Economics and Research Assessment Systems

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Abstract

This paper seeks to analyse the effects on Economics of Research Assessment Systems, such as the Research Assessment Exercise (or RAE) which was carried out in the UK between 1986 and 2008. The paper begins by pointing out that, in the 2008 RAE, economics turned out to be the research area which was accorded the highest valuation of any subject in the UK, even though economists were then under attack for failing to predict the global financial crash which had occurred a few months earlier. One aim of the paper is to explain this economics anomaly in research assessment. The paper goes on to point out a key difference between economics and the natural sciences. Most areas of the natural sciences are dominated for most of the time by a single, generally accepted, paradigm, whereas there are always in economics different schools of thought which have different and highly conflicting paradigms. Given this situation, it is argued that the effect of research assessment systems in economics is to strengthen the majority school in the subject (whatever that is), and weaken the minority schools. This conclusion is supported by empirical data collected by Frederic Lee for the UK. It is then shown that the greater the dominance of the majority school, the higher the overall valuation of the subject is likely to be, and this is used to explain the anomaly noted earlier. It is argued that research in economics flourishes better in a situation in which there are a number of different schools treated equally, than in one in which a single school dominates. The conclusion is that research assessment systems have a negative effect on research in economics and give misleading results. Instead of such systems, an attempt should be made to encourage pluralism in the subject.

1. Introduction. Research assessment systems in general

Research Assessment Systems began with the Research Assessment Exercise (or RAE) which was introduced into the UK by Thatcher in 1986 and continued by Blair. For many years, the RAE was the only Research Assessment System, but recently the situation has become more complicated. After the RAE had been in existence for more than 20 years, the UK government decided that it was unsatisfactory and should be replaced by a new type of Research Assessment System to be called the Research Excellence Framework (or REF). The UK government accordingly announced that the Research Assessment Exercise to be held in 2008 (RAE 2008) would be the last RAE. They also gave an outline of how the new REF would be conducted. The initial plans for the REF, however, gave rise to much debate, and have been altered several times. They may not even be fixed now. Meantime further complications have arisen, since some other countries, notably Italy, have decided to introduce Research Assessment Systems. These will be to some extent modelled on the UK RAE and REF, but are likely to differ in detail from both the RAE and REF. We are thus in a complex situation which involves several Research Assessment Systems which differ from each other in detail.

Despite these complications, I think it is possible to give a general definition of a *Research Assessment System (or RAS)* which captures the essential features in common to the various existing systems, while abstracting from the details. The definition runs as follows:

A Research Assessment System (or RAS) is a system in which groups of researchers are assessed at intervals. If the assessment is good, the group retains its funding or gets more, while, if the assessment is bad, the group's funds are reduced or perhaps removed altogether.

Obviously the central problem for any RAS is how the assessment of the value of the work of the group of researchers should be carried out. Two basic methods have been suggested. The first of these is *peer review*. This means that the value of a researcher's work is judged by a set of researchers working in the same field – the 'peers' of the given researcher. The original RAE was based entirely on peer review. The second method of assessment has arisen out of the development of computer technology which has created vast data-bases containing information about research being carried out. This suggests that instead of getting a panel of humans to judge the value of the research output of a group, one might be able to do it by extracting information from these data-bases, and introducing various measurement formulas. Such formulas are known as *bibliometrics*, or *metrics* for short. A simple example of a metric is a citation index. It works like this. The value of a paper is assessed by counting the number of times that paper is referred to by other papers in the field. Naturally a large number of different, and often much more complicated, metrics have been proposed for research assessment.[1]

For an excellent recent survey and critical account of these, see Baccini, 2010.

The UK's original Research Assessment Exercise turned out to be very expensive to conduct. Papers and books had to be collected from departments all over the country, and then read and assessed by panels of experts. This involved the employment of many extra administrative staff, while the time of academic staff was diverted away from the productive activities of teaching and research to the non-productive activities of preparing for the RAE, or carrying out the research assessment. The UK government saw the metrics approach as a way to avoid all this expense. The original idea of the REF, which was designed to replace the RAE, was that it should be based entirely on metrics. Thus the assessment could be carried out automatically by computers and would involve little cost. Unfortunately for the UK government, it turned out that an assessment based entirely on metrics is not feasible, and the present plans for the REF involve a curious mixture of peer review and metrics. The Italian Research Assessment System will differ from both the RAE and REF, and will, no doubt, have special Italian features.

I have already developed a series of criticisms of Research Assessment Systems in 3 papers (Gillies, 2006; 2007; 2009), and a book (Gillies, 2008) whose appearance was designed to coincide with the publication of the results of RAE 2008. Two of these works (2008 and 2009) sketch an alternative approach to research organisation which does not involve the use of Research Assessment Systems. The main thesis of these works is that Research Assessment Systems have the effect of lowering the quality of the research produced rather than increasing it. To illustrate this thesis, I give a variety of examples of research from different fields. There are examples from mathematics, the natural sciences, and medicine, and I also include some examples from research in the humanities – particularly philosophy. However, I do not discuss in detail any examples from economics. In this paper I hope to redress the balance by focussing on the effect of Research Assessment Systems on Economics. The thesis will be that Research Assessment Systems have an

even more negative effect on research in Economics than they do on research in the Natural Sciences. This is owing to the special features which distinguish Economics from the Natural Sciences, and which are described in Section **3** of the paper. These are that Economics is divided into a number of different schools and these schools are associated with political ideologies. I will try to show in section **4** that the negative effects of Research Assessment Systems on Economics do not depend on the precise details of the Research Assessment System employed. They occur whether the assessment is carried out using peer review or using metrics or using some combination of the two. My claims then apply to Research Assessment Systems in general, but still it will be useful to begin with a specific example. In the next section, I will give the results for Economics of the UK Research Assessment Exercise carried out in 2008 (RAE 2008). This will bring to light a curious anomaly in the RAE results for Economics, which I will refer to as the Economics Anomaly in Research Assessment (or EARA). Later in Section **4** I will try to explain how this anomaly came about.

2. The results of RAE 2008 for economics, given with some historical context

The results of RAE 2008 were published in the *Times Higher Education(THE)* on 18 December 2008.[2] Before I give the results for Economics, however, it will be useful to provide some historical context by describing some relevant events which occurred in the preceding three months of September, October and November.

On 15 September 2008, 3 months and 3 days before the results of RAE 2008 were published, Lehman Brothers filed for bankruptcy. This was the largest bankruptcy in the history of the USA with Lehman holding over $600 billion in assets. This major event soon led to a global financial crash, the biggest since the Wall Street crash of 1929, and this in turn brought about a recession in most countries. Of course the consequences of these dramatic events are not yet played out, and it looks as if, in the West at least, the next decade is going to be a period of economic difficulty similar to the 1930s.

On 5 November 2008, the Queen and the Duke of Edinburgh visited the London School of Economics (LSE) to open the New Academic Building for economics. At the opening ceremony, the Queen asked what has come to be known as 'The Queen's Question'. The general sense of what the Queen said is clear enough, but different sources report her words somewhat differently. According to the Daily Telegraph, 2008, after hearing about the Global Financial Crash, she asked: "Why did nobody notice it?", and went on to describe the turbulence of the markets as "awful". According to LSE Website, 2009, what she said was: "If these things were so large, how come everybody missed them?" Now strictly speaking the presuppositions of the Queen's Question, in either version, are not correct. As we shall see later, some people did notice that a Global Financial Crisis was in the offing. However, the Queen was certainly right that the majority of economists had failed to foresee the coming financial crisis. If it is not *lèse-majesté*, I propose to modify the Queen's Question to the following: "Why did the majority of economists fail to foresee the Global Financial Crash of 2008?" For the rest of the paper, I will consider the Queen's question in this form, and I will also try to answer it later on. The presuppositions of this modified form of the question are correct. Indeed many leading

[2] An article by Zoë Corbyn about my book criticizing the RAE was also published in the same issue of the *THE*. See Corbyn, 2008.

economists had denied that there was any risk of a serious financial crisis developing. I will here give two examples by way of illustration.

