TWO PUZZLES REGARDING THE REPLACEMENT RATIO IN THE CONTEXT OF RENEWAL THEORY

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DISCUSSION PAPER No. 201
June 2010

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Abstract

The models Feldstein and Rothschild, on the hand, and Jorgenson on the other adopted in 1974 to highlight the replacement ratio are identical. Yet, the authors reached opposite conclusions and that view prevailed, which is weaker in terms of theoretical and empirical foundations. This paper argues that both puzzles may be resolved by reference to the differences in the methodological preconceptions of the authors involved, the operational advantages of the theorem of proportionality, the accumulated data that facilitate research, the inertia of the status quo, the lack of a model leading to a more useful theorem, the lack of communication among economists and their aversion toward complex solutions and policy prescriptions. In this light it is concluded that the time has come for research efforts to be directed towards constructing and testing models in which the useful life of capital is determined endogenously in the presence of embodied technological change.

JEL Classification: B21, B41, E22

Keywords: Proportionality hypothesis, useful life, renewal theory, durability aggregation, replacement, instrumentalism, formalism.

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1. Introduction

Much of what we know about the structure and stability of contemporary economies may be meaningfully related to certain key ratios. When Klein (1962, p.183) was writing, his list of great ratios included: the consumption-income ratio (propensity to consume), the capital-output ratio (acceleration principle), the labor's share of output (income distribution), the ratio of cash to income (reciprocal of velocity of circulation), and the capital-labor ratio (fixed factor proportions). From this account it follows that at that time economic theorists and econometricians conceived of investment as additions to the capital stock that were induced by changes in output through a Koyck type adjustment mechanism. Actually, as it may be ascertained from Haavelmo's (1960) treatise on the subject, there was no theory of gross investment, whereas the body of theory on replacement investment emanating from the seminal contributions by Hotelling (1925), Preinreich (1940), Terborgh (1949), Smith (1957) and others, was considered unsuitable to serve as microeconomic foundations for constructing a comparable aggregate theory.[1]

This disparate state in the theory of investment started to change with Smith (1961, p.166). In particular, to formulate a model of replacement investment based on rational choice, he postulated that the capital-using firm behaves as if to minimize:

$$C = (m + bT)x + (\widehat{\delta} + aT + q/T + rq)K, \tag{1}$$

where the various symbols have the following meanings: $C =$ total current cost; $x =$ variable input like the amount of energy consumed; $K =$ stock of durable goods; $T =$ useful life of the stock of durable goods; $m =$ unit cost of variable input; $q =$ purchase cost of the stock of durable goods; $b =$ age related rate of deterioration in the usage of the variable input; $a =$ age related rate of deterioration in the services from the incumbent durables due to embodied technological change in newer vintages; $r =$ a constant rate of interest, and $\widehat{\delta} =$ a constant non-age related proportional rate of deterioration in capital services. Looking closer at this expression observe that the efficiency of capital declines for three reasons. The first of them is that as capital ages it may require more inputs of materials, energy, maintenance, etc., in order to yield the original level of output. This effect constitutes the so-called *input decay* and is captured in the model by the age-related term bT. The second reason has to do with *output decay* and springs from the observation that as capital ages it may become less efficient due to normal wear and tear. Even though this effect is age-related as well, in the model it is stipulated to be a proportion $\hat{\delta}$ of the outstanding capital stock. Finally, the third reason relates to technological change and implies that as the capital in place ages it becomes inferior relative to new capital that embodies the most recent advances in science and technology. This effect is identified as *technological obsolescence* and in the model it is approximated by the term aT. From this formulation it turns out that the only part of replacement investment that was conceived as proportional to capital stock was to counterbalance *output decay* and it was adopted only as a convenient mathematical approximation. Otherwise the model was very general because it accounted for losses in the efficiency of capital services from all possible sources of physical and economic deterioration.

Soon after this remarkable conceptualization of the fundamental replacement problem there appeared a highly influential paper by Jorgenson (1963) where in terms equivalent to (1) he demonstrated that:

$$C = mx + (\hat{\delta} + rq)K = mx + q(r + \delta)K \,. \tag{2}$$

But this restatement of the problem constituted a major break from all past endeavors in at least one crucial respect.[2] This was that, by abstracting completely from the impact of input decay and technological obsolescence and attributing all deterioration to output decay, which evolved at the constant proportional rate δ, replacement was rendered invariant with respect to the useful life, T. Understandably therefore the justifications that warranted this far-reaching departure from the received economic theory of replacement were of particular importance. In this regard, here is how Jorgenson supported his assertion that the rate of deterioration of capital services and hence of replacement investment is a constant proportion of the capital stock:

> "…The justification for this assumption is that the appropriate model for replacement is not the distribution of replacements of a single investment over time but rather the infinite stream of replacements generated by a single investment; in the language of probability theory, replacement is a recurrent event. It is a fundamental result of renewal theory that replacements for such an infinite stream approach a constant proportion of capital stock for (almost) any distribution of replacements for a single investment and for any initial age distribution of capital stock. This is true for both constant and growing capital stocks…" (p. 251).

Thus, based on the claim that it could be derived from renewal theory and the determination with which Jorgenson (1965) returned to support it empirically, the notion that replacement investment is a constant proportion of the outstanding capital stock begun to be accepted as a proposition of general validity.

However, at the same time, there started to appear evidence, which raised serious doubts as to whether this proposition applied in reality. In the United States, for example, such evidence was offered by Walker (1968) and Wykoff (1970), who looked into the scrappage and the price-age profiles of automobiles, respectively, and Feldstein and Foot (1971) and Eisner (1972), who investigated the variability of the replacement investment-capital stock ratio in the manufacturing sector. What these research efforts showed was that the replacement ratio varied systematically with changes in conventional economic forces. So the literature entered into a state of uncertainty because either Jorgenson's claims were unfounded or the evidence from the above empirical studies was marred by erroneous shortcomings.

In view of this ambiguity, theoretically oriented research efforts were expected to intensify.[3] True to this expectation, Feldstein and Rothschild (1972/1974) turned their attention in this direction. As a result, until their discussion paper was published two years later, the tide seemed to be turning in favor of the view that a constant replacement ratio could be obtained from renewal theory under so restrictive conditions that it might hold in reality only by numerical accident. But in the same period Jorgenson (1974) came out roaring with a powerful defense of his earlier claims. In particular, he established that a constant replacement ratio could be derived from renewal theory under quite general conditions (henceforth to be referred to as the "theorem of proportionality" or just the "theorem") and ever since this result has influenced economic theory and policy as if the arguments put forth by the former authors were irrelevant or misplaced. Thus what I wish to do here is to revisit that very important debate and try to set the record straight in light also of the findings in Bitros (2009a; 2009b), where I survey and assess the voluminous theoretical and empirical literature in this area.

