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Tautology and Testability in Economics

J. AGASSI

Economics is a science - at least positive economics must be. And science is in part applied mathematics, in part empirical observations and tests. Looking at the history of economics, one cannot find much testing done before the twentieth century, and even the collection of data, even in the manner Marx engaged in, was not common in his day. It is true that economic policy is an older field, and in that field much information is deployed for the purpose of prescribing a course of action. But this is not to say that the information procured for that purpose is either based on observation or has been tested. In the seventeenth century some alchemists and economists hoped to boost the economy by manufacturing gold, others feared inflation; and the British Parliament legislated against manufacturing gold. David Hume proved in the eighteenth century - quite *a priori* - that doubling the quantity of gold will only double the price of each commodity and he thus set things at rest for a while. Later the question was opened again when Marx, for example, showed historically how the gold robbed from the Americas started Europe's boom. Yet the theory - the quantity theory of money, as it is called - which Hume proved *a priori*, is still contested and still hardly tested to economists' satisfaction: is the price level fixed mainly by the amount of available money? Some economists answer yes, others no.

In the twentieth century, economic testing techniques, econometric techniques, have been developed. Much of their application went to prove that demand curves slope downwards (all things being equal, rising prices do not raise one's disposition to buy), and such profundities. Incredibly, the theory was proven: demand curves do slope downwards, we are told as a result of empirical investigations. Of course, we all know the *a priori* proofs that they do not always. The proofs are as yet unshakeable. Conspicuous consumption, bullish stock markets, and falling prices of staple commodities (Giffin goods) leading to dearer substitutes becoming available within low budgets, these are such obvious cases that there is no need to observe them empirically. But, by and large, the argument goes, demand curves do slope downwards. All this is very reassuring. It can be shown *a priori*, but it is reassuring to hear that it is *a posteriori* too.

This reassurance was shattered by a discovery which is so elementary that it is hard to put it briefly without being misunderstood. One can put it thus: anything provable *a priori* can also be supported *a posteriori*: a count of one's fingers shows that 5+ 5 = 10. In another sense, the opposite may be said: whatever can be proven *a priori* cannot be proven *a posteriori*. This is a stronger sense of ‘proof’ or ‘proof procedure’ which says, what you cannot disprove or refute by experiment you cannot prove or confirm or corroborate by experiment either.

Thus it was that certain economists became interested in sifting the mathematical from the empirical – notably Hutchison and Samuelson, in the late thirties and the early forties.

Two points strike one at once. First, that the discussion was characterized by an unusual obsession with sifting the empirical from the mathematical (or the tautological or the analytic). Second, that the sifting was erroneous. Klappholz and myself, in 1959, applied the general idea Popper expounded in his *Logik der Forschung* (*Logic of Scientific Discovery*) of 1935, to the special case of economics.[1] Our main point was that a theory that is not tautological does not have to be empirical. Our other point is, economists expend much effort in demarcating tautologies. This is because, I suggest, such things do bother them, and for two reasons. First, that most of traditional economics, to say the least, is in a no-man's-land between analytic and empirical; and second, that much of economics is involved with accounting or bookkeeping, which is replete with its own idiosyncratic conventions - and conventions breed tautologies, however idiosyncratic and esoteric.

Consider some examples. Take the law of diminishing returns, so fruitful in suggesting Marx's law of diminishing profits (and increasing misery) and the law of diminishing marginal utility of his more reformist colleagues. The law of diminishing returns says this. If we have more than one production factor - take two - and if we increase the one while keeping the other constant, then a moment will arrive when it will be much more profitable to increase the other rather than the one. Not much is said about this law in the literature, perhaps because it is trite, perhaps because it is somewhere in a no-man's-land, until recently unstudied field of industrial management - i.e. of optimizing on all sorts of production functions. Yet its very triteness fascinates a philosopher. Perhaps it is not much noticed because it is between the analytic and the empirical - since, like so many metaphysical statements - and metaphysics is the paradigm of between analytic and empirical - it says ‘sooner or later’. It looks like it, but it is not.

