A PENNY FOR YOUR THOUGHTS

A NOTE ON THE IMPACT OF IDEAS

Alan Freeman Thursday, 1 December 2016

Introduction: Booms, paradigms and the force of ideas

It is widely claimed, paraphrasing Victor Hugo, that no force is more powerful than an idea whose time has come. The assertion is so far unproven: this paper seeks to prove it. It draws on the two concepts of a 'long economic boom' or simply 'boom', and of a 'mental object'. The first is associated with the work of Nikolai Kondratieff, Joseph Schumpeter, Christopher Freeman, Carlota Perez and others, whose research has developed our understanding of the effects of innovation. The second is central to my forthcoming book on the creative industries.

The term 'long boom' refers to an historical event that has occurred at least four times (Freeman 2001, Perez 2003): a decades-long period of rapid economic growth and far-reaching social transformation, usually initiated in a particular country or group of countries, fuelled by pervasive new cost-reducing technologies and consumer products across an entire range of activities such that each innovation feeds on the others, as did textiles and water power in the industrial revolution, or the car, the road, and the oil industry in the postwar golden age. Booms are usually distinguished from the upswing of the normal 7- to 11-year business cycle, and may include a number of such cycles.

Booms alternate with 'great depressions' like that of 1870-90, called the 'great depression' until ceding this title to the second such, the 1930s. The present phase of prolonged stagnation in the industrialised world is a strong contender for the title of the third. This led earlier writers to suppose that booms arise automatically when depressions have played out, leading to the term 'long wave' and Schumpeter's well-known phrase 'creative destruction', which he claimed was the manner in which a depression prepares for the succeeding boom. This idea is at odds with the evidence, and pioneers such as C. Freeman and Carlota Perez have moved away from it, preferring to speak of 'surges' related more to invention and discovery than to anything caused by the preceding depressive phase. This paper's point of departure is that booms result from conscious human action. Every boom is historically concrete; however Desai and I (2016, see also Freeman 2014a, 2014b) have identified certain conditions that appear to be indispensable for all booms. These include, *inter alia*, public investment on a great scale. This paper focusses on a further condition that seems equally indispensable: the general integration, into both production and social life, of one or maybe two deep, new ideas.

The main examples of booms are the 'age of steam and coal' of 1848-1870, the feverish but expansive *belle époque* that started in 1893, the postwar 'golden age' of 1942-1968, and the industrial revolution itself which, whilst authors differ on the start date, is generally considered to have petered out around 1830. Authors have other minor differences on the start and end dates of these booms, but broadly agree on their essential elements including, in particular, an accelerated rate of growth compared with the intervening periods. They are characterised by systematic cost reductions achieved through new and more productive technology, leading to what Perez (2003) terms a 'socio-economic paradigm' when new kinds of consumption such as the railways, motor transport, or the household appliance fall within the reach of the mass of wage-earners, transforming social life.

Much is known about what drives them: innovation (Freeman and Soete 1997), which brings inventions to market, is a critical element. Something is known about why they stop; least clear is how they start. It is generally accepted that at the core of each lies a set of 'discoveries' linking its key innovations. Those of the industrial revolution, for example, all depended on *mechanization*: by repeating a single identical act, humans could achieve economies which, when either assisted or performed by devices, could be extended without apparent limit.

With each boom, the ideas that underlie its inventions become more sophisticated: hence, with the age of coal and steam, the idea of *motive power* required sufficient sophistication to grasp the concept of energy and its conservation and transformation. Yet the core ideas of every boom depend on phenomena that previous booms make commonplace. Thus, the science of *materials*, a transformative agency of the 'age of steel, concrete and electricity', matured fifty years after iron had forged the industrial landscape of the age of steam and coal, whilst quantum mechanics arose through the urgent need to explain either entirely manmade phenomena such as cathode rays, or natural phenomena observed in manmade conditions, such as electromagnetic radiation. Each boom thus yields great extensions of knowledge by contemplating not just nature but its transformation by a previous age.

