

Supplementary appendices of
***A Theory of Profits* fifty years on**

Cambridge Journal of Economics, 2025
Doi: 10.1093/cje/beaf036

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Appendix A: Correspondence with Nicholas Kaldor about *A Theory of Profits*

In the modern archive of King's College, Cambridge (references NK/3/30/212 and 212A).

8 November 1975. Letter from Kaldor

Many thanks for sending me your book. I shall read it with great fascination.

At the moment I am so busy with reading official papers that I don't get much time for anything else but I can see that I shall be carrying it with me to and from London for many weeks to come!

1 February 1977. Letter to Kaldor

I have just read your presidential address, and I think it is excellent.¹ It is so nice (and so rare) to read a paper from which one learns something new and important. I refer in particular to your 2-sector analysis on pp. 704-7, and to your suggested solution on pp. 712-14. I must admit that I have residual doubts about the analysis of inflation on pp. 708-10. It's not so much that I can put my finger on anything wrong, as that I remain unsatisfied – unconvinced that the whole truth has been uncovered. Not, I hasten to add, that I could have done any better, although I think I would have put it somewhat differently. Anyway, thanks again for sending me the offprint.

10 February 1977. Letter from Kaldor

Many thanks for your nice and highly complimentary letter about my presidential address. I share your doubts about the cause of the wage explosion in 1968-71 and I am eclectic about the creeping inflation to the extent shown in the last footnote on page 708, though I am pretty confident that there is something in the whole capitalist mechanism which invariably makes wages rise faster than productivity and the difference tend[s] to be greater when countries grow faster. At least I cannot think of any industrial country where wages consistently grew less than productivity.

As regards the wage explosion of 1968-71, I think we are still in the dark as indeed I admitted by saying "there is no fully satisfactory explanation on present evidence". It is very odd that when wages have been rising from 6-7% a year in most countries, they suddenly start rising to 10-15% a year in much the same group of countries more or less at the same time.

I have been looking at your book again and I should like to have a talk with you about the difference between my approach (in the neo-Pasinetti theorem) and yours. I don't regard the difference in assumptions about the "valuation ratio" as so important because once the margin of profit on turnover is determined in the manner you suggest, on both assumptions output will

¹ [To the Royal Economic Society, 'Inflation and Recession in the World Economy', *Economic Journal*, Vol. 86, No. 344 (Dec., 1976), pp. 703-714.]

also be determined. For supposing external finance is zero, and net household savings are also zero (owing to the neo Pasinetti mechanism) output and employment will still be determined by the basic equation if the capital/output ratio is given, since $\pi = \rho \frac{y}{k}$.

What I do think is very important, however, is that in my *original* formulation of the “neo Keynesian theory of distribution” I not only assumed full employment, but the full employment assumption was an essential prop of the explanation, since the rate of growth could not be determined except exogeneously (or quasi-exogeneously) by the natural rate of growth. But I later discovered that I was wrong in this, once one assumes that one’s generalisations about profits, corporate finance etc. only apply to those economic activities which are carried on by corporate enterprises under conditions of monopolistic competition. This is true of the “secondary” sector (i.e. industrial production) and not of the “primary” sector. On these assumptions one can explain that the level and the growth rate of industrial output is determined by “outside” demand and, as far as the industrial sector is concerned, the mechanism works in much the same way without assuming full employment (this mechanism of course would only explain the distribution of the industrial product between profits and wages in terms of industrial goods, not the terms of trade between industry and agriculture). I find that from the point of view of such a two-sector model the assumption that personal savings are zero and all savings are corporate savings is helpful rather than unhelpful.

[A phone call must then have been arranged for 14 February.]

15 February 1977. Draft letter from Wood

Reading your letter, especially the last part concerning the two sectors, it struck me that perhaps I also should have suggested on the phone that you read pp. 141-3 of the book. Otherwise, my impression is that we covered most of the points in the letter in our conversation last night – correct me if I am wrong.

Appendix B: Contents of Fellowship dissertation and PhD thesis

Fellowship Dissertation (Wood, 1968)

In the college archive of King's College, Cambridge, ref. GBR/0272/KCAC/4/11/1/Wood
<https://www.kings.cam.ac.uk/archive-centre/online-resources/guide-to-the-college-archives>
 Bound typescript on US letter-size paper, with an average of about 250 words per page.

THE DISTRIBUTION OF INCOME IN THE UNITED KINGDOM 1870-1965
 A Study in the Application of Post-Keynesian Theory

Adrian Wood, November 1968

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PhD thesis (Wood, 1972)

Available from the Cambridge University Library.

<https://www.openaccess.cam.ac.uk/theses/finding-and-accessing-theses>

Bound typescript on A4 paper, with an average of about 220 words per page, chapters numbered separately.

AN ANALYSIS OF INCOME DISTRIBUTION

Adrian John Bickersteth Wood, October 1972

PART 1	INTRODUCTION
Chapter 1	Income Distribution (3 pages)
PART 2	EARNINGS [Summary Table of Contents]
Chapter 2	Income Competition and Non Income Competition: Ideal Types (58 pages)
Chapter 3	Social Bases of Earnings Structure (46 pages)
Chapter 4	Income Competition and Non Income Competition: Coexistence and Interaction (34 pages)
PART 3	PROFITS [Detailed Table of Contents]
Chapter 5	<u>The Determination of Profits</u> (33 pages)
5.1	Profits in Relation to Capital Theory
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Chapter 6	<u>The Financial Behaviour of Companies</u> (34 pages)
6.1	Introduction
6.2	Debt
6.3	A Simple Model of Share Valuation
6.4	New Issues of Shares
6.5	The Retention Ratio
6.6	Company Finance Policies: Provisional Conclusions
Chapter 7	<u>Growth, Competition and Profits</u> (45 pages)
7.1	A Model of Company Behaviour
7.2	The Long Term Profit Margin Decision
7.3	Secular Disequilibrium and Adjustment
7.4	The Aggregate Share of Profits in the Long Run
7.5	Short Run Consequences of Cyclical Fluctuations
7.6	The Structure of Industry Profit Margins
	Appendix to Ch. 5: Classical Antecedents of the Cambridge Theories (5 pages)
	Appendix to Ch. 6: Company Taxation and the Retention Ratio (2 pages)
	Appendix to Ch. 7: The Influence of the Form of the Opportunity Function (10 pages)

