What Remains of the Cambridge Critique?
On Professor Schefold's Theses

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Abstract

Professor Bertram Schefold's recent papers on capital theory and the Cambridge critique argue that the very low likelihood of reswitching and reverse capital deepening that appears to emerge from empirical input-output tables is confirmed by theoretical results; these results, he concludes, largely rehabilitate traditional neoclassical views on capital and show that the Sraffian critics' insistence on reverse capital deepening as a fundamental criticism of neoclassical theory is misplaced. The present paper raises doubts about these arguments. In particular Professor Schefold does not give adequate consideration to the 'supply-side' problems with the measurability and the given endowment of the traditional notion of capital as a single factor. On the empirical evidence based on input-output tables, I agree with Professor Kurz that it suffers from very serious weaknesses. The more recent argument for an extremely low likelihood of double switching, advanced in Schefold (2016) and (2017), appears criticisable too.

1. Professor Schefold's main theses

1.1. Professor Schefold's papers on capital theory subsequent to Han and Schefold (2006) have argued that the extremely low likelihood of reswitching and reverse capital deepening that emerges (according to him) from the study of empirical input-output tables is confirmed by theoretical results; these results, he concludes, rehabilitate to a considerable extent traditional neoclassical views on capital, and show that the insistence of the 'Sraffian' critics on reverse capital deepening as a fundamental criticism of neoclassical theory is misplaced. In a Conference held in Graz in 2011, where Schefold pre-

sented a first version of (2013a),[1] in my oral intervention I expressed disagreement with those views. His numerous papers on the theme (2010, 2013a, 2013b, 2016, 2017, 2020, and 2020 co-authored with Professor Kersting are the ones I will cite, but there are at least five more) have not persuaded me that those views were correct, and here I try to explain why. Let me anticipate that one view (very few wage curves on the envelope), advanced in (2013b) and developed in greater detail in the very recent Kersting and Schefold (2020), I will not reject, although on the basis of a different argument as I explain at the end of this paper.

Luckily, in science disagreement can go together with the highest respect and esteem, and this certainly describes my feelings in this occasion.

The origin of the stream of papers I intend to discuss is indicated by Schefold in his surprise at how little the empirical exercise in Han and Schefold (2006) seemed to confirm the occurrence of reswitching and reverse capital deepening, phenomena he expected to be frequent. (Whether the surprise was justified is one of the topics I will discuss.) Since that paper, he has been exploring what might be the cause of the low likelihood of occurrence of reswitching and reverse capital deepening that seems to him to emerge from empirical enquiries. He has concluded that indeed there are theoretical reasons which point to a very low likelihood of those phenomena. Technological matrices, he argues, can be characterized as large and *as if* random, and it emerges:

> that, by and large, random properties of the systems of production suffice to justify the conceptions of capital of the 19th century economists, i.e. of those using the old neoclassical equilibrium, the production function and Marxian theory (2016, p. 4).

Let me say immediately with reference to the above quote that I see the potential relevance of Schefold's arguments as very different for the 'old neoclassical equilibrium' and for Marxian theory. For the latter theory, the acceptance or rejection of Schefold's conclusions would have no effect on the validity of the surplus approach, which is what Marx inherits and develops. This approach does not depend on the correctness of Marx's idea that in the aggregate there is a reciprocal compensation of the deviations of exchange ratios from relative labour values that allows the determination of the rate of profit as the ratio between labour value of physical profits and labour value of physical advanced capital. The magnitudes (normal production methods, the several real wages, the quantities produced), that the surplus approach takes as given when the question is the explanation of normal prices and of the distributive variables other than wages, and which *are the same magnitudes from which Marx starts in order to determine labour values and through them the rate of profit*, do allow the determination of the rate of

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[1] Articles and working papers referred to simply by their date are by Professor Schefold. A first version of the present paper was read at the STOREP Conference, Siena, 29-30 June 2019; a second version (December 2020), besides benefiting from the useful comments I had received on that occasion, tried to take into account some of the papers on the topic that had come out in the meanwhile; the present third version (June 2021) has been improved by very useful comments by Roberto Ciccone, Sergio Parrinello, Anwar Shaikh, Paolo Trabucchi, an anonymous referee, and the editors of Centro Sraffa Working Papers.

profit and of prices of production (the possible difficulties due to joint production or intensive rent are not what is at issue here). The accusation of inability to determine the rate of profit and normal prices, moved against Marx and Ricardo by Jevons, Walras, Böhm-Bawerk, is groundless. How much Marx's rate of profit differs from the correct one has no consequence on the robustness of his general approach nor on the validity of his analyses of alienation, fetishism, exploitation (Garegnani, 2018; Petri, 2015a). For this reason I will not discuss Professor Schefold's arguments on Marxian theory.

On the contrary the validity of the 'old neoclassical equilibrium' *is* at stake, and with it the validity of the *entire* marginalist or neoclassical picture of the working of capitalism, because the neo-Walrasian versions of general equilibrium theory cannot support that picture, owing to the impermanence problem, the price-change problem, the substitutability problem, the absence of justification for the assumption of investment adapting to savings (Garegnani, 1990; Petri, 2017). The inability of neo-Walrasian general equilibrium theory to discuss disequilibrium adjustments (and hence stability) except via fairy tales with no implication for actual economies should be enough totally to discredit it. And the absurdity of the assumption of perfect foresight, without which a neo-Walrasian intertemporal general equilibrium cannot even be *defined* (and therefore cannot be proved to exist)[2], should again be enough to judge that theory simply ridiculous as an explicative, predictive theory. Actually, neo-Walrasian general equilibrium theory is only a smokescreen to hide a continuing belief in the traditional marginalist forces and adjustments that supported the tendency to the 'old neoclassical equilibrium' (Petri, 1999, 2020; Dvoskin and Petri, 2017). These forces and adjustments rely on traditional capital-labour substitution, where capital is a single factor of variable 'form', necessarily to be measured as an amount of value, and yet assumed to behave as if it were a physically homogeneous factor. Sraffa (1960) and Garegnani (1960, 1970, 1990) found this conception of capital indefensible, and their arguments convinced many economists. So if Schefold is right and his results really 'suffice to justify' that conception of capital and the connected neoclassical picture, the thing is very important.

1.2. Schefold argues that his results provide

> a partial justification of the old neoclassical general equilibrium model, which used the idea of capital as an endowment of a quantity of value (2016 p. 4).

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[2] Intertemporal equilibria need not only 1) perfect foresight of the relative prices that will rule in each possible future state of the world, an assumption always admitted to be utterly unrealistic whenever one is called to express an opinion on the issue, but also 2) a complete list of *which future states are possible*, which means perfect foresight of all possible future novelties too (e.g. scientific discoveries), which is logically impossible. The alternative of temporary equilibria without perfect foresight appears to have been completely abandoned after the difficulties found in the 1970s and 1980s with formalizing these equilibria without arbitrariness (e.g. with deciding how firms choose their plans), and with proving their existence; now they are no longer mentioned in textbooks.

The justification consists of

(i) an empirical argument (to be discussed in Sections 3 and 4): the observation of nearly linear wage curves derived from Input-Output tables, and of a very low frequency of reverse capital deepening among the switches of technique on envelopes of these wage curves; the proposed explanation of this empirical observation is randomness of technical coefficients, which if accepted implies nearly linear wage curves, thus also explaining the infrequent reverse capital deepening, and making the construction of approximate aggregate production functions possible;

(ii) a theoretical argument (to be discussed in Section 5) of improbability of reswitching in my sense, i.e. of double switching,[3] because, when two wage curves cross *on the envelope* at a certain rate of profit, meaning that they differ in the method to produce a good and that the two alternative methods are equally profitable at that rate of profit, the probability can be argued to be practically zero that out of all possible vectors of coefficients causing the switch these two methods have one of the very few couples of vectors of coefficients that would cause the two wage curves to cross again (on or below the envelope) at some other feasible rate of profit.

I would have liked more explanation of what exactly that 'partial' justification justifies and what it does *not* justify. I had a similar problem with the 'by and large' in the previous quotation. But after a careful reading of Schefold's papers it seems to me that a view emerges which, even though not insisted upon with the same force in all papers, is still clear enough and important enough to deserve detailed discussion. Quotation (A), which relies only on argument (i), states that that argument suffices to support the traditional neoclassical conception of capital and of 'well-behaved' capital-labour substitution, with the implication of a tendency toward full employment if no rigidity prevents the working of supply and demand. This amounts to a comprehensive rehabilitation indeed, and the subsequent addition of argument (ii) in Schefold (2016, 2017) has only confirmed it. Thus, were it not for the insufficient capital-labour substitutability mentioned in quotations (D) and (E) below and recently insisted upon in Kersting and Schefold (2020), and implying that in all likelihood the demand function for labour is not

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[3] I use 'reswitching' as synonymous with double switching; Schefold does not call reswitching the double switching of two wage curves except when both switches are on the envelope of wage curves. With his terminology, *reverse capital deepening without reswitching* occurs when two wage curves double switch and only the switch associated with a positive real Wicksell effect is on the envelope, the other switch being dominated by some other wage curve. Absence of reswitching in Schefold's sense allows the use of Champernowne's chain-index to measure capital per unit of labour, or of an index of capital per unit of labour equal to the (absolute value of the) slope of the tangent to the envelope of wage curves. But these indices in no way supply the measure of capital that the marginal approach to distribution needs, they are not measures that allow conceiving the quantity of capital as capable of changing 'form' without changing in amount, which is what the approach would need. For this reason, I view double switching as more important than the return of the same wage curve on the envelope and prefer to reserve for it the evocative term 'reswitching'.

elastic enough to produce a credible equilibrium wage, apparently the traditional neo-classical approach of J.B. Clark and Wicksell would be defensible. I intend to question this claim.

1.3. The following quotations seem to me well to summarize Schefold's central contentions and will be referred to below:

(A) We are thus led to the general conclusion that the 'local' construction of the production function is not devoid of content: capital can be aggregated in more general cases than one-commodity systems....

 The construction seems sufficiently robust to support contentions such as those that I associate with Böhm-Bawerk (1914): suppose the book of blueprints is given and does not change, suppose both factors are fully employed, suppose that trade unions enforce a rise of real wages. The choice of techniques, guided by mere profit maximisation, will then lead to an increase in the intensity of capital and hence to unemployment. Can it be cured by Keynesian means, either because of a demand effect resulting from an increase in wages or by state expenditure, while real wages stay at their elevated level? The answer is 'no', since full employment of 'capital' was assumed; full employment requires a lowering of real wages in this case — the Keynesian remedy, which presupposes idle capacity, is not applicable. The idea of rigidly given levels of capacity, the stationary nature of the economy and other, hidden, assumptions of this story may be questioned, but that leads into different territory. Whatever other arguments may be adduced, the point is that this argument can no longer be simply dismissed *on the basis of the critique of capital* (2013a, p. 1181).

(B) A stochastic approach has been introduced to explain the empirically observed fact that wage curves calculated from input-output systems tend to be nearly linear and that the paradoxes of capital appear to be rare (2016, p. 1, from the Abstract).

(C) The argument, neoclassical theory is entirely wrong because of reswitching etc., cannot be sustained except at the level of the most abstract theorising, while new arguments are coming up. The core of the neoclassical propositions is: if there is unemployment in a closed economy, a fall of real wages will make *known* methods of production profitable which will use more labour; the conversion of existing capital goods into others with a lower intensity of capital will make it possible to absorb the unemployed without a (significant) volume of net investment. (2016, p. 41)

(D) Our analysis suggests that the neoclassical mechanism to ensure full employment is no*[t]* so much threatened by capital reversals, creating instabilities, as by a lack of suitable *efficient* techniques, that is, by a lack of wage curves *on the envelope*, which stand for techniques allowing to increase employment significantly, using existing capital resources which are transformed somehow in accordance with Clark's vision. For we have seen that, at the relevant low rates of profit, wage curves appear on the envelope with properties that do not deviate much from the neoclassical ideal, but they are few (2016, p. 42).

