

# The Empirical Success of Keynesianism<sup>1</sup>

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## Abstract

The main thesis of this paper is that the empirical success of Keynesianism shows it to be scientific. Keynesianism here refers not to a specific theory, but to a paradigm. It is argued that Kuhn's notion of paradigm can be applied to economics, but, in contrast to the natural sciences, in economics there are always competing paradigms. The principal ones in contemporary economics are the Neoclassical, Keynesian and Marxist. To investigate whether the Keynesian paradigm is scientific we need a criterion for the scientificity of a paradigm. The paper first considers one approach to this problem due to Kant, but this is rejected as incorrect and called 'the Kantian fallacy'. Another approach is then put forward: 'the empirical confirmation principle', and reasons are given for accepting this as correct. Using this principle, it is then argued that that Keynesianism is indeed scientific.

**Keywords:** confirmation, Kantian fallacy, Keynes, Kuhn, paradigm, Neoclassical economics

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## 1. Paradigms in Economics

In this section, I want to argue that Kuhn's concept of paradigm can usefully be applied to economics, although paradigms in economics differ in one crucial respect from those in the natural sciences. I will therefore begin by saying something about the notion of paradigm.

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 book, he makes some remarks about the social sciences, which, despite their brevity, contain, in my view, a very important insight.

Kuhn writes as follows:

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<sup>1</sup> This paper is the result of the many meetings of the Post-Keynesian school, which I attended over the years, and also of my discussions with leading members of that school, particularly Bradley Bateman, Victoria Chick, John Davis, Tony Lawson, Jochen Runde and Robert Skidelsky. That is not to say of course that these researchers will necessarily agree with the contents of the paper! I am also, as usual, grateful to my wife: Grazia letto Gillies for many helpful comments. The journal *Economic Thought* unusually operates an open review process with a discussion forum. I thought this worked well. The reviewers of my paper: Rafael Galvão de Almeida, Arne Heise and Dave Taylor were not inhibited by the openness from expressing quite strong criticisms of the first version of the paper, as did the editor: Michel Zouboulakis. These criticisms led me to make several alterations, including a change of title. I have acknowledged some of the suggestions of these reviewers and of the editor in footnotes.

'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 recognise the role in scientific research of what I have since called "paradigms"' (Kuhn, 1962, pp. ix-x).

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, p. xi): 'Each of the schools ... is guided by something much 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. 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. However there are several other schools of economics. We can mention the Austrian, Institutional, Keynesian, and Marxist. In addition there are Sraffian, or neo-Ricardian economists, evolutionary economists and economists who use complexity theory. Although this variety of different schools of economics should be borne in mind, it will be convenient in this paper to limit the discussion, for simplicity, to what I take to be the principal schools. These are Neoclassical economics, Keynesianism and Marxist economics.

Do these three principal schools of economics each have a paradigm in the sense of Kuhn? To answer this question, let us look at the passage in which Kuhn introduces the concept of paradigm. It is the following:

'Aristotle's *Physica*, Ptolemy's *Almagest*, Newton's *Principia* and *Opticks*, Franklin's *Electricity*, Lavoisier's *Chemistry*, and Lyell's *Geology* – these and many other works served for a time implicitly to define the legitimate problems and methods of a research field for succeeding generations of practitioners. They were able to do so because they shared two essential characteristics. Their achievement was sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity. Simultaneously, it was sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve.

Achievements that share these two characteristics I shall henceforth refer to as "paradigms"...' (Kuhn, 1962, p. 10).

Kuhn here mentions key texts in the natural sciences, but, if we make a similar list of key texts in economics such as Marx *Capital*, Keynes *The General Theory* and the books by the founding fathers of neo-classical economics, then we find that much of what Kuhn says applies also to these texts. These 'works served for a time implicitly to define the legitimate problems and methods of a research field for succeeding generations of practitioners'. Taking Keynes *The General Theory* as an example, it is true to say that its 'achievement was sufficiently unprecedented to attract an enduring group of adherents away from competing modes of ... [economic] ... activity' and that 'it was sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve'. This, I think, justifies the use of Kuhn's term 'paradigm' in economics, but a key difference between economics and the natural sciences must not be forgotten. Keynes *The General Theory* did not establish a consensus in the field of economics. Many practitioners remained loyal to older paradigms such as the Marxist and the Neoclassical.

There is a second important point of difference between economics and the natural sciences. If we consider paradigms in economics, they appear to be associated with political ideologies. Taking the Marxist, Keynesian and Neoclassical paradigms, it seems obvious that they are arranged on a political spectrum running from the left to the right.

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.<sup>2</sup> This is expounded in *Capital*, Volume 1, Chapter IX, Section 1, which is entitled: 'The degree of exploitation of labour power'. Marx does not here use the term 'profit' but 'surplus-value'. According to his account, surplus value is extracted from the workers and then divided up into profits, interest and rent. So profits are only a part of surplus-value. Regarding the rate of surplus-value, Marx says:

'The rate of surplus-value is therefore an exact expression for the degree of exploitation of labour-power by capital, or of the labourer by the capitalist' (1867, p. 218).

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 openly acknowledged, indeed emphasised, by Marxists.

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<sup>2</sup> Rafael Galvão de Almeida suggested that I elaborate this claim with a quotation from Marx himself which I do in the next few lines.

Let us next turn to Neoclassical economics. The core of the Neoclassical paradigm is equilibrium theory. In a Neoclassical equilibrium model, we have firms which organise their production in order to maximise their profits, given the existing technology; and households which organise their consumption in order to maximise 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, p. 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. Keen has the following interesting passage about the consequences of Neoclassical economics:

'If you believe that a free market system will naturally tend towards equilibrium – and also that equilibrium embodies the highest possible welfare for the highest number – then *ipso facto*, any system other than a complete free market will produce disequilibrium and reduce welfare. You will therefore oppose minimum wage legislation and social security payments – because they will lead to disequilibrium in the labour market. You will oppose price controls – because they will cause disequilibrium in product markets. You will argue for private provision of services – such as education, health, welfare, perhaps even police – because governments, untrammelled by the discipline of supply and demand, will either under or oversupply the market (and charge too much or too little for the service)' (Keen, 2001, p. 163).<sup>3</sup>

These consequences certainly make it look as if Neoclassical economics involves a right-wing political ideology.