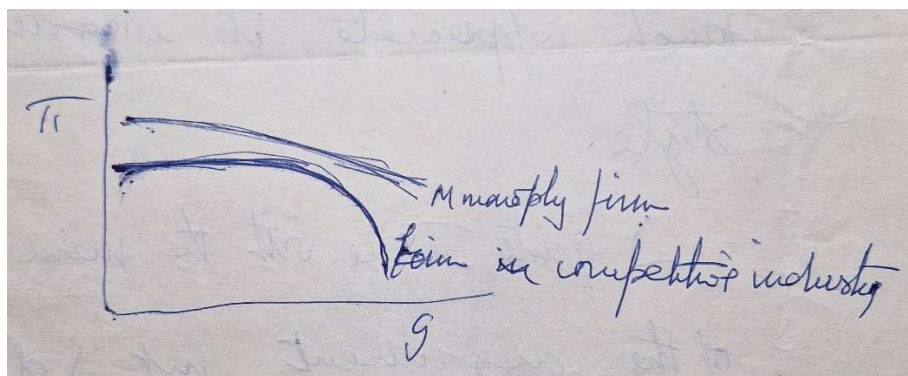
Appendix C: Correspondence with Joan Robinson about *A Theory of Profits*

This appendix is a transcription of the substantive content of 50 pages of mainly hand-written correspondence in November and December 1975 between Adrian Wood and Professor Joan Robinson about Wood's recently published book. In (and published with the permission of) the modern archive of King's College, Cambridge, refs. RFK/14/129 and RFK/14/129A.²

In the transcriptions of letters from Wood, which are mostly based on final drafts, abbreviations have been expanded and other minor clarifications made without any indication of having done so. In the transcriptions of letters from Robinson, such amendments are flagged by [bracketing] the words concerned, and in the few cases of uncertainty about writing or meaning there are also ? marks in the brackets. Algebraic notation has been italicised. Omitted non-substantive text is indicated by ...

Late October. Undated letter from Robinson.

Many thanks for sending the book. I have skimmed through it and will read it properly later. I very much appreciate its elegant style. I quite agree with the main line of the argument but I don't see how you can get on without animal spirits. What determines the expectations of overall growth of the market? Surely this is a case of thinking makes it so. Also I think the degree of monopoly is embodied in your curve.³



I hope you will have a big success. This is a really useful contribution and an exemplar of good exposition. [Note in margin] I think you might have given ... Eichner more of a look in

3 November. Letter from Robinson.

On p 115 you say that a higher k tends to reduce π . This is a fine paradox but I fear it won't hold. When an economy is growing at g , high k and low k industries are growing at more or less the same rate, and higher k 's require higher π 's. Similarly, if two economies have the same g , the one with a higher k has a higher π . Is not your theory that, whatever gk may be, π is adjusted so as to make l equal to it? Or have I missed a step in the argument?

² Unpublished writings of Joan Robinson copyright The Provost and Scholars of King's College Cambridge 2025.

³ Hand-drawn diagram with π on vertical axis and g on horizontal axis, showing downward-sloping curve labelled 'monopoly firm', and below it another curve, initially flatter but then declining much more steeply, labelled 'firm in competitive industry'.

4 November. Draft letter from Wood.

1. Animal spirits

Far from ‘getting on without’ animal spirits, I feel that they lie at the heart of my model, in that the main object of firms is to maximise the growth of sales. Were they, by contrast, lily-livered neoclassical profit maximisers, the finance constraint would in general not bind and thus my models (micro and macro) would become indeterminate. On the other hand, the equilibrium macro growth rate in my basic model (pp 111-116) doesn’t depend on animal spirits: it is determined, in old-fashioned Harrod-Domar (H-D) style, by l and k (or in old-fashioned H-D notation, by s and v), together with a . What the actual growth rate is, though, will of course depend on animal-spirit-based expectations, and unless these are such as to make the economy grow at the equilibrium rate, there will be disequilibrium – hence my discussion in section 4.5. (More on the macro growth rate below.)

2. Degree of monopoly

The degree of monopoly is indeed embodied in the demand-profit margin tradeoff, and is bound to affect the relative profit margins and growth rates of individual firms. But I argue (pp 108-11) that it won’t affect the average (or aggregate) profit margin. Indeed this is a fundamental and necessary aspect of my model.

3. The ‘paradox’ on p. 115

... in H-D terms, ... if k is higher in one economy than in another, and l is the same, the equilibrium growth rate of the former will be lower than that of the latter, since $g = l/k$. Thus because, in my model, π depends directly on g (for finance reasons), lower equilibrium g tends to cause lower equilibrium π . There is also an offsetting tendency, since π in my model also depends directly on k , also for financial reasons. But what the footnote on p. 115 shows is that the downward tendency on π of increased k (via lower g) is in general greater than the offsetting upward tendency. In the special case in which $a = 0$, the two tendencies are exactly equal in size, π is determined by equation 6, and k (and changes in k) have no effect on π .