(E) When the paradoxes of capital were brought to the fore in the reswitching debate, it seemed to many that economic theorising would have to start afresh. However, it now appears the paradoxes occur only rarely. Reswitching is improbable. Wicksell effects always exist; if anti-neoclassical, they create an instability, if 'neoclassical', they still disturb the idea that mere physical properties could allow to distinguish capital-intensive and labour-intensive techniques. The theoretical economist aiming at empirical relevance will perhaps not be impressed by such Wicksell effects, which have been known to exist since Ricardian times, since prices are somewhat sticky. So it is what we might call the poverty of production functions, the fear that the approximate efficient labour-using technique simply might not exist, that must worry the economist. The new facts that gave rise to this conclusion can be explained by the theory of random matrices (2016, p. 43).

(F) [under 'the assumption that the coefficients defining a Sraffa system are random'] it turns out that a neoclassical equilibrium with demand for and supply of capital as a value magnitude and with the composition of the capital stock determined endogenously (the so-called 'old' neoclassical equilibrium) then is macro-economically stable and quite similar to the surrogate production function (2017, pp. 172-173).

2. The supply of capital

2.1. Schefold argues that reswitching and reverse capital deepening are very rare, and from this concludes that they are irrelevant and that the traditional marginalist conception of capital is thereby rehabilitated (or would be, if there were not the problem of 'the poverty of production functions'). But is it really the case that a low empirical occurrence of these phenomena, if convincingly proved, would rehabilitate that conception of capital?

In several papers by Garegnani, by myself and by others a distinction has been made between two kinds of problems of the traditional marginalist conception of capital as a single factor: supply-side problems that concern the existence of a non-circular measure of capital for the determination of marginal products and of equilibrium, and the right to consider the supply of capital (its endowment) as given; demand-side problems that concern the legitimacy of assuming that demand for capital obeys the same laws as demand for labour or land in non-capitalistic economies. Now, Schefold appears essentially unconcerned by the supply-side problems.[4] In several places he puts forth sentences

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[4] He seems to conceive the Cambridge critique as consisting only of the use of reswitching and reverse capital deepening to deny the legitimacy of aggregate production functions and to argue the instability of equilibrium. But Garegnani (1970, p. 407), Joan Robinson (1971), Harcourt (1972, pp. 3-4, 18-20) all stress the difficulty with finding a measure of capital not guilty of circular reasoning — and if such a measure cannot be found, then the endowment of capital is indeterminable.

that suggest that if the demand-side problems can be considered negligible, that is, if the demand for capital or more precisely the capital-labour ratio can be considered a decreasing function of the rate of interest, then this 'suffices to justify the conception of capital' of traditional marginalist economists. And in several places he seems to accept the neoclassical picture of a change in real wages as needed in order to change in the opposite direction the amount of labour that the economy's capital endowment can employ (after appropriately changing its 'form'). For example the traditional neoclassical view summarized in quotation (C) above is followed by the assessment in quotation (D), where the notion of a given 'quantity of capital' that can change 'form' without changing in 'amount' seems to be fully accepted; the same holds for quotation (A). Apart from the lack of sufficient 'substitutability', Schefold seems to see problems with the mechanisms based on that notion of capital only if reverse capital deepening can be considered sufficiently frequent as to cause instabilities of the savings-investment market. The notion itself is not questioned.

It is important then to remember that that notion of capital was rejected not only by Garegnani's 1958 PhD thesis (or the Italian 1960 version), which rejects the entire marginal/neoclassical approach on the basis of the supply-side problems alone, but also by the marginalist authors responsible for the shift to neo-Walrasian formulations. After Wicksell's admission of an 'indeterminateness' of the amount of capital (Wicksell, 1934, p. 202), it was impossible for Lindahl, Hayek, Hicks to maintain the notion of capital as a given 'fund'; they were clear that this 'fund' would have to be measured as a quantity of exchange value and that this was illegitimate and that, in order not to abandon a supply-and-demand explanation of distribution, it was necessary to move to 'an improved analysis which will have more general validity' (Lindahl, 1929 [1939], p. 317), that is to neo-Walrasian formulations. Nor can one find in these authors any suggestion that the relevance of the difficulty depended on *how much* the value of capital depends on prices. There is no hint in them that the notion of capital as a 'fund' would be defensible if the value of capital goods were affected, but not much, by changes in distribution.[5] It would seem that they would have approved Sraffa's insistence in the

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[5] Nor can one try to argue that this 'indeterminateness' of the capital endowment is very limited. As shown by Sraffa, the normal value of a stock of durable machines of different ages is relevantly affected by the rate of interest even if the price of the machine when new is not. That is, if there is durable capital then even a near absence for new commodities of the price Wicksell effects mentioned in Schefold's quote (E) would not suffice to consider the *normal* value of given capital goods as approximately given. Even less would that be legitimate if some capital goods were rendered no longer part of the optimal technique by a change of the interest rate: their values would undergo very sharp falls; thus Hayek (1932), when he still accepted capital as a single factor, admitted that capital could be *destroyed* by a change in the interest rate. And the reasons, why the value of the vector of capital goods existing in an economy at a particular moment cannot be considered given for the determination of equilibrium, include not only the changes of *normal* prices when income distribution changes, but also the changes in the value of fixed plants due to non-normal capacity utilisation (e.g. because of recessions), and the devia-

Corfu conference on capital, appropriately remembered by Professor Kurz (Kurz, 2020, p. 261), that theory requires that notions be defined with absolute precision, and therefore either capital the single factor can be rigorously defined (and it cannot), or the notion must be abandoned.

2.2. It is not easy to be clear about Schefold's attitude toward this 'indeterminateness'. In several places he seems fully to accept the traditional given quantity of capital; in other places he admits there are problems, and in one article he advances considerations on the legitimacy of treating capital as capable of changing 'form' without changing in 'quantity' that clearly imply an untenability of the traditional notion of capital, but strangely this implication is not drawn and the analysis proceeds as if the problem did not exist. I am referring to (2013b) where Schefold finds the traditional neoclassical idea, of how capital can change 'form' without changing in 'quantity', valid only under very restrictive conditions:

> The neoclassical vision of the transition as substitution cannot generally hold [...] but the neoclassical conception of the 'supply of capital' *is* consistent under the following restrictive conditions: if the means of production are not specific for the industry, those used on the left of the switchpoint can be sold to other industries, and a somewhat different 'combination' of means of production ('factors') can be bought; activity levels will adapt. (2013b, p. 60)

Here, on the basis of his own (debatable, see below, but this is not the point now) reconstruction of how capital was believed capable of changing 'form', Schefold asserts that when techniques change a change in the 'form' of capital without change in 'quantity' is legitimate only under the extremely restrictive condition of non-specific capital goods (capital goods are specific more often than not!). Then clearly according to Schefold himself a given capital endowment capable of changing 'form' without changing in quantity is not an acceptable notion. This seems to be in contradiction with how Schefold reasons in quotations (A) and (D) above, which accept a given quantity of capital that through a 'transition' to a different 'form' can be combined with different amounts of labour. For example the reference to Böhm-Bawerk in quotation (A) is not accompanied by any criticism of his right to take the subsistence fund as given.[6] In (2013b) it-

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tions of prices from normal prices due to recent innovations, to changes in environmental conditions (e.g. climate changes, war destructions, earthquakes), and so on.

[6] Against occasional misinterpretations, it may be opportune to remember that in Böhm-Bawerk too the endowment of capital is an amount of exchange value: 'I, too [like J.B. Clark], believe that capital is a "fund" or "quantum" of matter. I think it clear that anyone who wishes to make an estimate of the size of this fund must measure it, not by counting the pieces or calculating their volume or weight, but by measuring it in terms of value — nowadays in terms of money' (Böhm-Bawerk, 1906, p. 5). His use of simple interest makes the dependence of the value of capital on distribution particularly easy to grasp. Simple interest allows Böhm-Bawerk to interpret the value of capital as a 'subsistence fund' because then the value of capital goods equals the wages paid to the labour embodied in them; but physically the subsistence fund does

self, on p. 66 one finds 'even conceding a given "supply of capital"', which implies that such a concession would neglect relevant problems, but then the analysis proceeds to concede this given supply of capital and standard neoclassical capital-labour substitution without raising any objection.

Incidentally, as the quotation just given makes clear, the picture of the 'transition' of capital to a different 'form' that Schefold (2013b, pp. 59-60; 2017, p. 173) attributes to traditional neoclassical authors is that, when income distribution changes and alters optimal methods, firms get rid of no longer desired capital goods by *selling* them. This cannot be correct: no one would desire these capital goods, and anyway the composition of capital would not be changed; this picture confuses the fact that single consumers or firms can change the 'form' of their capital by selling some of the capital goods they own and buying other ones at the given prices (this only changes the distribution of the property of the economy's given vector of capital goods among agents), with the traditional conception of how the economy's total capital changes 'form' i.e. composition, which was that the no longer optimal capital goods, *when used-up*, are not replaced by identical capital goods, the accumulated depreciation funds are used to buy different capital goods of same value (Wicksell, 1954 [1893] p. 103). This contraction of industries via non-replacement of their durable capital, and expansion of other industries, is what J.B. Clark argues in the example (Clark, 1899, p. 115) that Schefold quotes in (2016, p. 18). But Clark's and Wicksell's picture is unsustainable even when correctly interpreted. Firms will wish to change the 'form' of their capital stock only if relative factor prices and/or product prices change, but then the durable capital goods destined to non-replacement earn less and the accumulation of depreciation funds is altered, and furthermore to assume that the new capital goods have the same value as the old value of the non-replaced capital goods has no unambiguous meaning (it depends on how relative prices are changing, and on the numéraire); also, the purchasing power of the (necessarily nominal) cumulated depreciation funds depends on how the price level changes.[7]

Therefore I remain perplexed when Schefold (2017, p. 175) writes:

> The assumed identity of the value of capital goods in terms of long-run prices in both positions [i.e. before and after a price Wicksell effect, F. P.] and a given quantity of capital in value terms are problematic, but have been defended by Clark (1899) as caused by the accumulation of replacement funds in the context of technical change.

It seems to me that the given value of capital is nowhere 'defended' by J.B. Clark,

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not *consist of* wage goods, it is the vector of capital goods with which the economy starts each yearly production cycle; the fact, that with simple interest this vector of capital goods has the same value as the labour embodied in it times the wage, means that if, with no change in quantities produced nor in production methods, the real wage rises, then the subsistence fund — the value of capital — rises in proportion: clearly it is not independent of income distribution!

[7] Thus the assumption of non-specific capital goods would not in the least save the notion of a quantity of capital capable of changing 'form' without changing in amount.

who simply dogmatically neglects all price variations and treats the quantity of capital as a totally unproblematical notion. But anyway, shouldn't the robustness of this supposed 'defence' be discussed? Instead Schefold continues as if the 'defence' were no doubt solid, and problems could derive only from an increasing demand curve for capital.

The latter phenomenon, Schefold argues, can arise even in the absence of reswitching, owing to 'anti-neoclassical' price Wicksell effects (see 2016, pp. 31-32; 2017, pp. 175-176, 188; 2020, pp. 8, 18); here I find another correction of Schefold's interpretation of traditional marginalist authors necessary. His argument is that, when the wage curve is not linear, the normal value of capital in terms of the numéraire changes with changes in the real wage even if physically nothing changes; if the wage curve is concave, when w decreases the value of capital per unit of labour rises; this, Schefold argues, according to traditional neoclassical theory implies that the economy's given capital endowment, an amount of exchange value, now can employ less labour than before; instability can result. I doubt Schefold can find quotations proving that some marginalist/neoclassical economist really reasoned like this, treating the endowment of capital as a given quantity of exchange value *that does not change when the value of the economy's capital goods changes*. It is not easy to reconstruct conceptions that are intrinsically contradictory, but my understanding of the traditional neoclassicals is that the capital endowment was interpreted to be the value of the existing capital goods (see e.g. Clark, 1899, p. 124), but seen as a measure of a 'real' factor, whose presence increases the output obtainable from a given amount of labour and therefore contributes to production (Dvoskin and Petri, 2017, pp. 650-653); then a price-Wicksell-effect rise in the value of an unchanged stock of capital goods utilized in unchanged ways would have been interpreted as a purely nominal change reflecting in fact no change in the 'real' capital endowment, because then clearly the productive contribution of capital goods had not changed; capital owners would now own a greater wealth in terms of numéraire, not the same wealth as Schefold believes, but the optimal capital-labour ratio at the new real wage would have increased by the same percentage and therefore demand for labour would not change. In this perspective, price Wicksell effects cannot be a cause of instability of the savings-investment market if gross savings adjust to the changes in the value of depreciation, but they can if gross savings depend on other variables too, e.g. on a wealth effect[8] or on aggregate income or on the rate of interest (the issue is waiting for

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[8] Assume the economy to be in stationary equilibrium at a rate of interest r^*, with preferences and wealth levels of consumers such that total net savings are zero. Now for some accidental reason r rises and this does not cause switches to a different technique nor changes in the amounts produced but raises the value of the unchanged vector of capital goods by, say, 4% because of a positive price Wicksell effect and as a result gross investment (in value) increases by 4% (assuming circulating capital only); gross savings too would increase by 4% if net savings remained zero; but suppose that preferences are such that the wealth effect causes a lesser rise in gross savings: net savings become negative; the rate of interest will tend to increase further.

an exhaustive analysis). Schefold's peculiar interpretation of the traditional marginalist capital endowment as a quantity of exchange value independent of the value of the concrete capital goods in existence has perhaps contributed to making him neglect a clear critical implication of price Wicksell effects: their questioning the right to consider the quantity of capital as unaffected by changes in relative prices.