These great discoveries were moreover integrated into inventions with significant delays, especially when deployed in scale. Joule, Mayer, Helmholz and Clausius discovered the basic laws of energy conversion in the 1840s, but large-scale commercial electrical power had to wait until the 1880s. James Clerk Maxwell formulated the laws of electromagnetic radiation in 1861 but it was not until 1886 that Herz produced radio waves, whilst Marconi laid the foundations of commercial wireless nearly twenty years later. The principles of the transistor were known by the 1930s, yet solid state devices only entered general use in the 1960s.

Such delays are quantitatively comparable with the 30-40 year gaps that separate the booms. This makes it quite difficult to assess, in a direct causal sense, the economic consequences of 'great discoveries' whose history has been extensively researched. We can predict, accurately enough for investment purposes, the potential returns and risks of an innovation by assessing its potential impact on costs, and the likely scale of its adoption. It is more challenging, perhaps because rarely attempted, to assess the economic or social worth of an idea whose impact becomes known only a generation later. This makes it hard to determine the relative importance of the large sums spent on 'basic science'. Is it better to build a hadron collider, put a woman on Mars, or engineer an anticancer microphage?

Yet a method does suggest itself. It has two components: we can compare periods of rapid growth with those of slow growth, and we can identify the mental objects—of which more later—that appear to have been either causally decisive, or at the least practically indispensable, in the rapid growth periods. Since we can compare average growth in the rapid periods with that in the rest, we can then attribute the difference in growth rates to the ideas that underlay the boom. It remains only, then, to identify which ideas really mattered.

These differences in growth rates are measurable, because economics can attach meaningful numbers to the idea of growth, and tangible, because they correlate with socially recognisable phenomena. Rising prosperity is visible. The poor are sucked off the streets, work is plentiful, children are happy, plates are full, and hope abounds. Stagnation is equally visible, and the talk is of nothing but misery and complaint.

Chart 1a: Growth of world real GDP 1961-2005

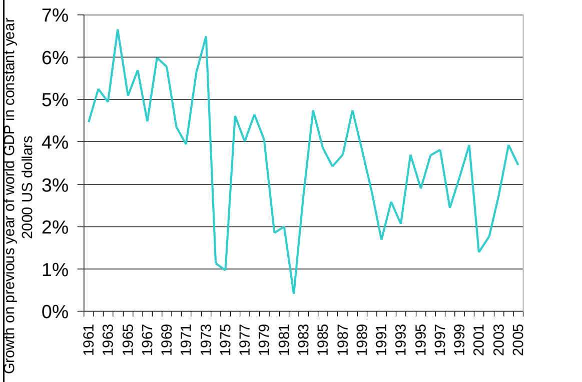
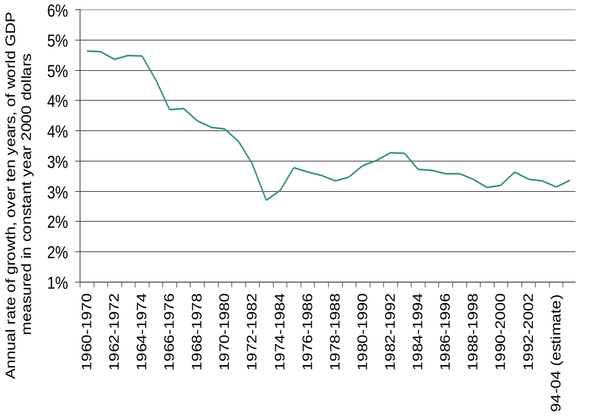


Chart 1b: 10-year average growth of world real GDP



Source: World Bank, IMF International Financial Statistics, and author calculations

Chart 1a shows annual growth in world real GDP since 1961—before then, growth was if anything higher, but statistics are incomplete and so we focus on the comparison with the following stagnant age. Chart 1b shows the moving average over the previous ten years. This never rose above 3.1 per cent in any ten-year period; before 1981, it never sank below this level. In 1960-70—the earliest year for which we have a complete figure—it stood at 5.4%, over two percentage points above the highest rate recorded after 1980.This suggests a standard for comparison: we can calculate the difference between the ages of growth—the booms—and everything else. This difference is at least two percent and possibly three. The ‘impact’ of a boom is thus the total excess growth, over the typical period of a boom, compared with growth outside of the boom periods, compounded over the typical life of a boom. As a conservative minimum this would be 1.02^{20}, that is to say 50%. More realistically it would be 1.025^{25}, that is to say 85%. An optimistic estimate would be 1.03^{25}, that is to say 109%; in short, a boom improves things by somewhere between 50 and 100%.