4. Causation in the macro model

... my theory is not, as you put it, ‘that whatever gk may be, π is adjusted to make l equal to it’ – that is what I call the neo-Keynesian theory, in which g is determined by the natural rate (Nicky) or by animal spirits (you), k is (for simplicity) exogenous, and π adjusts so as to make savings = investment (or, more accurately, to make the average saving propensity of the economy, which I call l , equal to the share of investment in output, which is equal to and determined by gk).

My theory, for what it is worth, is different. In the simplest case (pp 111-5), I take l as exogenously given. Thus exogenous l and (for simplicity) exogenous k (together with exogenous a) determine g , and g and k (and the exogenous variables f , x and r) determine π via

the finance function $\pi = \frac{(1+f-x)}{r} gk$.

On pp 116-9 I consider an additional complication, namely the feedback that is likely to exist from π to l , and I try to explain how the simple model must be modified. But the modified model is still not the same thing as the neo-Keynesian model, as I try to show on p 117. The reason in formal terms is that my model has an extra equation: ie even if I make l (savings) a function of π , π is still also a financial function of g and k (which it isn't in the neo-Keynesian model, which makes no particular assumptions about finance). Thus since I have one more equation in my model, I must have one more dependent variable – ... g , which is endogenously determined, à la H-D, rather than, as in a neo-Keynesian model, being exogenously determined by the natural rate or animal spirits. Hence my discussion of Marxian unemployment on p. 125 (including footnote).

5 November. Letter from Robinson.

I am still puzzled. Let us leave out a , which I would like to discuss later. (1) What is the relationship between the flow of retained profits accruing to firms and flow of saving in the economy as a whole? How do retentions affect l or s ? (2) Taking the case of a closed system with no government sector, what is the relation between the flow of borrowing, i.e. the excess of investment over retained profits and the flow of household saving? (3) When conditions in the capital market impose a lower retention ratio, does this tend to reduce g or to increase π ? I like your micro theory very much so I am anxious to get the macro bit clear.

6 November. Draft letter from Wood.

1. Retained profits and total saving

In the simple case in which retained profits do not cause capital gains on shares and/or household saving decisions are unaffected by such gains, an extra £100 of retained profits, cet. par., is an extra £100 of saving. If, to take another case, £100 retained profits caused £100 capital gains, and households increased their consumption by $\£(1 - s)100$ as a result, an extra £100 of retained profits causes only $\£s100$ extra saving. In this case, provided that households save the same proportion (s) of wages, dividends and capital gains, neither π nor r will have any effect on l , which will simply be equal to s . Of course there are many other possibilities. But see (3) below.

2. Company borrowing and household saving

in a closed economy with no government. The two must clearly be equal, as is stated in the equation in footnote * on p. 118. See also (3) below.

3. Effects of a lower retention ratio on π and g

A fall in r will always tend to raise π , ... from the finance function $\pi = \frac{(1+f-x)}{r} gk$. But will a fall in r also affect g (and thus have further repercussions on π)? Clearly, in the special case mentioned in (1) in which l is independent of both r and π , a change in r cannot affect g since it affects none of the variables (l, k, a) on which g depends.

Consider instead the case in which £100 of extra retained profits is £100 of extra saving. This case is analysed in equations 12 and 13 (in which, for simplicity, I assume $a = 0$). Equation 13 reveals that the net effect of a fall in r (taking both the direct financial effect and any possible indirect effects via the growth rate into account) is to raise π .

To discover the effect on g , one must substitute 13 into 12, thereby getting rid of π in 12, which becomes an expression for l dependent only on the exogenous variables (r, s, f, x) . Interestingly enough (and I hadn't spotted this before), r drops out, which implies that at the end of the day l isn't affected by the reduction in r , and thus g won't be either (since g depends only on l and k , assuming $a = 0$).

Moreover, this result isn't a special case caused by the particular form of 12 or the assumption that $a = 0$. If, for example, one adds a capital gains term to 12, such as $-(1-s)\Psi r\pi$, where Ψ is the ratio between retained profits and capital gains, or if one makes $a \neq$ zero, one gets exactly the same result.

Its meaning in economic terms is this: changes in r do not alter the total amount of retained profits, rP . To see why, suppose that companies are carrying out a given amount of I : given their average f and their average x , they need some definite amount of retained profits R . For a given retention ratio r this implies a particular P (and hence a particular π); but if r is reduced, P (and π) must be increased by an exactly offsetting amount.

So whatever the relationship between retained profits R and the saving propensity l , l won't be affected by changes in r because R isn't affected by changes in r . Moreover, because I , x and f haven't changed, the net amount of company borrowing hasn't changed, and thus (from point 2 above) the total amount of household saving can't change. Thus changes in r (in this simple equilibrium model) can't affect the total amount of (either company or household) saving, thus can't affect l , and hence can't affect g .

This, as I say, I hadn't really grasped before (mainly because I was focusing on what determined π and was only incidentally interested in what determined g), and I'm very grateful to you for forcing me to work it out. [Two pages of supporting algebra not sent to Robinson.]

7 November. Letter from Robinson.

... I think we are getting on. But everything you say confirms my reading of your theory. For instance: given I , a fall in r raises π . Why not: a rise in I , given r , raises π ? Suppose no household saving. Then $l = r\pi$. Each firm chooses π (given its r) to fit its I and for the economy as a whole, g determines l . What do you want to deny? The difference between your theory and the Anglo-Italian-Kaleckian one is in the mechanism by which g determines π , not in the fact. If we could get this clear, there are several other points I would like to discuss.

Undated draft letter from Wood.