When Robbins in 1930 ‘proves’ the law, he proves it as eminent commonsense. If it were not true, he argues, then we could grow all the corn the world needs on one field. Here land, as well as water, fertilizers, and perhaps also corn seed, etc., are the production factors. Now, first, there is a small logical ambiguity here: the law may apply to corn and yet be untrue - that is, if it does not apply everywhere it is false. This, of course, does not matter, as long as we remember that corn is here just an example, a pictorial name for any supposed violation of the law, which is soon proven to be false by a *reductio ad* commonsense. Second, metaphysical propositions are often very plausible; yet plausibility may be falsehood, and it often is. But consider this: if all the corn necessary may be grown on one piece of land, would we say the law is false? I suggest we would call the land ‘initial investment’, not a production factor. And instances do exist, even genuinely empirical ones. Bacteria for cheese factories; even land for cheese factories. And so, a precondition for a factor to be a production factor rather than an initial investment - or overhead, for that matter! - is that it obey the law of diminishing returns. Hence the law is a tautology or analytic: a precondition of our attempting to apply it is our knowledge that it applies successfully. To be precise, it is a part of the implicit definition of ‘production factors’ and as such it is a tautology.

So far so good. But here I must report the existence of a very common and oft used, simple test for analyticity - which is demonstrably erroneous.

If we blame ourselves for an unsuccessful application of a law the law is not empirical. And tautologies are not empirical. Hence, economists often conclude, in such cases the law in question is a tautology. But non-empirical statements need not be tautologies, as can easily be proven. Let me give an example.

Firms maximize profits. This is the fundamental law of microeconomics, if not the most fundamental law in all economics. Is it empirical? Let us employ our test. Apply it to our college refectory. If it applies successfully, fire the management. Apply it to the Salvation Army. If it applies successfully even there, then the end of the world is imminent.

And yet we uphold the law and criticize anyone who applies it to charitable firms for either applying it to the wrong firm or for applying it wrongly. The proper application will show that the general theorem of microeconomics is valid: the profits or losses of any firm equal zero: the books of our college refectory are balanced. If not, do not fire the manager - call the police!

There are other examples. Small businesses lose till they consume all their owners' savings, windfalls, etc. Frank Knight argues the losers buy entertainment.[2] Milton Friedman argues that the market ejects them.[3] It looks as if we always defend the theory by qualifying it again and again in the face of counter-evidence. Finally, when all possible counter-evidence is exhausted, the theory looks as if it tells us absolutely nothing, or as if it says firms do as firms do, i.e., the theory seems to reduce to a tautology.

The logical errors I have just paraphrased are very widespread. It is not true, however, that when a theory is reinterpreted, narrowing down its informative content, that content becomes zero. Even if in the process of narrowing it down it finally reduces to zero, it may not be zero yet. Moreover, while at the end of the process the empirical content may, perhaps, become zero, the informative content need not be zero - it can be too low for empirical tests but still too high for tautology.

Samuelson claims (in his *Foundations*) that the maximization principle is empirical because it is not analytic, as he analytically proves.[4] But the error should by now be obvious. The theory is neither analytic nor empirical, but quite metaphysical.

But what if we make income and expenditure matters of accounting and balance our books? Income equals expenditure both in properly balanced books and in perfect competition: of necessity. Does this make the firm maximize? It is hard to say. What exactly the maximization principle amounts to is a question which Friedman opened up,[5] and which is in debate still. To recapitulate: in economics at least a thesis may be lengthily discussed only to turn up later on as a tautology or as a putative tautology. Some other fields of inquiry share this problem. The paradigm, I would suggest, is Euclidean geometry. But such cases are at least complicated or sophisticated.[6]

Let us turn to an economic example that is neither complicated nor sophisticated, yet was lengthily discussed and seldom if ever proven, though most writers now agree that it is a tautology. I mean the quantity equation or the equation of exchange, MV = PT, where M is the quantity of money available, V its average frequency of exchange, P the average price of commodities and T their total volume of transactions. MV is what Hayek calls effective money and PT is the gross national product; the total effective money is, then, the GNP. Tautologously! Heilbroner says in his introductory text *Understanding Macroeconomics* (1965), the quantity equation is 'most famous' as it came to support the quantity theory, that is, we remember, the theory that price level is determined - largely or wholly - by the amount of money available (p. 141).

Heilbroner says it 'is not hard to grasp' that the equation merely equates payments and receipts. He does not give a proof - perhaps because his book is elementary. Eprime Eshag, in his *From Marshall to Keynes* (1963), opens with the demand for money and on page 3 he attributes the quantity-equation to Marshall, though the statement of Marshall that he quotes speaks of the quantity of gold and silver used as money, not of money in general, and certainly Marshall does not prove it even in its narrow version. But then, there is no need for Marshall to have proved it and all Eshag claims is that he knew it; which I suppose he did.