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 definitely 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-optimal 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 different forms depending on how much government intervention is seen as necessary. The right wing of the Keynesians have developed a version of Keynes' original theory which fits the Neoclassical framework and is known as the Neoclassical synthesis. Its adherents advocate rather little government intervention. The more left wing 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, even though Keynes himself was strongly opposed to Marxism and in his *Essays in Persuasion* expresses astonishment regarding Marxian socialism as to: 'how a doctrine so illogical and so dull can

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<sup>3</sup> In the second edition of the book, Keen (2011), this passage is on page 173.

have exercised so powerful and enduring an influence over the minds of men, and, through them, the events of history' (1931, p. 285).

These are my arguments for the conclusion that every paradigm in economics is closely associated with a corresponding political ideology. However, this view is not held by everyone. There is an opposite opinion that it is possible to construct a positive economics, which is entirely factual and does not involve any values or ideologies. A leading exponent of this idea was Lionel Robbins in his 1932 book. I will now examine how Robbins defends his position.<sup>4</sup>

Robbins does not speak of ideologies but use other terms such as 'valuations'. He writes:

'In recent years, certain economists ... have urged that the boundaries of the subject should be extended to include normative studies. Mr Hawtrey and Mr J.A.Hobson, for instance, have argued that Economics should ... take account of valuations and ethical standards... "Economics", says Mr Hawtrey, "cannot be dissociated from Ethics".

Unfortunately it does not seem logically possible to associate the two studies in any form but mere juxtaposition. Economics deals with ascertainable facts; ethics with valuations and obligations. The two fields of enquiry are not on the same plane of discourse. Between the generalisations of positive and normative studies there is a logical gulf fixed which no ingenuity can disguise and no juxtaposition in space or time bridge over.' (Robbins, 1932, p. 132).

Now, within economics, there may indeed be some propositions which could be described as 'ascertainable facts'. For example, if sufficient statistics are available, the average wage of a manual worker in a specific country and year may be an ascertainable fact. However, as soon as we move to the more theoretical propositions of economics, the picture changes. Such propositions involve concepts which do appear to be value-laden and so are not simply ascertainable facts. This can be seen by considering an example which Robbins deals with in detail, namely: minimum wage regulation.

It is surprising that Robbins should choose this example, since it does not seem *prima facie* to support his position very well. Debates about minimum wage have occurred regularly in the past few decades and they always provoke a furious and rather predictable debate between the right and the left. Right wing politicians characteristically argue that minimum wage regulation would only price workers out of jobs and so would really be worse for them. To this, left wing politicians characteristically reply that employers could easily afford to pay higher wages and they only refuse to do so in order greedily to increase their already large profits. Could this question, seemingly so fraught by political ideology, really be settled simply by an appeal to 'ascertainable facts'? Robbins thought that it could, for he writes:

'we may examine the case of minimum wage regulation. It is a well-known generalisation of Theoretical Economics that a wage which is held above the equilibrium level necessarily involves unemployment and a diminution of the value of capital. This is one of the most elementary deductions from the

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<sup>4</sup> Rafael Galvão de Almeida in his comment on the first draft of my paper objected that I had not taken account of the views of those who think that economics can be value free, and suggested that I should consider the arguments of Robbins who was one of the leading proponents of this position. The section on Robbins which follows, was added in response to this criticism.

theory of economic equilibrium. The history of this country since the War is one long vindication of its accuracy' (Robbins, 1932, p. 130).

When Robbins speaks of 'the history of this country since the War', he is referring to the UK in the years 1919-1931, but how during any specific year does one tell whether a particular wage is above the equilibrium level? These years were noted for the economy undergoing extreme fluctuations. A boom up to 1929 was followed by a slump thereafter. There was never any prolonged state of equilibrium. Thus to work out the equilibrium level of wages is not an easy matter. It involves hypothetical considerations, and so it remains unclear whether the result is really an 'ascertainable fact'. Moreover if we look more closely at the concepts involved in defining economic equilibrium, such as 'utility' and 'Pareto-optimal', they do appear to involve valuations and ideology.

Robbins was working within a version of Neoclassical economics strongly influenced by the Austrian school. Yet an example from the Marxist paradigm has many similar features. Let us compare Robbins' example: 'a wage which is held above the equilibrium level necessarily involves unemployment' with the Marxist example given earlier: 'profits arise from the exploitation of the workers'. A defender of Marxism would no doubt say: 'the data on 19<sup>th</sup> century capitalism given by Marx in *Capital* is one long vindication of the accuracy of this proposition'. However, as we have seen the concept of exploitation is defined in terms of the labour theory of value and the value of any commodity is not something that can be determined by observation in any easy or straightforward fashion.

Robbins regarded the theoretical proposition which he gives as an example as an ascertainable fact forming part of positive economics, but what would he have said about the Marxist proposition just mentioned which seems to have some features in common with Robbins? Of course, we can never know what Robbins would have said on this question, but it can be pointed out that none of the options available to him are very satisfactory.

He could have said that 'profits arise from the exploitation of the workers' is obviously an ideological value-judgement, whereas 'a wage which is held above the equilibrium level necessarily involves unemployment' is a purely factual statement which forms part of positive economics. But the obvious reply would be that he simply shows himself to be a man who is aware of the biases of others, but unconscious of his own. If a statement has left wing implications, he recognises it as ideological, but if it has right wing implications he regards it as just a matter of fact, not involving any ideology. The other option open to Robbins would be to say that 'profits arise from the exploitation of the workers' is a purely factual statement but a false one. However, this is hardly satisfactory. The Marxists themselves regard this statement as ideological, and it certainly looks highly ideological. Could it really be purely factual?

My conclusion is that the arguments of Robbins are not very convincing. However, the comparison I have drawn between statements within Robbins' own paradigm, and statements within the Marxist paradigm, is useful for the next topic I want to consider. Both Robbins and the Marxists regard the type of economics they do as scientific. Robbins speaks of 'Economic Science' both in the title of his book, and in the title of Chapter VI. This 'Economic Science' is of course economics done within the Neoclassical/Austrian paradigm. Famously in his speech at the graveside of Karl Marx in 1883 Engels claimed that while Darwin had produced a science of the biological world, Marx had produced a science of the human world.