I absolutely agree with you that the difference between my theory and the "neo-Keynesian" theory is not in the fact of g affecting π , but in the mechanism involved. The essence of my

mechanism is changes in internally generated company finance, not changes in l , and certainly not solely changes in l – it is this that I wish to deny. For even if l does not depend on π , my mechanism still works perfectly well. And in the special case which you mention in which l depends only on $r\pi$ (no personal savings) my mechanism collapses, as you can see by setting $s = 0$ in equation 12. (More precisely, the model becomes indeterminate.)

But this case is surely of little practical importance, since l in reality depends also on tax rates and the propensity to import – indeed this is why I used the letter l (for leakages) not the letter s (for savings). This incidentally touches on one of the reasons why I prefer my mechanism to the neo-Keynesian mechanism, namely that I find it hard to construct a plausible generalisation of the neo-Keynesian mechanism in the context of an open economy with a government, not least because of the problem discussed in the last para on p 133 (which restates a proposition of yours that in the context of a neo-Keynesian model ‘if the government underwrites full employment, the capitalists can extract any share of profits they desire’ [quote is probably from work cited as Robinson (1964) in Wood (1975)]).

11 November. Letter from Robinson.

‘Strong cases’ are not realistic but they are the best way of understanding the essence of a theory. You say your mechanism would collapse if household saving was zero. Would it work all right if it was .01 of the rate of investment. What is the critical ratio? I am learning a lot from this correspondence.

12 November. Draft letter from Wood.

The critical value of s is zero – anything above zero, no matter by how little, and my model works all right. If s is zero, what happens is that two of the equations in my model – the saving equation and the company finance equation – melt into one equation, and thus I am left with more variables than equations, and hence an indeterminate model. I do take your point about the role of ‘unrealistic’ cases in understanding the essence of a theory. All I was saying is that the practical significance of the difference between my mechanism and the neo-Keynesian one cannot be grasped properly in the context of a closed economy with no government.

14 November. Letter from Robinson.

You are now saying that your theory would be correct if 99% of business investment was financed out of retained profits, but not if 100% was. Is not this odd?

18 November. Draft letter from Wood.

In one sense it is odd that if a parameter of a model takes on a certain value the model breaks down. But there are many oddities of this kind. For example, the neo-Keynesian equation
$$\pi = \frac{1}{s_p - s_w} \frac{I}{Y} - \frac{s_w}{s_p - s_w}$$
 collapses if $s_p = s_w$. But it is all right if s_p differs from s_w by a minute amount. The important question is surely ‘does this mathematical oddity have any economic significance?’ My answer to this question with respect to the case $s = 0$ in my model (after considerable head-scratching) would be ‘no’. But I am open to persuasion.

19 November. Letter from Robinson.

We seem to have run into a dead end. May I start again at the beginning? Why do you say that higher k lowers g ? Compare two economies with the same labour force (not necessarily fully employed), same money wage rates and same rates of interest. The financial system is the same – r , gearing ratio, etc. One has a composition of output containing more high- k industries than Two. Suppose that g is the same for both (you have to show why it cannot be). In One consumption measured in labour embodied must be lower and investment higher. Investment in money terms is higher. Many firms require higher $r\pi$ to finance their investments. Aggregate π is higher so that money prices are higher (the stock of K in existence at any moment is greater and the rate of interest the same). With higher prices and somewhat higher money incomes, the household sector multiplier may be different from that in Two but whatever it may be, household saving per annum is greater in One, being equal to the greater excess, in money terms, of investment over internal finance. What is wrong with that? Please take for granted the simplifying assumptions that you use on p 111, no government etc. Also neglect a , though this is not necessary to the argument.

27 November. Letter from Wood.

... Let me try to explain why I say that higher k is associated with lower equilibrium g (and section 4.2 assumes equilibrium).

Consider an economy whose equilibrium growth rate is g_1 and whose investment coefficient is k_1 ; this implies that its average propensity to save, l_1 , must be equal to g_1k_1 – otherwise there could not be equilibrium (for $l = \text{planned } S/Y$ and $gk = \text{planned } I/Y$).

Now consider another economy whose investment coefficient k_2 is, say, twice as big. It is possible for this economy to have the same equilibrium growth rate as the first economy. But this can be so if and only if l_2 is twice the size of l_1 .

In your model, this could and would happen if the growth rate were exogenously fixed by animal spirits. So the chain of causation in your model could be schematically presented as follows [diagram with arrows from ‘exog g ’ and ‘ k ’ pointing at ‘necessary l ’, from which an arrow points at ‘ π ’, with a second arrow pointed at π from ‘saving props out of P and W ’].

Why couldn’t this happen in my model? Because the value of π given by your model, although it would generate the right total amount of finance, would (given f , x and r) generate the wrong mix of finance. That is, it would generate the wrong relative amounts of retained profits and household saving, ... inconsistent with firms’ desired financial structure. Either household savings would exceed firms’ desired borrowing, or they would fall short.

Thus in my model π has to be such as to generate both the right total amount of finance and the right mix. The need to generate the right mix is an additional constraint (ie not present in your model) which means that l cannot adjust to become equal to gk if g is exogenously given, as is

the case in your model. Hence, in my model, if economy 2 has a higher k than economy 1, it cannot have the same equilibrium growth rate (and it will in general have a lower one).

[Omitted from the above letter was a draft paragraph that read:

I am still not sure how to go about answering your specific questions in the most useful way. It would help me very much to have your answer to the following general question. Is the basic point you are making about my simplified model in section 4.2:

- (a) that it contains a logical flaw?
- (b) is logically ok but makes unrealistic assumptions about how the economy works?
- (c) is logically and economically ok but no different from your model?
- (d) something else.]