To sum up, Schefold offers no justification for his acceptance of the traditional neoclassical given supply of capital capable of changing 'form' without changing in amount, in spite of his own admissions that there are problems. But since *there are* problems and Schefold does not deny it, he appears to be contradictory. Perhaps he will supply an explanation, but I do not see how he can surmount the universally admitted inconsistency of a given quantity of capital measured as a quantity of exchange value. Or how he can surmount Sraffa's famous example of inversion in the direction of change of relative prices as the rate of profit rises, an example that denies the legitimacy of the notion of capital as a factor but then also of a given endowment of this nonexistent factor.[9] There seems to be no way to defend a given endowment of capital the single factor of variable 'form'.

2.3. In order to avoid the indefensible value endowment of capital, someone might perhaps turn to a Marshallian short period in which *durable* physical capital is given, and try to derive a short-period full-employment savings supply schedule, to be combined with a savings demand schedule (a gross investment function) to determine an equilibrium rate of interest without needing a given endowment of value capital. This Marshallian approach has been widely accepted in macroeconomics from Keynes onwards. In Dvoskin and Petri (2017) the popularity of this approach was cited as justifying the relevance of the criticism of the negatively interest-elastic investment function. For the purposes of that paper it was not necessary to discuss whether such an approach is really able to avoid supply-side problems. Some discussion of the issue is opportune now to confirm their presence.

That there must be problems is immediately suggested by the fact that this Marshallian approach was considered by no one an adequate terrain for a *self-sufficient alternative*, in the supply-and-demand approach, to the long-period equilibrium undermined by the indeterminable value endowment of capital. There must have been serious reasons why, having decided to drop long-period equilibria, Lindahl, Hayek, Hicks did not con-

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[9] Sraffa's example appears to have empirical counterparts. Mariolis and Tsoulfidis (2016) report on p. 95 to have found that '[p]rices change more often than not in a strict monotonic way': although the authors do not intend to stress it, an implication of this 'more often than not' is that the cases of relative prices *not* monotonic functions of the rate of profit are not so rare; indeed on p. 101 they report that for Greece 19% of prices are non-monotonic. These findings are derived from input-output tables, and any such derivation suffers from limitations (see below), but these limitations should render non-monotonic prices less probable if at all, so if nevertheless they can be observed the result is strong.

sider such an alternative and turned to a neo-Walrasian framework, that is, to treating *all* endowments of capital goods, even the very-quickly-modifiable ones, as given. I can think of two such reasons:

- the absence of a non-arbitrary criterion for deciding which capital goods have endowments that change slowly enough to be treated as given, and which do not, in the face of a continuum of presumable speeds of change of endowments;

- the difficulty with specifying the amount of the less durable capital goods if it is not treated as a given vector: to take it as given as an amount of value and of variable 'form' would re-introduce an illegitimate value endowment; to take it as endogenous both in 'form' and in 'amount' is legitimate in a Keynesian approach where production adapts to demand and savings adapt to a separately determined investment, but renders the level of income and hence of savings indeterminate in an equilibrium theory where investment is assumed to adjust to savings, because a totally adjustable vector of circulating (or anyway less durable) capital goods will generally permit a not insignificant variability of aggregate output even with a given labour employment and given plants.[10]

A further reason for finding this short-period approach undermined by supply-side problems (although *not* for preferring a neo-Walrasian approach) is the presence of the problems of neo-Walrasian equilibria although in a somewhat lesser degree. In all likelihood Hicks (1932) had the Marshallian short period in mind when rejecting, in a well-known passage, the notion of a short-period marginal product of labour owing to insufficient substitutability (see Petri, 2004, pp. 29-30). And lack of substitutability plus the impermanence problem is what emerges in Kaldor's conclusion (Kaldor 1956, Section 3A) that in the Marshallian approach to-day's marginal product of labour depends on the prices that yesterday influenced the 'form' of capital to-day, and therefore the approach permits no conclusion beyond that to-day's prices depend on yesterday's prices; no wonder that, as Kaldor notes, Marshall argued that distributive shares in the short period depend on long-period forces (thus implicitly admitting an inability of short-period analyses autonomously to determine income distribution).[11]

2.4. Another supply-side problem for the neoclassical approach, that Schefold neglects in his reappraisal of the traditional neoclassical picture, is the abundantly confirmed ample variability of short-period capacity utilisation in response to demand, with its very important implications (Petri, 2003; Garegnani, 1992; Kurz, 1992). Surveys of the degree of capacity utilisation are regularly carried out, e.g. in the USA by the Bureau of Census, and they show that generally most firms produce around 20% less than what they consider 'full production', which means they could easily increase production and

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[10] For example in agriculture, output from a given land and given labour significantly depends on the amount of irrigation, fertilizers, pesticides.

[11] Also deserving reflection are Garegnani's critical observations on the Marshallian approach in Garegnani (1978, pp. 347-348).

employment by at least 10% by simply utilizing the given durable capital for more hours per week; a further considerable increase of output (even 50-60%) is indicated as possible (clearly, by introducing night shifts) if needed by a national emergency. Therefore there is vast room for a decisive influence of aggregate demand on output. Of course an increase of aggregate demand would initially run down inventories of circulating capital, but these would be then reconstituted by the increased production of the firms producing circulating capital. The fact that the change in output is obtained by changing the hours of utilisation of durable capital per week means that output increase per unit of labour increase is constant over a wide range, that is, average labour productivity is not decreased by employment increases, more probably it increases owing to unchanged overhead labour; hence (within limits, but limits that leave room for a significant influence of policy) there is no need to decrease wages when employment increases in order not to let the rate of profit decrease.

This impressive short-period flexibility of production not only downwards but also upwards[12] undermines the marginalist/neoclassical picture of the capital endowment as a supply constraint that establishes a precise relationship between the real wage and the labour employable by a given capital endowment. This picture is that in order for employment to increase, the 'form' of capital must change (which requires a change of income distribution), or there must be a big change in tastes that increases the average propensity to save (and reduces consumption) and permits and causes capital accumulation. The different picture imposed by the above evidence is that the increase in employment (unless enormous) can be obtained already in the short period via a stimulus to aggregate demand, by raising the degree of utilisation of the given productive capacity and without any need for an increase of the average propensity to save; the need for a change of the 'form' of capital disappears too: over the same longer time horizon required for capital to change 'form' the higher-than-normal utilisation will stimulate the building of further productive capacity, that will utilise the same methods as before if the real wage has not changed — and there is no need for it to change; even a simultaneous rise of real wages and of employment is perfectly possible if aggregate demand increases (Petri, 2011, p. 411). The need of the marginal/neoclassical approach to view capital as a factor similar to land seems to have acted as blinkers that have long made economists blind to the ample margins of unused capacity utilisation in most industries. It cannot be by chance that the implications of these margins have been and are developed by economists who reject the marginal/neoclassical theory of distribution.

Schefold (see quotation (A)) states that the legitimacy of 'approximate surrogate production functions' supports Böhm-Bawerk's contention that in a situation of full employment a rise of wages induces the adoption of more capital-intensive methods, that cause unemployment which cannot be corrected by a stimulus to aggregate demand *be-*

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[12] Rapid production increases are less easy in agriculture, but food demand increases little with increases of employment — at least in not very poor nations.

cause capital is already fully employed. But full employment of a 'rigidly given level of capacity' does not mean *maximum* capacity utilisation, it can only mean *normal* capacity utilisation, which in nearly all industries leaves room for ample increases of output.

The last lines of quotation (A) may be interpreted as meaning that Schefold is aware of this fact but considers it to fall 'into different territory ... this argument can no longer be simply dismissed *on the basis of the critique of capital*'. This is of course false if one admits, as one should, that the critics in the Cambridge controversy also insisted on the problems with the meaning of 'quantity of capital', its measurability without arguing in a circle, the possibility to take it as a datum in the determination of equilibrium. But even if one were to concede to Schefold his restriction of the meaning of 'Cambridge critique of capital' to the instability implications of reswitching and reverse capital deepening and even if one were to concede his conclusion of too rare an occurrence of these phenomena, still this might mean that the Cambridge critique so intended is ineffective, but would not suffice 'to justify the conceptions of capital of the 19th century economists' and to rehabilitate the traditional neoclassical picture. The other aspects too of that picture would need to be solid. Besides the untenable given quantity of capital, the conception criticized in this subsection of the quantity of capital as a rigid *supply constraint* is also essential to that picture.

This has an important implication for Schefold's inquiry into the existence of 'approximate surrogate production functions' (2013a). Given the considerable adaptability of output to demand and the consequent flexibility of employment with little need for changes in real wages, even if the dependence of *normal* long-period relative prices, technical choice and value of capital on distribution could be approximately described by an 'approximate surrogate (aggregate) production function' this would have no implication as to the determinants of income distribution, employment and growth; therefore it would not rehabilitate the traditional marginalist conception of capital, in particular the capacity of its endowment to determine a necessary decreasing relationship between real wages and employment. The 'approximate surrogate production function' would only allow an approximate derivation of how the normal value of capital associated with a given net output would change with income distribution, but with no implication as to what causes income distribution or employment.[13] The (value) marginal product of labour resulting from such a production function would not be the determi-

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[13] Schefold (2013a, p. 1166) writes that the low frequency of reverse capital deepening implies that aggregate production functions can survive 'as an approximation ... we now want to create a theory for the approximations'; but this seems to forget what aggregate production functions are used for: to *explain* income distribution as *caused* by factor supplies and technology, according to a supply-and-demand mechanism which the aggregate production function can only aim at approximately grasping, and which in order to be credible needs specification devoid of 'approximations'. This mechanism can be based neither on long-period nor on neo-Walrasian nor on Marshallian general equilibria owing to their supply-side problems if for no other reason; no approximation to a non-existent mechanism can be supplied by 'approximate aggregate production functions', but then what can their use be?

nant of the real wage, it would be the result of the adaptation of technical choices and prices to it. The cherished conclusions of the neoclassical perspective, that more employment requires lower wages, and that faster growth requires giving up some present consumption (conclusions that Schefold seems to accept in these articles), would be in no way justified.

As I see no way to surmount these supply-side problems, I conclude that even if Schefold were right to argue the irrelevance of reswitching and reverse capital deepening and the determinability of approximate surrogate production functions, the marginalist/neoclassical approach to income distribution, employment and growth would remain indefensible.

3. The demand for capital: the relevance of input-output tables

3.1. But I have doubts on Schefold's arguments concerning the demand-side problems too. I discuss at the end of the paper Schefold's claim of a 'poverty of production functions' (see quotation (E) in subsection 1.3 above). Now I intend to discuss whether Schefold is right in his dismissal of reswitching and reverse capital deepening as powerful criticisms of the neoclassical picture.

A first thing to observe on this issue is that it would be wrong to limit the critical implications of reswitching to the possible instabilities caused by reverse capital deepening. Reswitching (double switching) is an implication of the possibility of inversion in the direction of change of the relative price of two products, or of the same product produced by different methods, as the rate of profit rises (Sraffa, 1960, pp. 37-38, 81-84). It illustrates in a different way the illegitimacy of '*any* notion of capital as a measurable quantity independent of distribution and prices' (*ibid.*, p. 38). So reswitching has a fundamental supply-side implication, discussed here rather than in Section 2 only to show that it was not necessary to bring reswitching in to reach the conclusion at the end of that Section. Reswitching makes it impossible to consider two reswitching techniques producing the same net product as utilizing capital in definite non-arbitrary ratios to labour compatible with the substitution mechanisms on which the theory rests. But if it is impossible to determine the relative quantity of capital employed by two techniques, then it is impossible for all other available techniques too. One implication is that, since it is impossible to consider the subsystems producing the goods in the net product vector as utilizing definite quantities of capital per unit of labour independently of income distribution, the total employment of capital is impossible to determine. This is one consequence of the illegitimacy of the conception of capital as a single factor.

