchers from within IS such as Dosi (2011 & 2013) and Perez (2013) also have pertinent observations to offer with respect to this particular challenge.

Rather than trying to add to all this, let me instead offer an observation. Like Dosi (2011), I sense that economics today is eerily reminiscent of Ptolemaic astronomy with its complicated epicycles. To the Ancient Greeks and Romans and many who followed over the next one and a half millennia, it was axiomatic that the heavenly bodies should move in perfect circles around the Earth. In order to explain why observations of planets and other bodies suggested otherwise, an ever more complicated set of epicycles was invoked (see Figure 1 below).

Figure 1. Epicycles to explain the observed movement of planets around the Earth



Source: Wikipedia (downloaded on 5 August 2013 from http://en.wikipedia.org/wiki/File:Cassini_apparent.jpg)

Likewise, neo-classical economics seeks to protect its core beliefs in equilibrium, profit maximisation, diminishing returns, rational expectations, perfect information, utility maximisation, Pareto optimality, efficient markets, representative firms, and the like.⁵⁹ But to do so in the face of accumulating inconvenient evidence to the contrary, not least from innovation studies⁶⁰ as well as behavioural economics (see e.g. Burnham, 2013), it has had to invoke an increasing panoply of ad hoc ‘fixes’ such as bounded rationality, imperfect information, information asymmetry, satisficing, prospect theory, cognitive bias, anchoring and the like – in short, an embarrassing accumulation of ‘epicycles’. As Kuhn (1962) pointed out, the accumulation of ‘anomalies’ is often the prelude to the end of a period of normal science and the onset of revolutionary science with the eventual transition to a new paradigm. If this is the case here, then IS scholars are surely well placed to respond to the challenge

of constructing a new and more effective paradigm for economics,⁶¹ perhaps even one incorporating neo-Schumpeterian or evolutionary economics (see also Dosi, 2011 & 2013).

3.20. Maintaining our research integrity, sense of morality and collegiality⁶²

For much of the 20th Century, many professional communities operated on the basis of ‘self-policing’. It was assumed that external rules and regulations were unnecessary – that the appropriate professional body could monitor its own activities and, in the rare cases where misconduct was spotted, it could investigate those cases, impose the necessary sanctions, and ensure that others were not tempted to stray down that route (Martin, 2012b). However, over the last decade or so, a succession of scandals have suggested that doctors, accountants, members of parliament, journalists and bankers are not immune to temptation, and that self-policing has all too often proved ineffective.

There are many in the academic community who like to think that ‘the Republic of Science’ remains one last shining bastion where misconduct is rare, low-level and self-correcting, where any misconduct is quickly detected by peer review and stopped, and where the risk of being caught and the severe repercussions that follow are such that few are tempted to err (Martin, 2012b). However, the growing incidence of plagiarism (Martin et al., 2007) as well other forms of research misconduct (Martin, 2013b) throws all this into question.

As a field, we were truly fortunate in our ‘founding fathers’ – individuals such as Chris Freeman, Richard Nelson and Nathan Rosenberg, who, besides making immense intellectual contributions, also shaped the culture and norms under which the IS community operates (cf. Schein, 1983). In particular, these individuals personified a spirit of openness, integrity and intellectual generosity. A striking example of the last of these qualities involves Freeman and Lundvall. As is well known, credit for the first published use of the concept of ‘national systems of innovation’ is usually given to Freeman (1987); however, he always stressed that the concept arose from his discussions at Aalborg and that ‘the first person to use the expression ‘National System of Innovation’ was Bengt-Åke Lundvall’ (Freeman, 1995, p.5). Lundvall fiercely resisted this attempt to give him the credit, even going to the lengths of tracing the idea back to an even earlier (unpublished) paper⁶³ written by Freeman around 1983 as an input to an OECD ‘Group on Science, Technology and Competitiveness’ (Lundvall, 2004, p.531). In this, admittedly somewhat atypical, case, the two main protagonists were each willing to cede priority to the other, a lesson to us all (Martin, 2010a)! Whether the same behaviour would have been exhibited in other fields is more doubtful.