27 November. Letter from Robinson.

I am trying to find out the structure of your model at one point in the process of growth i.e. when aggregate I is given. It seems to me that there are two cases where s approaches zero. (1) All households spend what they get. Then the multiplier is infinitely large and firms find that whatever capacity they create is always fully utilised. They fix prices so that they get an amount of retentions this year to finance investment next year. (Do you not need bank advances for $(I_{t+1} - I_t)$?) They can make I whatever they like, subject to a limit on real wages. (2) Households have a propensity to save subject to a bottom stop equal to minimum consumption, C [illustrated in simple diagram]. Then when $r\pi$ is very nearly equal to I/Y , income cannot rise above $I + C$. The overall multiplier is $1 + C/I$. I quite agree of course that government and foreign trade are important but I want to take the model for a walk before bringing them in.

28 November. Questionnaire from Robinson.

[In its first column, five questions, then Yes or No columns for Wood's answers (in italics).]

1. Is the split between borrowing and external finance for each firm given by $I - rP$?
No. Assuming $f = 0$ for simplicity, it is determined by x . Borrowing is xI . Internal finance is $(1 - x)I$.

2. For the aggregate of firms is borrowing equal to $I - r\pi Y$?
Yes, it is equal to it, but it is not determined by it. It is determined by x and I .

3. Is C (total consumption by households) = $Y - I$? *Yes.*

4. Is $C = \frac{1}{1-s}(I - r\pi Y)$ where s is the proportion of household income saved.

No. $C = \frac{(1-s)}{s}(I - r\pi Y)$ (proof attached – but this surely isn't of fundamental importance).

5. What is the problem? *The problem is that you ignore x . That is, you ignore the fact that in my model personal savings must equal desired company sector borrowing. In algebraic terms, $s(1 - r\pi)Y$ must equal xI , where x is an exogenously given parameter.*

2 December. Another questionnaire from Robinson.

[Headed 'Current income flows', again with Wood's responses in italics.]

1. Whatever plans may have been in mind, firms are carrying out a certain rate of investment I at a particular moment. *Yes.*
2. They have set prices yielding a certain π and are retaining $r\pi Y$. *Yes.*
3. They are borrowing at the rate $I - r\pi Y$. *Yes.*
4. They are injecting into income $I - r\pi Y$. *Yes.*
5. The household income multiplier applied to $I - r\pi Y$ generates an amount of saving equal to the borrowing of firms. *Yes.*

BUT

(i) Although this will be an equilibrium in the sense that $I = S$, it will in general not be an equilibrium in two other relevant senses. (a) Actual borrowing by firms ($I - r\pi Y$) will not in general be equal to desired borrowing (xI). (b) The actual level of aggregate demand will not in general be equal to the expected level of aggregate demand which induced I .

(ii) Thus firms will in general change both π and I in the next period, and thus, among other things, one hasn't explained what determines π . In 2 above, you take π as arbitrarily given. Yet only if (by chance) the arbitrary π happens to be the equilibrium π will it remain unchanged. And one cannot say (at any rate in my model) what would be the equilibrium π without introducing (inter alia) x , which does not appear in your analysis above.

(iii) More generally, I fear that the short run apparatus you are using here is not appropriate for analysing the properties of my model in section 4.2, which is explicitly set in a long run equilibrium context: long run in the sense that I is an endogenously determined variable, and equilibrium in the senses (a) that actual borrowing = desired borrowing and (b) that actual demand = expected demand. A more suitable apparatus, in my view, is the one set out in the note I sent you yesterday.

[On the back of the questionnaire sheet was the following note from Robinson]

Would you please re-read my letter about k (I think 3 ago). I was not conducting the argument in terms of s_P and s_W , but in the terms overleaf.

[In reply to which Wood wrote on the back of the questionnaire, before returning it to her]

... I have re-read the relevant letter, and my reply to it dated 27 November. I certainly didn't mean to imply that you were conducting the argument in terms of s_P and s_W , and I am sorry that I labelled my flow diagram in a misleading way. Let me redraw it as follows [diagram with arrows from 'exog g ' and ' k ' pointing at 'necessary l ', from which an arrow points at ' π ', with a second arrow pointed at π from 'company saving propensity (r) and household saving propensity (s)']. The entire argument of that letter of mine then follows as before. Would it be helpful if we talked at this stage? If so, I would suggest next week as a good time. Shall I telephone you to arrange a time?

2 December. Note from Wood.

[Referred to above as 'the note I sent you yesterday', which crossed with Robinson 2 December questionnaire.]

Definitions

We are dealing with two basic relationships.

(1) $I = S = \text{Retained profits} + \text{Household } S = rP + s(1 - r\pi)Y.$

(2) $I = \text{Company Finance} = \text{Retained } P + \text{Company Borrowing} = rP + xI.$

Together they imply a third relationship, namely

(3) $\text{Household } S = \text{Company Borrowing}.$

The Difference between the JR model and the AW model

Both models can be written down using the same pair of equations (which are derived from 1 and 2 above), namely

(1a) $gk = r\pi + s(1 - r\pi)$ The $S = I$ relationship

(2a) $\pi = \frac{(1-x)}{r} gk$ The company finance relationship, assuming $f = 0.$

For this to be a determinate system, two of the variables ... must be endogenously determined; the remainder must be exogenous parameters.

(i) JR and AW both treat π as one of the endogenously determined variables.

(ii) JR and AW both treat r, s and k as exogenously given parameters.