All that is needed for this conclusion is that reswitching be *possible*, not that it be observed. If a definition of 'quantity of capital' independent of relative prices were possible, reswitching would not be possible. Since reswitching is possible, the definition is

impossible: that notion must be discarded. There is no factor 'capital'.[14] This means that the characteristics of production with produced means of production cannot have been correctly grasped through that notion, they must be re-examined, and might be very different from how they are conceptualized under the belief in 'capital' (one example is the blindness to the ample variability of capacity utilisation). *All conclusions traditionally based on capital treated as a single factor of variable 'form' must be re-examined from scratch and discarded unless a different foundation can be supplied for them.* One example is the presumption that investment is a decreasing function of the rate of interest; the abandonment of the notion of capital as a single factor renders such a presumption gratuitous and requires a complete re-examination of the issue taking into account all the aspects involved, e.g. one must not *assume* that labour is fully employed (Petri 2015b).

It also means that articles uncritically using the notion of capital as a factor measurable independently of prices and income distribution should be banned from serious scientific journals.

3.2. During the Cambridge controversy the above critical implication of reswitching was for many years the only one discussed (and used to criticize the aggregate production function, non-existent because capital is undefinable); the implications of reswitching and reverse capital deepening for the investment function and the stability of the savings-investment market (the meaning Schefold appears to assign to 'Cambridge capital critique') are absent in Harcourt (1972) or in *Cambridge-controverse* (1977); as far as I am aware they are first argued (in English) by Garegnani (1978, pp. 350-351). So Schefold's meaning of 'Cambridge critique' is too restrictive; anyway the question remains, whether reswitching and reverse capital deepening have important instability implications for the neoclassical approach.

Schefold argues that they do not, because too unlikely. He bases this conclusion on empirical evidence, and on a theoretical argument. I comment first on the empirical evidence: the infrequent occurrence of reverse capital deepening in envelopes of wage curves derived from input-output (I-O) tables, and the near linearity of these wage curves.

But I-O tables do not appear capable of answering questions on the curvature of wage curves and on the likelihood of reswitching (even neglecting the doubtful reliability of their data, especially after the manipulations required to express I-O tables from different years and nations in constant prices and in the same currency).[15] Their limitations

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[14] We are then in a situation analogous to the discovery that the assumption, that certain biological processes are going on in some human bodies because of the effects of a certain retrovirus, must be abandoned because the retrovirus was wrongly thought to exist but in fact does not exist: the existence of those biological processes becomes doubtful too, the observations considered caused by those processes probably need a different explanation.

[15] As Oskar Morgenstern (1950) argued long ago, measurement in science should always be

vis-à-vis such aims are many, most of them already noted by Kurz (2020) but repeated here for the reader's convenience.

First, I-O tables cannot reveal the available non-adopted techniques, the methods that *would* be adopted with a different income distribution. To assume that the available alternative techniques are *only* the ones observed in one or other nation in one or other year would be nonsense. That these observed techniques produce or not reswitching tells us nothing on what one would obtain by including the non-observed but potential ones. (At most one can venture the suggestion that the presumable similarity of *adopted* production methods in economies with access to the same knowledge probably means a much greater similarity of the wage curves thus obtained, than if all the potential techniques could be considered, and hence presumably a lower observed occurrence of reverse capital deepening than on the unknowable real envelope. It is then interesting that the Han-Schefold exercise found that reverse capital deepening is not so rare, 4% of the switchpoints.)

Second, input-output tables describe flows of value sales among highly aggregated sectors. The number of different commodities produced in an advanced economy is around hundreds of thousands at least. To aggregate them even in 460 sectors and then to derive from the coefficients in value terms the technical coefficients means to treat each sector as producing a single good, that is, to aggregate *hundreds* of different productive processes into a single one, potentially an enormous averaging.[16] Inevitably the 'organic composition' of a sector is an average of those of the single goods produced by the sector, hence inevitably it will be much closer to the economy's average than the 'organic composition' of many of its products. And the closer one gets to a uniform 'organic composition', the less relative prices depend on distribution; the wage curve in all probability gets closer to linear.

Third, I-O tables are in value terms; to obtain technical coefficients from them one chooses for each good (= output of a sector, supposed homogeneous) such a unit as to render its price equal to 1 dollar. This is fine if one only wants to derive the wage curve corresponding to a given I-O table, but if one wants to derive techniques by combining methods from different I-O tables, in order to avoid having the unit in which a good is measured change with the table one must assume relative prices to be the same across these tables, which is hardly defensible (it conflicts with the claim that the methods are different).

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accompanied by an indication of its likely imprecision; it is so, for example, in physics. For economic statistics Morgenstern concluded that generally the indication should be ±5%. This would turn empirically derived wage curves into strips, and would render any conclusion on switchpoints rather uncertain.

[16] It suffices to enter an ironmonger's shop to see thousands of different products on sale, from very simple ones to highly elaborated ones, with the very different ratios of price to weight supplying a first indication of the differences in the methods that produce the several products.

Fourth, in concrete economies in each industry there is coexistence of less and more efficient firms, often (but not only) owing to the plants being older or newer; therefore even if the product of a sector were homogeneous its observed inputs would not indicate the best known method for its production in that economy.

Fifth, when fixed capital is neglected, as in Han and Schefold (2006), the 'method' attributed to a sector is radically different from the true one even conceding the sector produces a single homogeneous output. I am weak on empirical treatments of fixed capital, so I cannot assess the empirical studies of I-O tables that try to include it; but theoretically, fixed capital increases the likelihood of phenomena incompatible with the picture of capital as a single factor, for example it appears to make double and triple switching easier, at least this seems to be the message of Steedman (2011, 2020).

Sixth, if the I-O tables are for different years of the same economy, with high likelihood the more recent tables generate wage curves which are entirely outside the wage curves generated by older tables, owing to technical progress; when one compares methods from different economies, analogous effects are due to some nations being more advanced than other ones; so switches on the envelope are going to be few, and this will make it appear that the probability of reswitching is very low. 'Hence by construction, due to the shift factor of technical progress, the "probabilities" of reswitching, of reverse capital deepening and of consumption reversal assessed in this way are bound to be small. But is this a compelling proof of their unimportance? I wonder' (Kurz, 2020, p. 256).

Seventh, the composition of the output of a sector (e.g. agriculture) differs from nation to nation and from year to year. If in two nations the methods adopted to produce the several goods belonging to a given sector are the same, but the composition of the sector's output differs, it will appear that two different methods are being adopted, which is a misrepresentation.

Eighth, techniques formed by combining methods from different I-O tables are easily illegitimate. Since a given sector, e.g. agriculture, produces an output of different composition in different nations or years, the assumption that the different 'methods' adopted by a sector according to different I-O tables are substitutable, that is, that one can form a technique by combining sectoral methods coming from different nations or years, is seriously questionable. Agriculture in cold countries does not produce the same goods as in warm countries, it is then questionable that the agricultural inputs to the 'method' of a sector in one country can be assumed to have been produced with the I-O agricultural 'method' of any country. The same holds for I-O tables from different years: the goods produced e.g. in the electronics sector in a year can easily be incompatible with the production methods used in another year by the sectors needing electronic inputs.

Ninth, at least at the level of aggregation of the I-O tables used by Han and Schefold (2006), the I-O structure produces an indecomposable matrix, i.e. does not admit non-basic goods, and even when a separate consumption sector is considered which produces no input to other sectors, non-basic means of production are not admitted. The ne-

glect of non-basics has relevant consequences, as I argue later, for the probability of reswitching and reverse capital deepening and for the curvature of wage curves.

My conclusion is that the exercise in Han and Schefold (2006) appears radically misconceived. An envelope of wage curves derived from I-O tables cannot yield credible indications on the true envelope, in particular on the likelihood of reswitching and reverse capital deepening. If at all, it considerably underestimates this likelihood.

4. The search for explanations of the empirical results

4.1. Schefold on the contrary, although admitting some limitations of the exercise with Han, has never doubted the significance of its results. The single case of reswitching (as defined by him), the occurrence of reverse capital deepening at only about 4% of the switchpoints,[17] and the nearly linear wage curves have induced him to look for the reasons why his early expectation of numerous 'perverse' cases and of 'wiggly' wage curves were contradicted. Basically, he has concluded that, with reference to a standard Sraffian price system $\mathbf{p} = (1 + r) \mathbf{A}\mathbf{p} + \mathbf{a_L}w$, if:

(i) one treats the (indecomposable) matrix $\mathbf{A}$ of technical coefficients as random, more precisely, one assumes that the subdominant eigenvalues of matrix $\mathbf{A}$ are so close to zero that it is legitimate to assume that they are in fact zero;

(ii) one treats the column n-vector $\mathbf{a_L}$ of labour coefficients as having random deviations $\mathbf{v}$ from the vector that, given $\mathbf{A}$, would be required for the labour theory of value to hold, and one assumes that the mean $\bar{v}$ of these random deviations is very close to zero,

(iii) with $\mathbf{m}$ the row n-vector of deviations $\mathbf{m}$ of the numéraire's composition from the Standard commodity composition, one assumes zero covariance between $\mathbf{m}$ and $\mathbf{v}$,

then with high probability wage curves are very close to linear (perfectly linear on average). Approximate randomness of technical coefficients is then proposed as the main cause of the *observed* near linearity of wage curves; the implicit argument being, it seems to me, that randomness would presumably hold for the coefficients of non-observed techniques too, which would then generate nearly linear wage curves too, rendering reverse capital deepening infrequent on the true envelope too and therefore making the instability critique irrelevant.

In the papers before (2016), where a new argument (to be discussed in the next Section) is advanced for the improbability of reverse capital deepening, the argument is as summarized here. Its detailed examination appears unnecessary because, first, the purpose and justification of its assumptions is their capacity to explain empirical results which however, as argued, are not trustworthy; and second and decisively, step (i) does

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[17] So double switching was not very frequent but definitely not a very rare occurrence in their exercise.

not appear legitimate: the matrices **A** derived from empirical I-O tables do not confirm an assumption of all subdominant eigenvalues close to zero;[18] Schefold himself notes this fact in (2013b, p. 58; 2016, p. 6). Previous results to this effect by Shaikh and by others (see Shaikh *et al.* 2020, also for other references) have been confirmed by a recent New School PhD dissertation by L. D. Torres Gonzàlez (2018), which calculates eigenvalues of a United States I-O table with over 450 sectors, and concludes (p. 16) that 'there is an important number of eigenvalues with a magnitude that can hardly be considered to be close to zero ... around 10 eigenvalues have a magnitude at least a half of λ_1'. This suggests that the coefficient matrices derived from I-O tables are not random.

In fact it seems impossible to expect them to be random. Then different 'methods' for the same sector derived from different I-O tables *should have unpredictably different coefficients*; on the contrary, bread is going to need flour however produced, cars are going to need metals and paint, and so on; and most of the zeros, if I-O tables were sufficiently disaggregated to show them, would coincide.

4.2. Some other consequences of the aggregation in I-O tables deserve mention.

Aggregation certainly eliminates instances of reswitching. One simple example can be indicative. Let us consider the 'Austrian' example produced by Samuelson (1966) to confirm the possibility of inversion in the direction of change of relative prices and the possibility of reswitching. Champagne and whiskey are produced by different 'Austrian' techniques. Production is in periodic cycles, *period* t goes from *date* t (moment t on the continuous time line) to *date* t + 1, outputs of production processes carried out during period t are sold at date t + 1, and wages of labour employed in period t are also paid at date t + 1. Production of 1 unit of champagne requires the payment of 7 wages *two* periods before the sale of the product (i.e. if the product is sold at date t, which is the beginning of period t, the 7 units of labour were utilized during period t – 3 and paid at date t – 2). Production of 1 unit of whiskey requires the payment of 2 wages three dates before, and of 6 wages one date before the date when the product is sold. Then long-period prices p_c of champagne and p_w of whiskey must satisfy:

$$p_c = 7w (1 + r)^2$$
$$p_w = 2w (1 + r)^3 + 6w (1 + r).$$

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[18] Also, I am not clear on how the randomness assumed in the theorems on random matrices on which Schefold relies can be reconciled with the majority of zeros that the coefficients derived from a highly disaggregated input-output table would show. The probability of a zero in a non-atomic probability distribution is zero. Indeed in Han and Schefold (2006, p. 743) one reads: 'it would be a mistake to calculate a-priori-probabilities of reswitching on the assumption that the coefficients of the input matrix are random, for that would imply a positive input matrix'. Of course one can produce a random mechanism that also throws out zeros, but how many and where? The arbitrariness of any decision in this respect would question the randomness of the result. And would the theorems be still applicable?