However, as competition for funds, tenure and academic status intensifies, there are worrying signs that the stock of social capital may be eroding and the culture of our field may be changing for the worse, as some are tempted to ‘borrow’ or misappropriate the ideas or data of others. Journal editors now receive complaints from referees about how their data have been used without permission by the authors of papers they were sent to review. There are informal stories that some authors, fearful of their ideas being purloined, are no longer willing to present early drafts of their papers at conferences. Such behaviour, if widely emulated, risks weakening the ‘invisible college’, removing a key mechanism for improving the quality of new papers, and for stimulating the cross-fertilisation that is so essential to the future of our field (and indeed of any research field).

Occasionally, perhaps because of an impending deadline for a conference presentation or a journal article, individuals may succumb to the temptation to engage in outright plagiarism. Fortunately such cases appear to be rare, although there are some indications that serial plagiarisers such as Hans Werner Gottinger are becoming more common (Martin et al., 2007, p.910). Moreover, by definition we only know about the incidence of *detected* plagiarism – how much more remains undetected is what the noted American philosopher, Donald Rumsfeld, would term ‘a known unknown’ (quoted in Boardman, 2005, p.783).

Rather more common, and certainly on the increase, is the phenomenon of ‘salami publishing’. With the growing use of publications as a performance indicator comes escalating pressure to exploit one’s database, survey or study to the full with as many articles as possible. Hence, some authors resort to ‘slicing the salami very thinly’. The resulting papers are often sent to different journals. In some cases, the author may cite the other parallel papers. However, it is very difficult to persuade referees to read not only the paper in question but also the other parallel papers (which may not have been published yet and are therefore difficult to access) in order to establish whether the former represents a sufficiently substantial and original contribution to merit publication in its own right. In other cases, the author simply ‘forgets’ to cite the parallel papers. Sometimes, this may be picked up by a diligent referee. Other times, it may only be discovered after publication, leaving journal editors with a difficult decision as to whether that article should be withdrawn or subject to a ‘corrigendum’.⁶⁴ In the worst cases, ‘salami publishing’ shades into self-plagiarism, where the author re-uses material from one or more of his⁶⁵ earlier publications without drawing the attention of the reader to the existence of the earlier work (Martin, 2013b).

All this raises the question of where precisely is the boundary between acceptable research behaviour (full exploitation of one's data and findings) and unacceptable behaviour (salami publishing and self-plagiarism)? There is a challenge here for IS researchers not only to define that boundary in a universally agreed manner, but also to ensure that we maintain the norms, incentives and, if required, the sanctions to police that boundary effectively and hence ensure the continuing integrity of the field. This is the final of the 20 challenges for innovation studies over coming decades (see the summary in Table 6 on next page).

Table 6: Twenty challenges for innovation studies

1	From visible innovation to 'dark innovation'
2	From innovation in manufacturing to innovation in services
3	From 'boy's toys' to 'women's liberation'
4	From national and regional to global systems of innovation
5	From innovation for economic productivity to innovation for sustainability ('green innovation')
6	From innovation for economic growth to innovation for sustainable development
7	From risky innovation to socially responsible innovation
8	From innovation for wealth creation to innovation for well-being (or from 'more is better' to 'enough is enough')
9	From 'winner take all' to 'fairness for all'?
10	From government as fixer of failures to the entrepreneurial state
11	From faith-based policy (and policy-based evidence) to evidence-based policy?
12	Balancing the intrinsic tensions between intellectual property and open source
13	Balancing the intrinsic tensions between exploration and exploitation
14	Balancing the intrinsic tensions between closed and open innovation
15	Balancing the intrinsic tensions between competition and cooperation
16	Pricking academic bubbles
17	Identifying the causes of the current economic crisis
18	Avoiding disciplinary sclerosis
19	Helping to generate a new paradigm for economics – from Ptolemaic economics to ???
20	Maintaining our research integrity, sense of morality and collegiality

4. Conclusions

Now that the field of innovation studies is around half a century old, it is an appropriate time not only to reflect on what has been achieved, but also to look forward and to discuss what might be the next major challenges to be tackled and to ask what sort of field we want to be. The field of innovation studies has come a long way in 50 years, establishing itself as a vibrant research community with a long and impressive list of achievements (Fagerberg et al., 2012a & b; Martin, 2012a). However, attaining academic respectability brings with it the risk of also becoming ‘middle aged’, of becoming set in our ways.