Edwin Dean, *The Controversy over the Quantity Theory of Money* (1965), proves the quantity equation after defining GNP, namely after confining the quantity equation to goods and services; and his proof is not adequate even for this constrained case. He must have noticed this himself, since, later, he defines V = GNP/M or V = PT/M where T stands for the total volume of transactions; from which the quantity equation MV = PT follows at once; that is, after defining V that way, the proof is easy. Why, then, not start there? Because when we define V thus we do not at once have V as a velocity any longer. Or, to use Heilbroner's words, V is no longer defined as 'the number of times per period. . . that an average dollar was spent' (whatever 'an average dollar' may possibly be). And so, MV can no longer be viewed without further proof as 'effective money', to use Hayek's expression; but it was viewing MV as effective money which made Marshall use the equation to calculate the amount of precious metal which serves as money. That is to say, Marshall took MV = PT and deduced M = PT/V. If V only means PT/M as Edwin Dean says, then Marshall was using the equation M = M to estimate the amount of precious metal in circulation. Edwin Dean says, Marshall's definition of the equation of exchange is essentially the same as that of Irving Fisher of 1911! He means, not the 'definition of the equation of exchange' - this expression is simply ungrammatical - but the introduction of the equation of exchange in the form of a definition, or the claim that the equation is a mere implicit definition, or something of the sort. Since his volume contains a reprint of Irving Fisher's original paper, we may just as well glance at it.

Fisher defines V as E/M where E is the volume of exchange. He goes on to prove that E = PT in a closed system with no foreign trade. With foreign trade we have to add MV for domestic trade and MV for foreign trade, and then we get the equation in its generality.

I shall skip Keynes since he is a bit controversial, as my next two quotes will illustrate, I hope. So we arrive, soon after Keynes *Treatise on Money* (1930) to a paper by Hicks, 'A Suggestion for Simplifying the Theory of Money' (1935) - reprinted in the *American Economic Association's Readings in Monetary Theory* (1951).

'To anyone who comes over from the theory of value to the theory of money' confesses Hicks (pp. 13- 14),

> there are a number of things which are rather startling. Chief of these is the preoccupation of monetary theorists with a certain equation, which states that the price of goods multiplied by the quantity of goods equals the amount of money which is spent on them.

This is astoundingly unfair, and one can hardly say Hicks was not familiar with the arithmetic of it all. However, to continue,

> This equation crops up again and again and it has all sorts of ingenious little arithmetical tricks performed on it. Sometimes it comes out as MV = PT; and once, in its most stupendous transfiguration, it blossoms into

$$P = \frac{O}{E} + \frac{I' - S}{R}.$$

This last is Keynes' from his *Treatise on Money*, and is explained in the paper of Villard from which I shall quote next, at the length it certainly deserves. It is not self-evident why Keynes divides windfall from all other income, nor that the equation 'I – S' stands for windfall. Evidently, Hicks is appalled by these frills[7] and evidently he even thinks that the label MV for the designation of the amount of money paid is too frilly, though less so than in Keynes' formula; he prefers, you remember, to say money paid equals price times quantity of goods bought. If, to my mind, untutored in the history of economic thought as it regrettably is, the quantity equation MV = PT blossomed somewhere between 1900 and 1930, then certainly the equation Hicks cites is prehistorical: 'the price of goods multiplied by the quantity of goods equals the amount of money which is spent on them.' I repeat, the quote surely is a piece of knowledge nobody can date. Hicks' ironic tone gets thicker - to such a point that his meaning becomes obscure to an outsider such as myself. ‘Now we, of the theory of value’ he continues after quoting Keynes’ version,

> are not unfamiliar with this equation, and there was a time when we used to attach as much importance to it as monetary theorists seem to do still. This was in the middle of the nineteenth century, when we used to talk about value being ‘a ratio between demand and supply’. Even now, we accept the equation, more or less implicitly, in our systems. But we are rather inclined to take it for granted since it is rather tautologous, and since we found that another equation, not alternative to the quantity equation, but complementary with it, is much more significant, This is the equation which states that the relative value of two commodities depends upon their relative marginal utility.

Note the expression 'rather tautologous' (cp. 'rather pregnant').

Let me quote clarifications. *A Survey of Contemporary Economics* edited by Howard S. Ellis (1948) contains a famous paper on monetary theory by H. H. Villard, himself an economist on the Board of Governors of the Federal Reserve System in 1945-6, and author of a volume on deficit spending. Let me say at once that Villard says in his introduction (p. 314) that in his opinion ‘purely monetary devices for control … were found inadequate’ and so he is hostile to the quantity theory of money. But he starts his discussion (p. 316) by saying both that ‘few analytical devices in economics have been so useful over a long period as the quantity equation of exchange’ and ‘that there can be no analytical objection to [Fisher's] formulation.’ - To be precise, he says, even if there is no objection, application is problematic. At this, a student of scientific method must prick up his ears. A footnote explains.