Champagne costs less than whiskey for $r < 1/2$ and $r > 1$, whiskey is cheaper for $1/2 < r < 1$. Interpreting champagne and whiskey as the names of two different techniques to produce the same final good 'food', one obtains reswitching. But now let us suppose one aggregates the final good with the capital goods that its production requires as intermediate goods. The champagne method implies that seven units of unassisted labour produce a capital good $c1$ which after a period ripens (unassisted by further labour) into a capital good $c2$ which after a period ripens, unassisted, into a unit of food. In a stationary state the economy produces every period, for each unit of food produced, also one unit of capital good $c1$ and one unit of capital good $c2$. The whiskey method implies that 2 units of unassisted labour produce a capital good $v1$ which after a period ripens, unassisted, into a capital good $v2$; this is combined with 6 units of labour to produce a capital good $v3$ which after a period, unassisted, ripens into one unit of food. Then in a stationary state the economy produces every period, for each unit of food produced, also one unit of each capital good $v1$, $v2$ and $v3$. Now suppose one considers the champagne and the whiskey production to be two different techniques to produce food, but because of aggregation the yearly output of the sector according to each technique is considered to include all its yearly gross production treated as a single composite good: simple calculations (that it is unnecessary to report here) show that the price of the composite good (food+c1+c2) is and stays less than the price of the composite good (food+v1+v2+v3) as r rises from zero; aggregation has caused reswitching to disappear. No doubt similar phenomena can be shown to happen in more realistic cases too.

Also, no doubt the averaging caused by aggregation reduces the likelihood of the necessary condition Schefold indicates for reswitching when at a switchpoint only one method changes (two non-labour coefficients must differ and in opposite directions).

The need to specify 'when only one method changes' brings us to an assumption Schefold makes when explaining why a neoclassical economist need not worry about reverse capital deepening if it is rare. Schefold puts it as follows: 'each single small change of methods of production in different industries can only exert a small effect on the aggregates, and if the system is large and the changes are many, rare paradoxical changes will, as it were, disappear in the noise of frequent transitions' (2010, p. 122; also 2013a, pp. 1167-1168). But the presumption that a switch can only exert a small effect rests on the assumption that technical change induced by changes in distribution is always 'piecemeal', the term Schefold uses to indicate that at a switch point only one industry changes method, the rest of matrix A is unchanged (2013b, p. 59; 2017, p. 179). But this assumption is *very* restrictive. Bharadwaj (1970), the article often cited to support this unjustified assumption (e.g. Zambelli *et al.*, 2017, p. 41), says the opposite: the assumption that only one method changes at a switchpoint is only true if the two adjacent techniques have all basic goods in common,[19] but:

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[19] Actually, all goods (not necessarily basic) directly and indirectly entering the production of the good (which need not be basic) whose method changes.

The assumption that the two systems have the same number and kinds of basics while they differ in the methods of production is extremely restrictive since it is unlikely that two different methods will use identical materials and tools (Bharadwaj, 1970, p. 415).

That is, the more frequent case is that a new method to produce a commodity involves the use of specific capital goods previously not used in the economy, hence the activation of new industries together with possibly a closing-down of other industries that produced capital goods no longer needed with the new method. This means, first, that reswitching is much less difficult, there is more room for compound interest to produce effects; second, that the change in the value of capital can more easily be relevant: for example a change from coal to electricity as the way to move trains implies that lots of goods stop being produced and lots of totally new products start being produced. And technological revolutions of this type are not rare in the history of capitalism. The neglect of this possibility reduces the relevance of reverse capital deepening.

4.3. It is then worth remembering that Schefold concedes: '[t]he paradoxes are easy to generate, if only non-basics or "Austrian" processes are involved' (2013a, p. 1180). The reason why it is easy to produce reswitching with 'Austrian' processes is that in them a switch is of the type Bharadwaj considered more likely, it implicitly introduces new capital goods not previously utilized. But it is unnecessary for such an easy generation of 'paradoxes' that production be truly 'Austrian'. For example, Samuelson's 1966 example of champagne and whiskey continues to produce reswitching if its 'Austrian' nature is eliminated by assuming that the processes described by Samuelson commence, not with unassisted labour, but with labour and an amount k of corn, and corn is produced by corn and labour, as long as k is sufficiently small. And Sraffa's example of inversion of the direction of change of relative prices (1960, §48, p. 38) is carefully worded so as not to exclude the presence of basic goods among the indirect inputs of the two goods. We must be talking of non-basic final goods using many non-basics among their direct inputs, but these no doubt exist and are probably very relevant in actual economies, given the high number of non-basic consumption goods; I-O tables make it more difficult to perceive their presence, sometimes making them totally disappear because aggregation produces an indecomposable I-O table.

The aggregation in I-O tables mixes basics and non-basics. Non-basic capital goods are aggregated with the basic ones in I-O tables; and it is not easy to obtain sectors that contribute no inputs to other sectors, the clothes sector also produces heavy-work clothes, the food sector produces inputs to chemical and biological productions, music is mostly non-basic but some music is used to improve labour (and cattle) efficiency in firms that produce basic goods. Perhaps for this reason Schefold takes for granted the indecomposability of matrix **A**; but non-basics do exist. A majority, I think, of the commodities produced in advanced economies are non-basic: this is the case not only for pure consumption goods, military weapons, scientific research goods, goods used in teaching and in health assistance; all the capital goods specific to these productions are non-basic too. Some consumption goods are basic (electricity, petrol) but most are non-

basic; still, as argued, a 'consumption goods sector' cannot be considered to produce only non-basics. An implication is that the Standard Commodity corresponding to an indecomposable I-O table has no connection with the true Standard Commodity of that economy, which would not include non-basics. A numéraire of composition close to that of the Standard Commodity would render the wage curve close to linear, but if the numéraire is chosen to represent 'some composite unit of consumption goods capable of measuring the subjective satisfaction from which (according to these [marginalist] theorists) consumers abstain when they save' (Garegnani, 1978, p. 345), which is the choice required to derive the marginalist capital demand curve, then the numéraire will mostly consist of non-basics, so there will be no presumption of a near linearity of the true wage curve due to a numéraire not very different from the Standard Commodity. And since many of these non-basics will use a high percentage of non-basics among their direct inputs, they will be goods to which Schefold's opinion appears extensible that 'paradoxes are easy to generate', which I take to mean that they can easily happen.[20]

In view of the many aspects of reliance on I-O tables that reduce the likelihood of what Schefold calls 'paradoxes', it seems to me almost miraculous that Han and Schefold found one case of reswitching (in their sense) — I would have expected that none would emerge, and no or almost no reverse capital deepening either. That they found the latter in nearly 4% of the envelope switches seems to me to confirm that it is not difficult for reverse capital deepening to occur. Its real potential likelihood cannot be judged from the Han-Schefold results for the several reasons I have indicated.

In conclusion, Schefold's claim of an extremely low likelihood of empirical occurrences of reswitching and reverse capital deepening is unconvincing, it appears to derive from treating I-O tables

- as if they fully corresponded to the theoretical matrices that a rigorous discussion would need, which is far from being even approximately the case owing to the restriction of alternative methods only to observed ones, to aggregation, and to the several other reasons listed in §3.2;
- as if they were random matrices, which is a priori difficult to believe and is not supported by the empirical evidence;
- as if the economy produced only basic goods, which is far from true; and
- as if technical change at a switch could only be 'piecemeal', which is false.

4.4. Schefold (2013a, p. 1181) also argues that approximate aggregate production functions are not so indefensible a notion, that the near linearity of wage curves means that one can construct aggregate production functions that, at least 'locally', sufficiently approximate the actual long-period relationship between real wage, value of capital per

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[20] Therefore it is far from clear that Garegnani (1970, p. 425) was mistaken when interpreting as generalizable his result that 'the examples of the Appendix do not seem to indicate that the conditions in which a fall of *r* results in a relative cheapening of the less capital-intensive production processes are any less plausible than those in which the opposite would be true'.

unit of labour, and output per unit of labour. However, the assumptions needed for such a conclusion are so strong, that it seems very difficult to believe that they can be not far from reality. The matrix of coefficients of each technique must be very close to the form $\mathbf{A} = \mathbf{cf}$ with $\mathbf{c}$ a column vector and $\mathbf{f}$ a row vector, which implies all rows of $\mathbf{A}$ are proportional to each other: although Schefold does not say it, this implies that by an opportune choice of the units in which the goods are measured it is possible to render all rows of $\mathbf{A}$ identical. Furthermore, $\mathbf{f}$ must be approximately the same for all alternative techniques, which can differ only in the $\mathbf{c}$ vector (and in the labour coefficients): i.e. the composition of capital must be not only roughly the same in all industries but also roughly constant across the alternative techniques. (This means the composition of the Standard Commodity is (roughly) the same for all techniques, and the choice of that composition for the numéraire renders all wage curves (nearly) straight lines.) I find in particular the assumption of approximately uniform capital composition in all industries breathtaking, clearly contradicted by the evidence. Therefore I find it surprising that Schefold should argue that '[t]he construction seems sufficiently robust to support' the traditional marginalist picture, see quotation (A) in subsection 1.3. To me the construction seems generally totally illegitimate. Also, Schefold again seems to forget about non-basics: as noted by Garegnani (1970, p. 421), even the condition that all wage curves are straight lines is insufficient to ensure the existence of a surrogate production function if the economy also produces 'luxury goods'.

Anyway, contrary to what is stated in quotation (A) and in (2017 p. 189), the possibility to construct an approximate aggregate production function is not sufficient at all for the validity of the traditional marginalist picture, which does not require the determinability of an aggregate production function but does require (Section 2) that it be legitimate to consider the endowment of capital as given, capable of changing 'form' without changing in amount, and an effective constraint on the possibilities to change employment and output without changing the 'form' of durable capital nor the real wage.

5. A theoretical argument against the likelihood of reswitching

Schefold in (2016) and (2017) has put forth another argument, also mentioned in (2020), for an extremely low likelihood of reswitching: namely, that the probability of reswitching tends to zero as the dimension n of the coefficient matrix $\mathbf{A}$ becomes large. This argument is not derived from properties of I-O tables and does not seem to be questioned by subdominant eigenvalues not tending to zero. So it deserves specific attention. I remember the argument, so as to make my comments easier to follow.