To this effect the present paper is organized as follows. The first task is to preclude the possibility that the controversy emanated from technical reasons. In doing so Section 2 looks at the models that were adopted in the two studies and ascertains that they are identical. Section 3 describes how the authors employed their models to obtain necessary and sufficient conditions for replacement investment to be proportional to the outstanding capital stock and assesses the standing of the theorems they derived. Having excluded that the controversy is due to flaws in the analyses, Section 4 turns for clues to elucidate the first puzzle. In particular, it explains how the sharp conflict in the conclusions the two sides in the debate drew from exactly the same model may be reconciled by appeal to the methodological preconceptions of the researchers involved. Section 5 addresses the second puzzle, which has to do with the realization that, despite the serious criticisms it has been subjected to, the theorem has come to dominate economic theory and econometric applications. It does so by looking into the shifts that took place in the methodology of economics, the difficulties in formulating an alternative model of depreciation and replacement, etc. Finally, Section 6 closes with a summary of the main findings and conclusions.

2. The models in the two studies

Jorgenson (1974, pp. 211-213) assessed the empirical evidence that Feldstein and Foot (1971) had discovered against the theorem of proportionality and rejected it on the grounds that they had failed to define and measure the stock of capital consistently. But he ignored completely the results that Feldstein and Rothschild (1972/ 1974) had obtained using a model grounded in renewal theory.[4] From this observation one would be tempted to surmise that he did not find any fault with their model. On the other hand, after the appearance of Jorgenson's (1974) contribution, the latter authors did not care to revisit the puzzle that emerged, and hence one would be tempted again to surmise that they did not find any fault with his model either. Therefore, any attempt to reconcile their contradictory claims regarding the nature of the replacement ratio in the context of renewal theory must start with a description of the models in the two studies.

2.1 Rules, conventions and definitions of variables

On the way to this task, it is convenient to start with Table 1 below, which explains the rules, the conventions and the symbols used to denote the variables and the parameters in the two models. The rules and the conventions, which might affect the results, are shown in the top half of the table. From them it turns out that the only difference is in the length of time required for installed investment to become productive. In particular, notice that whereas in the Feldstein and Rothschild model (henceforth to be referred to as the F&R model) installed investment becomes productive in the next period, in Jorgenson's model (henceforth to be referred to as the J model) investment becomes productive as soon as it is installed. This difference though has to do only with the indexing of the installed vintages of investment and hence it leaves the results unaffected.

Table 1 Rules, conventions and symbols adopted in the two models

	Feldstein & Rothschild (1974)[1]	Jorgenson's (1974)
Rules and conventions		
Measurement of capital	Efficiency units	Efficiency units
Amount of services by a unit of capital in the first year of its life	1	1
Vector of surviving capital goods of various ages in the capital stock[2]	$M(t)$	…
Investment becomes productive in period	$t+1$	t
Symbols		
Stock of capital	$K(t)$	K_t
Percentage of surviving efficiency of vintage investment relative to the original	s_υ	d_τ
Surviving capital from vintage investment	$M_\upsilon(t)$	$d_\tau A_{t-\tau}$
Vintage investment	$M_\upsilon(t)(1/s_\upsilon)$	$A_{t-\tau}$
Replacement investment	$R(t)$	R_t
Replacement investment-capital stock ratio	$r(t)$	δ_τ
Mortality distribution defined as:	…	$m_\tau = (d_{\tau-1} - d_\tau)$
Vector of percentage contributions of the capital goods of various ages to the capital stock, age structure of the capital stock	$\alpha(t)$	…

Notes: 1. Henceforth reference will be made to the published paper.

2. Its components are: $M_1(t)\ M_2(t)\ ...\ M_\upsilon(t)\ ...$

2.2 Assumptions

Table 2 describes the assumptions on which the two models are based. Looking downwards at the two extreme right columns, observe that in the F&R model durable goods last for V periods. On the contrary, in the J model they last forever since their useful life is set equal to ∞ and their scrappage is forced through the condition that d_τ tends to zero as τ tends to infinity. Could this difference be responsible for the puzzle regarding the nature of the replacement ratio? The answer is no because drawing on Bitros and Flytzanis (2005) the puzzle has to do not with the possible differences in the level of the replacement ratios, but whether the replacement ratios that result from the two models are constant or variable.

Table 2 Assumptions embedded in the two models

	Feldstein & Rothschild (1974)	Jorgenson's (1974)
Types of durables goods in the stock of capital	Homogeneous	Homogeneous
Source of deterioration of capital efficiency	Output decay	Output decay
Technological obsolescence	Ignored	Ignored
Decay function of vintage investment	Time invariant (s_υ)	Time invariant (d_τ)
Re-investment opportunities	Ignored	Ignored
Services surviving from vintage investment	$s_{\upsilon-1}M_{\upsilon-1}(t)$ $s_1 = 1,\ s_V = 0$ $\upsilon = 2,\ldots,V$	$d_\tau A_{t-\tau}$ $d_0 = 1,\ \lim_{\tau\to\infty} d_\tau = 0$ $\tau = 0,\ldots,\infty$

2.3 Mathematical structure of the models

The main equations of the two models are shown in Table 3. Observe that with the exception of the difference mentioned above regarding the durability of capital goods, the definitional and behavioral equations are identical. However, while in the F&R model the replacement ratio $r(t)$ is expressed in terms of the age structure of the capital stock and the mortality distribution, in the J model the replacement ratio $\hat{\delta}$ is expressed as a weighted average of the vintage replacement

Table 3 Basic definitional and behavioral equations of the two models

	Feldstein & Rothschild (1974)[1]	Jorgenson's (1974)
Stock of capital services	$K(t) = \sum_{\upsilon=1}^{V} M_\upsilon(t)$	$K_t = \sum_{\tau=0}^{\infty} d_\tau A_{t-\tau}$
Replacement investment	$R(t) = \sum_{\upsilon=1}^{V} M_\upsilon(t)(1-s_\upsilon)$	$R_t = \sum_{\tau=1}^{\infty} (d_{\tau-1} - d_\tau) A_{t-\tau}$
Age structure of the capital stock	$a(t) = \frac{1}{K(t)} M(t)$	…
Replacement ratio	$r(t) = \frac{R(t)}{K(t)} = \sum_{\upsilon=1}^{V} \alpha_\upsilon(t)(1-s_\upsilon)$	$\hat{\delta} = \frac{R_t}{K_{t-1}} = \sum_{\tau=1}^{\infty} \delta_\tau \frac{K_t - K_{t-\tau-1}}{K_{t-1}}$

Notes: 1. From now on I will refer to their published version of the paper.

ratios with weights given by the relative proportions of net investment of each age in the beginning of period capital stock.

From the above it follows that, with the exception of their difference regarding the useful life of durable goods, which is unrelated to the puzzle under consideration, the two models are identical because they use the same rules, conventions, definitions and assumptions. Hence, even though the model was applied differently, i.e. by Feldstein and Rothschild (1974) to highlight the relationship of $r(t)$ to $\alpha_{\upsilon}(t)$ and s_{υ} and by Jorgenson (1974) to address the relationship of $\hat{\delta}$ to various distributions of δ_{τ}, the difference in their analytical approaches should reinforce rather than lead to conflicts in the results. So let us see whether this is indeed the case.