But to what can we legitimately attribute these results? In the works referred to above, Desai and I sought to distinguish what is indispensable from what is merely helpful. What emerges is that not merely the innovations, but the ideas behind them, are decisive. Without mechanisation, the industrial revolution would not just have been more difficult: it would not have happened. It *was* mechanisation, in an essential sense. The *idea* behind mechanisation—the indefinite replication of identity—was a precondition of the revolution itself.

But are all ideas equally important? They are interconnected. They depend on each other, and they progress from each other in time. Those ideas that lie behind other ideas, or which are preconditions for their existence, are clearly in some sense ‘more important’. Without algebra, modern computing simply could not have happened. It follows, however, that if we seek some kind of partial ordering for the importance of ideas, we have to invert the standard line of enquiry, which looks forward to their predictable commercial results, and instead look backwards to their identifiable logical origin.

We are aided by the fact that the logical connections leave their mark on history. When we study the sequence and timing of the apparently diverse inventions that studded the industrial revolution—Harrison's clock (1761), the Spinning Jenny (1764), Arkwright's Water Frame (1771), Cartwright's Power Loom (1784), and the subsequent evolution of the entire factory system—we cannot but notice the pervasive effect of the simple idea of identifying a single elementary operation and repeating it, under conditions that allow a large increase in the number of such operations that a given group of humans can conduct in a given time. The idea called mechanisation thus took root in social and economic life at a definite time, and under historically identifiable conditions.

In like manner, the railways, the factory system, and the industrial town are scarcely conceivable without that mastery of the 'forces of nature' summed up in Mayer's (1847) principle of the conservation of energy, Joule's (1841) discovery of the interconvertibility of electrical and kinetic energy, and a general application of those principles of force and power which underpinned an epochal transition from water-based sources in a fixed place, to mineral-based sources constrained only by the development of transport.

In the same way, without the generalised usage of steel, concrete, chemicals and electricity that characterised the *belle époque*, neither America nor Germany as we now know them would exist. Equally decisively, without the general application of quantum mechanics, almost none of the inventions that fuelled the postwar boom would have existed: modern lighting, nuclear power, modern chemistry, the transistor, the computer, appliances, the car—the list is not only long, but exhausts most inventions of the second half of the twentieth century.

Neither is the transformative function of such ideas confined to their productive power. They also shape the way we think about the universe. Mechanical analogies led directly to Laplace's 'billiard ball' model of the cosmos as a giant, predetermined automatic system, and his bold response to Napoleon that God was an unnecessary hypothesis, founding the deterministic positivist vision. The concepts of energy, power and force pervade Victorian thought, whilst futurist art expresses the quintessential idea of a new world 'made by man' that dominates the *belle époque.* Not least, the modern notion of a world of chance, chaos and complexity was—ironically—an almost inevitable postwar outcome of quantum physics' victory over 19th Century determinism.

An idea of this nature thus informs and connects the inventive spirit of an age, its productive and social consequences, and the way it makes sense of the universe. Re-deploying Perez's terminology, I propose the name 'paradigmatic idea' for this phenomenon.

The concept of a mental object

To develop these embryonic notions, we need to take two further points into account. The first, already alluded to, is that to study the usefulness, or impact, of an idea, we must reverse the direction in time employed by scholars of innovation, whose focus is on outcomes. An idea is recognised by its end results: it is embedded in an invention or group of inventions, comes to market, and becomes a success. But to see where it came from, we have to look at the problems it solved. If nothing else, the time lag in application is simply too long for any other mode of enquiry. In addition, however, the consequences of truly great ideas are far too complex to anticipate in any but the sketchiest terms. Working at the turn of the century, Edison or Marconi knew enough about electricity to 'put the idea to work', but they drew on knowledge that had gestated 30-40 years earlier. Neither Maxwell nor Mendeleev conceived of the modern broadcasting or chemical industries, yet without the periodic table or the laws of electromagnetic radiation that they set out in 1864 and 1869 respectively, these subsequent inventions would have been impossible. Indeed Herz himself (Norton 2000) said of his 1886 demonstration of radio waves: 'It's of no use whatsoever ... this is just an experiment that proves Maestro Maxwell was right.'