Now we know that there are scientific paradigms in the natural sciences. The Newtonian paradigm which was the basis of normal science for nearly two hundred years is an obvious example; as is the Darwinian paradigm of evolution by natural selection which is

still accepted today. But what about the paradigms of economics? Are any of them scientific? Or are they all purely ideological? If they are ideological to some degree, does this prevent them from being scientific? These questions are difficult to answer. It is straightforward to say that a paradigm is scientific if a number of theories developed within it are scientific, but when is a theory scientific? In fact there are a number of different suggestions about what makes a theory scientific, and these different suggestions lead to different results regarding economics. In the next two sections, I will expound two of these suggestions. Section 2 deals with one made by Kant. As we shall see, this suggestion, if accepted, would lead to the conclusion that Neoclassical economics is scientific, while Keynes' *General Theory* is not scientific. However, I will argue against Kant's suggestion, describing it as 'The Kantian Fallacy'. Then in section 3, I will consider a more empiricist suggestion that a theory is scientific if it is empirically confirmed. Using the criterion of section 3, I will, in section 4, argue for the main thesis of this paper, namely that the empirical successes of Keynesianism show that it is scientific.

## 2. The Kantian Fallacy

In his book: *Metaphysical Foundations of Natural Science*, published in 1786, Kant made the following claim:

'I maintain... that in every special doctrine of nature only so much science proper can be found as there is mathematics in it' (Kant, 1786, p. 6).

It is worth noting that Kant's claim here is about 'special doctrines of nature', i.e. the natural sciences. Let us, however extend the claim to the social sciences and see what results it would give regarding economics.

We have identified the three major paradigms in economics as the Neoclassical, the Keynesian and the Marxist. Of these three, it is clear that Kant's claim extended to the social sciences would favour the Neoclassical paradigm, because the theories of Neoclassical economists contain much more mathematics than those of the Marxists or of Keynes himself and the Post-Keynesians. I will, however, argue that Kant's claim is false, both as applied to the natural sciences and also as extended to theories outside the natural sciences.

Let me begin by considering the natural sciences. Here, as is so often the case with Kant's views, Kant's claim was quite plausible when he made it in 1786, but developments in science and mathematics in the 19<sup>th</sup> century went strongly against it. In 1786, the leading theory in the natural sciences was Newtonian mechanics. In fact Kant's book is really focussed on providing metaphysical foundations for Newton's theory, which he seems to regard as virtually the whole of natural science. Now Newton's theory was highly mathematical. So it is not so surprising that Kant concluded that a theory of nature would have to contain a lot of mathematics in order to be genuine science.

Another candidate for being a natural science in 1786 was chemistry, but Kant rules chemistry out, saying:

'chemistry can become nothing more than a systematic art or experimental doctrine, but never science proper' (Kant, 1786, p. 7).

Once again this view was not so implausible in 1786 when chemistry was still in a rather primitive state. However, things were soon to change. In 1789, Lavoisier published his *Traité élémentaire de Chimie*, in which he put forward his new definition of chemical element, and

his new oxygen theory of combustion. This book, which was translated into English the next year, became the basis of a new paradigm which was generally accepted by chemists. Research in this paradigm led to a tremendous development of chemistry so that by the middle of the 19<sup>th</sup> century, no one would have doubted that chemistry was a very successful science. Yet this new chemistry did not contain much mathematics. Chemistry involved a lot of weighing and measuring and examining of proportions. This required basic arithmetic, but that was really all the mathematics to be found in chemistry. Chemistry was a science proper, but involved very little mathematics, thus showing that Kant's 1786 claim was false.

Let us now generalise Kant's claim about the theories of natural science to theories in general, including those of the social sciences. This generalised Kantian claim takes something like the following form.

A theory is scientific to the extent to which it contains mathematics. (\*)

(\*) is of course still refuted by the example of 19<sup>th</sup> century chemistry, but it is also refuted by the development of mathematics in the 19<sup>th</sup> century.

Before the 19<sup>th</sup> century, nearly all of mathematics was such that it could be, and was, applied to the physical world. However, the 19<sup>th</sup> century saw the development of pure mathematical theories, which had no application either in physics or elsewhere. An example of this is Cantor's theory of transfinite cardinal numbers greater than aleph 0 ( $\aleph_0$ ). This is a highly mathematical theory, but it has never been applied in physics or in any other science. So it cannot be considered to be scientific. This again contradicts (\*). My conclusion is that (\*) is false, and I will refer to it from now on as 'The Kantian Fallacy'.

But if (\*) is fallacious, how then do we judge whether, and to what extent, a theory is scientific? In the next section I will argue for another approach to this question, which is empiricist rather than Kantian.

### 3. Empirical Confirmation shows a Theory to be Scientific

An empiricist approach to the question of whether a theory is scientific considers the relation of the theory to empirical data – that is to propositions that can be established as true by observation and/or experiment. However this general idea can be worked out in various ways. Let us start by considering Popper's notion of falsifiability or refutability.<sup>5</sup>

Popper's concept is usually illustrated by the example of the generalisation: 'all swans are white'. This was confirmed by all the observations made by Europeans in Europe, and so was generally held to be correct by them. However when the first Europeans went to Australia, they observed black swans and hence refuted or falsified the generalisation. Popper developed from this simple example the theory that science consists of conjectures and refutations. This in turn suggests that we could regard a theory as scientific if it is falsifiable by empirical data.<sup>6</sup> However, this quite plausible suggestion encounters a problem which has become known to philosophers of science as *the Duhem-Quine thesis*.

Duhem formulates his version of this thesis as follows:

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<sup>5</sup> In response to comments by Arne Heise and Michel Zouboulakis, I decided to include a discussion of Popper's falsifiability and the Duhem-Quine thesis, and this is done in the following five paragraphs. In order not to disturb the balance of the paper, I have given only a sketch of these matters. For the interested reader, there is a much more detailed treatment of these issues in Gillies, 1993, Ch.5, pp. 98-116, and Ch.10, pp. 205-230.