> If V is the use of M to buy T, T specific items sold for M, and P prices T when sold for M, then the equation [MV = PT] is valid because it is a truism. The charge that the equation was not valid arose because in some earlier presentation use has been made of such approximations as existing price indexes, which rendered the equation formally incorrect.

This is a gem, even if its last expression is puzzling and even if its beginning is a bit vague. True, the equation is a truism. Yet it was evidently viewed as empirical rather than as tautological: it has been hotly contested and no one knowingly contests a tautology. But an error was there: when the tautology was applied, erroneous values were put for P. Of course, other values had to be determined too to show that the values for P lead to mistaken results - including velocities of money. Here, then, in a footnote, Villard outlines a proof of the equation, different from Dean's introduction of it in Fisher's name as a definition of velocity. Villard's discussion is fascinating, and concerns the problem of identification and estimation of the various terms in the equation. I shall only pick one or two raisins from his cake. In a footnote on page 317 he says, still speaking of identification and estimation, I understand, 'Much interesting work has also been done by the Board of Governors of the Federal Reserve System and the Federal Deposit Insurance Corporation, particularly through the periodic surveys of the deposit ownership which are published in the Federal Reserve Bulletin.' This is truly fascinating: a purely pragmatic work, in the service of Mammon, helps clarify an issue which is of profound interest both intellectually and from the viewpoint of economic policy. Indeed, this goes well with Villard's general view and interest. 'For it was a paradox of Keynes' greatness', he says (p. 331), 'that he treated what was a minor clarification of concept as a great new discovery.' Of course, this means that Keynes' clarification was not complete; as Villard puts it (p. 329),

> Had the 'period analysis' character of the difference between saving and investment in [Keynes'] *Treatise* [*on Money*, 1930] been more fully recognized, it is possible that the advent of [Keynes'] *General Theory* [*of Employment, Interest and Money*, 1936]

> would not have been marked by the extended and largely limitless controversy as to whether savings and investment are equal or unequal.

And, of course, here is a similar equation, S = I, which can easily be proven, yet it has been hotly debated.[8]

It seems, then, that Villard has cleared the air: the equation is a tautology, but we can assess each side of the equation with different methods of estimate and so contradict ourselves. Indeed, it is this very possibility that makes the quantity equation so useful - regardless of what one thinks about the quantity theory. For, it offers us ways to make two independent assessments of the same quantity - which is a standard method of double-checking, in science and in practical affairs. This further point is concisely put - for the first time, it seems to me - by Milton Friedman in his essay, 'Money, the Quantity Theory', in the *International Encyclopedia of the Social Sciences* (New York, 1968, vol. 10, 432- 46), where he says (p. 435),

> However M is defined, … [MV = PT] remains valid, provided V is appropriately defined. The issue is one of usefulness of one or the other definition: what definition of M will have the empirical property of rendering the forces determining the other symbols in the equation as nearly independent as possible of those determining M?

Yet I confess I find the proviso 'provided V is appropriately defined' rather puzzling. And for the following reason. As Friedman says (pp. 434-5), MV as well as PT is the total volume of transaction as recorded on both sides of the double bookkeeping record, and this is why they equal each other. This, it will soon be transparent, is enough to define V, once M is given. So I do not quite comprehend the above statement.

There is more to my puzzle than a slip of Friedman's pen. In the same volume, a few pages later, Richard T. Selden makes the same point in his

'Money: Velocity of Circulation' (*op. cit*. 447- 452). And I find his remark puzzling too. He says (p. 447),

> V = PTM. However, the definition does not uniquely define velocity, since it fails to specify the meaning of 'spending' and 'money'. Actually, economists have worked with several broad types of velocities, and with countless minor variations thereof.

