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I. Introduction

Since the appearance of Kuznet's and Goldsmith's historical income statistics for the United States, considerable significance has been attached by several economists to the absence of any long-term trend in the savings ratio according to certain definitions. Kuznets [18] found first that the ratio of Net Capital Formation to Net National Product, averaged over overlapping ten year periods between 1879 and 1928, never varied by more than 1 point around a level of 11 per cent. Goldsmith's study [9] pointed to an absence of trend for the three decades preceding 1930, in both the national saving ratio which stood at around 14 per cent and the personal saving ratio which was roughly 12 per cent (both including consumer durables expenditure).

I shall argue in this note that there is no longer, and probably never was, any justification for attributing any great significance to these figures, that is, to the long-period constancy (LPC) of the savings ratio in the United States. That does not of course mean that the data are not of interest in themselves for the purposes of historical or possibly econometric enquiry. My argument is rather that no great theoretical construction may be justifiably put upon the fact of constancy as such. This will emerge as I discuss the point first in the context of the 1940's and 1950's debates on secular stagnation and on the theory of savings, and second in the context of present day knowledge.

II. The Early Response

Let us first deal with the relatively straightforward question of whether the discovery of LPC had much impact on the "secular stagnation" debate, carried on in the 1930s and subsequent decades in response to the Great Depression. Briefly, the issue concerns whether, in the long run, aggregate demand in a market economy will grow as fast as aggregate supply. If demand grows too slowly the economy, it is argued, will tend to chronic unemployment and under-utilisation of capacity. It was the expectation that the long term trend growth of demand would indeed be too low that suggested arguments in support of public expenditure programs in the United States. The question is, what prompted this expectation? It has sometimes been thought that a chief element was the fear that consumption would in the long run decline as a proportion of income.¹ In that respect, the discovery of LPC could pos-

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1. For example, according to Lindauer, "supporters of the New Deal favoured government spending because they felt that increases in purchasing of investors and foreigners were unlikely to be large enough to fill the gap left by the declining contribution of consumption" [21, 60].

sibly come as good news, for if the constant ratio were to reflect some fundamental law and were therefore to continue into the future, the amount of demand needed to come from investment and other sources would not have to rise more than proportionately.

However, the truth is that the secular stagnation debate was really about declining investment opportunities, and the argument over these was the crucial element in deciding the extent to which the market economy was expected to survive at full employment. Thus Alvin Hansen, spokesman for the new Keynesianism in the United States, argued: “The problem of our generation is, above all, the problem of inadequate private investment outlets” [3,10].² The reason for this inadequacy, as he saw it, was the fall in the population growth and in the claiming of new land, both of which would cause a fall in the investment rate. And Hansen’s opponents, such as King [17], contested the same issue, arguing, that is, that investment opportunities would be forthcoming. Moreover, Hansen [14] himself maintained³ that he had always thought in terms of a broadly constant consumption ratio in the long period, and had not found this inconsistent with the Keynesian consumption function which he rightly described as a short-run relationship. True, economists were for the most part surprised at the high level of demand and prosperity in the immediate post-war period. But they seem to have been neither surprised nor especially comforted by the evidence of LPC, and continued to look at the question of stagnation in terms of the prospects for investment outlets.⁴ At the same time, neo-Marxist analyses of stagnation were not affected by LPC, as these also depended on the expectation of falling investment and the inability of monopoly capitalism as a system to check this fall or to induce a proportionate rise in consumption and hence prevent under-utilisation [30; 29].

If the discovery of LPC had little or no influence on policy, the same cannot be said for the development of savings theory. As Sargan observed in 1958, with regard to the proportionality assumption made by Friedman in his Permanent Income Hypothesis and by Modigliani and Brumberg in their Life Cycle Hypothesis,

It seems likely that the acceptance of this kind of limitation on the model has been suggested by the observed constant proportion of savings to the national income in the U.S.A. over the last eighty years [27,172].⁵