<sup>6</sup> Popper's own formulation of his demarcation criterion is more complicated than this. For a full discussion with quotations, see the reference given in the previous footnote.



*An Experiment in Physics Can Never Condemn an Isolated Hypothesis but only a Whole Theoretical Group* (1904-5, p. 183, italics and capitals in original).

We can explain what Duhem means here by considering the example of Newton's First Law or the Law of Inertia ( $T_1$  say). This states that every body continues in its state of rest, or uniform motion in a straight line, unless it is compelled to change that state by forces impressed upon it. Now no one doubts that  $T_1$  is a scientific law. Yet it is not so easy to falsify by observation and/or experiment as was the case with 'all swans are white'.

Suppose, for example, we want to try to falsify  $T_1$  by observations of the planets. We cannot derive any statements about the movements of planets, which can be compared with observation, from  $T_1$  alone. We have to add to  $T_1$ , Newton's other two Laws of Motion ( $T_2$  and  $T_3$  say), his Law of Gravity ( $T_4$  say), and also some Auxiliary Assumptions ( $A$  say).  $A$  consists of assumptions like the following: no other forces but gravitational ones act on the planets, the mass of the Sun is very much greater than that of any of the planets, and so on. Once we have created this 'whole theoretical group', we can indeed deduce from it a statement  $O$  which can be compared to observations of the Solar System. Suppose now  $O$  is shown to be incorrect, we cannot deduce that  $T_1$  is false, but only that one or more members of the theoretical group  $\{T_1, T_2, T_3, T_4, A\}$  are false. The observational result does not tell us which members of the group are false, and so  $T_1$  is not falsifiable, despite being clearly scientific.

Duhem's formulation of his thesis given above could be criticised. There are, in physics, some hypotheses which are falsifiable. For example, Snell's law of refraction applied to glass. Similarly other natural sciences contain both falsifiable hypotheses and hypotheses which are not falsifiable. Broadly speaking it is the higher level hypotheses, containing more abstract theoretical terms, which are not falsifiable because of the Duhem-Quine thesis, but which should still be regarded as scientific. So falsifiability is not an adequate criterion for demarcating scientific theories from non-scientific ones.

In the reference given in footnote 5, I argue in detail that falsifiability should be replaced by confirmability as an appropriate criterion for demarcating science from non-science (see particularly Gillies, 1993, pp. 214-220). I will now apply this approach to economics.

My suggestion is to replace (\*) (the Kantian fallacy) by the following:

A theory is scientific to the extent that it is empirically confirmed. (\*\*)

I will call (\*\*) 'The Empirical Confirmation Principle'.

A theory  $T$  is empirically confirmed by evidence  $e$ , if  $e$  can be regarded as established by observation and experiment. The degree of empirical confirmation of  $T$  by  $e$  is written in symbols:  $C(T, e)$ , and there have been attempts to develop a formal theory of  $C(T, e)$  which shows its relation to probability  $[P(T | e)]$ . I discuss formal confirmation theory in my (1998) which also considers an area in which such a formal theory is useful, namely artificial intelligence. However, if our aim is to judge whether an economic theory is empirically confirmed, such formalisations and technicalities are more likely to obfuscate than to clarify the situation. What is needed is an informal, qualitative, method for assessing empirical confirmation. In fact such an informal qualitative approach is that taken by scientists themselves when they consider whether one theory in their field is better confirmed by evidence than another. I will now sketch an informal qualitative method for assessing empirical confirmation, and illustrate it by considering Kant's example of Newtonian theory.

The basic idea of this qualitative approach is to regard a theory T as empirically confirmed by successful explanations and/or successful predictions. In the second case, a prediction is made on the basis of T of an event which will occur in the future. If the predicted event is shown by observation and/or experiment actually to occur, the prediction is successful and T is confirmed empirically. In the first case, we suppose that a number of facts  $f_1, f_2, \dots, f_n$  have been established in the sense that they have been well confirmed by observation and/or experiment, and so can be assumed to be true (at least when interpreted as approximations) while the attempt at theoretical explanation is being made. In this case, if T is sufficiently simple and successfully explains these facts, it is confirmed empirically.

Let us now see how this applies to the example of Newton's theory as set out in his *Principia* of 1687. In this work Newton successfully explained a whole range of facts. He explained why Kepler's Laws and Galileo's law held approximately, but he also explained some observed deviations from these laws. In addition, Newton explained, with reasonable success, the laws of impact, the tides, and the inequalities of the Moon's motion. So Newton's theory was certainly well confirmed empirically by its successful explanations of established facts.

Newton was also able to make successful predictions, notably as regarded comets. His theory enabled the orbit of a comet to be calculated from a few astronomical readings. The predicted orbit of the comet could then be checked against its actual orbit as revealed by further astronomical readings.

It is interesting to compare the above account of empirical confirmation with that given by Friedman in his 1953 article.<sup>7</sup> Friedman writes:

'the only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience' (1953, pp. 8-9).

We see from this that Friedman differs from the view, which I have just expressed, by limiting validation of a hypothesis to successful predictions, and hence excluding validation through successful explanations of known facts. However, this exclusion runs counter to normal practice in the natural sciences, and so seems unjustified. As we have seen, Newton's theory was regarded in the early 18<sup>th</sup> century as confirmed just as much by its successful explanation of facts known to Newton, as by its successful predictions. If the community had limited themselves only to the successful predictions, then Newton's theory might well have not been accepted at all.

In the passage just quoted, Friedman speaks of the 'validity' of a hypothesis, but not of its empirical confirmation. In fact he seems rather doubtful about using the term 'confirmation'. He does use it in the next few lines, but only in scare quotes. This is what he says:

'Factual evidence can never "prove" a hypothesis; it can only fail to disprove it, which is what we generally mean when we say, somewhat inexactly, that the hypothesis has been "confirmed" by experience' (1953, p. 9).

Of course Friedman is quite correct to say that factual evidence can never prove a hypothesis. His scare quotes are here unnecessary. This is just a platitude with which all philosophers of science would agree. Nor do I see why he should use scare quotes for

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<sup>7</sup> Both Rafael Galvão de Almeida and Michel Zouboulakis suggested that it would be helpful, particularly for an audience of economists, to compare my views of empirical confirmation with those of Friedman (1953). The following two paragraphs were added as a result of this suggestion.