No doubt, Selden's first point is quite valid. Before we define transaction and money, we cannot speak of the velocity of the circulation of money in the market. The natural continuation of this point, then, should be obvious: define transaction and money, and you have defined circulation. This, however, is not what Selden says. And I think he is in error. What economists do is not, as Selden says, define the velocity of money in different ways; rather, as Villard says, they assess it different ways. It is admittedly possible, in principle, to define and even measure velocity of an undefined entity, and then look and see what that entity is. That process, however, is quite different - as works of Frank Paish may illustrate.[9]

The situation is quite puzzling. There is no doubt that Friedman is as fully in command of the material as possible. Indeed, he sees no problem at all in the tautologous character of the quantity equation. In his 'A Theoretical Framework for Monetary Analysis' (*Journal of Political Economy*, 78, 1970, 193-238) he says (p. 197) again and emphatically that the quantity equations

> are intended to be identities - a special application of the double-entry book-keeping, with each transaction simultaneously recorded on both sides of the equation. However, with the national income identities with which we are familiar, when the two sides, or the separate elements on the two sides, are estimated from independent source data, many differences between the two sides emerge.

This is the same view as Villard's. It is incredible to me that when presented in general the matter is so clear, yet I have found no detailed presentation which is not puzzling. Friedman himself seems to notice that confusion on this matter is puzzling; for, he seems to blame it on the general stupidity of mankind. He says (p. 193),

> the quantity theory of money … has probably been 'tested' with quantitative data more extensively than any other set of propositions in formal economics - unless it be the negatively sloping demand curve …

The downward slope of the demand curve may indeed be declared a tautology, if we confine ourselves to the normal and if the upward slope is declared abnormal; if, in addition, the non-economic motive is *a priori* excluded, etc. A better approach is to declare this a non-tautology which, however, is not worth testing. The quantity theory of money can be identified with the tautologous quantity equation - or else it is anything but a truism. Friedman, clearly, speaks of the 'tests' of the tautologous quantity equation as the frequent substitutes for tests of the quantity theory. These are the facts; they are peculiar to economics, and they are rather puzzling.

Perhaps the most interesting example, and my last, is Don Patinkin's charming 'The Chicago Tradition, the Quantity Theory, and Friedman' (*Journal of Money, Credit, and Banking*, I, 1969, 46- 70), where Patinkin contrasts the literature from the Department of Economics of Chicago University with both Friedman's written reports and his own personal impressions of the strength of that department (which, no doubt, made history). Patinkin's own impression is that there was a strong and beneficial 'oral tradition' there, the crux of which was the training to distinguish clearly between hypotheses and tautologies of similar appearances. Patinkin quotes, as an illustration, from a lecture by Lloyd Mints, delivered in Chicago in 1944 (p. 55):

> Some attempts to verify quantity theory … have to establish *causal relationship*. But formula itself is a truism – doesn't need verification. Formula $\neq$ quantity theory.

It seems quite obvious to me that had the proof idea about the double entry in double bookkeeping been presented in greater mathematical detail, there would be no need for all the stresses, emphases, subtle arguments, and nice distinctions which abound in the literature, and which have been exemplified here. And so, though neither an economist nor a mathematician, I venture to labor the obvious and offer a rather detailed proof - detailed in steps, not in any qualification to any specific case, not a proof by case after case.

We first assume that in every transaction payment equals receipt. We second assume that every payment involves n - one or more - units of money, each having a numerical value m, so that the total payment is the sum of this numerical value $\sum_{i-1}^{n} m_i$. Similarly every receipt is of a few items r - one or more - each having its price p, so that the value of the receipt is $\sum_{j=1}^{r} p_j$. Then, $\sum_{i-1}^{n} m_i = \sum_{j=1}^{r} p_j$. If we have a few kinds of commodity, we may find it easy to designate the quantity of a given commodity as q and write $\sum_{j=1}^{r} p_j q_j$, where r is now the number of kinds of commodity, not units. And, of course, $\sum_{i-1}^{n} m_i = \sum_{j=1}^{r} p_j q_j$. Let us try to add up transactions, whether arbitrary or over one day, or over one year. We may simply have

$\sum_k \sum_i m_{ik} = \sum_k \sum_j p_{jk} q_{jk}$ where k runs over all transactions. Notice that this is what, to Hicks, is the quantity equation, no more nor less. But the desire is to have on the right the gross national product and on the left means of estimating its amount. We have, then, one difficulty concerning both money and commodities: they are not unique. But without somehow adding them up we shall not achieve anything like the quantity equation.

There are many difficulties in the addition of quantities of commodities. Not even all grains, or all wheat, can be added up merely as bushels. Yet, somehow, we arrive at a rule of addition, and find a fictitious quantity Q = $\sum_j q_j$ of products over a unit of time, known as the gross national product or total production or the total quantity of commodities produced over that unit of time.