This discussion paper has argued that the focus of our empirical studies has not always kept pace with a fast changing world, in particular the shift from manufacturing to services and the growing need for sustainability as well as economic growth. Indeed, the very way we conceptualise, define, operationalise and analyse ‘innovation’ may be too rooted in the past, leaving us less able to grapple with other, less visible or ‘dark’ forms of innovation, whether in the area of services or in organisational or other non-technological forms. In particular, the relative neglect of financial innovations by IS scholars has left us with little to contribute to the analysis of the current financial crisis and the growing polarity between rich and poor, or to the debate on how economics needs to be fundamentally restructured or even shifted to a new paradigm if we are to avoid similar problems in the future. Governments may need to adopt a more entrepreneurial approach towards innovation policy, while we also require a better understanding of the interaction between policy research and policy making if the goal of more evidence-based policy is to be achieved. As innovation studies acquires more of the characteristics of an academic discipline, efforts will be required to maintain the vitality of the field and prevent the onset of disciplinary sclerosis. As the field takes on new challenges, attention must be devoted to the continuing development of its institutions and infrastructure if it is not to risk fragmenting into separate subfields (Fagerberg et al., 2012b). And as it continues to grow, the task of maintaining the integrity, morality and collegiality that characterised the field’s early decades will become more severe.

Let me end by re-emphasising that the list of 20 challenges presented here is not intended to be prescriptive. To paraphrase Hilbert (1902, p.478): ‘The [challenges] mentioned are merely samples of [challenges], yet they will suffice to show how rich, how manifold and how extensive [innovation studies] of today is’. My purpose is rather to join with others in a debate. Such a debate could shape the future of innovation studies for decades to come.

Notes

¹ A shorter version of this paper based on just 15 challenges (and without the empirical material in the tables below) is being published as a chapter in an edited book (Martin, 2013a).

² An attempt to do something similar can be found in Castellacci et al. (2005).

³ For a fuller definition and explanation, see Martin (2012a). Under this definition, Science and Technology Studies (STS) is regarded as a separate field, although there are interactions between the two (ibid.).

⁴ The term ‘science policy research’ is now normally reserved for a smaller, more specific subfield of research concerned with such questions as ‘How much should a nation spend on science? What kind of science? How much from private versus public sectors?’ (Marburger, 2005).

⁵ Using a different methodology based on analysing the references cited in review chapters of innovation handbooks, Fagerberg et al. (2012a) have also identified key advances in innovation studies.

⁶ This might be added to the list of challenges below, but instead I have subsumed it under Challenge 11.

⁷ Schumpeter’s definition of innovation was subsequently picked up by others, in particular Chris Freeman, who was responsible for the first OECD Frascati Manual as well as publishing what was, for over 20 years, the most influential textbook in the field, *The Economics of Industrial Innovation* (Freeman, 1974).

⁸ See http://www.oecd.org/document/37/0,3746,en_2649_33703_49364453_1_1_1_1,00.html (downloaded on 5 August 2013).

⁹ In addition, the huge amount of investment analysis conducted by financial institutions is to a greater or lesser extent research-based, but again it is not visible as such to the outside world. This may partly explain why the UK, with its heavy dependence on financial services, apparently devotes so little effort to ‘R&D’.

¹⁰ One prominent exception is Jansen et al. (2006).

¹¹ One such regulatory change was to permit firms to manipulate their stock prices through ‘buybacks’, a change that helped fuel the speculative bubble (Lazonick, 2009).