This lack of 'translatability' of ideas into practice—to use the ghastly term which modern research councils now use to judge what to fund next—is arguably the greatest obstacle to a modern, rational policy of ideas. For, if one enquires into what an idea can produce, one has already asked the wrong question. How should we identify the relative importance of ideas? By studying them in their habitat, which is the succession of booms. In this habitat, what leaps to the eye is that they were not designed to produce results, but to solve problems.

Ideas are, indeed, *produced by* problems: Mendeleev's synthesis followed an astonishing proliferation in the discovery of new elements, shown in table 1. One need only put oneself in the shoes of a chemist of the 1850s to see that her problem was not how to use or even discover the next new element, but to understand what an element actually was. The century-long process of discovery that lay between Lavoisier's 1789 recognition that all matter was composed of elements, and Rutherford's 1894 discovery of the atom, was driven not by the pressing desire to make these ideas work, but by the urgency of explaining why they did so.

Table 1: the number of elements isolated in each historical period

Scientific epoch	Number of new elements
Antiquity	12
Modern times before the definition of 'element' (1789)	16
After the definition of 'element' and before the periodic table (1864)	35
After the periodic table and before the discovery of the atom (1894)	14
After the discovery of the atom and before nuclear fission	13
Atomic Age	28

Source: https://en.wikipedia.org/wiki/Timeline_of_chemical_element_discoveries

A systematic approach to the impact of ideas requires therefore, not any new advance in the science of innovation but the foundation of a science of discovery. The question we should ask of a new idea is not what things it might produce, but what problems it might solve.

The second issue to address is, then, to actually pin down what an 'idea' consists of. Thanks, in part, to the discovery of software, but also the simple accumulation of fossilised facts deposited by three hundred years of industrial society, we are in a better position to define what an idea consists of than our predecessors. In particular, we can see that the 'paradigmatic ideas' under discussion share an important characteristic: they manifest themselves in behaviour. Arithmetic is a definite idea, not just because of abstract number theory but because at a definite time in history, large numbers of people learned it, and as a result, history was changed. The same applies equally to the 'idea' of mechanical engineering, the chemistry of materials, or the quantum theory of matter. At the risk of reductionism, we can single out a certain category of ideas that are identifiable through their economic effects, that is to say, the changes they bring about in the conduct of production and consumption.

The fact that ideas can have an economic impact is quite alien to economic thinking, which has become dependent on the notion for which the first two long booms are responsible, that productivity gains arise only from replacing men with machines. By degrees, machinery has come to be conceived of as the primary source of economic value, with humans as a disposable appendage. With Desai, I term this the 'machinocratic' mode of thought (Freeman 2014a), by analogy with 18th Century French physiocrats who held that the sole true source of value was the natural fructiferousness of the land, which the idle towns merely worked up unproductively into a variety of different forms to suit taste and fancy. Actually, from the dawn of capitalism, and indeed earlier, productivity has depended as much on ideas as on machines. This leads us to the concept of a mental object.

In 1478 a book appeared in Treviso, Venice, entitled the Treviso Arithmetic, in the vernacular of the time *Arte dell'Abbaco* (Swetz and Smith 1987). Its purpose was to make the Hindu-Arabic system of arithmetic available to the families of ordinary merchant classes. The resultant revolution in computation transformed commerce: the Treviso school was for over a century the educational destination of choice for the commercial families of Europe. The reason was simple: in passing from Sweden to Southern Russia, trading as he went, the merchant of the times needed to exchange his money as many as thirty times. Mental arithmetic, displacing the cumbersome abacus, was a decisive productive advantage.

The book impressed Napier, the inventor of logarithms. 'Napier's Bones' as they have become known, transformed the science of gunnery, which relied on the use of long tables of angles and distances which were arduous to calculate, because they involved multiplication. Napier reduced multiplication to the much easier addition. Although his original invention had a physical component to it—the 'bones' were a collection of inscribed cubes whose manipulation led to the result—the underlying principle of the logarithm, which is what survived into modern times, was a purely mental construction requiring only two sets of tables, which the schoolchildren were still using in the 1950s: logarithms and antilogarithms.