The first is Robert E. Lucas, who is a leading member of the Chicago School of Economists. He won the Bank of Sweden Prize for Economics[3] in 1995, and, according to Wikipedia, "is consistently indexed among the top 10 economists in the Research Papers in Economics rankings." He is famous for criticizing Keynesian economics, and developing the alternative 'Rational Expectations' approach. On 19 September 2007, be wrote a letter to the Wall Street Journal, in which he made the following statement (see Lucas, 2007):

> " … I am skeptical about the argument that the subprime mortgage problem will contaminate the whole mortgage market, that housing construction will come to a halt, and that the economy will slip into a recession. Every step in this chain is questionable and none has been quantified. If we have learned anything from the past 20 years it is that there is a lot of stability built into the real economy."

There can be little doubt that Robert E. Lucas failed to foresee the financial crash of 2008.

Robert E. Lucas is in the heart of the American economics establishment, and for my second example I have chosen a figure who has a similar position in the British economics establishment. This is Richard Portes, Professor of Economics at the London Business School since 1995. He has been (1992-2008) the longest serving Secretary-General of the Royal Economic Society since John Maynard Keynes, and was decorated as Commander of the British Empire (CBE) in the Queen's New Year Honours List 2003. In 2006, there was some turmoil in financial markets, and, as a result, doubts began to arise about the soundness of the Icelandic banks. They were given lower ratings than some of their Nordic peers, and a higher risk premium was place on their debt. Richard Portes was asked to investigate the situation of the Icelandic financial system, which he did with an Icelandic economist Fridik Mar Baldursson. Their report entitled: 'The Internationalisation of Iceland's Financial Sector' was published in November 2007. Section 4.1 of the concluding chapter of this report is entitled: 'The banks: successful and resilient'. The authors write (2007, 63):

> "The internationalisation of the Icelandic financial sector proceeded from market liberalisation, …, and privatisation, on the base of … an exceptionally healthy institutional framework. The banks have been highly entrepreneurial without taking unsupportable risks; good supervision and regulation have contributed to that, … Icelandic banks come out well in a comparison with Nordic peers – and their overall and core profitability is higher. … They are well hedged against volatility in the krona. Stress tests by the FSA indicate that the banks can withstand quite extreme movements in market variables specific to Iceland. The banks have negligible exposure to the US subprime market, structured finance products, and related financial

[3] The full title of this prize is 'The Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel'. This is usually abbreviated to 'The Nobel Prize for Economics', but this abbreviation has been recently questioned by the Nobel family (see the statement on their behalf made by Peter Nobel on 11 October 2010). Peter Nobel points out that, in his original will, Alfred Nobel did not specify that there should be a prize in economics, and that the economics prize was funded at a much later date by the Bank of Sweden. He goes on to say (Nobel, 2010): "What has happened is an unparalleled example of successful trademark infringement.", and adds: "I can imagine Alfred Nobel's sarcastic comments if he were able to hear about these prize winners. Above all else, he wanted his prizes to go to those who have been most beneficial to humankind, all of humankind!" The objections of the Nobel family seem fair to me, and I will therefore use the abbreviation: "The Bank of Sweden Prize for Economics".

> vehicles that have hit many financial institutions hard recently. Most fundamental, the banks exploit strong competitive advantage, arising from their entrepreneurial management, flat management structures, and unusual business models."

But what about the doubts regarding the solvency of the Icelandic banks expressed by some rating agencies? Portes and Baldursson state very clearly that these doubts are unjustified (2007, 63):

> "Yet in spite of their strong performance, Icelandic Banks have lower ratings than their Nordic peers, and a much higher risk premium is being placed on their debt during the present turmoil. *We see no justification for this in their risk exposure*." (My italics – D.G.)

Portes and Baldursson conclude their 'executive summary' as follows (2007, 4):

> "Overall, the internationalisation of the Icelandic financial sector is a remarkable success story that the markets should better acknowledge."

Less than a year after the publication of the report of Portes and Baldursson, all the major Icelandic banks collapsed. They were among the first casualties of the Global Financial Crash initiated by the bankruptcy of Lehman Brothers on 15 September 2008. Glitnir, Landisbanki and Kaupthing were placed into receivership in late September and early October 2008. Before the crash, the Icelandic banks held external debt of about €40 billion, which can be compared to Iceland's 2007 GDP of about €8.5 billion. In particular about £2.3 billion was owed to British individuals and institutions who had placed their money in Icelandic banks. This figure includes about £1 billion deposited by British local councils, and about £120 million by charities.

It is clear enough that Richard Portes did not foresee what was about to happen. The dramatic events of the Global Financial Crisis and the Queen's Question form the historical backdrop to the publication of the results of RAE 2008 on 18 December 2008. Let us next examine how economics fared in this Research Assessment Exercise.

The principal aim of the RAE was, for each subject, to grade the various departments in that subject throughout the UK. However, by combining the results in a different way, it was also possible to grade the various research subjects. The scale used ran from 4 (the highest possible grade) to 0. In this way an order of the various subjects such as Physics, Chemistry, Philosophy, History, and so on was produced in which the subject in which UK research was better, according to the valuations of the RAE, appeared higher. Where did economics appear in this order? The answer is given by the THE as follows (RAE 2008: The Results, 27):

> "Of all the subjects assessed, it is 'economics and econometrics' that shines as the UK's top scorer, nationally averaging more than three (on a scale of 0 to 4)."

In fact 'economics and econometrics' (which I will refer to henceforth simply as 'economics') scored 3.01. It was the only subject in RAE 2008 which scored above 3. For comparison, the next two subjects in the research ranking were Cancer Studies which scored 2.88, and Chemical Engineering which scored 2.86. These are averages over all the departments in the country which research into the subject in question. However, it is also interesting to examine the rating of the top institution in each of the subjects. For economics, the top institution was LSE, which scored an amazing 3.55. For Cancer Studies, it was Manchester which scored a much lower 3.20, while for Chemical Engineering the best institution was Cambridge which scored 3.15.

This result is very surprising indeed. On 5 November, the Queen must have embarrassed the economists at LSE by asking her Question, which presupposed that all of them had failed to foresee

the biggest Global Financial Crisis since 1929. The Queen's Question clearly implied that something had gone seriously wrong with research in economics. Yet in the results of the Research Assessment Exercise, published the next month, research in economics was declared to be, by some margin, the best research in any subject in the UK. Moreover research in economics at LSE was given a grade of 3.55 out of 4. Who was right in their judgement? The Queen or the RAE committee. It seems clear to me that the Queen was correct, and the RAE gradings were very seriously mistaken.

Now although I was very sceptical about the value of the RAE gradings, and had even published a book against the RAE, I still found the extent of the error in the case of economics very surprising. Here was a subject in which recent events had shown that current research was very much mistaken, and yet the RAE claimed that research in this area was not only good, but the very best research in the whole of the UK. How could such an erroneous assessment have occurred? After all, the RAE committee was composed of experts in the field, and they no doubt performed their allotted task carefully and conscientiously. Yet an outsider, such as the Queen, who presumably does not know a great deal about economics, could tell them in a few minutes that their judgment was completely wrong. I will describe this situation as the *Economics Anomaly in Research Assessment* (or EARA). Anyone interested in Research Assessment should, in my view, try to explain why this Economics Anomaly (or EARA) came about.

This section has provided us with two related questions which are both in need of answers. The first of these is the Queen's Question. The second is the question of how the Economics Anomaly in Research Assessment (or EARA) came about. In order to answer these questions, however, it will be necessary to examine a little more closely the nature of economics as a subject, and, in particular, to show how it differs from a natural science such as physics. These matters will occupy us in the next section.