When we have such a fictitious quantity as Q, we may assume that it was all exchanged in the market (despite the objection cited in note 2 above), i.e. that Q = T. Now we can assign it a fictitious price, and in two ways - the sum total of all money paid over the period, and the average price of a fictitious unit.

The average price of a fictitious unit is weighted by the number of fictitious units sold at this or that price. For example, when we have a fictitious units of one commodity at price P_a and b of another at price P_b and the gross product is Q = a + b, then the average price is $p = (a\, p_a + b\, p_b) / (a + b)$ and P Q is, of course, the money paid during the whole period to pay for Q - better designed as PT on the assumption T = Q as before.

We have avoided the repetition of sales of the same commodity, to repeat, so that we do not even know yet where to place second-hand cars. We can easily add services, including the services of second-hand car dealers; but not the price of second-hand cars. Indeed, services, we always assume, are unique. Now it is apparent that we can evade the problem of how to register the sales of second-hand cars, by registering the services of the second-hand car-dealer as a commodity and by pretending that the car was not sold in the market but in private, and by postulating that our equation pertains to the market alone. We have rescued Q = T.

This argument is intended to be off-putting; I find it so. I can even say I dislike any alternative solution to the problem I have myself raised because I dislike the problem itself - or at least that part of it, which is purely a problem of accounting.

Suppose we have added the right hand side of the set of sales equations we had $\sum_{i-1}^{n} m_i = \sum_{j=1}^{r} p_j q_j$, and we call Q = $\sum_j q_j$ and we also call PT = $\sum_{j=1}^{r} p_j q_j$; does this help us add the left? On the very contrary, it makes it virtually impossible. For, obviously, we have to register now every coin multiple times. Yet we should take care to avoid the circulation of money when there is no trade proper; e.g. when I enter a store and receive four quarters for a dollar. This is impossible, since we have agreed that a sale of a second-hand car is no trade, but money circulates. Moreover, we do purchase money, and at a price, and we do exchange liquid money for less liquid money, with varying degrees of liquidity and at distinct prices. The prices can always register as payments for services, but what about the circulation?

Let us demand that transactions are counted only where money is exchanged for a legitimate product, i. e. a ‘new’ commodity or a service. Now we have to define the velocity of money, or ‘the time the average dollar was spent’ to use Heilbroner’s quaint phrase again. The ‘average dollar’ is the dollar which has an average circulation, and the average is weighted again $V = \sum_i \sum_j m_{ij} \big/ \sum m_i = \sum v_i m_i \big/ \sum m_i$, so that when a ten dollar bill is spent once it counts ten times more than when a single dollar bill is spent once or as much as the single dollar bill when it is spent ten different times. Now $\sum_{i-1}^{n} m_i$ may be called the total money in the system, but this is a definition leading to confusion. For, there is always some money, carefully tucked away so as to appear nowhere in our equations of exchange. But we can go the other way round. Let us decide in advance what counts as money and how much of it is available; suppose we have z units of money, the value of which is M = $\sum_{i-1}^{n} m_i$. Now we observe each of them and see how many times it changes hands in a transaction proper (buying some goods or services from the list of national products) and provide for each unit of money whose value is mi with its own velocity V_i; now both M and V can easily be computed.

Here we have pretended to be able to identify q_i and its associated prices p_i; we have allegedly defined quasi-operationally p_i and q_i, which enable us to compute both P and T; the same holds for m_i and v_i, which enable us to compute M and V. Since, we postulate, payments equal receipts, $\sum m_i v_i = \sum p_j q_j$, or MV = PT .

Notice that as long as we stick to the accounting convention, payments equal receipts, the quantity equation, or the equation of exchange, holds for any kind of money, separate or compounded. For example, suppose only checks are money, and no one buys cigarettes by check. Then, the equation for the purchase of cigarettes will be the equation for all purchases of all cigarettes compounded: 0 = 0. Unfortunately, however, in this way cigarettes may drop out of the gross national product! Similarly, if only cars are considered, which are never bought with cash, we may but need not consider cash as money while considering car purchases. I should have thought this point obvious from the proof, but when I read in Pigou (in Dean’s collection, p. 37) the expression ‘leaving aside bank notes as being relatively unimportant’ I wonder. But at what cost is he ignorant of the fact that he may leave them even when they are important is hard to say, for the ignorance is of the fact that the quantity equation or the equation of exchange is a mere accounting device.

Similarly, the sum may be only of local transactions, only on export, only on import, etc. etc. Fisher’s ‘proof’ in stages looks highly suspect in the very same manner as Pigou's.