The argument proceeds by assuming a given technique $(\mathbf{A}, \mathbf{a_L})$, representable as an $n \times (n + 1)$ matrix by adding column $\mathbf{a_L}$ to $\mathbf{A}$, and supposing there is an alternative meth-

od to produce good 1 (using the same goods),[21] which if substituted for the method of good 1 in $(\mathbf{A}, \mathbf{a_L})$ forms a technique $(\mathbf{A'}, \mathbf{a_L'})$ whose wage curve $w'(r)$ yields a switch with the wage curve $w(r)$ of $(\mathbf{A}, \mathbf{a_L})$ at a given rate of profit r_1. The question is asked, how likely it is that this alternative wage curve produces a second switch with $w(r)$ at some other rate of profit. Schefold considers the set of all conceivable vectors of coefficients for methods for the production of good 1 which, when substituted into the first row of $(\mathbf{A}, \mathbf{a_L})$, produce wage curves $w'(r)$ that switch with $w(r)$ at r_1. These methods (plus the original one) represent points in R_+^{n+1} that form a simplex $M(r_1)$ in a hyperplane of R_+^{n+1} of dimension n, with vertexes on the coordinate axes of R_+^{n+1} corresponding for each good and for labour to the quantity which alone, at the prices and wage determined by technique $(\mathbf{A}, \mathbf{a_L})$ at r_1, causes the price of good 1 to be the same as with technique $(\mathbf{A}, \mathbf{a_L})$. He then asks what portion $M^*(r_1)$ of this simplex represents methods yielding also a second switch. He argues that certainly the latter portion does not include all, and actually generally none, of the vertexes of the simplex; so its 'area' (actually, hypervolume) is definitely only a fraction of that of $M(r_1)$. It must then exist a uniform shrinking of the $M(r_1)$ simplex, all vertex lengths being multiplied by $\gamma < 1$, that yields a simplex of same (hyper)volume as $M^*(r_1)$. The ratio of the volume of $M^*(r_1)$ to that of $M(r_1)$ is γ^n, and it is taken by Schefold to measure the probability that the method producing the switch at r_1 also produces a second switch. Schefold asserts that there must be an upper bound, less than 1, to γ and concludes that, for n large, the ratio of the two volumes is practically zero[22]. That is, if there is a switch on the envelope of available wage curves of a certain economy at some profit rate r_1, taking one of the two switching methods as given the probability that the coefficients of the other method be such that the two wage curves cross again is practically zero for n large, and one obtains practically zero again if one adds to this probability the probability of a second switch obtained by treating the other method as given. Schefold stops here, but clearly the intended implication is that for n large the fraction of switchpoints on the envelope associated with reverse capital deepening is practically zero.

To help the reader's intuition I reproduce Schefold's very clear graphical illustration of how one can obtain $M(r_1)$ and $M^*(r_1)$ in the two-good (plus labour) case.

The explanation of Figure 1 is as follows. Consider a two-good economy, hence 3 technical coefficients for each method for each good, the third being the labour coefficient. Let the coefficients for good 1 according to method s be (y_1^s, y_2^s, y_3^s). The good-1 method we start from, part of the initial technique distinguished by index k, $(\mathbf{A^k}, \mathbf{a_L^k})$, is point $(\mathbf{a^{1k}}, a_{L1}^k)$ in R_+^3 where $\mathbf{a^{1k}} = (y_1^k, y_2^k)^T$ and $a_{L1}^k = y_3^k$. This is indicated as $(\mathit{a_1, l_1})$ in Schefold's figure. At r_1, the triangle with vertexes $(z_1(r_1), z_2(r_1), z_3(r_1))$ is the simplex $M(r_1)$ of the points representing methods (y_1^s, y_2^s, y_3^s) for good 1 that, when replacing

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[21] Here too Schefold assumes A is indecomposable, and does not consider the possibility of alternative methods that use capital goods not appearing in matrix A.

[22] If $\gamma = 0.9$ and $n = 100$ the ratio is approximately 0.00002.

$(y_1{}^k, y_2{}^k, y_3{}^k)$, determine the same price for good 1, and therefore cause a switch. The vertexes are determined by having good 1 produced with such quantities of only good 1, only good 2, or only good 3 (labour) as to yield the same price for good 1 as $(\mathbf{a}^{1k}, a_{L1}{}^k)$, at the prices determined by $(\mathbf{A}^k, \mathbf{a_L}{}^k)$ at r_1. At a different rate of profit r_3, the simplex of good-1 methods yielding a switch with $(\mathbf{a}^{1k}, a_{L1}{}^k)$ changes, the new triangle again includes point $(\mathbf{a}^{1k}, a_{L1}{}^k)$, and the intersection of the two triangles is a segment, connecting points $P_{13}(r_1, r_3)$ and $P_{23}(r_1, r_3)$ in the figure. As r is varied from 0 (the corresponding triangle is shown too) to the maximum compatible with $(\mathbf{A}^k, \mathbf{a_L}{}^k)$, the intersection segment rotates in $M(r_1)$ around point $(\mathbf{a}^{1k}, a_{L1}{}^k)$, and the area one obtains is $M^*(r_1)$; this is as represented, if r_3 is the maximum profit rate.
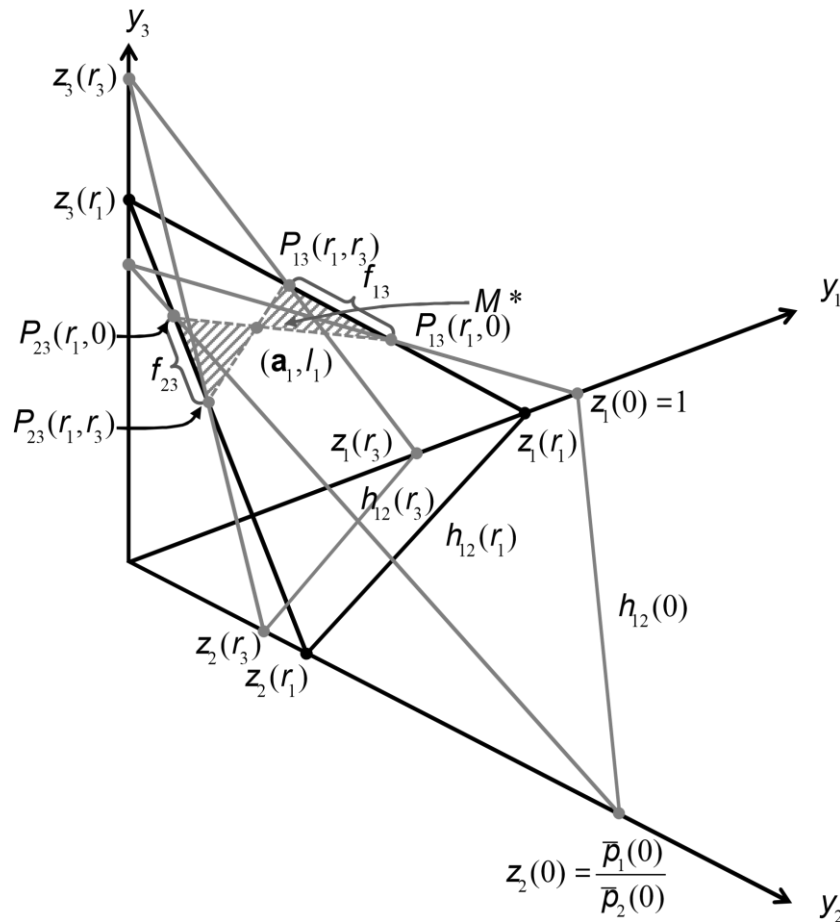

Figure 1 – Reproduced from (2016, p. 48); the graph is also in (2017, p. 186).

A fundamental assumption of the argument is that all alternative methods represented by points in $M(r_1)$ are equally probable. Schefold does not attempt to justify this assumption, which seems to me unacceptable. For example, if good 1 is bread and it necessarily requires flour and water as inputs, then all points in $M(r_1)$ where the quantity of flour and the quantity of water are zero must be assigned probability zero. Conversely, all points in $M(r_1)$, where commodities which cannot possibly be *direct* inputs to bread appear in positive amounts, must be assigned probability zero: steel, concrete, fertilizers, chain saws may well indirectly enter the production of bread, but not (one hopes!)

its direct production. That is, vast areas of $M(r_1)$, in most cases the vast majority of its volume I would argue, should be considered to have probability zero. This means that the direct inputs whose quantities can potentially change when passing to other methods for the production of a good are a very limited number relative to the number n of goods produced in an economy, and their number does not necessarily increase simply because n increases. I infer that the likelihood that an alternative method will produce double switch is not necessarily reduced by an increase of n, because the number of truly potential alternative methods need not increase.

This seems to be particularly true for non-basics, which as argued earlier cannot be neglected. Assume that a non-basic good necessarily needs some non-basic goods to be produced, e.g. all methods to produce bread need flour and some specific machines; flour and these machines are non-basic but for their production they need electricity and iron etc. which are basic. The alternative methods for the production of bread might be all identical in the services from basic goods that they need, and unaffected by whether these services are provided by basic goods which are part of an indecomposable matrix **A** with 100 or 10,000 rows. So let the dimension of the matrix **A** of basic goods increase. Without specific additional assumptions, this increase does not reduce the likelihood that the portions of the available alternative techniques for the production of bread which consist of utilisation of non-basics can produce reswitching.

The argument is also based on excluding 'non-piecemeal' switches which introduce new capital goods; these give more space to compound interest to produce inversion of the direction of change of relative prices as r rises, making reswitching easier. It is by excluding this possibility that Mainwaring and Steedman (2000) obtain a very low likelihood of reswitching, contrary to the opposite result in Petri (2011).

These observations appear seriously to question Schefold's argument.[23] Since the evidence derivable from I-O tables is untrustworthy too, the claim of an extremely high likelihood of 'well-behaved' demand curves for value capital per unit of labour appears neither empirically nor theoretically robust.

6. Too few techniques on the envelope

6.1. There remains to discuss Schefold's latest contribution to date, where in co-authorship with Professor Götz Kersting he returns to the claim of (2013b) that nearly linear wage curves cause very few wage curves to appear on the envelope, and that most likely except at rates of profit very close to zero or to the maximum a single technique dominates over all others, so that changes of the rate of profit do not cause changes in

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[23] The argument seems also to be *prima facie* contradicted by the 4% of envelope switches associated with reverse capital deepening (i.e. with reswitching in my sense) found by Han and Schefold, but I do not claim to have studied this issue very carefully.

technique except when extreme. So there is generally no possibility to speak of a 'substitution' between capital and labour induced by realistic changes of the rate of profit, even conceding (I would not) the legitimacy of such a 'substitution' terminology.

The supplied mathematical proof is incomplete (the full proof is reserved for a mathematical journal), but I have no doubt that the result is correct, given the assumptions. I attempt an intuitive presentation so as to make the assumptions clear and my comments easier to understand.

Suppose an economy that produces a given net product and knows a given finite number m of techniques for its production, all of which produce wage curves so close to linear that one can treat them as linear. The techniques are indexed from 1 to m in terms of *decreasing* maximum wage. No two techniques have the same maximum wage, nor the same maximum rate of profit; successive maximum wages are equally spaced between 0 and the highest maximum wage, and the same holds for successive maximum rates of profit.[24] It is assumed that the distribution of the maximum rates of profit among the techniques is random and uncorrelated with their maximum wages, so that the order of the n techniques in terms of *increasing maximum rate of profit* produces a succession of the indices of the techniques which can be any permutation of the integers from 1 to m. Some possibilities are then clear. If the permutation starts with 1 and ends with m, it means the technique with the highest maximum wage has the lowest maximum rate of profit, and the technique with the lowest maximum wage has the highest maximum rate of profit. In particular if the permutation is (1, 2, ..., m − 1, m) then we have the envelope of Samuelson's surrogate production function. If the permutation ends with a 1, it means the technique with the highest maximum rate of profit also has the highest maximum wage, therefore (given the linearity of wage curves) its wage curve dominates all others for the entire permissible range of profit rates. If all permutations are considered equally probable, and m is not very small, many techniques will be dominated by some other technique at all permissible profit rates, and will not appear on the envelope.