2. How economics differs from the natural sciences. The existence of competing schools of thought within eEconomics

Kuhn in his 1962 *The Structure of Scientific Revolutions* presents a view of the natural sciences which has become very well-known and quite widely accepted. According to Kuhn, mature natural sciences develop for the most part in a manner which he describes as 'normal science'. During a period of normal science, all the scientists working in the field accept the same framework of assumptions which Kuhn calls a 'paradigm'. However, these periods of normal science are, from time to time, interrupted by scientific revolutions in which the dominant paradigm of the field is overthrown and replaced by a new paradigm. This model is proposed by Kuhn for the natural sciences, and indeed Kuhn, who was originally trained as a physicist, worked almost exclusively on the history and philosophy of the natural sciences. Still, in the Preface of his 1962, he makes some remarks about the social sciences, which, despite their brevity, contain, in my view, a very important insight.

Kuhn writes as follows (1962, ix-x):

> "The final stage in the development of this monograph began with an invitation to spend the year 1958-59 at the Center for Advanced Studies in the Behavioral Sciences. … spending the year in a community composed predominantly of social scientists confronted me with unanticipated problems about the differences between such communities and those of the natural scientists among whom I had been trained.

> Particularly, I was struck by the number and extent of the overt disagreements between social scientists about the nature of the legitimate scientific problems and methods. … Attempting to discover the source of that difference led me to recognize the role in scientific research of what I have since called 'paradigms'."

So Kuhn developed the theory that natural scientists normally agree on the same paradigm by observing that this was not the case for social scientists. At first Kuhn seems to have thought that the term 'paradigm' should be restricted to fields where there was a unique paradigm, so that the social sciences could be characterised as having a number of competing schools of thought but no shared paradigm. Later, however, he reached the conclusion that each of these competing schools had its own paradigm. As he says (1962, xi):

> "Each of the schools … is guided by something like a paradigm."

I will adopt Kuhn's second position here, so that the difference between the natural sciences and the social sciences can be put as follows. In the natural sciences, outside revolutionary periods, all the scientists accept the same paradigm. In the social sciences, however, social scientists are divided into competing schools. Each school has its own paradigm, but these paradigms are often very different from each other. The contrast is between a single paradigm and a multi-paradigm situation.

As far as the natural sciences are concerned, we can illustrate this with the example of theoretical physics. In this field, all scientists accept a paradigm whose core consists of relativity theory and quantum mechanics. It is not that contemporary theoretical physicists are excessively dogmatic. Most of them probably think that, at some time in the future, there will be another revolution in theoretical physics which will replace relativity and quantum mechanics by some new, and perhaps even stranger, theories. However, they would argue, relativity and quantum mechanics work very well, and so it is sensible to accept them for the time being. If we turn now to economics we find a very different situation.

Economics is a social science, and Kuhn's analysis proves to be quite correct. The economics community is divided into different schools. The members of each of these schools may indeed share a paradigm, but the paradigm of one school can be very different from that of another. Moreover the members of one school are often extremely critical of the views of members of another school.

The school of economics which has the most adherents at present is neoclassical economics. The majority of economists are neoclassical, and this approach can justly be referred to as the mainstream. Indeed Weintraub in his 1992 says, 1:

> "When it comes to broad economic theory, most economists agree. … 'We're all neoclassicals now, even the Keynesians,' because what is taught to students, what is mainstream economics today, is neoclassical economics."

There is some truth in what Weintraub says here, and yet he also exaggerates in some respects. While most economists are indeed neoclassicals, there is a small, but very vocal, minority who reject the neoclassical approach completely. They are known as heterodox economists. Weintraub is also correct to say that some Keynesians do accept neoclassical economics. Versions of Keynes' original theory have been produced which fit in with the neoclassical framework. This is known as the neoclassical synthesis. However, Keynes himself did not accept neoclassical economics, and many Keynesians both in the past and today have been sharply critical of neoclassical economics.

Of course Weintraub is aware of this, and he goes on to say (1992, 2-3):

> "Some have argued that there are several schools of thought in present-day economics. They identify (neo)-Marxian economics, neo-Austrian economics, post-Keynesian economics, or (neo)-institutional economics as alternative metatheoretical frameworks for constructing economic theories. To be sure, societies and journals promulgate the ideas associated with these perspectives. … But to the extent these schools reject the core building blocks of neoclassical economics … they are regarded by mainstream neoclassical economists as defenders of lost causes or as kooks, misguided critics, and antiscientific oddballs. The status of non-neoclassical economists in the economics departments in English-speaking universities is similar to that of flat-earthers in geography departments: it is safer to voice such opinions after one has tenure, if at all."

One can certainly agree with Weintraub that it is difficult for heterodox economists to obtain permanent posts in universities, and that, even if they do obtain such a post, they may well be treated badly by their neoclassical colleagues. However, despite these handicaps, there still remain a significant number of heterodox economists who are active in the academic world. They are divided into a number of schools. Leaving out some of the 'neos', Weintraub mentions: Marxist, Austrian, Post-Keynesian, and Institutionalist economists, and one could add some more. There are Sraffian, or neo-Ricardian economists, who are followers of the Italian economist Sraffa who worked at Cambridge with Keynes, but developed his own system. There are also evolutionary economists and economists who use complexity theory.

Weintraub states correctly that neoclassical economists have a low opinion of heterodox economists, but equally most heterodox economists have a low opinion of neoclassical economics. Every few years a book appears by one or more heterodox economists denouncing neoclassical economics as intellectual rubbish. A well-known example of this genre is Keen 2001 *Debunking Economics*. *The Naked Emperor of the Social Science*. Steve Keen is a Sraffian economist. The economics which he debunks is neoclassical economics. According to him it is like the naked emperor of Hans Christian Andersen's fairy tale. Another more recent example is Edward Fullbrook (ed.) 2004 *A Guide to What's Wrong with Economics*. This is a collection of papers by contributors most of whom criticize neoclassical economics very sharply. The general scene in economics then, with its different schools which criticize each other harshly, is very different from that in theoretical physics. There just is no group of heterodox physicists who spend their time denouncing relativity theory and quantum mechanics as valueless theories.

Indeed what is striking is the harshness of the comments which the members of one school of thought in economics make about the other schools of thought. We remarked earlier that a synthesis has been formed between Keynesianism and Neoclassical Economics. Yet some neoclassical economists regard even this very moderate variety of Keynesianism as worthless. This attitude was held, for example, by Robert E. Lucas whom I quoted in section 2. According to Krugman 2009, Robert E. Lucas of the University of Chicago declared in 1980: "At research seminars, people don't take Keynesian theorising seriously any more; the audience starts to whisper and giggle to one another." Another nice example of this phenomenon is provided by the Japanese economist Morishima who wrote in his 1973, 1:

> "In Japan, for example, Marxian economists have formed an association called Keizai Riron Gakkai (Economic Theoretical Association) in opposition to the Riron Keizai

Gakkai (Theoretical Economic Association) of non-Marxists. In spite of the similarity of the names of the societies, no fruitful conversation has ever been held between them. They are at daggers drawn and describe each other as a society for reactionaries and a society for economists with lower I.Q.s."

Now it is a general tendency among academic researchers to have a high opinion of their own approach to a subject, and a much lower opinion of alternative approaches. However, this tendency is much more pronounced in economics than it is elsewhere. To explain why this is so, we must introduce a new consideration.

Kuhn claimed correctly that economics is divided into competing schools, each with its own paradigm. However, we can add another point. These different schools are associated with different political ideologies. To examine this question, let us confine ourselves for simplicity to the following schools: Neoclassical Economics, the various versions of Keynesianism, and Marxist Economics. It is easy to see that these schools are arranged on a political spectrum running from the right to the left. There are in fact two compelling arguments for this conclusion.