LPC may also have earlier influenced Duesenberry in his formulation of the “emulation” effect in his Relative Income Theory [3]. He chose to do this by making utility a function of the ratios of consumption (in different periods over a lifetime) to a linear index of other peoples’ consumptions. In consequence he was able to derive a consumption function which automatically gave long-run proportionality of consumption with income, as long as all other peoples’ consumptions rose proportionately in the long run also, thus assuming a stable distribution of income. Duesenberry claimed that the discovery of LPC was consistent with the Relative Income Theory. He considered whether trend factors such as urbanization, interest rate changes, the introduction of new products, and changes in the structure of the population would be expected to affect the consumption ratio much, and argued that they probably would not. Hence LPC was adduced as (weak) evidence in support of the Relative In-

2. See also, for example, [24] and [15].

3. See also Samuelson [26] for a similar viewpoint, and for a healthy dismissal of all statistical economic laws.

4. See, for example, [16], especially Chs. 4 and 9.

5. But note that neither Friedman [8] nor Modigliani and Brumberg [25] state that LPC was what suggested to them the proportionality hypothesis.

come Theory against the Keynesian consumption function which expected a falling consumption ratio.

However, Duesenberry's argument is a very weak one. It depends crucially first of all on accepting his proposition that the trend factors were not sufficiently important in the long run to affect the consumption ratio. This question is of course very difficult to answer, owing to the sheer complexity of the changes occurring in the economy over many decades. To be sure that trend factors are absent one has to have some pretty strong arguments. And yet there are on the face of it good reasons to expect the consumption ratio to have increased over many decades. To continue the same quotation from Sargan:

The relative increase in taxation and at the same time the increase in insurance and in government sponsored relief schemes together with increases in estate duties all tend to discourage savings as a proportion of the national income. At the same time, technological developments particularly in consumer durables alter the whole nature of the utility function, increase the range of consumer possibilities and may be assumed to have encouraged consumption. Disposable income has also become perceptibly more equally distributed. But more important than these is the decline of the nineteenth century idea that abstinence is morally good and that thrift is a prime virtue of the serious citizen, combined with the growth of advertising as a serious and scientific business of persuading the consumer to consume. It would need a considerable opposing force to combat all these factors, and this is surely only provided by the effect of the general rise in real income [27,172].

Duesenberry's argument also depends on the particular formulation he chose for the utility function. It would be possible to have the "emulation effect" without the particular utility function he utilises in order to obtain a constant consumption ratio.

In view of this, and of the complex question of how far trend factors are important in the long run, it would seem that the discovery of LPC is of no use in discriminating between the Relative Income Theory and competing theories, and that only a weak claim of consistency can be made—weak in the sense that just about any long term trend of consumption ratio could be made consistent with each of the competing theories. The emphasis placed on the LPC by Duesenberry was thus not justified.

A more cautious approach was adopted by Friedman, who, although devoting a few pages to LPC, concluded that "the observed rough constancy of (the consumption ratio) is about as much of a puzzle as substantial variations in it would be" [8, 120]. He argues that the trend factors tended to act in contrary directions, ignoring the "declining thrift" argument and pointing to the fall in average family size which would tend to raise the savings ratio, counteracting the effect on the increased urbanisation. Hence LPC is consistent with but "hardly strong evidence for" the Permanent Income Hypothesis.

Thus, although LPC may have had a suggestive influence, as Sargan claims, it can hardly have been decisive in prompting Friedman and Modigliani and Brumberg to frame proportional consumption functions. Indeed the "proportionality hypothesis" is introduced from a theoretical angle, depending as it does on the likely form of the utility function. It is a hypothesis about an individual's consumption function at a point in time, and is not the same thing as LPC itself. LPC should have been and, as Friedman's book shows, was only a very small part in the argument. Moreover, the proportionality hypothesis is *not* an essential part of the theory; it is only an additional conjecture. Friedman resolved "tentatively (to) accept it, subject as always, of course, to the possibility that empirical evidence will be discovered that turns out to be inconsistent with it and that will therefore require complicating the hypothesis" [8, 13]. As is well known, a good deal of the early work tended to treat the pro-

portionality hypothesis as virtually synonymous with the Permanent Income Hypothesis. But by now it should be recognised that it is quite feasible to reject the former and to defend the latter, if that is the way the evidence points. What was of great importance in the data of Kuznets and Goldsmith was the difference between the short-run behaviour of savings and its long term trend, and it is to the explanation of this difference between the short and the long run that the Permanent Income, Life Cycle and Habit Persistence Theories are addressed. However this should not be confused with the fact of LPC which may or may not require explanation. If LPC were not the case, it would still be necessary to explain the difference between the short and long run marginal propensities to consume.