Arithmetic and logarithms together transformed all aspects of early technology; nothing was left untouched. Starting with artillery, arguably the most powerful transformative technology of early modern times, they laid the successive basis for astronomy, navigation, commerce, and all those mechanical inventions in which precision in numbers, rather than crude trial and error, was essential. Yet almost no machinery was involved in them, except the bones and the occasional slide rule. They were a mental construct which transformed production.

Equally critical were the general natural discoveries of the Victorians: were it not for Newton's laws of motion, Maxwell's laws of electromagnetics, and Mendeleev's periodic table, not one of the innovations that launched the Twentieth Century would have been conceivable.

The existence of ‘mental constructs which transform production’ is thus empirically almost beyond doubt. The problem is: what explains them? The facts speak for themselves: their effect on production leads, quite naturally, to the desire to replicate them. The ‘repetition of the identical’ applies to ideas as well as things. Unlike material objects, however, mental objects are replicated not by *manufacture* but by *communication*. They are transferred from person to person and society to society by the many means we have developed for this purpose: teaching, writing, painting, singing, speaking, dancing and googling to name but a few.

A mental object is in summary a replicable, and by this token communicable, mental construct. As such it necessarily assumes an existence external both to the individual humans in whose minds and actions it takes root, and to the objects that these individuals use to replicate and communicate it. A poem does not live on the leaf of a single folio, but on every page on which it was once written, in the words of every poet who once spoke it, and the heart of every being touched by it. The Papyri on which ancient Egyptian builders first inscribed what is now known as Pythagoras’ theorem lie in dust, but the theorem itself lives in the books of all ages, in masonic monuments throughout the world, in ceaselessly-multiplying internet pages propounding or explaining it, and in the lessons drummed into the unwilling heads of a hundred generations of schoolchildren.

The Western industrial approach to the history of ideas, originating in the German Romanticism of Schiller, Friedrich, Klopstock, Herder and their companions (Safranski 2004), obscures their true nature. It associates ideas with the mechanisms of the brain, as if psychology held the keys to the secrets of the universe. It focusses obsessively on genius, on the individual to be named as discoverer, displacing the actual content and social function of the idea to a sideshow.

Actually, ideas are fundamentally social. Galileo and Copernicus built on a cosmological discovery set down by Aristarchus in 246 BC.[1] We celebrate the ‘genius’ of Darwin but forget that countless fossil hunters were already homing in on the same notion. We all know, because Newton tells us, that he stood on the shoulders of giants; but he also rubbed shoulders with the somewhat lesser mortals who founded the Royal Society in 1660 as a “College for the Promoting of Physico-Mathematical Experimental Learning”. Einstein’s carefully scripted commendation of Newton’s theory of gravity (Capra 1983: 49) as ‘the greatest advance in thought that a single individual was ever privileged to make’ needs to be moderated by Fara’s (2004) observation that ‘Newton's reputation, surprisingly limited in his day, was carefully cultivated by devoted followers so that Newton's prestige became inseparable from the explosive growth of science itself.’ Landau and Lifshitz’s (2013) remarkable ten-part course in physics establishes all the principles covered by Newton’s laws of motion on the logically superior foundation of d’Alembert’s principle of Least Action. Newton, and all his works, are in short no more than a literary phenomenon inscribed on the surface of the true history of ideas, which are the product of ages, not genii. Without

[1] for which Cleanthus called for him to be burned at the stake, on the grounds of 'saving the phenomena by displacing the earth from its rightful position at the centre of the universe.’

demeaning the 'privileged individuals' who set ideas on paper, we should ask how, and why, these same ideas get up off the paper and haunt the world.

The answer to this is complex, and I don't give it here: rather I propose it as a branch of study, in which society should invest resources. Nevertheless, there are pointers. Mental objects are deployed in production itself; examples would be schematics, formulae, instructions, and of late, computer programmes. As such, they modify and animate the instruments of production; without the instructions conveyed in the weaver's punch cards, the Jacquard loom would have been little more than a device for making sacks. Inhabiting no medium independent of any other, mental objects thus mediate between humans and their constructions, ranging from scribbled notes to gaunt factories and vast electronic warehouses, all of which both bear the imprint of ideas passively, and actively transmit them materially.