The first of these arguments looks at the main features of the paradigms adopted by the various schools. Let us begin with Marxist Economics. Perhaps the central claim of the Marxist paradigm is that the profits made by capitalists arise out of the exploitation of workers. The political message of this point of view is clear enough. No one wants to be exploited. So, if the Marxist claim is correct, then workers should seek to abolish capitalism and replace it by socialism. This is just what the Marxists advocate. Here the connection between economic theory and political ideology is quite transparent and is acknowledged by both Marxists and non-Marxists. Let us next turn to Neoclassical Economics. The core of the neo-classical paradigm is equilibrium theory. In a neoclassical equilibrium model, we have firms which arrange their production in order to maximize their profits, given the existing technology; and households which arrange their consumption in order to maximize their utility, given their income. It is then shown that, if there is a market with free competition, this behaviour leads to an equilibrium which is Pareto-optimal. Pareto-optimality means that no redistribution of goods or productive resources can improve the position of one individual without making at least one other individual worse off. The political implications of this are again clear. A market with free competition delivers the best (the Pareto-optimal) results for everyone. Hence politicians, in the interest of society as a whole, should to use the phrase of Portes and Baldusson (2007, 63) introduce "market liberalisation … and privatisation". Indeed this is just what politicians the world over have been doing in recent decades, justifying their actions by an appeal to neoclassical economics.[4] Keynesians hold a position intermediate between Marxist and neoclassical economists. Keynesians would not agree that markets with free competition always deliver the best results for society. Keynes himself reached this conclusion during the 1930s when the free market seemed to have delivered the Great Depression with massive unemployment and under-utilisation of capacity. In his economic theory Keynes showed how markets could lead to this sub-optimality situation, and also how government intervention could correct the flaws of the market. However, Keynes did not go as far as the Marxists in advocating the complete abolition of capitalism and the market. He argued for a limited degree of government intervention which would correct the defects of the market while leaving a great deal of economic activity to the market. Naturally this kind of compromise position can occur in

[4] This is of course only a very concise sketch of the ideological implications of neoclassical economics. A much more detailed account is to be found in Keen (2001, 163). Here Keen shows why neoclassical economics leads to opposition to minimum wage legislation and social security, and support for anti-union laws.

different forms depending on how much government intervention is seen as necessary. The right wing of the Keynesians, those who support the Neoclassical Synthesis, advocate rather little government intervention; while the more left-wing Keynesians, the so-called Post-Keynesians, support more radical government interventions in the economy. Indeed the left wing of the Post-Keynesians overlaps to some extent with the Marxist school. We see from all this the very close links between economic theory and political ideology. These links are further confirmed by our second argument.

This second argument points to the close correlation between the political regime in power and the type of economics taught in the country. This was obvious during the Cold War era when economists in the Soviet Union were all, except for a few dissidents, members of the Marxist school, while economists in the U.S.A. were all, except for a perhaps somewhat larger number of dissidents, members of the Neoclassical school. The contrast between economics and physics is here very striking, since physicists in both the Soviet Union and the U.S.A. adopted exactly the same theories of physics and used those theories in the construction of nuclear weapons. Another striking example is provided by the situation in Britain since the Second World War. Politically speaking we can distinguish two different periods (1945-75) and (1980-2010) with a brief interregnum in 1975-80. In both periods the Conservative party was more to the right of the Labour party. However in the late 1970s, there was a general shift to the right which affected both parties. In 1945-75, the political debate was Tory 'wets' versus old Labour. In 1980-2010, the debate was between Tory Thatcherites, and new Labour. New Labour was to the right of old Labour, and Thatcherites were to the right of Tory 'wets'. This significant shift to the right in the political spectrum was accompanied by a change in the character of the economics taught in British universities. Keynesianism had been very important in British universities in the period 1945-75, but in 1980-2010 the neoclassical economists ousted the Keynesians to a large extent.

The connection between the various schools of economics and political ideologies explains the harsh judgements which economists of one school make of economists of other schools. These are quite similar to the harsh judgements which politicians of one party make about politicians of another party. There is a striking contrast here with subjects, such as physics, which are, at the moment at least, relatively ideology free. In physics, the various schools of thought do not attack each other with the bitterness to be found in economics. Now the bitterness of the feuds between economists is very relevant to the question of the assessment of research in economics. We will show what effect it has in the next section (section 4), but before turning to this, we will close the present section by trying to answer the Queen's question, since the discussion of this section puts us in a position to do so.

We earlier stated our modified version of the Queen's question as follows: "Why did the majority of economists fail to foresee the Global Financial Crash of 2008?" We can now answer this question quite simply as follows. The majority of economists were members of the Neoclassical School. They therefore accepted the core of the neoclassical paradigm, namely equilibrium theory. According to equilibrium theory, a market governed by free competition moves into a Pareto-optimal equilibrium. Now the financial markets throughout the world had in the decades before 2008 been de-regulated and so made to approximate to a freely competitive market. Hence neoclassical economists deduced from their economic paradigm that these financial markets would move towards equilibrium rather than crashing in a catastrophic fashion.

What we see here is a feature of paradigms which applies just as much in the natural sciences as it does in economics. The original empiricists thought that reality could be observed without any theoretical presuppositions, but this is not the case. We always see reality in terms of a framework

provided by a paradigm. Often the paradigm acts like a magnifying glass and brings to light features of reality which we would not otherwise have noticed. However, sometimes the paradigm acts like a set of blinkers, and makes us overlook features of reality which contradict the assertions of the paradigm.

Kuhn gives a wonderful example of a paradigm in astronomy blinding scientists to phenomena which they might otherwise have observed. According to the Aristotelian-Ptolemaic paradigm, which dominated astronomy from Ancient Greek times up to the beginning of the Copernican revolution in the 16th century, the heavens were perfect and unchanging. Heavenly bodies such as stars and planets did not change in any way, but continued to carry out their uniform circular motions through the centuries in exactly the same fashion. During this period, European astronomers did not notice any of the changes, such as exploding stars, which in fact did occur in the heavens. However, changes of this sort were noticed and recorded by Chinese astronomers who were not working within the Aristotelian-Ptolemaic paradigm. As Kuhn puts it (1962, 115):

> "Can it conceivably be an accident, for example, that Western astronomers first saw change in the previously immutable heavens during the half-century after Copernicus' new paradigm was first proposed? The Chinese, whose cosmological beliefs did not preclude celestial change, had recorded the appearance of many new stars in the heavens at a much earlier date. Also, even without the aid of a telescope, the Chinese had systematically recorded the appearance of sunspots centuries before these were seen by Galileo and his contemporaries."

The situation was just the same for neoclassical economists. They failed to observe very dangerous imbalances building up in financial markets because their paradigm told them that these would just be fluctuations which would disappear as market forces brought the system back to equilibrium. This then is my proposed answer to the Queen's question. The majority of economist failed to notice the coming financial crash because they were looking at economic reality through the glasses provided by the neoclassical paradigm. I will support this answer later on by showing that those economists who did correctly foresee the financial crash were members of economic schools different from the neo-classical school, and so were looking at economic reality through the glasses provided by paradigms which differed from the neoclassical one.

4. The effect of research assessment systems on economics

Our examination of the community of researchers in economics has led us to the following picture. This community is divided into a number of different schools of thought A, B, C, …, each with its own paradigm. The members of each school have a very low opinion of the research work produced by the other schools. Now if a Research Assessment System (or RAS) is applied to such a community, what result will it give? My claim is that the research work of the members of whichever school has the largest number of members will receive the highest valuation. So if school A is in the majority, the members of school A will receive the highest valuation. If school B is in the majority, then the members of school B will receive the highest valuation, and so on. The valuation received by the members of a particular school X will be roughly proportional to the number of members of that school.