3. Main results

The authors employed their models to tackle two issues. These were, first, to obtain necessary and sufficient conditions under which $r(t) = r$ and $\hat{\delta}=\delta$, and, second, to assess the applicability of these conditions in real world situations. The plan here is to present the results that they obtained with regard to the former issue.

3.1 Feldstein and Rothschild (1974, pp. 397-399)

Observe from the middle column of the last row in Table 3 that the replacement ratio would be constant if: a) either $r(t)$ is independent of the age structure of the capital stock, $\alpha_{\upsilon}(t)$, or b) the latter assumes only certain limited values. Consider first the conditions for $r(t)$ to be independent of the age structure of the capital stock. This would transpire if:

$$r(t) = \sum_{\upsilon=1}^{V} \alpha_{\upsilon}(t)(1 - s_{\upsilon}) = r \text{ for all } \alpha_{\upsilon}(t) \geq 0 \text{ such that } \sum_{\upsilon=1}^{V} \alpha_{\upsilon}(t) = 1. \quad (3)$$

On close inspection it is easy to ascertain that (3) would be satisfied if and only if:

$$s_{\upsilon} = s, \text{ for } \upsilon = 1, 2, \ldots, V. \quad (4)$$

Now from Table 2 it is seen that $s_V = 0$. Hence, (4) can hold only for $V = 1$ or $V = \infty$. In the former case, capital would last only for one period and the problem would become economically uninteresting. So the authors exclude it from further consideration. As for the later case, i.e. the case in which capital lasts forever, (4) implies constant exponential output decay. This proves:

> ***Theorem 1***. *The necessary and sufficient condition for the replacement ratio to be independent of the age structure of the capital stock, and thus give rise to* $r(t) = r$, *is that all capital must deteriorate at the same constant exponential rate.*

If output deterioration is not exponential, in order for the replacement ratio to be constant, the age structure of the capital stock must remain unchanged throughout the horizon of the renewal process. So what these authors investigated next was the conditions under which $\alpha_{\upsilon}(t)$ remains constant. In doing so they focused on the solution of the equation:

$$M(t+1) == B[q(t)] \cdot M(t)\ . \qquad (5)$$

where $q(t)$ is the ratio of gross investment to the capital stock, called the expansion coefficient, and B is a matrix given by:

$$B[q(t)] = \begin{bmatrix} q(t) & q(t) & q(t) & \cdots & q(t) \\ s_1 & 0 & 0 & \cdots & 0 \\ 0 & s_1 & 0 & \cdots & 0 \\ \vdots & & & & \\ 0 & 0 & 0 & s_{V-1} & 0 \end{bmatrix}. \qquad (6)$$

To this effect, they applied two lines of analysis. In the first they proved the following theorem:

> ***Theorem 2.*** *If a)* $M(0) \geq 0$, $q(t) > 0$, $s_{\upsilon} > 0$ for υ=1,$\cdots$,V-1, *and b)* $q(t) = q$ *for all t, there is a non-negative vector* $E(q)$ *such that* $\|E(q) - a(t)\| \to 0$.

This implies that, if the capital stock does not decay exponentially, $r(t)$ converges eventually to the constant r only in the very special case in which gross investment is a constant fraction of the capital stock and in which the capital stock eventually grows at a constant exponential rate.

In the second line of analysis their attention turned to the converse of the above theorem and the one below summarizes the results:

> ***Theorem 3.*** *If a)* $M_1(t) \geq 0$ for all t, *b)* $0 < q \leq q(t) \leq \overline{q}$ for all t, *and c)* $Lim_{t\to\infty}\alpha(t) = \alpha$, *then the sequence* $q(t)$ *converges.*

What it asserts is that the age structure of the capital stock is or tends to a constant only if the sequence of expansion coefficients also converges to a constant. By implication, once again, but in a more important way, they ascertained that if the deterioration of capital is not exponential, the replacement ratio tends to a constant only in the very special case in which gross investment becomes a constant fraction of the capital stock and therefore in which the capital stock eventually grows at a constant exponential rate. With the above in mind, let us turn now to summarize the results that were obtained in the second study.

3.2 Jorgenson (1974, pp. 191-204)

As in the above case, Jorgenson investigated the conditions under which the sequence of vintage replacement ratios δ_τ, for τ=1,2,$\cdots$, converges to $\hat{\delta} = \delta$ for exponential and non-exponential output decay functions.[5] To illustrate the former case, he assumed that the decline in the relative efficiency of capital follows the geometric distribution:

$$d_\tau = (1-\delta)^\tau \text{ for } \tau=0, 1, 2,\ldots; \tag{7}$$

Inserting (7) into the mortality distribution yields:

$$m_\tau = d_{\tau-1} - d_\tau = \delta(1-\delta)^{\tau-1}. \tag{8}$$

Next, using (8) in conjunction with the definitions of R_t and K_t gives:

$$R_t = \sum_{\tau=1}^{\infty} \delta(1-\delta)^{\tau-1} A_{t-\tau} \ , \tag{9}$$

$$K_t = \sum_{\tau=0}^{\infty} (1-\delta)^\tau A_{t-\tau} \ . \tag{10}$$

Consequently, the change in the capital stock may be written as:

$$\begin{aligned} K_t - K_{t-1} &= A_t - R_t = A_t - \sum_{\tau=1}^{\infty} \delta(1-\delta)^{\tau-1} A_{t-\tau} \\ &= A_t - \delta K_{t-1}. \end{aligned} \tag{11}$$

This proves that the replacement ratio is equal to δ.

Next, he went on to investigate the more general case of non-geometric mortality distributions. He did so in four regimes involving: a) a single investment with fixed capital; b) multiple investments with fixed capital; c) a single investment with changing capital, and d) multiple investments with changing capital.[6] The results are summarized in Table 4 below. From them it

Table 4 Results for non-geometric mortality distributions

<table>
<tr><th></th><th>Single investment</th><th>Multiple investments</th></tr>
<tr><td>Constant capital</td><td>If sequence $\{\delta_\tau\}$ is non-periodic:[1]
$$\delta_\tau = \frac{1}{\mu}$$If sequence $\{\delta_\tau\}$ has period θ:
$$\delta_{\tau\theta} = \frac{\theta}{\mu}$$</td><td>If sequence $\{\delta_\nu\}$ is non-periodic:
$$\delta_\nu = \frac{1}{\mu}$$If sequence $\{\delta_\nu\}$ has period θ:
$$\delta_{\nu\theta} = \frac{\theta}{\mu}$$</td></tr>
<tr><td>Changing capital
(Increasing or decreasing)</td><td colspan="2">In all cases:
Gross, net and replacement investment grow at the same constant rate.
The sequence of vintage replacement ratios approaches a constant.</td></tr>
</table>

Notes: 1. The symbol μ denotes the expected value of the time to replacement

turns out that in all cases the replacement ratio is or tends to a constant irrespective of the nature of the mortality distribution, thus leading to:

> ***Theorem 4.*** *Irrespective of whether: (a) the capital stock is fixed or changing, and, (b) it is periodic or not, the sequence of vintage replacement ratios* $\{\delta_\tau\}$ *approaches a constant fraction* δ *of capital stock for (almost) any mortality distribution and for any initial age distribution of the capital stock. The result that the replacement is a constant fraction of the capital stock, which holds exactly for the geometric distribution, holds asymptotically for (almost) any distribution."(p. 195)*

In this way Jorgenson (1974) made good on the claims he had advanced several years earlier in Jorgenson (1963; 1965) to rationalize the use of the hypothesis that replacement investment is a fixed proportion of the outstanding capital stock.