Moreover, mental objects naturally divide into two classes: *predicative* objects which serve to make judgements on the nature of things we encounter, and *algorithmic* objects that are employed to produce things that conform to definite predicative objectives, generally by repetitive means. When we ask if a piece of clothing is red, pretty, reprehensible, or lawful, we make a predicative judgement, calling on the ideas associated with colour classification, aesthetics, cultural stigmatisation or political and social regulation. When we build machines to spin, weave, print and shape it, we employ algorithmic knowledge, of chemical process, the source and behaviour of fibres, and of manufacture, that can generally be translated into a set of instructions to be followed by a community of humans and machines, working together to achieve something that meets the given predicative judgement.

The separation of ideas into predicative and algorithmic is not only widely recognised, but lies at the heart of modern mathematics and logic. It has many deep consequences, not least the profound theorems of Church, Turing and Gödel which set limits on what algorithms can achieve, and establish that there will always be 'new questions'—predicative descriptions of problems whose answers can only be obtained through the creation of new ideas.

The historians of invention, for the most part labouring under the weight of machinocratic prejudice, tend to triumphalise the algorithmic over the predicative, to the extent that their narrative conceals evident truths. As the subsequent development of the wool, linen and cotton industries shows, the industrial revolution, in which mass-produced fabric freed the middling classes of Europe to imitate the dress of their betters for the first time in history, might with equal justification be called 'the age of clothing'. But we do not think about industrial history in terms of social outcomes, only in terms of productive means. This same narrow focus, in the sphere of the production of ideas, obscures the most fundamental characteristic of discovery, namely the creation of new predicative mental objects. When we seek to understand science, from the moment we focus on *method* as opposed to *explanation,* we are lost.

To digress for but one paragraph, it is for this reason that the entire mystique that surrounds 'artificial intelligence' and the fear of robots inverts the issue: once we recognise that mental objects acquire a life outside the mind of any particular human, it is an inescapable conclusion that they will enjoy a life outside the mind of all humans, and therefore, they will in part reside in the interior of inhuman objects which they thereby infect with humanity. The secret fear of the robotophobe is not that machines will become human, but that humans will become machines, and this risk has been with us since the dawn of the mechanical age.

However, the key point which concerns us here is that predicative objects make their presence felt at both ends of the chain that leads from scientific discovery to practical innovation. At the future end, predicative objects tell us 'what society wants'. They shape and define what products we wish to display in the front office of material production. But they not only haunt the back office of mental production; they run it. The solution to a 'problem' is defined predicatively. We only know when somebody has 'made sense' of the facts when the explanation makes sense to us; there is no great machine which we can set in motion, which will explain them on our behalf while we relax and tend the garden. There is no other road to comprehension than unrelenting cognitive vigilance.

This leads us to the fundamental point: *mental objects are engaged in the production of other mental objects.* This is, indeed, the reason that research makes any sense at all (Bakhshi, Desai and Freeman 2009). Ideas do not arise out of nothing but are produced, through the combined interaction of humans with nature, with their own prior natural productions, and with their own prior mental productions. This may seem obvious, and indeed it is; however, recognising the importance of an obvious fact is one of the few universal keys to truly significant mental advances.

The role of ideas in production needs, finally, to be re-connected to a part of the picture which we have alluded to without developing: the role of ideas in changing society. Ideas shape not just the way we understand the products of nature, or even our own products; they shape the way we understand ourselves. In this sense, a system of laws, a set of aesthetic principles, or a religious tract, is every bit as much a mental object as a scientific theory. All genuinely paradigmatic ideas have, for this reason, been cosmological in character: as I attempted to set out in Freeman (2007), they are used simultaneously to explain the world and our own society.

Is there a next big idea?

The clues are all in: let detection commence. We can now pin down what a deep, new, paradigmatic idea might consist of, how it might help us shape our somewhat uncertain future in a way that improves its prospects, and where we might start looking for it. In particular, we are in a position to ask a question which, although we did not make this clear at the outset, is the purpose of this paper. Is it possible that a paradigmatic idea or ideas, capable of promoting change and growth to the same extent as the ideas we have been discussing, *already exist,* in possibly embryonic form, and are they accessible on the basis of what we already know? And if so, is it possible both to modify the prospects of finding them, and also to foresee, and either ameliorate or remove from the future horizon, the maleficent effects that have so far accompanied the benefits of all such ideas?