The Kersting-Schefold result can then be put in intuitive terms as follows. Indicate the maximum rate of profit of technique k as R_k and its maximum wage as W_k. Assume all permutations are equally probable. Then the greater the number m of alternative techniques, the more probable it is that, whichever the permutation, one of the techniques with a very low index (i.e. with one of the highest W's) has its index appearing among the last ones of the permutation (i.e. has one of the highest R's). This technique will then dominate nearly all other techniques except at very low and very high rates of profit, and will have a wage curve very close to the diagonal that connects the highest W with the highest R. The likelihood rises with the number of alternative techniques

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[24] That is, given the highest maximum wage W_1, the segment in ordinate from 0 to W_1 is divided into m equal segments whose upper extremes, starting from the top segment, correspond to the highest maximum wage W_1, to the next maximum wage W_2, etc., and the same holds for the distance between successive maximum rates of profit once the highest one is given. This is a discrete approximation to a uniform continuous distribution.

that there will be one such technique that is on the envelope for most of the permissible range of the profit rate, being only dominated by the few techniques with a higher W for r close to zero, and by the few techniques with a higher R for r close to its highest permissible value. The shape of the envelope of wage curves is then with high probability as in Figure 2, and the probability of such a shape increases with the number of alternative techniques.
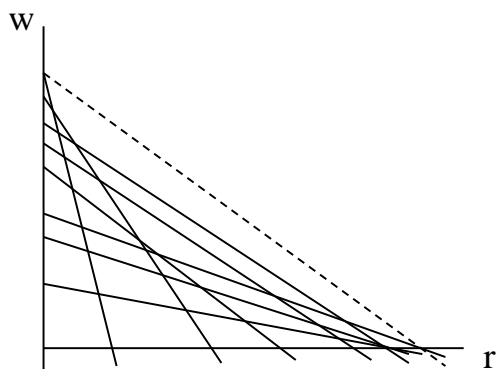

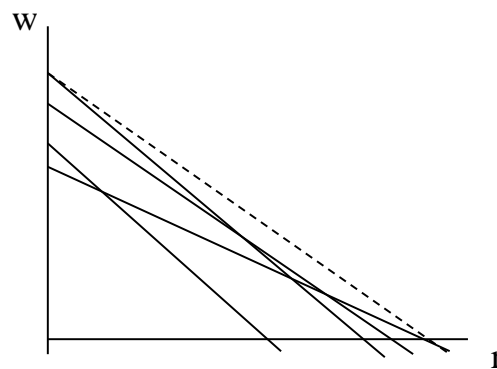**Figure 2**   **Figure 3**

If one accepts that as the number n of goods the economy produces increases, the average number of alternative methods for each good does not decrease or not much, then the number of alternative techniques increases exponentially or almost (with n industries each one with two alternative methods, one can form 2^n techniques), so maintaining the assumptions the shape of Figure 2 becomes the more likely the greater is n.

The result is not surprising. Assume 100 different techniques. Take the ten with the highest W, hence with indices 1, 2, ..., 9, 10. They are randomly assigned ten of the 100 different R's, therefore each one has a roughly 10% probability of having a maximum rate of profit among the ten highest ones. The probability that at least one of them does is high, about 67%.[25] Suppose this is the case. If more than one does, consider the one of them with the highest R. Suppose it is technique 6. The five techniques with a higher maximum wage dominate technique 6 in a neighbourhood of r = 0. The random assignment of R to these five techniques makes it improbable that any of them has an R very close to R_6, therefore it is likely that technique 6 dominates them for r only a little above zero. Analogous considerations apply to the few techniques (nine at most, in this exam-

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[25] This is the complement to 1 of the probability that none of those ten techniques has an R among the top 10 ones. Call NO the property that a technique does not have an R among the top 10 ones; if NO holds for one of the first ten techniques, then for a second one of those techniques NO holds with probability 89/99, so it holds for both with probability (90/100)·(89/99), therefore the probability that NO holds for all ten techniques is (90/100)·(89/99)·(88/98)·...·(81/91) ≈ 0.33. The analogous procedure produces a probability roughly 0.027 that if there are 100,000 techniques none of the 1000 techniques with highest maximum wages has a maximum rate of profit among the top 1000 ones, which explains the 97% probability that appears later in the text.

ple) that dominate technique 6 for r close to the permissible maximum; it is therefore likely that technique 6 dominates them for r not much less than R_6. It is then likely that technique 6 is the dominant one except at values of r close to zero or close to its admissible maximum. If a technique with higher W than W_6 has a maximum R close to R_6, or a technique with a higher R than R_6 has a W close to W_6, one obtains Figure 3, but it is improbable. If m = 100,000, the probability is about 97% that at least one of the 1000 techniques with a W in the top 1% of maximum wages also has an R in the top 1% of maximum rates of profit; that is, as m grows, a shape of the envelope as in Figure 2 becomes a near certainty. Schefold (2013b) with a different procedure had concluded that the most probable number of techniques on the envelope of an economy with 100 sectors and 10 alternative methods for each sector, hence with 10^{100} alternative techniques, and a random uncorrelated distribution of R's, is at most 243.

6.2. The argument is the more likely, the more wage curves are close to linear and the more the distribution of maximum R's is random and independent of maximum W's. Therefore it is unclear how likely it is: even the wage curves derived from I-O tables are only close to linear, and some of the arguments I have advanced suggest the actual wage curves are probably rather less close to linear; more importantly, the assumption of random distribution of the R's appears difficult to justify: if n is great a change of technique corresponding to a change in just one method is hardly going to change W much or R much, in all likelihood most of the techniques with a W close to that of technique j will be those that differ from technique j in only one or few methods and then will have an R also close to that of technique j. However, the conclusion of a single technique dominant for an ample interval around the given rate of profit can perhaps be based on different considerations, connected to the fact that the available techniques are the result of cumulated technical progress.[26] I have only intuitive arguments to this effect, but they seem to me to open interesting avenues for further reflection and so I dare submit them.

A new technique is introduced if, at the ongoing income distribution, it causes its wage curve to be 'outside' the envelope of pre-existing wage curves. Historically, since the mid-19th century the normal rate of profit does not seem to have undergone drastic changes, if following Adam Smith one takes the rate of interest on loans to firms as some indication of it; for my purposes I think I can take it as having seldom been outside a 15%-25% corridor, since many, more or less casual, observations suggest a normal expected rate of return on risky industrial investments around 20%. The average real wage on the contrary has increased considerably. Thus new techniques in order to be introduced have had to have wage curves that went through a higher and higher real wage for an approximately constant rate of profit. Furthermore, 'productivity' per unit

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[26] Schefold (2013b, p. 63) mentions a similar suggestion coming from an anonymous referee and rejects it, but with arguments I find unconvincing: the suggestion does not rely on 'linkages' between sectors.

of labour (the vertical intercept of wage curves in terms of a representative consumption basket) has historically increased too.[27] This means that the wage curves of new dominant techniques have been associated on average with a higher and higher vertical intercept *and* a higher and higher wage at $r = 20\%$. This renders a decrease of the R of successive dominant techniques unlikely. If wage curves were linear, a decreasing R associated with a rising w and a rising W would require labour productivity to increase faster than the real wage, which as far as I know does not seem to have been historically the case except in restricted periods; a non-decreasing R associated with a rising W would imply that the dominant technique at the given real wage is dominant also at all feasible wages: only one wage curve on the envelope; wage curves are not linear, but they would have to be very non-linear (and with different curvatures) in order for a wage curve no longer dominant at the ruling w to remain long on the envelope (most likely, at lower w's) if w and W keep rising while R does not decrease.

This reasoning would appear to reach, without the doubtful assumption of randomness of the association of different R's with different W's, the same presumption that the dominant technique remains dominant for ample variations of the real wage. In actual fact, if one truly stops treating capital as a factor analogous to labour or land, then one no longer has any reason to expect the spectrum of alternative wage curves in the presence of capital goods to be similar to the one with production with labour(s) and land(s) in a non-capitalistic economy. As long as factors are labour and land the idea of a variability of factor proportions entailing gradually decreasing marginal products appears not entirely implausible, so the assumption appears defensible of a gradual substitution among such factors activated by changes in relative factor rental rates, and of wage-rent curves (with the rate of land rent in abscissa) such as those assumed in Samuelson's surrogate production function, determining a rather smooth convex envelope. But this assumption loses all foundation if the change of technique requires change in the heterogeneous capital goods employed. Samuelson (1962) had no right to assume that a set of alternative production techniques of his special variety (each technique using a different capital good produced by itself) and generating his convex envelope of wage curves was more likely than one of those techniques being dominant over all the others at all or nearly all feasible wages. In drawing the envelope of wage curves of his Surrogate Production Function as similar to the one derivable from production of a single good with labour and land he revealed a deep persuasion that production with labour and capital goods had to be similar to production with labour and a single factor 'capital' (thus on this I agree with Kersting and Schefold, 2020, p. 5: 'How many possibili-

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[27] It seems not impossible to find a single numéraire in terms of which to measure the real wage across two centuries of changes of goods produced and consumed. This numéraire would have to consist of goods forming basic subsistence consumption: food (in terms of calories and vitamins), water, lodging, winter heating, basic clothes, some transport services. It seems indubitable that labour productivity in terms of such a numéraire has secularly increased. The enormous decrease of the share of employment allocated to producing food suffices to see this.

ties of substitution do we encounter on the envelope? ... neoclassical economists repressed the entire discussion in their mind and regarded the idea of substitution as obvious').

The previous considerations are reinforced, I believe, by the following one. New methods are introduced when at the going income distribution they are more convenient than previous methods; if they are not, no one knows whether they would be more convenient at the prices associated with a different rate of profit, which would change input costs in unknown ways. Now, most innovations become utilisable only after specific efforts to improve them and make them commercially viable; there is no incentive to produce efforts to develop productive methods that require a different income distribution in order to be convenient. This means that there is little chance that technical progress develops new 'alternative' methods, that is, methods not convenient at the going income distribution but that would generate techniques dominant at a different income distribution. The presumption must be, after centuries of innovation motivated by its convenience at the going income distribution, that there is little availability of 'alternative' methods in the above sense; the known but non-adopted methods are in all likelihood methods which were once convenient but were then rendered obsolete by technical progress, i.e. they would generate techniques with wage curves in all likelihood below the envelope at least for an ample interval of the wage around the going wage.[28] Even if 100 years ago the envelope of wage curves resembled Samuelson's, the absence of birth of new methods except dominant ones at the going income distribution has certainly caused the methods that were on the envelope 100 years ago no longer to appear on the envelope now. For this reason, the presumption is strong that Joan Robinson was right: it is highly likely that the production methods that are dominant at the going income distribution would be dominant even at considerably different income distributions. Even very low wages are not going to make airplanes and trains replaceable with bicycles.

Thus on the conclusion of a likely invariance of technique for even ample changes of income distribution from the current one, I agree with Schefold, and I thank him for the stimulus to think more on the issue. The conclusion is of course one further nail in the coffin of the marginal/neoclassical approach.

7. A summing up and a concluding comment

The really important question is not, what remains of the Cambridge critique, but rather, is the marginal/neoclassical picture of the forces operating in a capitalist market economy defensible? The criticisms of the 'supply-side' problems of that picture, remembered

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[28] Clearly alternative methods are known in many fields, that would be more convenient if the wage of the sole labourers employed with those methods were greatly reduced, but the question is about the effect of a *general* reduction of wages.

in Section 2 and subsection 3.1, are perfectly valid (Schefold does not attempt to refute them), and sufficient to answer in the negative. And they *were* part of the Cambridge critique; it is because of them that the neoclassicals turned to claiming that the truly 'rigorous' versions of neoclassical theory are the neo-Walrasian ones, which (apparently) do not need the notion of capital that the supply-side criticisms undermine.

However, the use of reverse capital deepening to criticize the picture of the rate of interest as the price capable of adapting investment to savings was also important, and Schefold's rejection of that criticism on the basis of a claimed improbability of reverse capital deepening has had some impact (see e.g. Lavoie 2014, p. 64). My re-examination of his arguments has found them unconvincing. The true likelihood of reverse capital deepening cannot be derived from I-O tables; if anything there are several reasons suggesting that envelopes based on alternative techniques derived from I-O tables tend to underestimate it; but even if the average likelihood on the true envelope were not much greater than the 4% of occurrences found by Han and Schefold, that would be sufficient a) to conclude that there is a significant probability that sometimes much more than 4% of the switches are anti-neoclassical, which would destroy the belief in the generality of a sufficient negative interest-elasticity of investment (Petri, 2011, p. 408), and b) to suggest that at least in some occasions instability should have been observed, which does not seem to be the case, implying that forces other than supply and demand must be considered to explain the relationship between savings and investment (*ibid.*, p. 406 and footnotes 24 and 25, p. 415). So *all* of the Cambridge critique seems to remain.

True, I have found the conclusion of a high probability of very few techniques on the envelope probably acceptable, although on the basis of a different argument relying on the effects of technical progress. Now, certainly the fewer the techniques on the envelope, the less likely the occurrence of reverse capital deepening. But an additional criticism does not mean that the previous criticism becomes invalid; it would return to full relevance if for example further reflection were to conclude that the 'too few techniques' argument is invalid, or not often plausible. Analogously the argument, that intertemporal equilibria are indeterminable because perfect foresight of novelties is logically impossible, does not render the problems with uniqueness and stability of general equilibrium irrelevant.

No doubt the debate will continue; I think it will be helped if Schefold clarifies his attitude vis-à-vis the problems of the notion of a given quantity of capital capable of changing 'form' without changing in amount, problems that again and again he hints to, but then neglects, as if finally that notion were acceptable. The ambiguity is evident also in the last and perhaps most explicit description of his general view to date, the last section of (2017, pp. 188-192). There he admits that there may be problems by writing: 'If there is truth in the idea that the nominal amount of capital limits employment' (p.