At this point there arises the question: Are theorems 1-3 different from theorem 4? The answer is that all four theorems have been obtained consistently from the same model and that they complement and reinforce each other in asserting that the replacement ratio is a constant fraction of the capital stock or tends to a constant, if and only if the ratio of gross investment to capital stock is or approaches a constant. But then, how can we explain that: a) Jorgenson (1974), on the one hand, and Feldstein and Rothschild (1974), on the other, arrived at diametrically opposite conclusions regarding its applicability; and b) even though the theorem is weak in terms of conceptual and empirical foundations, it has come to dominate economic theory and econometric applications? The task below is to elucidate these puzzles.

4. Towards a resolution of the first puzzle

The protagonists in the debate were already leading authorities in this field. Also, in both studies the theorem was proved from different perspectives by applying a result of renewal theory in the framework of the same model; and moreover, if one expected that the controversy would be settled on empirical grounds, this has not happened. Therefore, the explanation of the first question-puzzle posed above must be sought in reasons other than the status of the personalities involved, the sophistication of the mathematical tools used, or the nature and convincing power of the empirical evidence that has accumulated. With these possibilities out of the way, one based on methodological considerations comes to the forefront. But before turning to it, the following digression is in order.

When Schumpeter (1954) was writing his monumental *History of Economic Analysis*, he characterized the method by which economists study economic phenomena as follows:

> " Economic theory… cannot indeed, any more than can theoretical physics, do without simplifying schemata or models that are intended to portray certain aspects of reality and take some things for granted in order to establish others according to certain rules of procedure. So far as our argument is concerned, the things (propositions) that we take for granted may be called indiscriminately either hypotheses or axioms or postulates or assumptions or even principles, and the things (propositions) that we think we have established by admissible procedure are called theorems" (p. 15).

This passage describes precisely the way theorems 1-4 were obtained. But it does not give any hint as to how economists select better over good "models," nor does it explain why economic theorists who adopt this methodological approach have split into three groups. The first group, called instrumentalists, consists of those who maintain that the appropriate selection criterion is the ability to *predict* the phenomena to which "theorems" pertain, without regard to the empirical validity either of the "models" themselves or the "hypotheses or axioms or postulates or assumptions or even principles" on which they rest.[7] The second group, called realists, comprises all those who place the emphasis on the ability of "models" and "theorems" thereof to *explain* as well as *predict* the phenomena under consideration.[8] In turn, what this requires is that both the "models" and their "premises" must be empirically valid.[9] Finally, the third group, called formalists, is composed of researchers who emphasize the form or language in which the premises and the models are expressed and analysed. For them, the more sophisticated in terms of mathematical and other techniques is the whole process, the more powerful the results.

Now, equipped with these clarifications, suppose that in interpreting their results Jorgenson (1963; 1965; 1974) acted as an instrumentalist, whereas Feldstein and Rothschild (1974) acted as realists.[10] Could the difference in their methodological preferences explain the sharp conflict in their conclusions? My view is that it can shed considerable light. Let us see why. Apparently Jorgenson was interested to explain *net* investment. But the latter cannot be observed directly and the only way to factor it out from *gross* investment is to estimate somehow the *replacement* component. In doing so he had two options. The first was to construct a model like the one embedded in equation (1) and use it to estimate how much of gross investment is undertaken rationally for replacement purposes each period. Even today this would be hard to apply and not alone because of the lack of pertinent data. The second option was to break away from the classical theory of replacement, invoke the theorem of proportionality in lieu of an engineering rule to estimate replacement investment as a fixed proportion of the capital stock, and ignore the possibility that if, for example, the premises in Table 2 do not hold, the applicability of the theorem may be vitiated. By now we know that he adopted the second option, which suggests that he did take a rather instrumentalist posture.

However, for the above explanation to be credible, one must confront the following issue. If Jorgenson acted as a Friedmanesque instrumentalist, he would be expected to accept or reject the theorem according as the evidence confirmed or refuted it. So we must ask the question: are there grounds to support his view that the theorem is consistent with experience? The answer is that over the years Jorgenson himself and his associates carried out extensive empirical tests with data covering various industries and time periods and their steadfast claim has been that the empirical evidence is in line with the theorem. But my assessment in Bitros (2009b) of the voluminous empirical literature showed that the bulk of the evidence is against it. As a result, while there is little doubt that Jorgenson acted as instrumentalist, the lack of a mechanism by which to judge when a hypothesis should be accepted or rejected on the basis of a series of confirmations and refutations based on data from various industries, periods and countries is a problem that remains unresolved to the present day.[11]

By contrast to the above, acting as realists Feldstein and Rothschild would be expected to place the emphasis on the empirical validity of the premises and the structure of the model from which the theorem derives. For, if it fails when a more realistic assumption is substituted for a less realistic one or the structure of the model is made slightly more realistic than it is, then the usefulness of the theorem for explanatory and predictive purposes would be called into question. So let us see whether their conclusions were consistent with this methodological approach by relaxing the patently unrealistic assumption that in the economy there is a single homogeneous type of capital. To proceed it would suffice to recall that actual economies employ innumerable categories of capital goods and that within each category there are old and new ones, which are differentiated by the technological progress that they embody. By implication, the assumption in the model that durable goods are homogenous may be relaxed in at least two ways. That is, first, by recognizing the existence of more than one category of durable goods, which are replaced in a like-for-like fashion, and, second, by allowing durable goods to be replaced by ones that incorporate the most recent advances in science and technology. Feldstein and Rothschild (1974, p. 401) did investigate the former case and found that in a two-sector model without technological change the required necessary and sufficient conditions for the aggregate replacement ratio to be constant are unlikely to be met in reality. Moreover, in the presence of embodied technological change the results presented in Appendix A show that the theorem would not hold even in the one-sector model that Jorgenson adopted. Therefore, Feldstein and Rothschild were justified to conclude that the theorem might apply only by numerical accident.

To summarize, at times it may be possible to resolve theoretical controversies by appeal to the methodological preconceptions of the researchers involved. In the present case we were able to explain the conflict in the views for and against the theorem of proportionality by attributing to the two parties in the debate different methodological inclinations.

