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The Cambridge Critique and Professor Schefold: Some Clarifications on the Remaining Disagreements

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Abstract

This is a rejoinder to professor Schefold's 2022 reply to my 2021–2022 criticism of his views on what is left of the Cambridge critique of neoclassical capital theory. I concentrate on the main disagreements. I find Schefold's reply unconvincing on the usefulness of aggregate production functions, and also on the empirical evidence of a rarity of reswitching. I clarify better than in 2022 my reasons for rejecting his a priori approach to the probability of double switching. I insist that the impossibility to determine the endowment of capital destroys the neoclassical labour demand curve and the neoclassical investment function, leaving real wages and employment in need of a different theory, which renders a turn to a classical-Keynesian approach inevitable.

Keywords: capital theory; Cambridge controversy; reswitching; aggregate production function.

JEL codes: B51; D24; D50.

1. Structure of the paper. Some important agreements. On aggregate production functions

Professor Bertram Schefold has written numerous papers, from Han and Schefold (2006) onwards, in which he argues that empirical evidence and theoretical arguments suggest that reswitching of techniques and reverse capital deepening are definitely possible, but their empirical occurrence is of such low likelihood — so rare, to use his terminology — that the criticism of lack of uniqueness and stability of equilibrium of the savings-investment market is unconvincing, and then little is left of the Cambridge critique. My detailed disagreements, presented in P2022a (earlier version P2021b), have been answered by him with a long working paper (S2022a) whose second half has been reformulated and published as S2022b. (For brevity my papers are referred to by their date prefixed by P, and those by professor Schefold, except the co-authored ones, by the date prefixed by S). Relevant for the debate are also further papers by him or in co-authorship: Kersting and

Schefold (2021), S2023, Kalb and Schefold (2024). This rejoinder advances a few observations centred on the fundamental disagreements, which I hope clarify some aspects of the discussion.[1]

The present section discusses Schefold's claim that even though neoclassical 'pure' theory is unsustainable, some neoclassical 'tools' can be nonetheless useful, in particular aggregate production functions. The next two sections discuss the empirical evidence in support of a rarity of reswitching: section 2 concentrates on Han and Schefold (2006), section 3 on contributions by other authors. Section 4 disputes the right to speak of an *a priori* probability of reswitching, because there is no random mechanism determining available techniques. Section 5 rejects the argument that rare cases of reswitching affect the demand curve for capital too little to question traditional neoclassical arguments; the reason for my stress in P2022a on piecemeal switches is clarified. Section 6 argues that a theory must be capable of dealing with 'paradoxical' cases too however rare, and uses Marshall's treatment of Giffen goods as an example. Section 7 reminds readers that reswitching shows that there is no factor 'capital' whose endowment can be treated as given, and this destroys the labour demand curve and the decreasing investment function. The conclusion is that reswitching has fundamental critical implications; nor should it be assumed not to happen in applied economic enquiries, since whether it happens or not has little relevance for non-neoclassical applied studies.

I start with some important agreements between myself and Schefold.

There is agreement between Schefold and myself on the distinction between (i) a *traditional* marginalist (or 'neoclassical') theory resting on the treatment of capital as a single factor of variable 'form' and on a notion of equilibrium as a long-period position determining the composition of capital endogenously, and (ii) a modern *neo-Walrasian* theory resting on the notion of intertemporal equilibrium with initial given endowment of *each* capital good, a notion of equilibrium whose deficiencies are an issue outside the debate under consideration here (my views on this topic are made clear in P2017, P2022b, P2025).

There is also agreement on the fact that the equilibrium of traditional marginalist theory requires a given endowment of this factor 'capital', independent of relative prices and unaffected by time-consuming disequilibrium adjustments, and that such an endowment is indeterminable. Owing to the uniformity of the interest rate the condition, that the quantity of capital 'embodied' in different capital goods must be proportional to what the capital goods earn as net rentals, implies that this quantity must be proportional to their