To argue for this claim, let us first suppose that the RAS is conducted (like the UK RAE) entirely by peer review. I am supposing throughout that the RAS is conducted fairly, so that the

committee which reviews research has representatives from the various schools in exactly the same proportion in which members of these schools occur in the research community as a whole. Now let us suppose that a piece of research work by a member of school A is judged by the committee. Those members of school A on the committee are likely on average to take a favourable view of the piece, while those members of the other schools will take an unfavourable view. I am assuming, of course, that the members of the committee are completely honest in their judgements, and also the rule, for which I have provided very strong evidence in the preceding section, that members of each school in economics have a very low opinion of the research work of other schools. On average then, a piece of research work from school X, will receive favourable judgements from members of school X, and unfavourable judgements from members of the other school. It follows immediately that the valuation received by members of a particular school X will be roughly proportional to the numbers of members of that school.[5] Members of minority schools with few members will on average receive low ratings, while members of whichever school is in the majority will receive on average the highest ratings.

Let us next suppose that the RAS is conducted using bibliometrics instead of peer review. The simplest bibliometric is the citation index in which the value of a piece of research is evaluated by counting the number of references made to it in other published pieces of research. Now the members of each school of economics refer, most frequently, to the papers and books of members of the same school. It follows that if a school has a large number of members, the chance of a piece of research work by a member of that school getting a large number of references is much higher than the chance of a piece of research work by a member of a school which has few members. So, once again, using citation indices, the average evaluation receive by the members of a particular school X is going to be roughly proportional to the number of members of that school. In particular, the members of whichever school is in the majority will, on average, receive the highest valuations.[6] Now, of course, there are a whole variety of different metrics, and a RAS can combine the use of metrics with peer review in complicated ways. However, it is easy to check that, whatever the complicated formula employed, the result will turn out to be the same.

These then are my theoretical reasons for the claim that the valuations of the members of a particular school of economics produced by a RAS will be roughly proportional to the number of members of that school. However, it is always as well to check theoretical arguments against

[5] David Corfield pointed out to me that in many evaluation procedures the discrimination in favour of the majority school is more than proportional to the number of members of that school. He wrote (private communication): "Take a very simple model for publishing in a top journal where there are two schools A and B, and 2/3 of researchers belong to A, and 1/3 to B. If to be accepted you need two yes votes, and let's imagine … that reviewers from one school accept papers if and only if they're from their school, then the acceptance rates will be in the ratio 4:1 not 2:1, assuming referees are assigned at random from the population." In what follows, I will continue to assume the 'rough proportionality' model, but, in view of Corfield's point, it should be remembered that this underestimates the discrimination in favour of the majority school and against minority schools.

[6] This was queried by Alberto Baccini (private communication). Baccini agreed that all the citation indices currently in use do increase in value with the number of citations obtained by a paper. This would seem to favour the majority school. However, against this he points out that members of a minority school could cite each other's work more than frequently than members of the majority school, which would act as a countervailing tendency. I am not sure that members of minority schools usually cite each other more frequently than members of the majority school, and, even if they do in some cases, this is unlikely to have a significant effect on the general tendency for members of the majority school to score more highly on citation indices than members of a minority school. In an independent comment (private communication), Frederic Lee argued in the opposite direction. His point was that heterodox economists frequently cite mainstream economists (in order to criticize them), whereas mainstream economists ignore heterodox economists and their criticisms, not citing them at all. Therefore "citations to mainstream economists will be more than proportional to their number", and citation indices correspondingly biased.

empirical data about what happens in practice. Here there is some good information about what happened in a research assessment of economics conducted in Italy in 2005. This assessment was carried out as a trial run for the introduction of a full research assessment system for Italy. Such a system is indeed now (2010-11) being introduced by the Berlusconi government. The 2005 assessment was carried out by the CIVR, or Comitato di Indirizzo per la Valutazione della Ricerca (Committee for the Purpose of Assessing Research). I will refer to the results of this assessment as regards economics as CIVR, 2005. The committee were able to agree in a valuation for two thirds of the research items they considered; but, for the remaining third, a consensus valuation proved to be impossible. In particular one member of the committee (Luigi Pasinetti) strongly criticized some of the valuations of the other members of the committee). The report on the deliberations of the committee was written by its coordinator (Guido Tabellini), but Pasinetti wrote an appendix (Number 4) in which he expressed his points of dissent. Tabellini then replied to this in Appendix 5.

Now Pasinetti is well-known as a leading critic of mainstream neoclassical economics. He has based his own approach to economics on a development of the tradition of Keynes and Sraffa. We would expect on our analysis that Pasinetti's judgments would differ sharply from those of mainstream economists and so it proved to be. Pasinetti and his supporters on the committee objected, according to the report (CIVR, 2005, 9) that

> "The valuations given have results on average better … for items with a high mathematical content and of mainstream theory, than for those of the school of economic thought, economic history, institutional economics, and heterodox theory."

This is exactly in accordance with our claim.

So far I have considered the effect of a RAS in a single evaluation. I will now turn to the question of the long term effect of a RAS, if such a system is in operation for many years. This was the case with the UK RAE which ran for twenty two years from 1986 to 2008. To discuss this issue it is useful to distinguish between *monistic* and *pluralistic* research communities in economics. Of course, as we have argued, research communities in economics are always to some extent pluralistic, since it is never the case that one paradigm becomes acceptable to all researchers. However, there are undoubtedly different degrees of pluralism. The highest degree of pluralism occurs when researchers of each of the various schools are treated on a par. Members of every school are to be found in the best institutions, on important governmental advisory and decision-making bodies, and so on. The opposite situation, which I will refer to as monism, occurs when members of the majority school hold virtually all the places in the best institutions and have virtually all the good research grants; when governmental advisory and decision-making bodies are composed almost exclusively of members of the majority school; and when the very few remaining members of minority schools of thought lead a beleaguered existence in low grade institutions with poor research facilities. My thesis is that if a RAS operates for a long time, it will push the community of researchers in economics away from pluralism and towards monism.

To see why this is so, let us consider what happens once an evaluation has been carried out. Those groups which have low evaluations have some or all of their research funds removed, while those with high evaluations keep their research funds or have them increased. As far as economics is concerned, this means that members of the majority school have their research funding increased, while the members of minority schools will on average have their research funding reduced. In the specific case of the UK RAE, those departments which had a large proportion of members of minority

schools would receive low ratings and so have their research funding cut. This would have the effect of reducing the research time of the members of those departments and forcing them to do more teaching and administration. Meanwhile their rivals in the majority school would have better conditions and more time for research. However, this is the effect of just one evaluation. Once such evaluations become regular, every department will seek to gain a high evaluation and so more research funds. The best strategy to achieve this is, whenever a post becomes available, to appoint someone who belongs to the majority school. Thus the numbers in the majority will tend to increase while those in the minority schools will tend to diminish, leading in turn to even worse evaluations of their work in the RAS. This trend is likely to be further reinforced by the attitudes of graduate students hoping to get a post as a researcher in economics. The best strategy for them to achieve their goal is obviously to join the majority school. The better institutions have a wider choice of candidates for their posts, and so the percentage of members of the majority school is likely to be highest in those institutions, while, conversely, the few remaining members of minority schools will be forced into accepting posts in low grade institutions where the time and facilities available for research are very limited. Even if a member of a minority school does retain a post at a high grade institution, he or she is less likely to get promoted, since he or she will get a lower rating for his or her research than a rival from the majority school. Note that all these effects will occur whichever school is in the majority at the time that the RAS is introduced. If, for example, the Post-Keynesians had been the majority school in economics in the UK when the RAE was introduced in 1986 (which was clearly not the case), the workings of the RAE would have made them the overwhelming majority. We are facing here something which is a feature of many developmental or evolutionary processes. What starts as a small initial advantage is amplified by the working of the process until it becomes an enormous advantage.