5. Towards a resolution of the second puzzle

Feldstein and Rothschild (1974) were neither the first nor the last to criticize the theorem of proportionality. For example, before them Eisner and Nadiri (1968, p. 380) had challenged its validity from both theoretical and empirical standpoints, whereas after them the papers by Zarembka (1975) and Brown and Chang (1976) showed how unlikely are the conditions for it to hold in reality. Yet, their objections as well as those in the voluminous literature surveyed recently by Bitros (2009a; 2009b) failed to win much following. Quite amazingly, and one might say against all odds, the theorem has proved invincible. The objective below is to shed some light on the reasons for its dominance and staying power.

5.1 Tilting of economics towards instrumentalism and formalism

If one searched for clues that would explain the origins of Jorgenson's (1963, p.251) justification of the replacement hypothesis in equation (2), one would find no clear cut evidence linking his methodological views to those of Friedman (1953). This does not imply that he may have not been influenced by the latter's instrumentalist convictions. Rather, from the reference in the first page of his paper to "naïve positivists", the footnote in the same page that he conducted his research while he served as Ford Foundation research Professor of Economics at the University of Chicago, where Friedman was a distinguished member of the faculty, and the grand abstractions from reality that he adopted, the likelihood is that Jorgenson did succumb to instrumentalism. But, at the same time, one should not preclude that he was encouraged to do so by following the trend towards instrumentalism and formalism that was taking hold in economics.

To corroborate the existence and importance of this trend, it suffices to call attention to two fundamental changes in the theory and practice of economics from a methodological standpoint. The first has to do with the shift in emphasis from the models themselves to the form and the techniques for their analysis. Revealing in this regard is the following passage from the presidential address of Leontief (1971) to the American Economic Association:

> "In the presentation of a new model, attention nowadays is usually centered on a step-by-step derivation of its formal properties. But if the author-or at least the referee who recommended the manuscript for publication-is technically competent, such mathematical manipulations, however long and intricate, can even without further checking be accepted as correct. Nevertheless, they are usually spelled out at great length. By the time it comes to interpretation of the substantive *conclusions*, the assumptions are easily forgotten. But it is precisely the empirical validity of these *assumptions* on which the usefulness of the entire exercise depends"(p. 2).

From this we are informed that in the period when the replacement controversy erupted research economists were interested more in the mathematical manipulations by which theorems were deduced, rather than the empirical validity of the assumptions on which the corresponding models were based. Therefore, in this intellectual environment it was not surprising that the abstractions from reality that Jorgenson (1974) introduced in order to prove the theorem of proportionality were received with subdued silence by the proponents of the classical replacement theory and minimal scrutiny from researchers who worked in the neoclassical tradition.

Even more apparent than the above was the second change, which had to do with the usage of mathematics. Reflecting on the findings by Grubel and Boland (1986), here is how Boland (1987) characterized the shift that took place in this regard:

> "Formalism in economics has also been active for several decades although its growth was greatest in the 1970s. The excessive formalization of recent mathematical economics is of immediate concern to many economists today. Much of mainstream economics has been taken over by formalists who are quite willing to assume anything to make their models formally complete. Realism and relevance are virtually of no concern in the many journals which devote most of their space to mathematical economics."(p. 385)

Clearly, from 1963, when it was introduced heuristically into the literature, to 1974, when the renewal result was established as a theorem, economics was becoming increasingly formal and mathematical. In turn this facilitated the acceptance of the theorem and made it difficult for many who objected to its applicability to be heard. Moreover, the contributions by those few who managed to air their objections through major journals were effectively quashed. If there is any doubt about this predicament, one does not have to look further than the results obtained by Zarembka (1975) and Brown and Chang (1976). Both these studies established theorems showing that it is impossible to obtain a measure of "capital-in-general" by aggregating over two or more durable goods that depreciate at different rates. However, even though in the light of this finding the replacement controversy could have been resolved, both studies passed unnoticed.

In conclusion, the objections that Feldstein and Rothschild (1974) and others raised regarding the applicability of the theorem had little chance to succeed because they were addressed from a realist perspective, which was going out of fashion. This again corroborates the conjecture ventured above that the methodological preconceptions of researchers matter a great deal in the resolution of theoretical issues.

5.2 Operational advantages

Equation (7) gives the geometric distribution, which constitutes the discrete analog of the exponential one. Switching for convenience to the latter, the percentage of capital that survives to time τ is given by the so-called reliability function: $R(\tau) = \exp(-\delta\tau)$, for $\tau \geq 0$ and $\delta > 0$. Corresponding to the reliability function there is another function, $h(\tau)$, called the hazard function or instantaneous failure rate function. The relationship between these two functions is $h(\tau) = -R'(\tau)/R(\tau)$, where the prime indicates the derivative of R. Thus in this case:

$$h(\tau) = \frac{\delta e^{-\delta\tau}}{e^{-\delta\tau}} = \delta. \tag{13}$$

Namely, the hazard function does not change over time. This is a unique property of the exponential distribution because it is the only one having a constant instantaneous failure rate. That is why we say that used means of production whose output efficiency deteriorates exponentially are as good as new or, otherwise, that the exponential distribution has no memory. On the contrary, if deterioration follows the reliability function: $R(\tau) = \exp(-\tau^2)$, then $-R'(\tau) = 2\tau\exp(-\tau^2)$ and $h(\tau) = 2\tau$. This implies that, the decline in output efficiency worsens linearly with time and used durable goods are not as good as new. This property indicates that the distribution underlying these reliability and hazard functions has memory.[12]

Viewed in the context of these remarks, the study of depreciation and replacement is far easier under exponential than non-exponential laws of deterioration. To corroborate it, recall from above that under exponential deterioration new units of capital are as efficient as used ones. This implies that, while the quantity of capital units evaporates as by radioactive decay, the output efficiency of those that survive remains intact. As a result, since each surviving unit of capital has the same output efficiency, its age or durability or longevity or service life or useful life is immaterial and it may be ignored. In turn this yields a far-reaching simplification for the following four reasons: a) if all units of capital deteriorate at the same constant exponential rate, in the absence of embodied technological change, producer durables can be consistently aggregated into a measure of "capital-in-general" by invoking Theorem 1; b) the computation of capital stocks at any level of aggregation is greatly facilitated through the perpetual inventory method; c) as Hulten and Wykoff (1981) and Hulten, Robertson, and Wykoff (1989) have pointed out, using a single number to characterize the process of deterioration helps achieve "a major degree of simplification", because it transforms a problem which is essentially non-stationary into a stationary one; and d) depreciation is dual to replacement and thus capital as a factor of production and as a measure of wealth coincide. All these advantages enhance the tractability of the problem and hence they may explain why economic theorists and applied researchers have embraced the theorem with such unquestioned enthusiasm.