It appears then that the discovery of LPC was not important to the secular stagnation debate, since that debate was mainly concerned with investment opportunities, and it can only have been a minor factor influencing the development of the neo-Fisherian savings theories. Nonetheless there was in the 1950's considerable interest in the long term movements of savings and along with this a certain pragmatic interest in the size of the savings ratio. Writing of the national savings ratio, Kuznets asks, "Why has it averaged 12 to 14 rather than 25 or 5 per cent?" [19, 507] and tries a preliminary answer, in terms of such things as income distribution, size of corporate sector, life-cycle behaviour and so on. No attempt is made at any *simple* fundamental explanation.

III. LPC in the Light of Later Evidence

If LPC were ever thought to have any significance for the proportionality hypothesis, it ought to be clear in the light of subsequent evidence that it does not now do so. For whatever has happened to the Permanent Income and Life Cycle Hypotheses in the light of empirical testing—this not being the place to attempt an assessment⁶—the proportionality hypothesis has been pretty decisively rejected. The evidence has been most convincingly assembled by Mayer [23], who finds that of all the many tests that had been used which utilise a variety of different techniques and data sources, there are several which disconfirm the proportionality hypothesis. Moreover "what is even more persuasive, of all the many tests which have been undertaken by friends of the hypothesis, *not a single one supports it*"⁷ (his emphasis). Although each test which does so may be open to some specific objection, the collection of tests together constitute strong evidence. And although studies on the Permanent Income Hypothesis continue to appear, no published work since 1972 has to my knowledge challenged Mayer's findings on this point. This being the case, LPC ceases to be of any significance in any simple or fundamental way for savings theory, for only if the proportionality hypothesis had been found to hold true in other tests could a simple explanation of LPC have been offered. Moreover, with continued historically high employment levels in Western economies through the 1950's and 1960's the secular stagnation debate naturally faded, again leaving little direct significance to long term movements of the consumption ratio.

It is difficult therefore to see why the fact of LPC is still raised on a pedestal in modern discussions of the consumption function and of savings behaviour, both in textbooks and in research. A brief survey of a number of macroeconomics textbook treatments of the consumption function shows that LPC is still very much a part of the folklore of the history of

6. For an empirical survey see [10] or [12; 33–60].

7. See Mayer [23, 347–8], which also lists such tests.

the consumption function. There are, it is true, some books which treat the theory of savings as an unresolved and open matter, but the majority tend to be favourable towards the neo-Fisherian theories; and in most cases LPC is adduced as strong evidence in their favour. As a typical example, Lipsey states that “the average propensity to consume does not decline as income rises,” [22, 525], declared this to be inconsistent with Keynes’ consumption function and proceeds immediately with an exposition of the permanent income theory. Later he concludes: “permanent-income type theories do succeed in reconciling the various empirical observations of consumption functions referred to above” [22,531]. For Lipsey, as for other textbook writers, LPC is generally regarded as showing that the “true” consumption function is a proportional one, and that therefore Friedman is right in his tentative conjecture. To take another author, Sawyer writes: “Empirical research by statisticians such as Simon Kuznets and Raymond Goldsmith in the United States has established that the ratio of aggregate personal consumption to income has been constant over long periods of time. The implication of this finding is that the long-run relation between consumption and income (appropriately defined) is of the form $c/y = a$ ” [28,185]. The trouble with this statement (and many others like it that are not put so clearly) is that it is either purely tautological, or if not—that is, if it is meant as a statement of a “true” consumption function—it is incorrect. Friedman himself, as we have seen, never argued that this implication could be drawn. It is in any case an unwarranted implication because we simply do not know how important are the manifold long-run changes in the structure of the economy in their impact on the savings ratio. And, to repeat, there is abundant evidence from elsewhere, as Mayer has documented, that the consumption function is not proportional.