*confirmed*, or regard the use of that word as an example of speaking 'somewhat inexactly'. 'Confirmation' is a term widely used by both scientists and philosophers of science, and there seems no reason to regard it as somehow inappropriate.

Returning now to the example of Newton's theory, we can say that it was certainly well confirmed empirically when it came to be accepted by scientists early in the 18<sup>th</sup> century. Further results in the 18<sup>th</sup> and 19<sup>th</sup> centuries added still further to its empirical confirmation. Thus Newton's theory had two significant features. (1) It contained a great deal of mathematics, and (2) it was strongly confirmed empirically. Kant's mistake was to choose the wrong feature as his criterion for the scientificity of Newton's theory. He thought that it was (1) which made Newton's theory scientific (the Kantian fallacy), while, in reality, it was (2) (the empirical confirmation principle).

That concludes my discussion of the empirical confirmation principle. In the next section, I will examine how it applies to Keynesianism.

#### **4. Keynesianism is Scientific**

Keynes, as a student of Marshall's, started as a fairly orthodox economist; but he came to regard standard economic theory as inadequate, especially in the light of the Wall Street Crash of 1929 and the Great Depression which followed. The magnitude of this depression for the period 1929-1935 is shown by the unemployment figures given in Table 1.

**Table 1** Unemployment in the USA 1929-1944 (per cent of civilian labour force)

<u>Year</u>	<u>Unemployment</u>
1929	3.2
1930	8.7
1931	15.9
1932	23.6
1933	24.9
1934	21.7
1935	20.1
1936	16.9
1937	14.3
1938	19.0
1939	17.2
1940	14.6
1941	9.9
1942	4.7
1943	1.9
1944	1.2

Source: Baran and Sweezy, 1966, p. 228.

As can be seen, unemployment in the USA rose from 3.2% in 1929 to a peak of 24.9% in 1933, and was still over 20% in 1935 when Keynes was writing *The General Theory*. In this book, Keynes speaks (1936, p. 33) of a 'doctrine ... remained unquestioned by orthodox economists up to a late date' and 'its signal failure for purposes of scientific prediction'. These remarks are quite appropriate. Orthodox Neoclassical economics claims that a market economy will be propelled by market forces to a full employment equilibrium. Now the USA

had a market economy in the period 1929 to 1935, but the figures just quoted show that its behaviour was not at all that predicted by Neoclassical economic theory.

In these circumstances, Keynes set himself the task of devising a theory, which, unlike Neoclassical economics, would explain the Great Depression. He thought that such a theory was needed to restore confidence in the economics profession, because, as he wrote:

‘... professional economists, after Malthus, were apparently unmoved by the lack of correspondence between the results of their theory and the facts of observation; - a discrepancy which the ordinary man has not failed to observe, with the result of his growing unwillingness to accord to economists that measure of respect which he gives to other groups of scientists whose theoretical results are confirmed by observation when they are applied to the facts’ (Keynes, 1936, p. 33).

An interesting feature of this passage is that Keynes implicitly accepts what in section 4 has been called the empirical confirmation principle (\*\*), namely that ‘a theory is scientific to the extent that it is empirically confirmed’. So Keynes’ aim was to produce a theory, which explained the Great Depression and was empirically confirmed and so scientific; but did he succeed in this project? This is what I will next investigate.

Keynes’ theory is well-known, but it will still be useful to give a brief outline of it based on his 1936 book, and a very clear summary of the theory which he published in (1937). The title of Keynes’ 1936 book is: *The General Theory of Employment, Interest and Money*. However, I will focus on those aspects of the theory, which deal with employment. A key concept of Keynes’ theory is that of effective demand, which he divides into demand for consumption goods and demand for investment goods. Keynes produces the key equations (1936, p. 63):

$$\text{Income} = \text{value of output} = \text{consumption} + \text{investment}$$

Since, however, saving = income - consumption, it follows that saving = investment. Part of the aim of *The General Theory* is to show how the equality of saving and investment is brought about.

The next concept which Keynes introduces is that of *the Propensity to Consume*. About this he says:

‘The fundamental psychological law, upon which we are entitled to depend with great confidence both *a priori* from our knowledge of human nature and from the detailed facts of experience, is that men are disposed, as a rule and on the average, to increase their consumption as their income increases, but not by as much as the increase in their income. ... it is also obvious that a higher absolute level of income will tend, as a rule, to widen the gap between income and consumption. For the satisfaction of the immediate primary needs of a man and his family is usually a stronger motive than the motives towards accumulation, which only acquire effective sway when a margin of comfort has been attained. These reasons will lead, as a rule, to a *greater proportion* of income being saved as real income increases’ (Keynes, 1936, pp. 96-97).

So in the equation:  $\text{income} = \text{consumption} + \text{investment}$ , as income rises the proportion spent on consumption declines, and hence the level of income can only be sustained if there is a corresponding increase in the proportion of investment. As Keynes says (p. 98) we need 'an increase in investment to fill the gap'. After a period of boom based on massive investments, there may come a point where there are no more profitable investment opportunities. Investment will fall rather than rise, and this will produce a fall in output and a rise of unemployment. Income will decline to a level at which, assuming that the propensity to consume remains constant throughout, savings will become equal to the new lower level of investment. This then is Keynes' explanation of the Great Depression. He himself gives the following summary of it:

'The theory can be summed up by saying that, given the psychology of the public, the level of output and employment as a whole depends on the amount of investment. I put it in this way, not because this is the only factor on which aggregate output depends, but because it is usual in a complex system to regard as the *causa causans* that factor which is most prone to sudden and wide fluctuation. ... of these several factors it is those which determine the rate of investment which are most unreliable, since it is they which are influenced by our views of the future about which we know so little.

This that I offer is, therefore, a theory of why output and employment are so liable to fluctuation' (Keynes, 1937, p. 121).

This then is Keynes' theory, but what empirical evidence does he offer in its favour? And does this evidence in fact empirically confirm the theory? To these questions I now turn.