5.3 Availability of data

All publicly available information that has accumulated in the post-war period regarding stocks of fixed capital comes in the form of estimates obtained with the help of the so-called perpetual inventory method in conjunction with some assumption about the factor of proportionality, δ. Thus, if an empirically oriented economist wishes to acquire data on certain capital stock series for his research, the chances are that he will be able to get them or to construct them quickly and without much investment in time and resources. On the contrary, if he wishes to compute capital stock series on the basis of another methodology, say, like the one suggested by Prucha (1997), the task would require a significant diversion from the primary purpose of his investigation, and this only if he has the knowledge and the resources to accomplish it. What all this implies is that there is a built-in inertia in empirical research that favors the dominance of the theorem.

Moreover, this inertia is propagated further by the fact that changing over to a new approach would render obsolete much of the investment that has gone into the publication of capital stock series by national and international organizations. Certainly, if these data were produced in the private sector under competitive conditions, one would hope that at some point capital stock series based on a more fruitful approach would start to emerge and perhaps also supply might create its own demand. Yet under the present government driven system of producing and distributing such data, the rate of obsolescence of perpetual inventory based capital stock series is bound to be slow, if not nil. So this may be the hardest impediment to confront, if the incumbent theorem is to give way to one that would provide for an endogenously determined rate of depreciation.

5.4 Inertia of the status quo

How do economists come to believe what they believe, and to alter these beliefs over time? What part do empirical findings play in determining and affecting this web of beliefs? These are the two questions that Goldfarb (1997) posed and tried to elucidate by undertaking a detailed comparative assessment of the results in several fields of economics. At the end he concluded that:

"The relative fragility of empirical findings suggested by the existence of so many

> 'emerging recalcitrant results' makes it more likely that theoretical preconceptions will be relatively impervious to empirical onslaughts."(p. 238)

But is the empirical evidence regarding the replacement ratio fragile? According to the assessment presented by Bitros (2009b), it is anything but fragile. More specifically, in the four decades from Jorgenson (1963) to Bu (2006) there appeared over 60 studies, which tested the theorem at different levels of aggregation using various methodological approaches, sets of data, and estimating techniques. From them not more that 5% might be classified as inconclusive, around 12% confirmed the theorem, whereas in the remaining 83% it was refuted with considerable degrees of confidence. From these figures it follows that the empirical evidence is overwhelmingly against the theorem and that, if this were the case in the hard sciences, the theorem would have been abandoned long ago. Hence, that this has not happened indicates that, aside from the processes already mentioned above, there may have been at work even stronger forces of inertia.

One of these forces may have been the view that the beliefs of economists are determined by theoretical considerations. Hirschman (1970, pp. 67-68) introduced it into economics by drawing on the ideas about scientific revolutions advocated by Kuhn (1962). Its main argument is that a theory can be beat only by another theory, and not alone by "data". Or, expressed differently, a theory is not set aside due to conflicts in its predictions with reality, but because another theory is in better alignment with experience. Therefore, perhaps, research efforts aimed at falsification of the theorem by reference to "data" would have proved more successful in preventing its dominance in contemporary economics, if they had been oriented towards building a model leading to a more fruitful theorem.

Another force may have been the way in which graduates of economics departments, particularly in the United States, are taught and advance their academic careers. A cursory view in the curricula of leading universities would suffice to reveal that they pay lip service to education in the methodology of science. In my years of graduate education one might chose methodology as one of his fields and even write his Ph.D. dissertation in this area. However, since then related courses have dwindled to extinction and mathematicians and engineers have taken over the education of academic economists, neglecting the concerns that previous generations of economic theorists expressed about the proper approaches to confirmation or refutation of theoretical propositions in economic research. Hence, drawing also on the findings by Goldfarb (1995), it is not unlikely that the bias towards neoclassical replacement theory in the education of academic economists and in the publication of their research papers by leading economics journals may have played a significant role in the survival of the theorem over the onslaught of the empirical evidence referred to above.

Lastly, a significant source of inertia may have been the lack in economics of an apparatus by which to keep track of the empirical refutations and confirmations of a theorem and combine them into an index of acceptance or rejection. Very illuminating in this regard are the following views that Koopmans (1979) expressed in his 1978 presidential address to the American Economic Association:

> "The "if … then … " statements are similar to those in the formal sciences. They read like logical or mathematical reasoning in the case of economic theory, and like applications of statistical methods in the case of econometric estimation or testing. The heart of substantive economics is what can be learned about the validity of the "ifs" themselves, including the premises discussed above. "Thens" contradicted by observation call, as time goes on, for modification of the list of "ifs" used. Absence of such contradiction gradually conveys survivor status to the "ifs" in question. So I do think a certain record of noncontradiction gradually becomes one of tentative confirmation. But the process of confirmation is slow and diffuse….
> I have not found in the literature a persuasive account of how such confirmation of premises can be perceived and documented. How do we keep track of the contradictions and confirmations? How do we keep the score of surviving hypotheses? And what are we doing in those directions…. Meanwhile, unresolved issues, sometimes important from the policy point of view, and mostly quantitative ones, drag on and remain unresolved. Do they have to?"(pp. 11-12)

The answer to the last question is that certainly important issues do not have to remain unresolved and this explains my research in Bitros (2009a; 2009b) regarding the replacement ratio. However, before economists acquire the mindset of researchers in the hard sciences, it will take a variety of changes along the lines suggested by Teixeira (2007).

5.5 Lack of a better model

The tasks Feldstein and Rothschild (1974) pursued were first to obtain necessary and sufficient conditions for the theorem of proportionality to hold, and, second, to establish that these conditions are unlikely to be met in reality. As indicated in the preceding, perhaps their research efforts would have proved more successful if they had presented a model leading to another more fruitful theorem. Yet this was not their plan and the field remained without an appropriate model that would challenge the established orthodoxy. Therefore, given that the theorem of proportionality has survived the massive empirical evidence against it, the time is quite ripe to redirect research efforts towards building a model capable to *explain* as well as *predict* replacement investment; In other words, to expand on the efforts of researchers in the tradition of the classical theory of replacement.

The starting point in this endeavor is to recognize that from a methodological standpoint successful research in empirical sciences quite often involves reviewing an established model and dethroning its non-reliable assumptions. In the present case, Table 2 shows that the model from which the theorem derives is based not on one but at least on three such assumptions. Consequently, a model in which they would be relaxed has good prospects to make a significant contribution in the field. Working in this direction, Bitros (2008; 2009c) constructed a model in which all three assumptions are replaced by premises much closer to reality. For example, in this model two types of capital heterogeneity replace the assumptions that capital is homogeneous and that there is no embodied technological change. The one type of heterogeneity distinguishes durable goods into two categories according to their use, whereas the other differentiates durable goods within each category on the basis of the amount of technological change that they embody. Its analysis has shown that the theorem of proportionality fails (See also the proof in Appendix A). Moreover, it is argued that it ignores several thorny conceptual and methodological issues; it may have restrained seriously the progress towards developing models of capital based on more general approaches to production, and not the least that it is alien to the thinking of researchers in industrial organization, operations research, operations management and other fields neighboring to economics that treat the durability of capital goods as a choice variable.