(iii) JR and AW disagree over which of the two remaining variables (x and g) should be treated as exogenous. AW says x is exogenous and g endogenous: ie company borrowing policy is an exogenous factor which affects π (or more accurately $r\pi$) and thus the value of the multiplier and thus the rate of growth. JR, by contrast, says g is exogenous (determined by animal spirits, possibly as a function of π); this implies that x must be endogenous, ie that companies will passively borrow whatever households happen to want to lend.

Undated response from Robinson

[to Wood's 2 December note, returned to her with his comments (in italics) as follows.]

1. How do firms know in advance how much household saving there is going to be? *They don't.*

2. I also allow x to be exogenous. It determines the excess of investment over internal savings.
* *[indicating simply that I had noted the point]*

3. I do not need any particular theory of I , only that, at any moment, I is running at some level. However, I like your theory of the determination of I , that is, each firm is planning to provide capacity for the flow of sales it expects to achieve (allowing for its price policy). *

4. The operation of the multiplier on $I - r\pi Y$ causes the public to save an amount equal to the desired borrowing of firms. *The multiplier doesn't necessarily generate the level of demand (Y) that firms expected when they decided on a particular I and a particular π . Thus actual RP will not equal expected RP . Thus actual borrowing will not equal desired borrowing. Thus actual savings of households (while equal to actual borrowing of firms) will not equal desired borrowing. Moreover, since actual demand is not equal to expected demand, I must be altered. My theory of I is a vital part of my model.*

Another undated response from Robinson

[which again Wood returned to her with his comments (in italics).]

I. Can you set up a model to show how causation runs from saving to investment? *I don't believe that it does.*

II. All this is crude because it ignores time lags. If we make an artificial period of a year. This year's I is financed by last year's $r\pi Y + S$. Finance must be available before it is spent and saving accrues afterwards. You cannot finance I out of the saving generated by the multiplier attached to itself. For this reason I think that if I is growing from year to year there must be borrowing from banks. See Accumulation [Robinson's book *The Accumulation of Capital*] section on Finance. *I absolutely agree. Hence all my discussion of short-run finance. But my model is a long-run model, in essence. "Extra" borrowing from banks will occur to finance I in the short run, but it will be repaid later so that firms can get back to their desired long run gearing ratios.*

III. My interpretation of your theory can accommodate extreme cases. Suppose $r = 0$. Then $Y = \frac{1}{s}I$. Then $S = I$. *My theory collapses. In particular, the equation $\pi = \frac{(1-x)}{r}gk$ collapses.*

I can change with many circumstances. Does a change in I have to wait for a voluntary change in S ? Or in s ? *Actual* I will of course be affected by anything that changes firms' expectations of future demand. *Equilibrium* I will however depend on s , among other things.

I have shown how it works when $S = 0$. Then $r\pi Y = I$. *If $s = 0$ my model breaks down. For if $x > 0$, household savings cannot be equal to desired company borrowing.*

I gave two cases, one where the public does not save and one where firms do not borrow. Do you accept this? *In my model, both s and x have to be > 0 . If one is zero and the other positive, or if both are zero, the model collapses.*

Have you simply forgotten that x determines the excess of investment over retained profits, ie the multiplicand for household income? ** No. On the contrary, I had thought (clearly erroneously) that this was something that you had forgotten.*

Undated note by Wood to himself

[about the preceding responses from Robinson.]

1. Don't seem to be divided on any point of substance about how economy works. See *'s. I suspect that one problem is that I tend to talk in terms of equilibrium growth in discussing the section 4.2 model, while JR uses disequilibrium examples.

2. Nor does JR appear to be saying that there's anything logically wrong with my model. Hence somehow all this confusion has been generated by the correspondence itself.

Another undated note by Wood to himself with numerical examples.

8 December. Wood's diary shows a meeting with Robinson.

Undated note from Robinson

[possibly after the meeting]

Adrian Wood's Model

An isolated pure capitalist economy is an abstraction. The assumptions about its behaviour are as follows.

Firms plan gross investment to provide capacity for what they hope to sell. In any one year, I is taking place. Conditions in the money market constrain firms to borrowing xI . They plan to save $(1-x)I$. They are constrained to maintain a certain $(1-r)$ (omit f). Thus they must get profits such that $r\pi Y = (1-x)I$.

Households save a proportion s of income. Household income is $(1/s)(I - r\pi Y)$. Household saving is $I - r\pi Y$. Household expenditure for consumption is $\frac{(1-s)}{s}(I - r\pi Y)$.

Harmony requires that s should be low enough to permit π to be high enough to make $r\pi Y$ sufficient to cover $(1-x)I$. Missing link: influence of $(1-r)$ and π on s .

[And on the back of a piece of graph paper.] The formulae are wiser than the men who thought of them. Poincaré

20 December. Aerogramme from Robinson [at Centre for Development Studies in Kerala.]

In the discussion we broke off you were contending that there is a theoretical upper limit to the share of saving in Y , therefore to the level of k with which a given rate of growth [~~that~~] can be sustained. In von Neumann there is a technical maximum to the rate of profit on capital, given the real wage rate. How does the limit work in your system? You do not seem to have discussed the influence of π on s . Given r , a higher π increases the share of rentier income relative to wages. Does this increase s ? If so, what is the limit? I think the catch is the other way round. If s is too high (ie demand for consumption goods is too low) it is impossible for the firms to get sufficient $r\pi$ to match an indefinite level of desired investment (which depends on expected growth, not on level of Y []). However, I think golden ages are out of date. We must go back to fluctuations. Have you seen Minsky on Keynes? His approach should be sympathetic to you.