Keynes has argued that the key factor in determining the level of employment is the amount of investment. In his 1936, he considers some statistics regarding investment during the period of the great depression, and these, in effect, are a test of his theory. Before comparing his theory to the data, Keynes stresses that the relevant factor is *net* investment, that is to say gross investment less amounts which are set aside in the form of sinking funds to finance the eventual replacement of the capital equipment which has just been installed. The money in these funds is not spent on consumption goods, and will not be spent on investment goods until the capital equipment is eventually replaced some time in the future. So this money does not contribute to effective demand. As Keynes himself says

'Sinking funds, etc., are apt to withdraw spending power from the consumer long before the demand for expenditure on replacements (which such provisions are anticipating) comes into play; i.e. they diminish the current effective demand and only increase it in the year in which the replacement is actually made. If the effect of this is aggravated by "financial prudence", i.e. by its being thought advisable to "write off" the initial cost *more* rapidly than the equipment actually wears out, the cumulative result may be very serious indeed.

In the United States, for example, by 1929 the rapid capital expansion of the previous five years had led cumulatively to the setting up of sinking funds and depreciation allowances, in respect of plant which did not need replacement, on so huge a scale that an enormous volume of entirely new investment was required merely to absorb these financial provisions; and it became almost hopeless to find still more new investment on a sufficient scale to provide for such new saving as a wealthy community in full

employment would be disposed to set aside. This factor alone was probably sufficient to cause a slump. And, furthermore, since 'financial prudence' of this kind continued to be exercised through the slump by those great corporations which were still in a position to afford it, it offered a serious obstacle to early recovery' (Keynes, 1936, p. 100).

Keynes employs economic statistics for the United States in the years 1919-1933, compiled by Kuznets. Kuznets uses 'capital formation' for what Keynes calls 'investment', but this is just a terminological point, and I will continue to use Keynes' terminology. Kuznets does, however, distinguish between gross and net investment, along the lines of the passage from Keynes just quoted. Kuznets' figures for net investment in the years 1929-1933, as quoted by Keynes, are given in Table 2.

**Table 2** Net Investment in the United States 1929-1933 (Millions of Dollars)

1929	25,481
1930	19,036
1931	11,098
1932	1,237
1933	6,675

Source: Keynes, 1936, p. 103.

Comparing these figures with the figures for unemployment in the same years, we can see that they strongly confirm Keynes' theory. Table 2 shows a catastrophic decline in net investment which fell to less than 5% of its 1929 value in 1932, and had only recovered to 28% of its 1929 value in 1933. Such a fall in net investment would, according to Keynes' theory, certainly produce a dramatic increase in unemployment such as is shown in Table 1 for the corresponding years.

Keynes also quotes figures for Great Britain compiled by Colin Clark for the years 1928-1931. These are given for the years 1929-1931 in Table 3.

**Table 3** Net Investment in Great Britain 1929-1931 (Millions of £)

1929	296
1930	183
1931	43

Source: Keynes, 1936, p. 102.

The picture here is the same. Net investment by 1931 had dropped to less than 15% of its value in 1929. The conclusion then is that Keynes' theory explains the Great Depression, and is empirically confirmed by economic data. However, as has been stressed above, empirical confirmation arises out of both successful explanations and successful predictions. Let us next examine how Keynes' theory fared as regards predictions.

In response to the great depression, the US president Franklin D. Roosevelt introduced the New Deal (1933-36). This involved a considerable increase in government spending, a large part of which was for investment in public works. The most notable of these public works was the Tennessee Valley Authority, a project of dam construction to curb

flooding and generate electricity. This is quite striking in view of current concerns about global warming. In addition to this public investment, the New Deal provided relief for the poor and unemployed. According to Keynes' theory this too would be helpful since it would tend to increase the demand for consumption goods, since the poor and unemployed have a very high propensity to consume. In fact it is likely that all the relief received by such groups would be spent on consumption. So Keynes' theory definitely predicted that the New Deal would decrease unemployment. According to Baran and Sweezy:

'Measured in current dollars, government spending increased from \$10.2 billion in 1929 to \$17.5 billion in 1939, more than 70 per cent' (1966, p 162).

Table 1 shows that unemployment decreased from its peak of 24.9% in 1933 to 17.2% in 1939, a decrease of 31%. The result of the New Deal thus confirms Keynes' theory. On the other hand, an opponent of Keynes might argue that this confirmation is not very striking since, despite the big increase in government spending, the recovery was not so great, and perhaps might have happened anyway. To this, a Keynesian would obviously have replied that the increase in government spending was not sufficient to restore full employment. Events after 1939 showed that this Keynesian argument was correct.

Following the entry of the United States into the Second World War, government spending increased from \$17.5 billion in 1939 to a maximum of \$103.1 billion in 1944. This time the increase was not 70%, but 489%. Table 1 shows that unemployment dropped steadily from 17.2% in 1939 to 1.2% in 1944 – a very striking confirmation of Keynes' theory.

As we have seen then, Keynes' theory was strongly confirmed both by its successful explanation of the Great Depression, and by the successful predictions it made regarding the recovery from this depression. This, I think, is sufficient to show that Keynes' economic theory should be regarded as scientific. However, the same conclusion is also reached by more recent work within the Keynesian paradigm. In this context, the analysis of Chick and Pettifor (2011 and 2016) is particularly striking, because it uses economic statistics collected over a long period of time. I will therefore conclude this section with a brief account of Chick and Pettifor's work.