5.6 Other reasons

. The last sentence implies that the theorem's dominance may have been facilitated by a lack of communication among economists working on depreciation and replacement who operate at various levels of theoretical abstraction and empirical sophistication and across subdisciplines. Following Lipsey (2001, p. 173) this is a genuine possibility because segregation and isolation among researchers in economics leads frequently to two undesirable consequences. First, accepted results emerge that are in fact anomalous.[13] Second, there is resistance to correction of these anomalies because, compared to the natural sciences, anomalous results in economics take longer to recognize and remedy.

Relevant are also two other reasons that Lipsey (2001) analyzed under the headings: a). Elegant error is often preferred to messy truth; and b) Economists often prefer theories that produce unambiguous policy results to theories that do not, irrespective of their evidential bases. To see why these attitudes may have been at work in the present case, consider the following. Bitros and Flytzanis (2002; 2009) and other researchers have demonstrated that the analysis of depreciation and replacement in a non-stationary environment yields solutions that are quite messy and policy prescriptions that are hardly clear-cut. On the other hand, from Eisner and Nadiri (1968, p. 380) to Prucha and Nadiri (1996), numerous studies have shown that, by invoking the theorem to reduce a problem that is essentially non-stationary into a stationary one, we may commit errors of unknown magnitudes and directions. In a similar situation in the natural sciences, the possibility of such substantial deviations from observed reality would encourage scientists to shift research approaches. But not in economics because, according to the above precepts, the mindset of economists is such that they prefer the certainty of simplicity over the uncertainty of complexity, even when their choices are liable to be accompanied by errors. In this light then it is not surprising that economics has remained locked for so long in the confines of the stationary neoclassical theory of depreciation and replacement.

6. Summary of findings and conclusions

On account of their assumptions, conventions and definitions, the models that Feldstein and Rothschild (1974), on the one hand, and Jorgenson (1974), on the other, adopted to investigate the nature of the replacement investment-capital stock ratio turned out to be identical. Moreover both were cast in the context of renewal theory. But the authors used them to highlight the issue from different analytical perspectives. In particular, whereas the former authors focused on the relationship of the replacement ratio to the age structure of the capital stock and how the process of deterioration affects it, the latter author addressed the implications for the replacement ratio of various distributions describing the decline in the output efficiency of the capital stock. Thus, given that the theorems derived from the model were complementary and reinforced each other, one would have expected the authors to arrive at roughly similar conclusions. Instead not only did they reach diametrically opposite conclusions, but also in the controversy that erupted the view prevailed which was weaker in terms of conceptual and empirical foundations. In this light the task set in this paper was to highlight the reasons that may have been responsible for these two puzzles.

To resolve the first one, which concerned the sharp conflict in their conclusions, an appeal was made to the presumed methodological preconceptions of the researchers in the two sides of the debate. In particular, assuming Jorgenson acted as a Friedmanesque instrumentalist and Feldstein and Rothschild acted as realists, one would expect them to conclude exactly as they did. This in turn led to the suggestion that at times it may be possible to settle controversies surrounding theoretical issues by reference to the differences in the methodological approaches to which the researchers involved might subscribe. The second puzzle had to do with the dominance and staying power of the theorem in economic theory and econometric applications, despite the voluminous empirical evidence against it. Our analysis suggested that contributing reasons included the following. First, there was a discernible shift from realism to instrumentalism and formalism in the methodology of economics. Second, the theorem had operational advantages that reduced an essentially non-stationary problem into a stationary one and helped in the construction of series of capital stocks using the perpetual inventory method. Third, the data that accumulated on capital stock series at various levels of aggregation facilitated research in various fields without having to face the difficulty of generating appropriate series from scratch. Fourth, there was the inertia of the status quo, which is fed and sustained by the lack in economics of a process by which to decide when a proposition is in conflict with experience and should be replaced or revised. Fifth, there was lack of a model leading to a more useful theorem than the one under consideration, lack of communication among economists working on depreciation and replacement who operate at various levels of theoretical abstraction and empirical sophistication and across subdisciplines, and aversion of economists towards the solutions and policy prescriptions that would emanate from the non-stationary nature of the problem. Therefore, in the light of these findings, the time has come for research efforts to be directed towards constructing and testing models in which the useful life of capital is determined endogenously in the presence of embodied technological change.

Appendix A

Impossibility of the theorem of proportionality in the presence of embodied technological change

Consider an economy with a representative firm which consists of two lines of production, one constructing an intermediate durable good called capital solely by means of labor and another producing a final good by combining each unit of capital with one unit of labor. In year υ, the firm uses capital capable of producing $K_X(\upsilon)$ units of output X. Usage does not wear out capital because its effects are exactly offset by maintenance. But from the one period to the next $K_X(\upsilon)$ becomes more productive because newer vintages embody the most recent advances in science and technology. So to capture the impact of technological change, let the productivity of $K_X(\upsilon)$ increase at the constant exogenous rate μ_X. Then newer vintages of capital would present a competitive advantage to other firms that might wish to enter into business. For this reason, assume that to deter potential entrants the firm reduces the price of X at the rate of technological change. The question that arises is: Would the theorem of proportionality hold in this economy?

To answer it, I shall use an adaptation of the model presented in Bitros (2008, 2009c).[14] More specifically, assume that the above firm operates as if to maximize the value of its net worth over an infinite number of investment cycles, each of which lasts as many periods as the useful life of its capital T_X. If so, from the analysis in the above mentioned papers, it can be shown that one of the necessary conditions that must be satisfied is given by:

$$\sigma e^{-\mu_X T_X} - \mu_X e^{-\sigma T_X} = (1+\beta\sigma)(\sigma-\mu_X)\,. \qquad \textbf{(A.1)}$$

where σ is the rate of interest and β stands for the minimum labor required for building one unit of $K_X(\upsilon)$. From the expression (A.1), but also from its graphic solution in Figure 1, it follows that the useful life of capital T_X depends, among other economic influences, on the rate of technological change μ_X. This proves that the useful life of capital in this economy would not be invariant with respect to the rate of technological change and thus vitiates the theorem of proportionality.