188);[29] but he then proceeds as if the answer were yes, and uses the randomness assumption to conclude that

> a description of the technological possibilities by means of approximate surrogate production functions is not absurd. Since these conditions may be realised sufficiently well in reality, they may help to explain why so many economists adhere to neoclassical doctrine: not only for ideological reasons, not only because they feel that the world must be such, if the market system is to be just and efficient, but because they perceive that reality appears to conform to the picture: profits are maximised, wages seem to be paid to compensate for the contribution made by the worker, people find employment and less developed countries use more labour-intensive methods of production than those more developed — for example old cars circulate longer in the underdeveloped world because repair work is cheaper etc. (2017 p. 189).

The first sentence in this passage implicitly accepts the given 'quantity of capital': unless one believes that income distribution between wages and profits is determined by the marginal products of given endowments of labour *and capital*, one will have no interest in approximate aggregate production functions, whose determinability is neither sufficient nor necessary for the validity of the marginal theory of income distribution, and loses all relevance if the forces determining distribution are other. The continuation of the passage deserves comment too: it states that marginalist/neoclassical economists could find confirmation for their approach in the observation of the economy. Schefold seems to be stating here that he believes that the behaviour of market economies is not far from the neoclassical picture. Still, the elements cited in this passage as supporting the claim that reality 'appears to conform to the [neoclassical] picture' cannot but leave one perplexed, they are far from persuasive: that profits are maximised is also in Marx, nor would Marx have been surprised by old cars circulating longer if wages are low;[30] as to 'wages seem to be paid to compensate for the contribution made by the worker', certainly this conclusion is not something that one can derive from observation: the equality at firm level between wage and (where determinable) value marginal product of labour results from cost minimization whatever the determinants of the wage; to interpret this equality as meaning that the wage equals the *contribution* of labour requires the entire marginalist edifice with its interpretation of profits and rents too as the contributions of factors.

The reader's uncertainty about what Schefold believes is not decreased by the fact that after this passage Schefold immediately proceeds to argue that 'another argument comes in to re-enforce the critique': the 'too few curves on the envelope' argument. But if this argument is valid, then the economy *cannot* function as neoclassical theory ar-

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[29] It has been argued in §2.2 that the question is misconceived, but this is not the important point now, it still raises the problem of the meaning of 'given quantity of capital', and Schefold admits that there may be a problem but does not stop on it.

[30] Still it is surprising that Schefold should mention this example, which is rejected as not necessarily correct in Han and Schefold (2006, pp. 748-749).

gues, so reality *cannot* appear 'to conform to the picture'; but then what is the sense of the above passage? It seems as if Schefold wished to claim at the same time that the economy does, and that it cannot, behave pretty much like J.B. Clark thought.

References

Bharadwaj, K. (1970). 'On the maximum number of switches between two production systems'. *Schweiz. Z. Volkswirtsch. Stat.* 106(4), 409-429.

Böhm-Bawerk, E. von (1906). 'Capital and Interest Once More: I. Capital Vs. Capital Goods'. *Quarterly Journal of Economics* 21(1), 1-21.

Böhm-Bawerk, E. von (1914). 'Macht oder ökonomisches Gesetz?'. *Zeitschrift für Volkswirtschaft, Socialpolitik und Verwaltung*, 23, 205-271.

Cambridge-controverse sur la théorie du capital (1977). Special issue (with articles by J. Robinson, G.C. Harcourt, L.L. Pasinetti and others) of *Revue d'Économie Politique*, 87(2).

Clark, J.B. (1899). *The Distribution of Wealth.* New York, NY: Kelley. Reprint, 1965.

Dvoskin, A. and Petri, F. (2017). 'Again on the relevance of reswitching and reverse capital deepening'. *Metroeconomica* 68(4), 625-669.

Garegnani, P. (1960). *Il Capitale nelle Teorie della Distribuzione*. Milan: Giuffré.

Garegnani, P. (1970). 'Heterogeneous Capital, the Production Function and the Theory of Distribution', *Review of Economic Studies* 37(3), 407-436.

Garegnani, P. (1978). 'Notes on Consumption, Investment and Effective Demand: I'. *Cambridge Journal of Economics* 2, 335-353.

Garegnani, P. (1990). 'Quantity of capital'. In J. Eatwell, M. Milgate, and P. Newman (eds), *The New Palgrave: Capital Theory*. London: Macmillan, pp. 1-73.

Garegnani, P. (1992). 'Some notes for an analysis of accumulation'. In J. Halevi, D. Laibman, and E. Nell (eds), *Beyond the steady state: a revival of growth theory*. London: Macmillan, pp. 47-71.

Garegnani, P. (2018). 'On the Labour Theory of Value in Marx and in the Marxist Tradition', *Review of Political Economy* 30(4), 618-642.

Han, Z. and Schefold, B. (2006). 'An Empirical Investigation of Paradoxes: Reswitching and Reverse Capital Deepening in Capital Theory'. *Cambridge Journal of Economics* 30(5), 737-765.

Harcourt, G.C. (1972). *Some Cambridge Controversies in the Theory of Capital.* Cambridge: Cambridge University Press.

Hayek, F.A. von (1932). 'Money and Capital: A Reply'. *Economic Journal* 42(166), 237-249.

Hicks, J.R. (1932 [1963]). *The Theory of Wages* (as reprinted with additions, 1963). London: Macmillan.

Kaldor, N. (1955-6). 'Alternative Theories of Distribution'. *Review of Economic Studies* 23(2), 83-100.

Kersting, G. and Schefold, B. (2020). 'Best Techniques Leave Little Room for Substitution. A New Critique of the Production Function'. *Centro Sraffa Working Papers* No. 47.

Kurz, H.D. (1992). 'Accumulation, Effective Demand and Income Distribution'. In J. Halevi, D. Laibman, and E. Nell (eds), *Beyond the steady state: a revival of growth theory*. London: Macmillan, pp. 73-95.

Kurz, H.D. (2020). 'The Theory of Value and Distribution and the Problem of Capital'. *European Journal of Economics and Economic Policies: Intervention* 17(2), 241-264.

Lavoie, M. (2014). *Post-Keynesian Economics: New Foundations*. Cheltenham, Glos.: Edward Elgar.

Lindahl, E. (1929 [1939]). 'The place of capital in the theory of price'. In E. Lindahl (1939), *Studies in the Theory of Money and Capital*. London: George Allen & Unwin (reprinted 1970 by Augustus Kelley, New York, pp. 271-350. Originally published in Swedish in 1929).

Mainwaring, L. and Steedman, I. (2000). 'On the probability of re-switching and capital reversal in a two-sector Sraffian model'. In H.D. Kurz (ed.), *Critical Essays on Piero Sraffa's Legacy in Economics*. Cambridge: Cambridge University Press, pp. 323-354.

Mariolis, T. and Tsoulfidis, L. (2016). *Modern Classical Economics and Reality*. Tokyo: Springer Japan.

Morgenstern, O. (1950). *On the Accuracy of Economic Observations*. Princeton: Princeton University Press.

Petri, F. (1999). 'Professor Hahn on the "neo-Ricardian" criticism of neoclassical economics'. In G. Mongiovi and F. Petri (eds), *Value, Distribution and Capital: Essays in Honour of Pierangelo Garegnani*. London: Routledge, pp. 19-68.

Petri, F. (2003). 'Should the theory of endogenous growth be based on Say's Law and the full employment of resources?'. In N. Salvadori (ed.), *Theory of Growth: A Classical Perspective*. Cheltenham: Edward Elgar, pp. 139-160.

Petri, F. (2004). *General Equilibrium, Capital and Macroeconomics: A Key to Recent Controversies in Equilibrium Theory*. Cheltenham, UK and Northampton, MA: Edward Elgar.

Petri, F. (2011). 'On the Likelihood and Relevance of Reswitching and Reverse Capital Deepening'. In: N. Salvadori and C. Gehrke (eds), *Keynes, Sraffa and the Criticism of Neoclassical Theory: Essays in Honour of Heinz Kurz*. London and New York: Routledge, pp. 380-418.

Petri, F. (2015a). 'On some modern reformulations of the labour theory of value'. *Contributions to Political Economy* 34, 77-104.

Petri, F. (2015b). 'Neglected Implications of Neoclassical Capital-Labour Substitution for Investment Theory: Another Criticism of Say's Law'. *Review of Political Economy* 27(3), 308-340.

Petri, F. (2017). 'The passage of time, capital and investment in traditional and in recent neoclassical value theory'. *Oeconomia* 7(1), 111-140.

Petri, F. (2020). 'Capital Theory 1873–2019 and the State of Macroeconomics'. *History of Economics Review*, DOI: 10.1080/10370196.2020.1722411.

Robinson, J. (1971). 'The Measure of Capital: The End of the Controversy'. *Economic Journal* 81, 597-602.

Samuelson, P.A. (1962). 'Parable and Realism in Capital Theory: The Surrogate Production Function'. *Review of Economic Studies* 29, 193-206.

Samuelson, P.A. (1966). 'A Summing Up'. *The Quarterly Journal of Economics* 80(4), 568-583.

Schefold, B. (2010). 'Families of strongly curved and of nearly linear wage curves: a contribution to the debate about the surrogate production function'. In J. Vint, J.S. Metcalfe, H.D. Kurz, N. Salvadori, and P.A. Samuelson (eds), *Economic Theory and Economic Thought*. London: Routledge, pp. 117-137.

Schefold, B. (2013a). 'Approximate Surrogate Production Functions'. *Cambridge Journal of Economics* 37(5), 947-983.

Schefold, B. (2013b). 'Only a Few Techniques Matter! On the Number of Curves on the Wage Frontier'. In: E.S. Levrero, A. Palumbo, and A. Stirati (eds), *Sraffa and the Reconstruction of Economic Theory: Volume One*, *Theories of Value and Distribution*. Basingstoke, UK: Palgrave Macmillan, pp. 46-69.

Schefold, B. (2016). 'Marx, the Production Function and the Old Neoclassical Equilibrium: Workable under the Same Assumptions? With an Appendix on the Likelihood of Reswitching and Wicksell Effects'. *Centro Sraffa Working Papers* No 19.

Schefold, B. (2017). 'The Improbability of Reswitching, the Certainty of Wicksell-Effects and the Poverty of Production Functions: The Cambridge Critique of Capital Transformed'. In L. Fiorito, S. Scheall, and C.E. Suprinyak (eds), *Research in the History of Economic Thought and Methodology*, vol. 35B, including a Symposium on New Directions in Sraffa Scholarship. Bingley, UK: Emerald Publishing Limited, 2018 (online publ. 2017), pp. 171-194.

Schefold, B. (2020). 'What Remains of the Cambridge Critique of Capital Theory, if Reswitching and Reverse Capital Deepening Are Empirically Rare and Theoretically Unlikely?'. *European Journal of Economics and Economic Policies: Intervention* 17(2), 220-240.

Shaikh, A., Coronado, J.A., and Nassiz-Pires, L. (2020). 'On the empirical regularities of Sraffa prices'. *European Journal of Economics and Economic Policies: Intervention* 17(2), 265-275.

Sraffa, P. (1960). *Production of Commodities by Means of Commodities*. Cambridge: Cambridge University Press.

Steedman, I. (2011). 'Capital "perversities" in a one (new) commodity model'. In V. Caspari (ed.), *The evolution of economic theory*. Abingdon, UK: Routledge, pp. 100-110.

Steedman, I. (2020). 'Fixed capital in the corn-tractor model'. *Metroeconomica* 71, 49-56.

Torres Gonzàlez, L.D. (2018). *Essays on Prices of Production and the Interindustry Structure of Technology and Demand*. PhD dissertation, New School for Social Research.

Wicksell, K. (1934). *Lectures on Political Economy*, vol. 1. London: Routledge and Kegan Paul.

Wicksell, K. (1954 [1893]). *Value, Capital and Rent*. London: Allen and Unwin (transl. of *Über Wert, Kapital und Rente*, Jena: G. Fischer, 1893).

Zambelli, S., Fredholm T., and Venkatachalam, R. (2017). 'Robust Measurement of National Technological Progress'. *Structural Change and Economic Dynamics* 42, 38-55.

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