On 15 September 2008, Lehman Brothers collapsed provoking a worldwide economic crisis. This was obviously quite similar to the Wall Street crash of 1929, which led to the Great Depression. Now policies based on Keynes' theory had, as we have seen, been very successful in producing a recovery from the Great Depression. So it seemed to many Keynesian sympathisers that similar policies could be used to deal with the aftermath of the Lehman Brothers collapse. This would have meant increasing public expenditure through investment in public works. In May 2010 a coalition government of the conservatives with the liberal democrats came to power in the UK. Its chancellor of the exchequer, George Osborne rejected this Keynesian approach to the crisis. In fact the Conservative Party, of which Mr Osborne was a member, had campaigned on the need to cut public expenditure. Their argument was that the national debt had been greatly increased by the need to bail out the banks in the wake of the Lehman Brothers collapse. In 2009 it stood at 68% of GDP, and thus, the conservatives argued, must be reduced, which could only be done by cutting public expenditure. Keynesian policies of expanding public expenditure could simply not be afforded. If a household falls into debt, they argued, it has to retrench and cut back its expenditure until the debt is paid off. Similarly the UK as a whole must retrench and cut public expenditure. Naturally the Keynesians replied that this was an instance of what is called the fallacy of composition. This fallacy is based on the assumption that, if a whole is composed of parts, what is true for each part must be true for the whole. However, there are many counter-

examples to this assumption. For example, animals and plants are composed of atoms. Atoms are not living, but it does not follow from this that animals and plants are not living.

Still, the argument that households have to cut back in order to pay off debts, and that the same must therefore hold for the country as a whole had great appeal for common sense, and persuaded a lot of citizens of the UK to vote for the conservatives. It also persuaded Mr Osborne to adopt the austerity policy of cutting back on public expenditure.

This situation led Chick and Pettifor to investigate, within a Keynesian framework, the question of whether it was really the case that cutting public expenditure would reduce the level of the public debt. What is interesting to note here is that there has been a shift of focus away from the question of reducing unemployment to that of reducing the national debt. The approach which Chick and Pettifor adopted was first to examine over a hundred years of data about the UK economy 1909-2009, and to see what this data showed about the relationship between government spending and the national debt. Their results are rather surprising. They define (p. 3): 'fiscal consolidations' as 'episodes when government spending actually fell in money terms' and go on to say:

'Outcomes for the public finances are seen as running almost entirely contrary to today's conventional wisdom, which is derived from microeconomic thinking: when sustained, fiscal consolidation increases rather than reduces the public debt ratio and is in general associated with adverse macroeconomic conditions. The analysis is extended to the post-war era, in which government expenditure never actually falls, but the pattern is sustained: when expenditure rises comparatively rapidly, the debt ration falls and the economy prospers, and when it levels off, the debt ration worsens and macroeconomic indicators are less favourable' (Chick and Pettifor, 2011, p. 3).

And again:

'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*. ... Even in war, when the debt ratio rises, it does so by a good deal less than the increases in government expenditure' (Chick and Pettifor, 2011, p. 5).

Chick and Pettifor express the relationship between government expenditure and national debt by means of a regression equation (2011, p. 5), but, rather than laying exclusive emphasis on this mathematical approach, they give a qualitative analysis of the data for the UK economy from 1909 to 2009, dividing these years into periods of fiscal consolidation and fiscal expansion. I will give a brief summary of some of their results.

The first period of fiscal consolidation is 1918-1923, known as 'Geddes Axe'. Here government expenditure fell from £1850 million (35.3% of GDP) in 1918 to £483 million (11.0% of GDP) in 1923. Government debt as a percentage of GDP rose from 114% in 1918 to 180% in 1923 (Chick and Pettifor, 2011, p. 8).

The second period of fiscal consolidation is 1931 to 1933, and was the UK government's initial response to the Great Depression. Government expenditure fell from £575 million (13.2% of GDP) in 1931 to £514 million (12.1% of GDP) in 1933. Government debt as a percentage of GDP rose from 173% in 1931 to 183% in 1933 (Chick and Pettifor, 2011, p. 10).



The third period of fiscal consolidation is 1944-47, when the UK was switching back from a war economy to a peacetime economy. Government expenditure fell from £5,206 million (50.7% of GDP) in 1944 to £2,156 million (20.2% of GDP) in 1947. Government debt as a percentage of GDP rose from 194% to 245% (Chick and Pettifor, 2011, p. 13). However, Chick and Pettifor also point out that the particular years 1946 and 1947 do constitute an exception to their general rule. In 1946, government expenditure was £2,575 million (25.9% of GDP), and Government debt was 252% of GDP. Comparing this with the figures for 1947 given above, we see that government expenditure was reduced between 1946 and 1947, while most unusually government debt also fell. This anomaly is, however, easily explained by the switch from war production, much of it carried out by the government, to more normal peacetime consumer production, much of which was carried out by the private sector.

By way of contrast, we can give the long period of post-war fiscal expansion from 1947 to 1975 (Chick and Pettifor, 2011, p. 14). Here government expenditure rose steadily from £2,156 million (20.2% of GDP) in 1947 to £28,963 million (27.1% of GDP) in 1975, while government debt declined equally steadily from 245% of GDP to 45% of GDP. It is worth noting that at the beginning of this period in 1947 government debt stood at 245% of GDP and yet it was perfectly possible to apply Keynesian policies of increasing public expenditure while at the same time reducing national debt. This shows the falsity of George Osborne's claim that the national debt of 68% of GDP in 2009 made it impossible for the country to afford Keynesian policies of increasing public expenditure. How could a national debt of 68% of GDP be too high for policies which were applied so successfully when the level of the national debt as 245%?

Chick and Pettifor sum up by saying:

'The empirical evidence runs exactly counter to conventional thinking. Fiscal consolidations have not improved the public finances. This is true of all the episodes examined, except the consolidation after World War II, where action was taken to bolster private demand in parallel to public retrenchment' (2011, p. 18).

Chick and Pettifor's conclusions are certainly established by the empirical data collected over so many years. Yet they are also very surprising, and, as we have seen, run counter to common sense. How are these conclusions to be explained? Chick and Pettifor go on to show that they are explained by Keynes' theory (2011, p. 21). This successful explanation of results based on data for the UK economy from 1909 to 2009 clearly gives strong empirical confirmation to Keynes' theory. This is not the end of the matter, however, since Chick and Pettifor's analysis leads to predictions, as I will now explain.