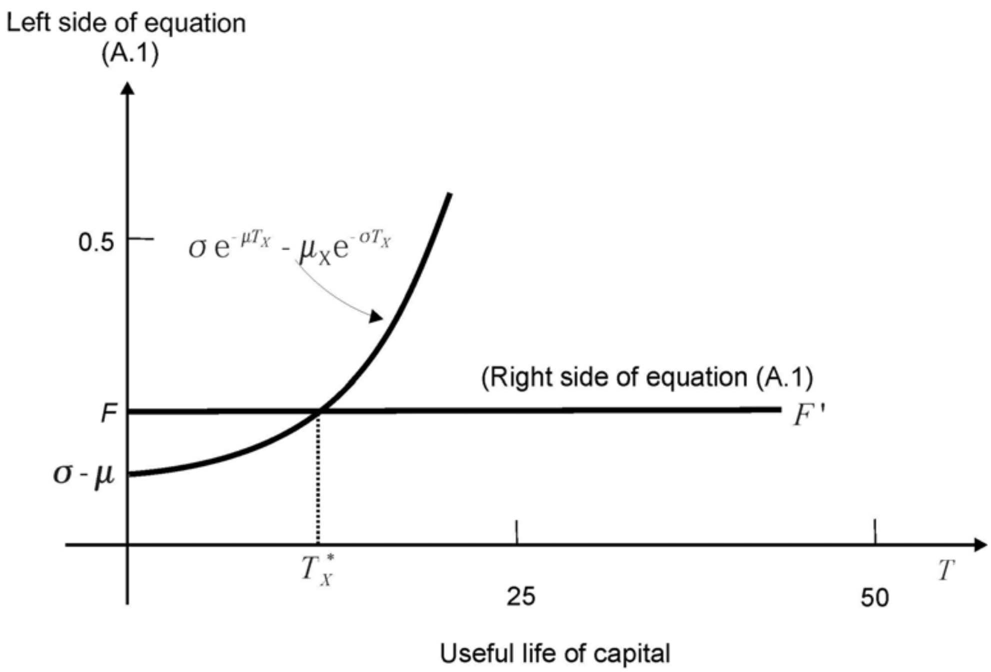

Figure 1

To summarize, in periods of rapid technological change that differentiates consecutive vintages of capital, the rate of replacement investment is not a constant because it depends on the rate of technological change and the rate of interest. The followers of the classical theory of capital and its replacement knew this result. But references to it eclipsed after Jorgenson's (1963) paper.

Appendix B

Contemporary methodological guidelines for research in the empirical sciences

Notwithstanding important disagreements among philosophers of science, what is accepted today as appropriate methodological approach to science can be laid down briefly in the following four principles:

Principle I. A scientific theory (physics, biology, economics, sociology, medicine but NOT mathematics, logic, philosophy and other non-empirical disciplines) must be empirically testable. It must be verifiable said the logical positivists in the 1930's, falsifiable as Popper (1935) insisted then and later. The two are not equivalent: there is an asymmetry between verification and falsification, but that need not bother us here. The important thing is that scientific theories must be empirically testable. We can call this principle, the principle of empirical accountability. No empirical accountability, no science. Instead of science you have metaphysics.

Principle II Some metaphysics is instrumentally useful. It can serve heuristically. One may engage in a 'metaphysical' research programme from which certain empirical hypotheses can be deduced. We may call this principle, the principle of scientific speculation or hypothesis construction. One can use experience or imagination or metaphysical ideas as background; Certainly experience, which serves as background knowledge; But not induction.

Principle III There is no induction. What we call induction is unwarranted generalization from a finite number of observations. Whenever you believe you are using inductive thinking, you are really engaged in an activity described in Principle II above. There are no neutral observations. They are always theory-laden (or theory-impregnated). They contain theory. So you can't use a number of supposedly neutral observations to form a universal theory.

Principle IV What this boils down to is that usually theories (hypotheses) in empirical sciences are to be compared, say T_1 (the old one) and T_2 (the newly proposed one) and we judge their merits and demerits using various criteria. If we opt for T_2 and decide to discard T_1, it will be because the newer one has greater explanatory and/or predictive (or 'postdictive') power.

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Endnotes

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[*] This paper proved quite lucky. First, because it was read by unknown referee(s) who devoted more than their fair share of time and effort in finding even minute blemishes and pointing them out to me; and, secondly, because they went out of their way to suggest ideas as to how I could deal with issues of major concern. Consequently, I am grateful to the editor for his encouragement and patience and to the referee(s) for their generous help. Finally, I wish to extend my appreciation to my friend and colleague A. D. Karayiannis, who read various versions of the paper and helped me improve it significantly.

[1] So indifferent was Haavelmo regarding the usefulness of received replacement theory that he did no make even a single reference to the contributions by these writers.

[2] For the sake of historical accuracy it should be noted that at about the same period other leading contributors to the neoclassical theory of capital adopted various ad hoc approaches to modeling depreciation. For example, Solow (1956) ignored depreciation altogether, whereas Samuelson (1962) introduced proportionality on the grounds that:

> "To keep the alpha good homogeneous independently of age, one has to assume a force of mortality independent of age (or an exponential life table). This means that physical depreciation is always directly proportional to the physical stock of alpha, K_a : Depreciation equals δ_a times K_a where the average length of life of alpha is the reciprocal of the δ_a factor."(p. 197)

[3] At that time prevalent among economists was the view that the only way to beat a theory is by another theory, not by "data" alone. An exposition of the foundations of this view is found in Hirschman (1970, 67-68). However, in the following decades mainstream economists shifted to the view, which is consistent with Friedman's (1953) famous methodology essay, that theories stand or fall on the basis of their ability to predict what the data reveal.

[4] Moreover, it may be of some interest to mention that Jorgenson's (1974) ignored also the sharp criticisms of his arguments by Feldstein (1972/1974) .

[5] Actually Jorgenson used the geometric distribution. He did so on the grounds that he employed discrete analysis. Had he applied continuous analysis, he would have assumed that the decline in the relative efficiency of capital followed the exponential distribution. But the results would have been just the same.

[6] A single investment is defined as one completed all at once. On the contrary, multiple is an investment completed piecemeal over a certain period.

[7] Friedman (1953) introduced this approach into economics following the epistemologist Duhem (1908), who recommended using theories as instruments and without concern if they are true or if their assumptions are realistic. According to the latter, what is important is whether the predictions derived from theories match appearances (phainomena), thus implying that models are useful not as causal explanations, but 'as if' ways of highlighting what appears before us.

[8] Drawing on the debate that took place in the American Economic Review in the 1960s and the subsequent appraisal by Caldwell (1982), one would be justified to conclude that leading authorities in the group of realists were Machlup (1955; 1964) and Samuelson (1963; 1965).

[9] For a brief but more detailed account of the principles that guide contemporary research in the empirical sciences, see the Appendix B.

[10] To be sure, both parties in the debate employed mathematical techniques that were admirably advanced at the time. But in no way can they be categorized as formalists, because their emphasis was not on the techniques of the analysis but on the premises and the structure and the results of their models.

[11] Note that this is exactly the problem that Koopmans (1979, pp. 11-12) and Goldfarb (1997) have discussed and to which I will return shortly below.

[12] In particular, the probability distribution function that underlies the reliability and hazard functions in this case is Weibull with *shape* and *scale* parameters equal to 2 and 1, respectively.

[13] As defined by Lipsey in the above reference, an anomaly arises when conflicts exist within a subject. Consequently, the controversy surrounding the applicability of the theorem of proportionality constitutes a genuine anomaly in economics.

[14] The two-sector model analyzed in these papers is much more general in the sense that it provides for two sources of capital heterogeniety. That is, capital that belongs in different categories, like say laths versus electricity generators, and capital that differs from one vintage to the next, like laths and electricity generators built in 2007 versus those built in 2008.

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