This then is the proposed model of the long term effects of a RAS on a community of researchers in economics. If it is correct, it would lead us to expect that in the period from 1986 to 2008 when the RAE was operating in the UK, the dominance of the majority school in economics (i.e. neoclassical economics) would become stronger and stronger in the UK. This conclusion is supported by the empirical findings presented by Frederic S. Lee in his excellent 2007 paper. Lee begins his paper as follows (2007, 309):

> "Previous research (… Lee and Harley, 1998) on the impact of the Research Assessment Exercise (RAE) on heterodox economics and heterodox economists in the UK arrived at a discouraging set of conclusions and predictions. … we concluded that the RAE would continue to drive economic departments to discriminate positively in terms of their hiring, promotion and research strategies in favour of mainstream economists, and their research in order to maintain or improve their ranking (and hence their research funding). As a consequence, we predicted there would, in time, be no or only a token presence of heterodox economists in an increasing number of departments. And, in turn, the near absence of heterodox economists in many economic departments would result in undergraduate, post-graduate and research students only being taught mainstream economics and writing neoclassical doctoral dissertations."

Lee's 2007 paper, written about a decade after his original predictions, shows them to be amply confirmed.

In his 2007, Lee analyses the results of the 2001 Research Assessment Exercise, and comes up with an interesting result which is shown in our Table 1, which is an extract from Lee's Table 1 given on p. 312 of his 2007.

Table 1

Departmental Grades	5*	5	4	3
Percentage of Diamond List publications in total RAE publications	42.57	29.18	23.18	11.81
Diamond List publications per active research staff	1.67	1.16	0.87	0.45

The RAE 2001was supposed to be conducted by Peer Review, that is to say the committee were supposed to read all the items submitted and grade them. However, Lee shows that the grades given to the departments could have been obtained by a very simple metric, namely calculating the percentage of the total RAE publications which appeared in a particular set of journals, known as the 'Diamond List'. This is seen clearly in Table 1 above. The first line gives the departmental grades which in RAE 2001 were given on the scale 5*, 5, 4, 3. It will be seen that this grading exactly corresponds to the grading obtained in the second line by calculating the percentage of diamond list publications in total RAE publications of the department. The third line gives an alternative, but equivalent, metric, which consists of calculating the number of diamond list publications per active research staff.

Now what is this 'Diamond List' of economics journals. It consists of 27 journals which are given in Lee, 2007, 311, Ftnte 1. As we would expect, they are journals which publish almost exclusively mainstream economics.

Now what of heterodox economists? Obviously they do not publish in diamond list journals which would not accept their papers. However, they do publish their research in the form of books, or in collections and journals which specialise in heterodox economics. Lee in his 2007 examines how this research was judged in RAE 2001 by introducing a category of H-HET-M publications. H-HET-M stands for 'Heterodox, History of Economic Thought, and Methodology'. Lee gives his results in his Table 3 on p. 313, an extract from which constitutes our Table 2 below.

Table 2

Departmental Grades	5*	5	4	3
Percentage of H-HET-M publications in total RAE publications	1.79	2.71	4.75	6.83
H-HET-M publications per active research staff	0.07	0.11	0.17	0.26

Once again a simple metric would produce the departmental grades of RAE 2001. This time the metric consists of calculating the percentage of H-HET-M publications in total RAE publications, but, in contrast to the previous case, the inverse of the metric has to be used for the grading, that is to say the higher this metric, the lower the grade given to the department.

Tables 1 and 2 provide, so I would claim, very strong empirical support for the model I presented earlier. According to this model, in a RAS the members of the school which constitutes the majority will receive on average the highest grades. This is clearly the case for RAE 2001. On average researchers who publish work in the majority school (neoclassical economics) received much higher grades than those who published work in a minority school. It is also striking that the grading in RAE 2001 was in accordance with the principle to which Pasinetti objected in his dissident appendix 4 to CIVR, 2005. He there says (CIVR, 2005, Appendix 4, 6):

> "Cases like the following was the first cause of my denying consensus (without any effect, being always in the minority). Quality of the product: 'This paper is published in a top field journal, the IF (Impact Factor – D.G.) of the journal is high, hence the paper is excellent.' Or conversely (always on the quality of the paper) 'this paper is published in my opinion in a non serious journal [in the specific case of the quotation it was the *Journal of Post Keynesian Economics*], hence the quality is 'limited.'"

This is exactly the principle which was operating in the RAE 2001. If a work was published in the 'Diamond List' of mainstream economic journals it received a high rating. If it was a H-HET-M publication, it received a low rating. Naturally Pasinetti objected to this kind of valuation, but his protests were "without any effect" since he was "always in the minority". This shows clearly the extent to which a RAS in economics consists in the oppression of minorities by the majority.

Given the results of RAE 2001 just analysed, it was clearly in the interest of any economics department in the UK at that time to appoint someone who published papers in the Diamond List, i.e. a mainstream neoclassical economist, and to avoid appointing anyone who produced H-HET-M publications, i.e. a heterodox economist of some kind. This indeed would have been the only sensible strategy for improving the RAE rating of the department and hence obtaining more research funding. One would therefore expect there to be very few heterodox economists in the UK in 2007, and the data given by Lee bears this out. Here is what he says (2007, 321):

> " … we find that over 60% of British economics departments and 68% of the ranked departments have none or only one heterodox economist on their staff; and in contrast,

> less than 16% of the departments and 12% of the ranked departments have a sustained presence of four or more heterodox economists. … we find that over 77% of the departments and 88% of the ranked departments include only mainstream economics in their course aims and objectives, and that 63% of economic students and 76% of students in ranked departments reside in departments with no or one heterodox economists and which include only mainstream economics in their course aims and objectives. Again in contrast, 5% of economics students and 3% of students in ranked departments reside in departments with four or more heterodox economists and which include both heterodox and mainstream economics in their aims and objectives. These starkly contrasting figures fully support the conclusions and predictions Harley and I made."

This passage shows how much the economics scene in the UK has changed from how it was in the 1960s and 1970s. At that time, the UK economics research community was very pluralistic. There were many neoclassical economists, but the UK was also famous for the Cambridge school of economists, most of whom were sharp critics of neoclassical economics, and supported radical versions of Keynesianism or the ideas of Sraffa. The position of most members of this school would now be classified as 'heterodox', and anyone supporting or trying to develop such a point of view would probably be having a beleaguered existence in some university low down in the academic hierarchy. *Sic transit gloria mundi.* What is surprising, in view of all this, is that groups of heterodox economists, which are small in size, but nonetheless vigorous and active, do continue to exist in the UK. This is a remarkable triumph of independent thought in unfavourable circumstances.

The next consequence which I want to draw from the model presented is the following. If a group of economists gets a very high score on a RAS, this must mean that it is almost entirely monistic in character. It is clear that this must be so, since the presence of any non-mainstream economists in the group will inevitably lower the overall score of the group. We saw earlier that the department of economics at the LSE obtained the remarkably high rating of 3.55 out of 4 in RAE 2008. If our theory is correct, this must mean that that in the period covered by the valuation, the LSE department of economics consisted almost entirely of mainstream neoclassical economists. This is an empirical consequence of the theory which could be checked, though I have not done so.[7]

I will now attempt to use our model to explain the Economics Anomaly in Research Assessment (or EARA). EARA it will be remembered arose because RAE 2008 gave economics the highest research rating of any subject in the UK. Indeed economics was the only subject in the UK to

[7] I do, however, have a piece of anecdotal evidence which supports this conclusion. For many years, I gave a Master's course on the *Foundations of Probability* at LSE. I was not actually a member of staff of LSE, but rather of King's College London. However, the course in question was part of a MSc in Philosophy and History of Science which was run jointly by LSE and King's College London. Now Keynes's first piece of research before he turned definitely to economics was in the philosophy of probability. In 1921 he published his *A Treatise on Probability*, which gives a classic account of the logical interpretation of probability. I naturally covered the logical interpretation and Keynes's *Treatise* in my course. At that time, there was a great deal of discussion of Keynes's views of probability by the Post-Keynesian school of economists. This school argued that the standard 'neoclassical synthesis' version of Keynesianism differed from Keynes's own views because it omitted any considerations of uncertainty and probability, whereas uncertainty and probability played an important role in Keynes's *The General Theory*. In fact the Post-Keynesian school thought that Keynes's early work on the foundations of probability had a profound influence on his later economics, though there were disagreements among its members as to the nature of this influence. I discussed some of the Post-Keynesian work on this topic and published a paper on this subject [Gillies (2003)]. I was once chatting with a well-known economist at LSE and mentioned to him that I discussed the Post-Keynesians' views in my course on *Foundations of Probability*. He looked at me with some surprise, and said that he had never heard of the Post-Keynesians.