[1] This rejoinder has taken a long time owing to personal difficulties (unrelated to the topic). But the importance of the topic has not decreased, so I dare submit this further belated contribution. The present version of this paper has benefited from useful comments from Roberto Ciccone, Stefano Di Bucchianico, Saverio Fratini, Enrico Sergio Levrero, Antonella Palumbo, Daria Pignalosa, Franklin Serrano, Antonella Stirati, Paolo Trabucchi, Attilio Trezzini, and an anonymous referee. Let me repeat that the disagreements I advance leave totally unscathed my great esteem for and friendship toward Bertram. Admiration is deserved by his intellectual honesty and courage in putting forth theses which contradicted earlier beliefs of his and which he knew would not be easily accepted.

exchange values,[2] but these are not independent of relative prices, which is what the given endowment of capital should contribute to determining. So the capital endowment, since it cannot but be proportional to the value of the capital goods present in the economy, does not have the persistency, and independence from prices, required of a factor of production whose given endowment should contribute to determining normal distribution and prices: any change in prices, e.g. due to a change in distribution, would alter the value of the existing capital goods. Furthermore, during disequilibrium adjustments the quantities of the several capital goods change, and their value cannot be assumed equal to that of the capital goods they replace: it is price changes that push to alter the capital goods demanded. The impossibility to take the 'quantity of capital' as given implies that the system of equations of long-period general equilibrium lacks one equation, so equilibrium is indeterminable (P2021a, pp. 539–551); the failure of the approach to determine income distribution implies that its characterization of wages and interest rates as reflecting the scarcity (and marginal contribution to social welfare) of labour and of capital is untenable.

But on the implications, for economic theory, of this agreement some differences of opinion appear to remain. To my question (P2022a, p. 112) "is the marginal/neoclassical picture of the forces operating in a capitalist market economy defensible?" Schefold replies: "The general answer is a clear 'No'. Marginalism as pure and complete theory has been discredited" (S2022b, p. 117). This phrasing implies that marginalism can also be something else than "pure and complete theory", and that whether this 'something else' too is discredited remains to be ascertained. In the earlier working-paper reply, S2022a, Schefold admits that even when, because of a randomness assumption, one can construct aggregate production functions where capital is treated as a single factor of variable 'form' ('jelly' capital, he calls it)[3] , this "does not solve the supply problem from the pure theoretical point of view" because "an explanation of distribution in terms of supply and demand for capital and labour must be able to say what the supply of capital and labour is prior to the determination of equilibrium", and for jelly capital this "is impossible in pure theory" (S2022a, pp. 19–20). Again the problem is limited to "pure theory".

[2] There is a logical necessity behind the measurement of this factor "capital" of variable 'form' as an amount of exchange value. All units of a factor tend to earn the same rental rate. Thus take two lands A and B of same quality and different surface; if land A earns, as total rent payment, twice as much as land B, this must mean that land A has a surface twice the surface of B, because it must contain twice as many units of land as B. Now consider two different capital goods A and B; assume capital good A earns as (*net*) rental twice as much as capital good B; *if* one wants to see these earnings as rewarding the productive contribution of a single factor 'capital' embodied in them, *then* one must conclude that A contains twice as many units of 'capital' as B. But the *net* rental earned by a capital good is the interest on its value, and the rate of interest is uniform, so the value of A is twice the value of B. Hence the "quantity of capital" 'embodied' in different capital goods is necessarily proportional to the value of those capital goods; so "quantity of capital" is necessarily measured by the *value* of capital goods; nor is there any other physical way to specify this quantity that would respect this proportionality: weight or volume of capital goods have no univocal connection with earnings and value. (The 'average-period-of-production' approach too respected this proportionality, under the assumption, needed by the approach, of simple interest).

[3] In older articles by Schefold a tendency appears to identify the treatment of capital as a single factor of variable 'form' by neoclassical authors with the use of aggregate production functions. In his more recent writings this seems no longer to be the case.

The ‘impure’ neoclassical theory is not clearly illustrated. Schefold speaks of “tools […] used [by neoclassical economists] in teaching applied economics”, and of “partial theories” whose coherence, validity and applicability must be assessed (S2022b, p. 116, in the Abstract and p. 117). But he gives no example of “partial theories”, and mentions only one such “tool”, the aggregate production function. So the latter is all I can comment upon. His view on the defensibility of this “tool” is stated at the end of S2022b:

> In his book (Petri, 2021a), Petri has chosen to deepen the critique at the level of pure theory. To me, it seems necessary to meet modern economics also at other levels of abstraction, as Petri himself does when he deals with more applied subjects such as wage bargaining (for instance Petri 2021a, chapter 13). Capital theory, discussed at a similar level of abstraction, leads to the approximate surrogate production function.