Despite this the fact of LPC continues to be considered important. The explanation may lie in a confusion between two observations. The first is the observation that the long period consumption function differs from the short period function, in other words, that it is necessary to distinguish cyclical from trend behaviour. This parallels another discovery that static cross-sectional data will give a different picture from that of time-series data. The second observation is that, in the United States, the trend behaviour of consumption is that it has remained a roughly constant function of income. The first observation is undoubtedly correct, and it has been instrumental in pushing forward the study of the consumption function from the initial pre-war “Keynesian” relationships to the development of the Relative Income Theory, the Habit Persistence Theory and the post-war neo-Fisherian theories. The second observation is relevant evidence in respect of savings theories only in so far as it is an aspect of the first. That the consumption ratio has no trend implies its long-run movement is different from its cyclical behaviour. Suppose instead that the consumption ratio had either an upward or a downward trend. As long as the marginal propensity to consume was less in the short than in the long term then a need arises to explain this: an indeed the post-war theories are designed to do so. Nobody (to my knowledge) has yet suggested that the Permanent Income Hypothesis is not applicable to the United Kingdom just because in the course of five decades or so the national savings ratio *has* risen (from around 5% to over 20%).⁸

Finally, we may note a continuing misuse of the search for simple explanations in a recent argument that raises the fact of LPC in a new form, attempting to place interpretations on it that are quite untenable. David and Scadding [2] recall an observation by Denison in 1957, now elevated to the status of a “law,” that the ratio of gross private savings to GNP in

8. See Feinstein [4]. Incidentally, the fact that another country’s experience is different raises further doubt that LPC in the U.S. is the expression of any fundamental relationship based on individual preferences.

the U.S. exhibits no trend over a seventy year period, and deviates from its constant value by only small amounts except during the world wars and the Depression years. They argue that the best explanation is what they consider to be the simplest one—namely that consumers planned it that way. Thus they ignore all the many factors which under highly plausible assumptions could affect the savings ratio. “Simplicity is best” is their guideline. The first part of their behavioral theory is that personal and corporate savings are perfect substitutes for each other. Hence consumers, wishing always to save a constant proportion of income, adjust their savings in the opposite direction to changes in corporate saving so as to achieve the desired level of total private saving.

Given that the proportionality hypothesis has elsewhere been rejected strongly, it is not satisfactory to make that implicit assumption. There are other ways to attempt to test whether corporate and personal savings are substitutes which do not necessarily impose a proportional savings function. Such tests as exist, [5; 7; 1; 20; 10; 11], present a conflicting picture, some showing little or no substitutability while others that indicate some substitutability find it is far from perfect. It would seem impossible therefore to put upon this “law” the interpretation offered initially by David and Scadding.

But they argue further that not only do consumers treat corporate savings as part of their rational savings plan, they also take government savings into account: consumers are “ultra-rational.” If this is so, and the proportionality hypothesis were true, why then is not the total national savings ratio the stable number? They offer as a possible explanation the proposition that consumers regard the government expenditure that is financed by borrowing as investment expenditure while tax-financed spending is on consumer goods. In that way, it would turn out that the gross private savings ratio would be constant while the national savings ratio would vary in so far as consumers miscalculate government savings.

There is of course no evidence (either concrete or intuitive) that consumers do behave in this way, and taking this with what has been said about the first stage of their argument it seems that the search for fundamental explanations is again a fruitless one. As a final point of criticism, there is also no reason given as to why *gross* saving is the key variable to be explained. Are we to assume that the rational consumer simply ignores depreciation?

In conclusion, I share the view of Feldstein [6] on this point, that the stability of the U.S. gross private savings ratio is just a fortuitous outcome of complex offsetting forces, “including the growth of taxes, the higher level of incomes, the increase in retirement and the expansion of social security.” If foreign saving and/or capital consumption is introduced, the apparent stability is changed. As with the earlier observation of LPC by Kuznets and by Goldsmith, the discovery of magical constants in historical economic statistics should be treated with great reserve as regards how much they tell us of real economic behaviour.

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