The answer is a surprisingly simple yes. First, we can modify the prospects of discovering any given idea merely by stating that it is possible—in short, by getting people to think about it. This paper itself is a modification of possibilities. Everyone that shares, disseminates, or builds on the ideas herein, will further modify these prospects and, by so doing, encourage also some thinking about how to reduce the dangers. Second, we can identify the general area of knowledge in which this idea, or these ideas, might arise, by asking 'where are the biggest problems?' Desai, I and our collaborators have written extensively (Bakhshi et al 2013, Freeman 2014a) on the role of human creativity in production, and have shown that this functions, in the dynamic new economic sector known as the 'creative industries', in a way that distinguishes it sharply both from previous forms of material production, and from the role of scientific labour as generally engaged in modern production. This distinction rests, however, on a demarcation between the *application* of scientific ideas in innovations, and the *discovery* of scientific principles, which is the subject of this paper. As I hope it has clarified, these are two very different aspects of the role of science in production.

Discovery, unlike the mechanical application of known principles, is undoubtedly a creative activity, for all the reasons just explained. A rational policy of discovery, however, cannot ignore the question we posed at the outset, namely, how do we know which ideas are important? The answer, I would suggest, is to identify the key problems of our understanding of the universe that have come to light as a result of the practical work of the previous age of invention. Which complexes of phenomena did the long boom of 1942-1970, dependent as it is in almost all its socio-economic consequences on the great achievements of quantum physics, fail to explain?

To bring this line of enquiry to a (temporary) close, we need to examine two further closely related points: the extraordinary, and under-recognised, dependency of modern production techniques on the discoveries of quantum physics, and the equally extraordinary failure of quantum theory itself to explain the very discoveries to which it has given rise. The 'crisis in quantum physics' is expressed in the diversity of 'interpretations' of quantum theory, the number—at least forty-seven (Gillespie 2013)—of recognised 'unsolved problems' in the field and not least, the growing number of leading experts in the field who daily throw up their hands and admit that the problems they once considered solved, now appear in the light of current theoretical understanding to be greater mysteries than ever.

It is widely proclaimed that quantum physics differs from its predecessor branches of knowledge in that it produces immensely practical and accurate results, whilst at the same time either eschewing, or failing to produce, a coherent explanation for them. Yet, I hope this note has established, this combination is not unique; to the contrary, it characterises the major paradigmatic advances of the past three centuries. We gain little from exoticising the difficulties facing quantum physics, and much from the ample precedents for paradigmatic ideas which render bewildering past achievements coherent and understandable.

Albert Einstein was the lead proponent of the idea that laid the foundation for the fourth long economic boom. His 1905 paper on the photon set quantum theory in motion. His idea built on the work of Max Planck to give physics a window into the behaviour of atoms. Yet he was one of the deepest and most persistent sceptics as to the 'theoretical completeness' of the emerging new system, as his lifelong correspondence testifies (Einstein and Born 1971, Bohr 1949).

In 1954, Einstein built further on Planck's work to speculate about the next window into something even smaller: Planck-scale physics. He wrote: 'I consider it entirely possible that physics cannot be based upon … continuous structures. Then *nothing* will remain of my whole castle in the air including the theory of gravitation, but also nothing of the rest of contemporary physics.' He was adverting to Planck's 1899 calculation of a smallest possible size—shades of the ancient Greek's idea of an indivisible atom. Today, physics is increasingly pursuing Planck-scale physics in its search for answers to far deeper problems and contradictions than those that drove change a century ago.

This is a deep new idea of the same ilk as the one that led to the fourth Kondratieff boom. Indeed, it is more fundamental. Will it help lay the foundation of a fifth boom? If so, we may suggest, on the basis of the calculations above, that it could unlock the way to a quarter-century-long improvement in the well-being of the planet between 50 and 100% of its (presently $80 trillion per annum) economy: well worth a throw of the dice.

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