Whether the last sentence in this quote is correct or not (and what “at a similar level of abstraction” precisely means) is rendered of secondary importance by what I will now proceed to argue.[4]

Schefold continues:

> Is the neoclassical theory of distribution therefore in the list of admissible alternative theories of distribution? The choice, it seems to me, has to be made on the basis of logical and empirical criteria. Since reswitching is rare, what matters is the result that the capital-output ratio for the efficient techniques is given. The theory based on substitution then — if the result holds — is ruled out (S2022b, p. 133).

These lines admit that whether approximate surrogate production functions can be constructed is not sufficient for deciding on the theory of income distribution, and then conclude that neoclassical theory “is ruled out” because it would need ample technical substitutability, which on the contrary is lacking as Schefold argues in several recent papers. (As an aside, the words “what matters”, which suggest that the lack of substitutability is all that matters, are not appropriate: given the admission that a given supply of capital is not determinable, the lack-of-substitutability argument must be considered an ‘even conceding’ or ‘overkill’ argument:[5] *even conceding* a given capital endowment of variable ‘form’, *still* the theory would not hold because it needs one more indefensible element). But Schefold omits to ask whether the rejection of the neoclassical theory of distribution has implications for his view of the surrogate aggregate production function as a lower-

[4] On Schefold’s discussion of randomness as justifying aggregate production functions (S2013) I maintain reservations, but to discuss them would be premature without a preliminary answer to a request for clarification I must advance: given the importance of zero technical coefficients in real economies, can one really rely on the theorems by Goldberg *et al.* (2000) and Goldberg and Neumann (2003)? Schefold himself seems to be uncertain, in S2022b, p. 119 he admits the problem but does not clarify his view on its relevance, and adds a perplexing sentence not accompanied by proof or further elucidation: “The Goldberg-Neumann approach admits that up to half the coefficients of the input matrix can be zeros.” This, if true, would question Schefold’s reliance on these theorems in S2013 and elsewhere: a theoretically rigorous, hence disaggregated, matrix of input coefficients of an economy with thousands of products certainly has much more than half the coefficients equal to zero.

[5] See P2022b, pp. 1039, 1040.

abstraction-level ‘tool’ possibly useful independently of the defensibility of the general theory. Let us now do the asking.

Suppose one accepted that discussion of capital theory at a lower “level of abstraction” than in the “most abstract” general theory of value and distribution does lead to the approximate surrogate production function. But suppose real wages were determined not by supply and demand but by institutionally determined relative bargaining power in the class struggle, and that employment were determined by investment and the multiplier, with investment determined independently of full-employment savings, and with average capacity utilization of plants amply variable. As pointed out in a passage of P2022a on which Schefold does not comment, without the validity of the neoclassical theory of distribution and employment, in particular the validity of a given supply of capital and of full labour employment, “[t]he ‘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 actual income distribution or employment in any given economy in any given year” (P2022a, pp. 95–96). The reason is that the aggregate production function determines the normal average K/L once net output and the rate of profit are given, but does not explain what determines the latter variables, nor offers reasons to assume a given labour employment, nor to treat K as given (on this Schefold agrees). Thus even conceding the determinability of approximate surrogate production functions, the use of aggregate production functions in neoclassical macroeconomic and growth models would in no way justify those models without an assumed validity of (traditional) neoclassical ‘pure’ theory supplying a theory of employment and of income distribution. And given the difficulty — admitted by Schefold (S2006, pp. 793–794) — with deriving the *form* of a surrogate production function from empirical data, even the remaining possible use of this ‘tool’ indicated in the above P2022a passage comes out to require arbitrary assumptions about that form, so the ‘tool’ allows no quantitative conclusions, its use amounts simply to agreeing with the ‘pure’ theory on the sign of the change in K/L if the rate of profit changes: an agreement of no relevance without an acceptance of the entire ‘pure’ neoclassical approach, since distribution and outputs remain open to non-neoclassical determinations. Furthermore, it is strange that Schefold can consider the approximate surrogate production function a useful ‘tool’, given his conclusion of nearly zero ‘substitutability’.