To see how much the equation of exchange is an accounting device, let us ignore the convention 'payments = receipts' and see what happens. We have determined q, p, m and v independently, and so we have thus far given no argument for the truth of the equation. We can imagine a world in which the equation holds without assistance from double-bookkeepers. We can imagine a world in which every commodity has a price-tag and every transaction is made in deference to the price-tag. This will assure the truth of the equation of exchange in a very simple, practically strictly operational, sense. Once we retain the interpretation of p to be the price on the price-tag, but allow price reductions - of perishable goods or due to haggling or during pre-Christmas sales - our equation ceases to hold.

How is it possible, then, that the truth of the equation was contested? As we saw just now, without the aid of accountants the equation may be false, but we are little enlightened by the fact that it may thus be false. As we also noted, with the aid of accountants the equation may be true, but the gross national product it describes may not indicate what we usually want to consider as the gross national product, indication of changes of levels of productivity, etc. Nor would the equation tell us what is used as money.

The reason the quantity equation was contested, we remember, is that the quantity theory was contested, which blames the mint for inflations. I should think that here the quantity equation may help the debate since it enables us to calculate the effective quantity of money of one kind or another. But this is, indeed, Villard's point recently repeated by Friedman.

Here we come, I think, to the crux of the difficulty of econometrics. First, much of it is trite. This holds for all fields of empirical study, not only econometrics, especially during the current boom in research. Second, there are highly complicated accounting conventions which lead to unusual problems of identification.

There is a theory of science which views the problem of identification as central and universal. This is conventionalism, the theory that all theoretical science is true by convention. Since terms all too often get identified by equations which define them (explicitly or implicitly), we may not know the limits of their applicability - these have to be found by trial and error.

Conventionalism is a highly sophisticated doctrine. It sharpens our ability to discriminate, particularly since it tells us that one word often has two or more meanings which often diverge, one common, one as implicitly defined by one theory or more. Nowhere is this truer than in accounting, yet in accounting the fact looks more natural and so less sophisticated and so highly misleading - especially where the accountant defines and the econometrician observes.

Conventionalism warns us not to test tautologies, or even statements open to modification on the way to becoming tautologies. But it encourages us to test both generalizations and auxiliary hypotheses, especially the auxiliary hypotheses which solve identification problems.

Now econometrics is infested with auxiliary hypotheses, such as ones utterly essential for the observation of velocities of money. And here, clearly, the simplicity and testability of an original theory, say of the demand for money, such as its being proportional to the inverse velocity, may become too hampered to signify. Especially when misunderstandings of accounting conventions and the absence of large scale accounting conventions play havoc, as Villard gently indicates.

To conclude, the difficulty that I found when I waded through the literature seems to me to relate simultaneously to various ingredients. There is much inertia to difficulties that cause little trouble yet whose removal from the literature demand much effort. In our case, the main ingredients are these. First, the avoidance of conventionalism as a philosophy is an essential prerequisite of the desire to invest effort in empirically testing a given economic theory. Second, understanding the nature of conventions in science is necessary in order to remove the risk of unwittingly testing a tautology. In particular, one has to notice that *however idiosyncratic a convention is, when it is consistently applied it is a tautology* (and when inconsistently applied it is a contradiction). Few philosophers have avoided conventionalism while yet absorbing the conventionalist teaching on this point. As I say, economists have managed thus far in spite of occasionally succumbing to the risk of debating and testing a tautology. I hope now they will be ready (a) to state accounting conventions more explicitly - even when they are idiosyncratic and (b) to use them to prove fairly rigorously the tautologies they employ. This will ensure future avoidance of debates about tautologies, at least for those who agree that even though a tautology is certain, it need not be employed. A tautology is based on conventions adopted for the sake of convenience, and in economic science the convenience is either that of offering explanatory economic hypotheses, or that of testing such hypotheses.

Boston University The Hebrew University of Jerusalem

NOTES

[1] K. Klappholz and J. Agassi, 'Methodological Prescriptions in Economics', *Economica*, 26, 1959; reprinted in D. R. Kamerschen (ed.), *Readings in Microeconomics*, New York, 1967. See also Hutchison's comments and authors' rejoinder in *Economica*, 27, 1960.