9 February 1976. Letter from Wood [with apologies for the delay.]

1. Von Neumann and Wood

As I understand it, von N assumed a given real wage – which determines the (maximum) profit rate, and also the share of profits (if one uses output per man as numeraire, the share of profits is simply $(1-w)$ where w is the real wage rate measured in terms of the numeraire). What my model is designed to do (and what, as I sadly recognise, you think it fails to do) is to explain what determines the share of profit and thus (in a given state of technique) the real wage.

2. The influence of π on s

You are absolutely right to suppose that π will in general affect s (household saving propensity) because it will affect the share of rentier income in total household income. I deliberately left this out of the model to avoid making the algebra any more complicated. For I do not believe

that introducing it would make any fundamental difference to the properties of the model. Suppose I rewrite equation 12 on p 117 as

$$l = r\pi + s_r(1-r)\pi + s_w(1-\pi)$$

Where s_r and s_w are rentier and worker saving propensities. Then equation 13 becomes

$$\pi = \frac{s_w}{\frac{r}{(1+f-x)} - [r(1-s_r) + s_r - s_w]}$$

which is simply a nastier-looking version of the same thing. [Detailed algebraic working was not sent to Robinson.]

I should add that I am very sorry that our discussion has focused so exclusively on pp 111-119. For although that is where I set out my macro framework, the point of what I am doing, the purpose of my assumptions and equations, really only emerges in pp 124-48, and even more in the last chapter on government policy. I sometimes wonder if my theoretical model would seem clearer or more plausible to you if you watched it in action, so to speak.

Perhaps I ought also to say how sorry I am that you seem to regard my book as an attack on the Cambridge theories of distribution. It is not – at least it is not intended to be – it is a development and extension of them. But I now fear that, in my efforts to make clear the points at which I differ from you, Richard, Nicky and Kalecki, I have seemed ungrateful. For without you, I literally could not have begun.

Appendix D: Geoff Harcourt correspondence and thesis comments

This appendix contains transcriptions of the substantive content of correspondence in 1975-76 between Adrian Wood, Peter Kenyon and Geoff Harcourt, and of Harcourt's written comments in 1972-73 on Wood's PhD thesis. Non-substantive omissions are indicated by ... The originals of these documents are in the National Library of Australia (MS 10817; MS Acc24.097).

Letter of 15 December 1975 from Peter Kenyon and Geoff Harcourt to Adrian Wood

From Kenyon: Recently, I saw a review of your book: *A Theory of Profits*. From the review it seems that parts of your book are concerned with some issues that I am working on in my M. Ec. thesis. I have sent under separate cover a copy of a paper that Geoff Harcourt and I wrote and would very much appreciate any comments that you may care to make. As yet, I haven't seen your book; it doesn't seem to have reached Australia yet, though Geoff did say something about having read your Doctoral dissertation. However, he couldn't recall the details. We have submitted the paper to *Kyklos*, though we haven't had a decision on it as yet.

From Harcourt: You'll receive under separate cover a paper which Peter Kenyon (a research student of mine) and I have written. It arose out of a seminar Peter gave last year. I must confess that at the time I sketched out the skeleton of the argument, I'd completely forgotten about your work – a reflection of my age not your work. But seeing your book advertised and reviewed in *The Economist* aroused in me the horrible suspicion that we may have crossed your paths more than somewhat. Anyway, if you have the time, please do have a quick look at it. If we have, it is entirely my fault, not Peter's, and it was truly quite unconscious. I'm enclosing also the little note which I wrote after Peter's seminar, which drew, I thought, on work I'd first done in 1966 [the note was not enclosed, because the letter was a one-sheet aerogramme]

Letter of 5 January 1976 from Wood to Harcourt and Kenyon

Due to the vagaries of the Christmas mail, I received your letter and papers only two days ago. ... I sent you (Geoff) a prepublication copy of my book (which does indeed overlap quite extensively with your paper) at the very beginning of November, but by sea mail; had I realised how long it would take to get to you, I would have sent it by air. I hope it has now arrived.

Anyway, I very much enjoyed reading your paper. I think the economic argument is basically correct, and I didn't detect any slips. There are a lot of comments that I would make, mainly of the 'I would have gone about it differently' variety, but most of them, I hope, will be self-evident once you have read the book.

One point that is perhaps worth noting here is the difference in our micro frameworks: yours is a model of price and volume of sales, mine a model of profit margin and value of sales. I adopted this framework for various reasons, but principally because most firms are multi-product firms (and also because it seems to me very unlikely that a firm would maximise the volume rather than the value of its sales in any situation in which the two objectives were in

conflict). The two frameworks are not inconsistent, and there is no problem of principle in proceeding from your framework to mine, or vice versa (*cf.* p. 62 [of my book]), although the algebra would be complicated.

A point that slightly puzzles me concerns the choice of technique. As you will see, this presents no special problems in my model (at any rate at the level of a single firm), and thus I have a feeling that perhaps you are making unnecessarily heavy weather of it. That is, in my model there is always some technique which maximises growth, and this is the one which a growth-maximising firm chooses: I suspect that this is probably also true in your model, and thus that you are too modest (and too convoluted) in Section III.

It is very kind of you (Geoff) to worry about having inadvertently appropriated some of my ideas. It's hard for me to say if your worries are justified, not least since I'm not the only person who is working (or who has worked) along these lines. But in any event I would certainly appreciate it if you were to make some reference to my book.

Letter of 8 January 1976 from Harcourt to Wood

I have just received the copy of your book which you so thoughtfully sent me – many thanks indeed. I've had a quick look at the opening chapter in order to refresh my memory and I'm looking forward very much to reading it (the book!) in depth. I was relieved to see that while Peter Kenyon and I do use the *general* idea of an investment-profits link, and its influence on the mark-up, I had already done this in a rudimentary way in my 1972 book and 1965 *Ec. Rec.* article and the details of our paper owe more to the work I was doing in 1966-67, drawing on Salter, than to any (unconscious) pinching of your views. But we will, of course, add your book to our references and footnote any overlaps in the text.