¹² The term ‘social innovation’ is often used here, although different authors have employed it for rather different purposes (e.g. Gershuny, 1983; Kanter, 1999; Mulgan, 2007).

¹³ See also Jensen (2007) on the adoption of mobile phones by fishermen in Kerala in Southern India.

¹⁴ No pejorative sense is intended here – the term is simply adopted by analogy with cosmology. An alternative would be ‘hidden innovation’, a term previously used by SPRU colleagues (Diana Hicks, Sylvan Katz and Michael Hopkins) and others (e.g. Ian Miles).

¹⁵ Recent work by my colleagues, Alex Coad and David Storey, using a new database on small businesses suggests that there is a similar issue in the field of entrepreneurship studies, with researchers having previously tended to concentrate on larger and more visible forms of entrepreneurship rather than less easily measured but perhaps more extensive forms.

¹⁶ Researchers in other research fields may be giving rather more attention to innovation in services.

¹⁷ The data in this and subsequent tables are crude but sufficient for illustrating the broader themes raised here.

¹⁸ And there have been a few high-impact studies focussing on innovation in services (e.g. Evangelista, 2000; Tether, 2005; Castellacci, 2008).

¹⁹ In the 1950s, it took around two hours to prepare each meal whereas the average figure now is a matter of minutes (The Economist, 2003; Pelupessy & Van Kempen, 2005, p.360). See also the data on the dramatic fall in time spent by women on ‘cooking, cleaning, laundry’ in Kan et al. (2011, p.239, Figure 1).

²⁰ There is a book on the shipping container but written by a journalist (Levinson, 2006) rather than an IS researcher. Also relatively neglected by innovation scholars are the enormous diesel engines used to power container ships and bulk carriers, which have likewise played a crucial role in globalisation (Smil, 2010).

²¹ Science and Technology Studies (STS) has a rather better record of studying mundane innovations.

²² There have, for example, been significant innovations in the area of water sterilisation, but these have received little attention from innovation scholars.

²³ The proposal by Lundvall (2013b) for the establishment of a Norwegian Observatory for Global Governance Innovation would provide an institutional environment in which IS scholars might address this challenge.

²⁴ I am indebted to Lundvall (2012) for this particular challenge.

²⁵ Others refer to the concept of ‘inclusive innovation’ (e.g. Utz and Dahlman, 2007). In that it incorporates the notion of both including all those affected in the innovative process, as well as playing a central role in development (by ensuring that fewer are excluded from learning), inclusive innovation spans my challenges 6 and 7 (and even elements of challenge 8 with its emphasis on enhanced well-being).

²⁶ Over the 20th century, life expectancy in advanced countries increased by 25-30 years (Mackenbach, 2013a & b). This phenomenal achievement often goes unremarked. While there may have been some slowing in recent decades, every day we live our life expectancy increases by several hours (Oeppen & Vaupel, 2002).

²⁷ Some would go further, arguing that concerns with defence and security have resulted in ‘bad’ innovations that reduce rather than increase welfare. Indeed, one might include certain financial innovations in this category. However, as noted later, the financial innovations were initially introduced for the best of reasons (e.g. reducing risk); only later when the scale increased did the unintended consequences become clear.

²⁸ Research by psychologists suggests that once the number of choices for a good exceeds half a dozen or so, customers are more likely to be stressed by choosing between them as well as more likely to later conclude that they made the wrong choice (the ‘choice overload’ hypothesis – see e.g. Iyengar & Lepper, 2000).

²⁹ Some might even challenge the assumption that ‘the more innovation, the better’, arguing that we have perhaps become addicted to innovation for its own sake (Westley et al., 2011, p.8).

³⁰ Although contested by some (e.g. Inglehart et al., 2008), it was recently re-affirmed by Easterlin et al. (2010). In the case of women, the paradox is particularly pronounced, with substantial gains in income and other ‘objective’ measures of well-being having been accompanied by an absolute decline in subjective well-being (Stevenson & Wolfers, 2008).