score over 3 (out of a maximum of 4). However, at the time the results of RAE 2008 were announced, it had become obvious to any outsider (such as the Queen) that there was something seriously wrong with economics research in the UK. The preceding few months had witnessed the worst global financial crash since 1929, and yet the great majority of UK economists had altogether failed to foresee, or give any warning about the possibility of such a crash. The anomaly was that a carefully conducted valuation of research such as the RAE should give a result which any outsider could see was obviously wrong. The analysis of this section provides an explanation of EARA. The very high rating for the economics research community given by RAE 2008 actually meant that the economics community of the UK was very monistic. Because of the nature of economics, a highly monistic research community can go seriously wrong. Everyone is looking at economic reality through the spectacles of the same paradigm, and so everyone can miss some important developments which are concealed by the nature of the paradigm. In a more pluralistic community, there would be several representatives of other schools, operating with other paradigms. The dangerous developments might be rendered more visible by these other paradigms, and so the community would get some warning of approaching disaster, and so could take steps to avoid it. But in a monistic community, where a single school dominates, and the representatives of other schools are ignored, no warning is given, or, if it is given, no one pays any attention. In this section I have argued informally, but it is possible to produce a mathematical version of the model being used. This is done in the Appendix for those who like a mathematical approach.

5. Arguments for Pluralism

The first and most important argument for pluralism is that history shows that often minority schools give correct results while mainstream schools give wrong results. Hence banning or marginalising minority schools is never a good strategy for developing reliable knowledge. This historical phenomenon of the frequent success of minority schools applies just as much in mathematics and the natural sciences, as it does in economics. In my 2008 (33), I give a typical recent example, taken from research in the bio-medical sciences.

In 2008, Harald zur Hausen was awarded the Nobel prize for the discovery that a form of cervical cancer is caused by a preceding infection by the papilloma virus. In the research which led to the discovery, however, the majority of researchers favoured the view that the causal agent for cervical cancer was a herpes virus and not a papilloma virus. Zur Hausen and his group were the only ones who favoured the papilloma virus.

One of the reasons why the research community supported the idea that a herpes virus was the cause of cervical cancer was that it had been shown that a herpes virus, the Epstein-Barr virus, was the cause of another cancer: Burkitt's Lymphoma. The dominance of the herpes virus approach is shown by the fact that, in December 1972, there was an international conference of researchers in the area at Key Biscayne in Florida, which had the title: Herpesvirus and Cervical Cancer. Zur Hausen attended this conference and made some criticisms of the herpes virus approach. He said that he believed that the results indicate at least a basic difference in the reaction of herpes simplex virus type 2 with cervical cancer cells, as compared to another herpes virus, Epstein-Barr virus. In Burkitt's lymphomas and nasopharyngeal carcinomas, the tumor cells seem to be loaded with viral genomes, and obviously the complete viral genomes are present in those cells. Thus a basic difference seems to exist between these 2 systems. (cf. Goodheart, 1973, 1417). It is reported that

the audience listened to zur Hausen in stony silence (Mcintyre, 2005, 35). The summary of the conference written by George Klein (Klein, 1973) does not mention zur Hausen. At that time zur Hausen's position seemed eccentric and unjustified. Yet zur Hausen and his group continued their research on the papilloma virus, and, after more than twenty years, the research results did convince the majority that the papilloma virus was indeed the cause of many forms of cervical cancer. This has most important consequences since a simple vaccination against infection by the papilloma virus gives protection against cervical cancer.[8]

What happened in the case of research into the viral causation of cervical cancer, also happened in the case of economic research in the decade or so before the great financial crash of 2008. As we have seen, the economists in the mainstream neoclassical paradigm failed to foresee this crash, but, by contrast, several economists working in minority paradigms did successfully predict the crash. In 2010, The Real-World Economics Review decided to give a prize to be called the *Revere Award for Economics*.[9] It was named in honour of Paul Revere and his famous ride through the night to warn the Americans of the approaching British army. Participants in the Real-World Economics Blog were asked to vote for "the three economists who first **and** most clearly anticipated **and** gave public warning of the Global Financial Collapse **and** whose work is most likely to prevent another GFC in the future." 2,500 people voted and the winner by a considerable margin was Steve Keen. As we have seen, Steve Keen is a member of the minority Sraffian school of economics, and has written a book criticizing the mainstream neoclassical paradigm. Steve Keen shared the Revere Prize with Nouriel Roubini and Dean Baker, and there were several other finalists who were judged to have predicted the Gobal Financial Collapse. An analysis of these finalists shows that they were all, like Steve Keen, critics of neoclassical economics, and belonged to schools which opposed the mainstream. It would take too much time to analyse every economist who was a finalist for the Revere award, but I will here discuss one more example: George Soros, and will mention some recent work of another of the finalists (Ann Pettifor) later in the section.

George Soros has developed a boom/bust model of how financial markets work, which he has used in his own financial activities. This model is based on what he calls 'reflexivity'. Soros criticizes neoclassical economics for failing to recognise reflexivity, and for consequently producing a theoretical construction with little relevance to the real world. Whereas according to neoclassical economics, free markets have a built in tendency to move towards equilibrium, Soros denies that there is any such tendency, and even goes so far as to argue that markets tend towards excess and disequilibrium.

In 1998, Soros published a book entitled 'The Crisis of Global Capitalism' in which he applied his model to the financial sector as a whole. He reached the alarming conclusion that global capitalism was heading towards a very serious crisis. In the introduction, Soros makes clear his objectives in writing the book (1998, xxviii-xxix):

> "I want to make it clear that I do not want to abolish capitalism. In spite of its shortcomings, it is better than the alternatives. Instead, I want to prevent the global capitalist system from destroying itself."

Soros argues that, in the global financial system, there is a prevailing bias which he calls 'market fundamentalism' defines as follows (1998, 126-7):

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[8] This very brief sketch of zur Hausen's work is based on the much more detailed account to be found in Clarke (2011).

[9] Further details about the award can be found on http://rwer.wordpress.com/2010/05/13/keen-roubini-and-baker-win-revere-award-for-economics.

> "The global capitalist system is supported by an ideology rooted in the theory of perfect competition. According to this theory, markets tend toward equilibrium and the equilibrium position represents the most efficient allocation of resources. Any constraints on free competition interfere with the efficiency of the market mechanism; therefore they should be resisted. In previous discussion, I described this as the laissez faire ideology but market fundamentalism is a better term."

Soros was writing his book at a problematic time. There was the Asian crisis, the Russian melt-down, and the collapse of Long Term Capital Management. This was an investment firm so large that its demise put the whole global financial system at risk. Despite all these negative events, Soros pointed out that we might only be at the 'period of testing ' in his boom/bust model. If the global financial system survived testing by the Asian crisis and its aftermath, the boom would, according to his model, continue in a more exaggerated fashion than ever, leading eventually to a much bigger crash some years down the line. This, of course, is exactly what happened. It should be stressed, however, that this prediction of Soros' was conditional on the system remaining as it was. It could, he goes on to argue, be corrected in a way which would reduce the chances of a major crash. As he says (1998, 134):

> "I have no hesitation, however, in asserting that the global capitalist system will succumb to its defects, if not on this occasion then on the next one – unless we recognize that it is defective and act in time to correct the deficiencies."