[2] Knight does not present his idea *ad hoc*. He claims that it is because of religious prejudice that all labor was constructed as evil; and he uses the refutation of this prejudice to various purposes. He shows that this refutes Marx's system by Marx's own criteria; that the slogan 'equal work, equal pay' is thus rendered problematic; that the existence of optional self-service makes it hard to assess income, especially since self-service is often group activity; that the fact that 'resources may receive different remuneration on crossing an indifference line separating alternative occupations … upsets the cost theory of price in its simple form' (which, incidentally, is still the dominant theory in economic texts). See part one of his most stimulating 'Notes on Utility and Cost' reprinted in his *The Economic Organization*, New York, 1951, esp. notes 6,7, 9, 16, 17 and 21. The point that a job can be partly a source of income, partly 'consumption', is made explicitly only in part 2 of that essay, in note 15 and text to it.

[3] For the *ad hoc* of Friedman's view of the non-competitive establishment and its position in the market, see G. C. Archibald's enlightening 'The State of Economic Science' British Journal for Philosophy of Science, 10 (1959-60) 58-69, esp. 69-3.

[4] Paul A. Samuelson, *Foundation of Economic Analysis, Cambridge (Mass.) 1947 (paper, New York, 1965).* See the summary of Chapter IV at the end of the chapter (p. 88).

[5] For a discussion of Friedman's position, see reference in note 1 above. Friedman's methodological view is today widely quoted in literature as the official view of the profession – whatever that may mean.

[6] The question, "is Euclidean geometry tautologous?" was opened by Kant, if not by Leibniz. It was finally answered by various writers, more or less simultaneously, particularly by Henri Poincaré, David Hilbert, Bertrand Russell and Albert Einstein. It is no agreed that Euclidean geometry can be viewed either as tautologous or as superseded (by Einstein), but in two very different interpretations. It should be noted that Keynes considered Say's Law superseded the way Euclidean geometry was. See J. M. Keynes, *The General Theory of Employment, Interest, and Money*, London, 1936, p. 21.
Whereas in the past, social scientists aspired to be the Newton of the social science, in the sense of establishing general principles, Keynes

aspired, with no mean justice, to be the Einstein of economics.

[7] *This is said not in order to endorse Hick's view of the formula as a sheer fancy, restatement of the obvious. Without expressing a view on an issue which is beyond me, let me note that Sir Roy Harrod, in his recent 'Reassessment of Keynes' Views on Money',* Journal of Political Economy *78 (1970), 617-25, views the same formula which Hicks quotes here with so much distance, as a part of the 'Keynesian breakthrough'; because it avoids all explicit reference to the money supply', he says (p. 620). Of course, it is one thing to transform a formula so as to eliminate from it explicit dependence of one variable (price level) to another (money supply), and quite another to use the result as part of the (Keynesian) theory that bank rates do not directly influence price levels. The directness in the sense of explicit mathematical dependence is something alterable by a mere logical transformation, whereas Keynes' claim regards casual nexus, which signifies in economics because casual influences may be slightly delayed, and in rapid processes they may 'miss the train' of events. I am aware of the fact that Sir Roy is fully aware of this point. I wish however that his paper was longer and more explicit on this point – as it is on the difference between the equality of saving and investment as a conversion and as an equilibrium condition within a given theory (see next note).*

[8] Sir Roy Harrod seems to corroborate to the full Villard' s gentle suggestion that Keynes himself was unclear here. Harrod's 'Reassessment' (op. cit.) suggests that in retrospect the Treatise should be viewed as the better line to pursue than the General Theory. See particularly p. 619: 'There is a question of terminology. In the General Theory Keynes . . . lays stress on the fact that investment must always and necessarily be equal to saving [whereas] in the Treatise these magnitudes are taken to be unequal. . . . Of course, Keynes knew perfectly well the book-keeping identity that *ex post* investment must be equal to *ex post* saving, and he stated this more than once in the Treatise. He is able to postulate the inequality by providing a special definition of income.' Now clearly, were Keynes' terminology clear enough, much debate would have been avoided: nobody voluntarily contests a tautology.

[9] See for example, Frank W. Paish, *Long Term and Short Term Interest Rates*, Manchester, 1966, p. 37: "My approach to this question (what determines interest rates) is basically Keynesian, though with some variations. I hold the view that the long-term rate of interest depends primarily on the relationship between the quantity of money and the national money income. I do not share the view of the Radcliff Committee that money, in this country and at the present time, is such a nebulous concept that attempts to measure its quantity have no meaning. It is true that all assets or, at any rate, transferable assets, share with money the function of a store of value – indeed in recent years many of them have been better stores of value than money. But the essential function of money, which distinguishes it from all

other types of assets, is that of a medium of exchange – that it is widely and generally accepted in payment of debts. There are no doubt places where … the distinction between money and non-money is blurred …' etc.