³¹ For a broader discussion of the complex relationship between globalisation and inequality, see e.g. Held and Kaya (2007) and Milanovic (2012).

³² For an analysis of ‘the economics of superstars’, see Rosen (1981).

³³ Besides the ‘winner takes all’ effect among producers, there is the related issue of who benefits among the customers, with many innovations primarily benefiting those at the ‘top’ of the economic pyramid rather than those at the bottom (Soete, 2013). Senker and Cudworth (2012) have examined ‘dysfunctional innovation’ – i.e. innovation that ‘sustains economic and social inequality rather than stimulating economic growth’. In contrast, the term ‘frugal innovation’ has been used to describe innovations providing ‘‘good-enough’, affordable products that meet the needs of resource-constrained consumers’ (Zeschky et al., 2011, p.38), another area where the IS community has yet to devote much attention, not least because frugal innovation remains largely invisible to traditional IS metrics (Bound & Thornton, 2012, p.6).

³⁴ I am indebted to my SPRU colleague, Mariana Mazzucato, for this challenge.

³⁵ As the ‘varieties of capitalism’ literature reveals (e.g. Hall & Soskice, 2001), besides liberal market economies, there are other types, in particular coordinated market economies and state-influenced market economies. In these, the state plays a rather different role than in liberal market economies, but for reasons of space I am not able to deal with these here.

³⁶ The formulation of this challenge was prompted by my colleague, Ed Steinmueller.

³⁷ Where evidence is taken into account, it is frequently of an economic character, in particular that stemming from some form of ‘cost-benefit analysis’. Such an approach all too often reflects some hidden agenda, while the crudeness of many such efforts (for example, with regard to what counts as a ‘cost’ or a ‘benefit’) results in more complex aspects being systematically ignored. For a critical discussion, see Palfrey et al. (2012), especially Chapter 5.

³⁸ As noted earlier, we also need a better understanding of the complex interaction between policy researchers and policy makers.

³⁹ According to Scholz (2012), data from the British Medical Journal indicate that ‘51% of medicine is of unknown effectiveness ... [with] 3% ... likely to be ineffective or harmful, 5% unlikely to be beneficial, 7% [offering a] trade-off between benefits and harm, 23% likely to be beneficial, and only 11% beneficial’.

⁴⁰ For suggestions as to how randomised controlled trials might be applied in the world of public policy, see Haynes et al. (2012).

⁴¹ In fairness, Chesbrough’s concept of open innovation recognises the use of internal as well as external ideas.

⁴² A related challenge concerns the balance between Burt’s notions of ‘brokerage’ and ‘closure’ with regard to the exploitation of social networks for the purposes of innovation – in other words, the extent to which one builds new connections *across* groups in order to increase one’s exposure to differing perspectives and practices, compared with the extent to which one focuses on strengthening existing links *within* a group in order to focus on a limited set of perspectives and practices, with the former likely to stimulate more radical advances, while the latter is generally better suited to generating incremental improvements (Burt, 2009).

⁴³ It could be argued, for example, that the number of studies focusing on new ideas or products in creative businesses is disproportionate to those focusing on innovation in large, complex organisations such as public sector institutions (Finn Hansson, private communication).

⁴⁴ Initially, it was probably better characterised as ‘multi-disciplinary’, but as researchers from the different disciplines began to communicate more directly with each other and to integrate the inputs from different disciplines, it became more ‘interdisciplinary’ (Martin, 2012a, p.1237).

⁴⁵ A similar concern has been raised with regard to management research (Tsui, 2007).

⁴⁶ Even researchers from management or business studies seem to be driven by an aspiration to be more econometric than economists in terms of devotion to theories, hypotheses, models and statistical analysis (Alan Hughes, private communication).

⁴⁷ For a sardonic anthropological account of the Econ tribe and their devoted worship of ‘modls’, see Leijonhufvud (1973).