In order to reduce the UK's national debt in 2010, Chick and Pettifor advocated the Keynesian policy of increasing public expenditure, while George Osborne adopted exactly the opposite policy of cutting public expenditure. The prediction of Chick and Pettifor, on the basis of Keynes' theory and their empirical data, was that Mr Osborne's policy would increase rather reduce the UK's national debt. By 2016 it was possible to decide on the basis of economic statistics whether this prediction was correct. In fact, as Chick and Pettifor (2016, p. 5) point out, George Osborne relaxed his policy in late 2012. Nonetheless cuts in public expenditure continued though not on such a grand scale as originally planned. What were the results of this policy? The relevant data are given in Chick and Pettifor (2016, p. 6). Public spending as a percentage of GDP steadily decreased from 25.0% of GDP in 2010 to 22.2% of GDP in 2015. Public debt steadily increased from 77% of GDP in 2010 to 89% of GDP in 2015. So it turned out that the prediction of Chick and Pettifor was shown to be completely

correct, and that of George Osborne entirely wrong. As Chick and Pettifor themselves say:

‘On the basis of what happened to the public debt ratio, our conclusions have been wholly vindicated. ... Rather than improving, the debt ratio has not stopped rising and in 2015 was within a whisker of 90 per cent of GDP’ (Chick and Pettifor, 2016, p. 6).

We have then a successful prediction, which provides yet more empirical confirmation of Keynesianism.

Two further points can be added. First it is usual to think of an economic theory being confirmed because policy recommendations based on it have proved successful. This was indeed the case with Keynes’ theory and the policy recommendations based on it in the Great Depression. However, in the case of Mr Osborne’s austerity policies, we have a different situation. Mr Osborne’s policies were exactly the opposite of those recommended by Keynesianism, and it was the complete failure of these policies to attain their objective that confirmed the Keynesian paradigm.

A second point is that the success of Chick and Pettifor’s prediction in 2011 gives particularly strong confirmation to Keynesianism, because of a principle which has been developed in philosophers of science working in confirmation theory. The principle states that if a theory T predicts an event e, then if e is very improbable on background knowledge but is actually observed, T receives strong empirical confirmation from the success of this prediction. In Gillies, 1998, p. 157, I attribute the basic idea behind this principle to Popper (1959, Appendix \*ix, p. 410), but then go on to show that, with a slightly different formulation from Popper’s, it is accepted by the other main account of confirmation, namely Bayesianism. The standard illustration of this principle comes from physics, and is the following:

‘A nice historical example is provided by the famous test of Fresnel’s wave theory of light. Poisson deduced from this theory that if a ball bearing cast a circular shadow, then, under some circumstances, a bright spot of light should appear at the exact centre of this shadow. This result was regarded as highly improbable on background knowledge, and yet when the experiment was carried out, the bright spot did indeed appear at the centre of the shadow’ (Gillies, 1998, pp. 157-8).

The success of this very surprising prediction was regarded by the physics community as giving very strong empirical confirmation to Fresnel’s theory, and was an important factor in that theory coming to be accepted by most physicists.

Let us compare this prediction in physics with the prediction made by Chick and Pettifor in 2011. As pointed out above, the common sense view was based on the analogy between a household and the country as a whole. If a household falls into debt, it must cut back its expenditure until the debt is paid off. Similarly it is argued (and the argument sounds very plausible): if a country falls into debt, it must cut back its expenditure until the debt is paid off. The claim that cutting back on expenditure will actually increase the national debt runs just as contrary to common sense as the claim that a bright spot will appear in the centre of a dark shadow. In both cases a correct prediction contradicting common sense gives very strong empirical confirmation to the theory on which the prediction is based.

The empirical confirmation of the recent work of Chick and Pettifor shows once again that Keynesianism is scientific. This additional recent empirical confirmation is important for the following reason. It may be that the laws of physics do change, but, if so, it is at a rate,

which is very slow by the scale of human life. Galileo obtained empirical confirmation of his law of falling bodies in the late 16<sup>th</sup> and early 17<sup>th</sup> centuries. In the 400 or so years, which have passed since then, it is most unlikely that the law of falling bodies would have changed. On the other hand, the world economy has changed out of all recognition during this time. Economic theories can only apply to a particular economic system, and may therefore no longer apply if that economic system changes. Keynes' theory applied very well to the UK and US economies in the period 1930 to 1945. It was empirically confirmed by data about these economies and was shown to be perfectly scientific. Since 1945, however, there have been very considerable changes to both the UK and US economies. Above all, there has been the rise of the transnational companies that now have a dominant role in the economies of both countries. It is thus no longer certain that Keynesianism still applies today. However, the work of Chick and Pettifor shows, that, despite the changes mentioned, Keynesianism does still apply. A theory in this paradigm has been empirically confirmed just as much by recent evidence as by earlier evidence, and so should definitely be regarded as a scientific.

## 5. Conclusions

In section 4 I have argued that that Keynesianism is scientific, while in section 1 I argued that all paradigms in economics, including the Keynesian, are ideological. If both these claims are correct, it follows that a paradigm (or theory) in economics can be both ideological and scientific. This conclusion may seem surprising at first, but, if we consider the criteria of being ideological and the criteria for being scientific, it becomes clear that both sets of criteria can be satisfied in a particular case. A theory is ideological if its conclusions, e.g. government intervention in the free market can be justified, form part of a political ideology contradicted by other political ideologies; and also if some of its theoretical concepts, e.g. a shortfall in investment, involve values which are ideological in character. By contrast a theory is scientific if it is empirically confirmed by data. Now reflection shows that it is perfectly possible for a theory to satisfy the criteria for being ideological, but also those for being scientific.

This point is an important one, because many economists, e.g. Friedman and Robbins, have thought that a theory being ideological prevents it from being scientific. They have therefore tried to create a 'positive economics' which consists of factual statements involving no values or ideology. These attempts have ended in failure. In fact the 'positive' economics of both Friedman and Robbins turns out to involve a strong right wing ideology. Moreover the attempt to construct a positive economics was unnecessary since an economic theory can be scientific even if it is ideological.

Although the Neoclassical paradigm cannot constitute a positive economics, Neoclassical economics might, like Keynesianism, be scientific as well as ideological. To investigate whether this is the case lies beyond the scope of this paper, but some material relevant to the question is contained in Gillies (2012).

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SUGGESTED CITATION:

Gillies, Donald (2020) 'The Empirical Success of Keynesianism' *Economic Thought*, 9.1, pp. 24-43. <http://www.worldeconomicsassociation.org/files/journals/economicthought/WEA-ET-9-1-Gillies.pdf>