Letter of 13 January 1976 from Harcourt to Wood

Many thanks for your letter which arrived today, ... crossing yours. I was delighted with your response to the paper. When Peter returns from ... Melbourne, we'll take your comments into account. The paper is still with *Kyklos*, who are taking a devil of a time to process it.

Transcript of Harcourt's 1972-73 comments on Wood's PhD thesis

There are seven pages of hand-written comments. The number before each comment refers to the page in the thesis (its first digit identifies the chapter – chapter titles have been added here). The thesis, An Analysis of Income Distribution, 1972, is in the Cambridge University Library. <https://www.openaccess.cam.ac.uk/theses/finding-and-accessing-theses>

Square brackets around text without ? marks indicate expansion of an obvious abbreviation or an explanatory comment. Square brackets containing words and ?s indicate guesses about

illegible words (more ?s mean more uncertainty), and those with only ?s indicate inability even to guess. The 'my book' to which Harcourt refers is his Some Cambridge Controversies in the Theory of Capital, Cambridge University Press, 1972. 'Hahn and Matthews' refers to a widely-referenced survey of growth theory cited in the thesis.

Chapter 3. Social bases of earnings structure

3.2 I don't believe that the [moving averages] do [? provide this information]; I think the whole notion of [moving averages] is inappropriate to the concepts you are after, which is what [would] be established eventually under [*ceteris paribus*] conditions which in fact never hold long enough to be able to draw them out of the statistics.

Chapter 5. The determination of profits

5.1 Equality of *ex ante* with *ex post* profits at [equilibrium] only is a key point.

5.2 [refers to footnote 1] I object strongly to your implied assumption that my book is a superficial if comprehensive and timely survey while [Hahn and Matthews] are more basic. I agree that I did not go into the whys and wherefores of the determination of [the rate and share of profit] as much as I did into other topics but that was because I regarded the work on these from my point of view as tying up loose ends which emerged from the central core of what I'd been looking at. But I think the use I made of [wage-rental] relationships and the same model the book through in [? order to sort out their elasticities] and the basic issues involved (when the sophisticated [?? theorising] has been removed) is more than superficial – in the same way as [Hahn and Matthews] [? simplify] the various Harrod issues by the use of their production [function] model. Kahn's work is, of course, quite basic and in a class of its own as original.

5.3 [refers to footnote 1] Again, I agree with you that these points have been neglected (but not, I might say, by me in other places; I regard them as central issues). [refers to footnote 2] In your statement about fixed coefficients assumption – does this still stand in the light of the [? reversal] [now] very [??] claimed for [general equilibrium]? See [?Arrow/Hahn].

5.4 [refers to footnote 1] If you look up my bibliography, you will see that the Treatise and the General Theory are included though not explicitly with reference to ch 5 which I accept is an omission (though I do refer to Keynes' widow's cruse theory). You are right about Hahn; it was not until I read his PhD thesis this year that I realised why he claimed to be part of this scene. I'd tried his [Oxford Economics Paper] paper several times but always boggled at it.

5.5 Should you not bring the important role of [?expected] profitability (which is associated with expectations of expanding output) in the Keynesian [investment function].

5.6 Why can't the two mechanisms be complementary, ie occur together, rather than substitutes?

5.7 I can see that with Kalecki the level of income does not affect [the share of profits]: but why not [the share of investment]. Are you assuming that there is no autonomous item in the saving function? Thus if $Y = I/s$ then $I/Y = s$ for all I , but if $Y = (I + A)/s$ then $I/Y = s(I)/(I + A)$ which is not independent of I .

5.7 Doesn't [? Kaldor] have I/Y rather than I as the determining variable?

5.9 Depends on your view of secular growth, i.e. is it more than the cumulative outcome of successive [short period] happenings?

5.17 When you say the value of the multiplier may be increased don't you really mean two conflicting factors are at work? The fall in investment tends to reduce [???]. And also to shift the distribution of income towards the wage earners. Hence what happens is the outcome of whether $-\Delta I$ is greater than $+\Delta C$.

5.19 But if $O^* = \bar{O}$ aren't you wrong about $q = \bar{q}$?

Chapter 6. The financial behaviour of companies

6.6 What is C and what is λ ? In (6.3)

6.7 Very unclear what is going on and what the variables are – need reminding.

6.9 Let me ask an [idiot boy?] question. How in a one-commodity model, do you have price changes? [????] I understand! I should like to see an empirical solution of (6.7).

6.16 ambiguity: you imply that it is rational to pay the same [?? tax] rates on [dividends] and [capital gains].

Chapter 7. Growth, competition and profits

7.1 I don't like this moving average approach – it seems to me a statistical outcome which has little behavioural significance, i.e. it is not something to be explained.

7.2 I agree very much with what you say on his page but think that the [moving averages] are an inappropriate reflection of it.

7.4 [refers to footnote 1] Is that the correct [definition] of g_o ? Shouldn't it be $(g_c - g_p)/g_p$?

7.9 [refers to footnote 1] I'm confused here: I thought it was at under capacity that external finance targets were reached, in which case it should be the more attractive PTO I'm confused.

I thought that at full capacity the target could not be fulfilled – in which case it should be the less attractive ones who do fill their targets

7.19 What is the intuitive explanation of your interesting result?

7.24 Again I don't get this moving average concept.

7.28-30 Your aggregation [? parameters] may differ very markedly from those of the [?individual companies] which make up the aggregate?