Soros goes on in chapter 8 of his book, entitled: 'How to Prevent Collapse' to suggest a number of reforms which he thinks might prevent the crash which he has predicted. Needless to say, these reforms were not adopted, and the predicted crash duly occurred.

As we have seen, monism in economics had a very negative effect regarding the advice which politicians and policy makers received in the decade leading up to the great financial crash of 2008. Naturally politicians and policy makers chose as their advisers, those economists who were the most prestigious under the current system of valuation. This meant that they did *not* hear the views of any members of a minority school, and so were not given any warning of the dangers of a financial collapse. If, instead, they had chosen advisers from each of the minority schools, as well as advisers from the mainstream, they would at least have heard these warnings.

At present we are faced with the problem of how to deal with the consequences of the crash of 2008, and, in particular, with the high levels of public debt which have resulted from governments bailing out the banks. Mainstream economic models claim that to reduce public debt, it is necessary to cut public expenditure. However, it will hardly come as a surprise that these models of the majority school in economics are being sharply criticized by some of the minority schools in economics. Perhaps the most striking such criticism is to be found in Chick and Pettifor (2011). Victoria Chick is one of the leaders of the Post-Keynesian school of economics in the UK; while Ann Pettifor was one of the heterodox economists who correctly predicted the great financial crash of 2008. She was one of the finalists for the Revere prize. In their 2011 paper, Chick and Pettifor argue, using Keynes's multiplier theory, that cuts in government expenditure will increase rather than decrease government debt, and that to reduce government debt is would be necessary to increase government expenditure. A remarkable feature of their paper is that it supports this conclusion by a detailed consideration of statistics of the UK economy from 1909 to 2009. From these statistics, the authors conclude (2011, 3):

> " … there is a very strong negative association between government expenditure and the government debt, excluding the two outliers for the World Wars. As public expenditure increases, public debt falls, and *vice-versa*. A simple regression (excluding the World Wars) shows an R^2 of -0.98 …"

Apart from their overall analysis of the statistics, the authors give a detailed analysis of particular episodes. For example, Chick and Pettifor, write (2011, 7):

> " … between 1931 and 1933, government expenditure was cut by about 10 per cent. Nominal GDP fell by 2.3 per cent, and government debt rose from 173 to 183 percent of GDP."

After 1933, however, the UK government was persuaded by Keynes and other economists to replace the cuts policy by one of increasing government expenditure. This policy was implemented from 1934 until the outbreak of the Second World War in 1939. Chick and Pettifor comment (2011, 8):

> "The extent of this expansion (in public expenditure from 1933 to 1939 – D.G.), from 12 to 23 per cent of GDP, is not widely appreciated, … . The economy recovered: real GDP rose by an average annual rate of 4 per cent, the unemployment rate was halved and the public debt fell from 183 to 141 per cent of GDP."

It is not of course my aim here to compare the merits of Chick and Pettifor's approach as opposed to the models based on mainstream economics on which the various political parties in the West are relying at the moment. A study of which approach is better confirmed by the data is a job for economists. The question I am raising is the following much simpler one. In seeking advice about the economy, would the government get better advice if they consider only the models based on mainstream economics (monistic strategy), or if they consider not just those models but also the approaches of some heterodox economists such as Chick and Pettifor (pluralist strategy)? This question is of course easy to answer for it is obvious that the pluralist strategy is likely to produce better results.

7. Conclusions

I have argued that the effect of a research assessment system on economics is to drive the economics community away from pluralism and towards monism. I have also argued that a pluralistic economics community is much more likely to produce good research than a monistic one. Putting these two claims together, the conclusion is that a research assessment will make research in economics worse rather than better. Hence there is a strong case for abolishing research assessment systems in economics where they exist, and not introducing any new ones.

One result which emerged in the course of the paper was that, if an economics community gets a very high score in a research assessment system, this should be interpreted as meaning that the community is very monistic, and hence in a problematic state as far as research in the subject is concerned. So a high score for an economics group in a research assessment system does not mean that that group is producing a lot of good research – rather the opposite. The fact that scores in a research assessment system can be so misleading is another reason for eliminating such systems.

Of course there are many ways of organising research which do not involve research assessment systems. In my 2008 book, I sketch one possible approach (Gillies, 2008, Part 3,

Chapters 7-11, 65-129). This 2008 book is mainly concerned with research in mathematics, the natural sciences, and the humanities. In the case of economics, an additional point needs to be stressed. The most important factor in designing a system of research organisation for economics is to ensure that pluralism is protected and encouraged. Members of minority schools should have the same chance as members of the majority school of obtaining posts in leading institutions, promotions, research grants, and time and facilities for research. On the teaching side, all economics degree courses should present the views of the minority schools as well as those of the majority school; while committees of economists who advise the government should always include representatives of minority schools as well as representatives of the majority school. In short, pluralism should be encouraged in every context.

Appendix A. Mathematical model

It is possible to construct a mathematical model which shows the relation between the valuation which a group of economists receives in a RAS and the degree of monism/pluralism of the group. This model, which I call *the MPS model*, is based on simplified assumptions, but nonetheless gives results which I believe to be qualitatively correct. The first simplification is to suppose that there are only two schools of economics – School A (Marxism) and School B (Neoclassical Economics). I suppose that a proportion x of the economists in the group being evaluated belong to School A, where $0 \leq x \leq 1$, so that a proportion 1-x of the economists belong to School B. Let us further suppose that when a research assessment takes place, each economist of the group submits the same number of items for evaluation and that the total number of such items is n. A committee is formed to carry out the evaluation. We suppose that it contains m members, and that the proportion of members of the committee belonging to School A on the evaluation committee is the same as the proportion in the group being evaluated, namely x. We further suppose that the members of the evaluation committee belonging to School A give 1 to each item of research produced by a member of School A, and 0 to each item of research produced by a member of School B; while the members of the evaluation committee belonging to School B give 1 to each item of research produced by a member of School B and 0 to each item of research produced by a member of School A. Naturally this is a simplification, but our earlier discussion shows that it is qualitatively on the right lines.

It is now easy to calculate the total score which the n items of research produced by the group obtain. There are nx items produced by School A, and mx members of School A on the committee. So these nx items score mnx^2. Similarly there are n(1-x) items of research produced by School B, and m(1-x) members of School B on the committee. So these n(1-x) items score $mn(1-x)^2$. Hence the total score obtained by the n items of research is

$$mnx^2 + mn(1-x)^2$$

Let us suppose that to obtain the RAS valuation of the group of economists (y say), we normalize by dividing through by mn to produce a scale which runs from 0 to 1. We then obtain

$$\begin{aligned} y &= x^2 + (1-x)^2 \\ &= 2x^2 - 2x + 1 \end{aligned}$$

≤ x ≤
I. A

The point P (Pluralism) is the point which represents the maximum degree of pluralism when the two schools A and B are equally represented in the group. It corresponds to the lowest value of the RAS evaluation of the group y, namely y = ½. If the group moves away from pluralism by an increase in the value of x, i.e. an increase in the proportion of the members of School A in the group, then the RAS valuation y steadily increases, reaching its maximum of 1 at the point S (Stalinism), when all the members of the group belong to School A. If the group moves away from pluralism by a decrease in the value of x, i.e. an increase in the proportion of the members of School B in the group, the RAS valuation steadily increases, reaching its maximum of 1 at the point M (McCarthyism), when all the members of the group belong to School B.

I argued in section 5 of the paper that a pluralist group of researchers in economics is likely to produce better results than a monistic group. If we accept this assumption, then the MPS model has a remarkable consequence, namely that to judge the research potential of a research group in economics, we should use not the RAS valuation y, but its inverse 1/y. This is because y reaches its minimum value at the maximum of pluralism, where the potential for good research is highest; while it reaches its maximum value at the maximum of monism, where the potential for good research is lowest. 1/y behaves in the opposite fashion, and so gives a better measure of the research potential of a group of economists.

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