⁴⁸ As Giovanni Dosi recently remarked, this tendency seems to be most pronounced not among the older economists but those in their 30s and 40s – the ‘Taleban tendency’ of economics, as he provocatively described them (comments at the FINNOV Conference, London, 1-2 February 2012).

⁴⁹ In the UK, this process has been intensified by successive Research Assessment Exercises (Lee, 2007).

⁵⁰ This process began in the early part of the 20th Century before the field of innovation studies began to emerge but has intensified since (Ed Steinmueller, private communication).

⁵¹ As we now know, while individual institutions believed they were minimising the risk to themselves, the overall effect was to greatly magnify the risks to the financial system as a whole (Haldane & May, 2011).

⁵² See <http://www.finnov-fp7.eu/events/finnov-final-conference-2012> (downloaded on 5 August 2013).

⁵³ At the same time, it should be noted that the financial innovations, and their widespread impacts, would not have been possible without the intensive use of ICT (Engelbrecht, 2009).

⁵⁴ As should be clear from the text, I am indebted to Lundvall (2012) and others for this particular challenge, although the specific phrasing owes something to Dosi (2011).

⁵⁵ See also Stiglitz (2012, p.32); ‘economists (and their models) also bear responsibility for the crisis. Flawed monetary and regulatory policies were guided by economists’ models, and the dominant models failed to predict the crisis and said that such a crisis could not or would not happen.’

⁵⁶ These days, contributions by economists seem to be judged more on the elegance of their methods than on their relevance to addressing real economic challenges – in other words, they seem to have mistaken beauty for truth (Krugman, 2009). Of the ten most highly cited economics articles identified by Kim et al. (2006), no less than seven were econometric methodology (or statistical) papers. As Lawson (2009, p.760) caustically notes, the over-emphasis by neo-classical economists on mathematical deductivist models ‘mostly gets in the way of understanding’. Or as Coase (2012, p.36) observes, ‘The degree to which economics is isolated from the ordinary business of life is extraordinary and unfortunate.’

⁵⁷ See also Stiglitz (2012, p.33): ‘Whenever markets have imperfect information and incomplete risk, the markets are almost never efficient. They are also not stable ...’.

⁵⁸ He also sets out some ideas on what this might involve (Lundvall, 2012, pp.11-16).

⁵⁹ ‘Economics, as a field, got in trouble because economists were seduced by the vision of a perfect, frictionless market system’ (Krugman, 2009).

⁶⁰ As Lundvall (2012, p.11) acerbically notes, ‘If the world was neo-classical – with pure markets and populated with rational agents and representative firms – very little innovation would take place.’

⁶¹ This is not to underestimate the magnitude of the task. As Kuhn would predict, there will be considerable opposition from within economics, at least from the current generation of leading economists. The IS community will need to join forces with newer and more sympathetic sub-fields of economics such as behavioural economics, experimental economics, and ecological economics (cf. Colander et al., 2004).

⁶² This challenge might be extended to include Lundvall’s (2012 & 2013b) plea that IS researchers, besides conducting research to further their own careers, emulate illustrious pioneers like Freeman and Nelson (and, one might, add Lundvall himself) in contributing collectively to the ever necessary work on developing and strengthening the ‘infrastructure’ of the field (in the form of research centres, journals, conferences, professional associations, networks, mentoring young scholars, and so on) – in other words, ‘public good’ contributions rather than those aimed at enhancing individual careers.

⁶³ The paper was never published by OECD, perhaps for the reasons identified by Lundvall (2004, p.531). However, it finally appeared in print 20 years later in *Industrial and Corporate Change* (Freeman, 2004).

⁶⁴ All this is based on the experiences of *Research Policy* over recent years (Martin, 2013b). While the details of such cases must remain confidential, miscreants should be aware that journal editors now often informally share information on such misdeeds in order to be on the lookout for repeat offenders.

⁶⁵ Nearly all those found guilty of plagiarism have been male – an interesting phenomenon for sociologists of science or perhaps evolutionary biologists to explain.

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