Nor am I able to think of other ‘tools’ used by the neoclassical economist when teaching applied economics, which can be considered neoclassical and yet do not suffer from an analogous dependency on the validity of the general theory. For example the decreasing labour demand curve requires a given capital endowment, ‘well-behaved’ capital-labour substitutability, and investment adjusting to savings.[6] Schefold writes in S2023, p.

[6] The need for investment to adjust to savings is not always made clear to students. But if a decrease of real wages induces firms to hire more labour, raising total output and hence savings, unless investment rises as much as savings there will be insufficient aggregate demand and the sales difficulties will induce firms to decrease labour employment; the labour demand curve can be argued to indicate the labour employment

129 that “the neoclassical approach might survive as a set of tools for applied economics, if reswitching is rare *and if one abstracts from problems of neoclassical theory on the supply side*” (my italics). But which economist who understands these supply-side problems is going to abstract from them? They destroy the logical tenability of the approach, to abstract from them is totally illegitimate. Let me remind readers that the given quantity of capital in traditional neoclassical theory had as one main effect (and purpose) the need, in order for employment to increase, of a decrease of real wages. The adaptability of capacity utilization to demand even when it requires a capacity utilization 20% above normal, with essentially no change in the amount of (non-overhead) labour per unit of output, destroys this view;[7] and the accompanying changes in the quantities of circulating capital goods present in the economy render a given K simply ridiculous.

The vague terminology ‘pure, or most abstract, theory’ appears to be anyway out of place. The relevant thing is the *general* theory, how it defines the equilibrium, how it specifies the forces pushing the economy toward it. It is *not* ‘purer’ or ‘more abstract’ than applied economics, its ambition at generality does not make it abstract, its statements on what determines equilibrium quantities and distribution, and on comparative statics, aim at being general and hence at indicating only the *sign* of changes if the data of equilibrium change, but are very concrete.

2. On my objections to Han and Schefold (2006)

The remaining major disagreement concerns the central aim of Schefold’s more recent papers: to persuade critical economists to stop insisting on reswitching and reverse capital deepening (RCD) as criticisms of the neoclassical approach, because they happen so rarely that one has the right to assume that they are absent: “the focus of the critique of capital theory has to shift from the reswitching argument to other aspects of the debate” (S2023, p. 145); “Our results confirm that reswitching is rare [...] reswitching may be assumed away in the relevant applications [...] reswitching may henceforth be excluded by assumption in exactly those contexts, where the possibility of reswitching provoked a reconsideration of received doctrines, such as the existence of aggregate production functions, marginal productivity theory or the Keynesian hypotheses about the direction of technical progress” (Kalb and Schefold 2024, pp. 23–24).[8]

Note the ambiguity in the last quotation: do Schefold and Kalb mean that the “reconsideration of received doctrines” has come out to be ill based and those doctrines are thereby rehabilitated? Don’t those “received doctrines” anyway rely on an indefensible

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sustainable at each real wage only if investment adjusts to savings. The capital endowment behind that curve must be of variable ‘form’, or there would be too little substitutability, as admitted by Hicks both in 1932 and in 1980–81, see P1991, pp. 270–271, 283.

[7] See P2022a, pp. 93–96.

[8] The point about technical progress raises different issues which need not be discussed here, concerning the legitimacy of the more common assumptions in growth models about technical progress over very long periods.

treatment of capital? How can the marginal productivity of a factor be defined, if the amount of that factor is indeterminable? But these obvious objections are not discussed. Future mentions of reswitching, the authors continue, deserve to be confined to teaching, as a curiosum with no practical relevance: “Reswitching will also be discussed in the future, because the phenomenon is of didactic interest, if one teaches capital theory. Its empirical relevance for the analysis of real world situations has never been shown, however” (*ibid*. p. 24).[9]

I will argue that there are excellent reasons to keep insisting. These concern both (i) whether reswitching is really as rare as Schefold believes, and (ii) the relevance of the possibility of reswitching even if infrequent.

A first reason is that the *empirical* evidence on the rarity of reswitching, or rather of DS (Double Switching of two techniques, with no need for both switches to be on the envelope), is highly dubious. To start with, my objections advanced in P2022a, pp. 98–100 to the Han&Schefold 2006 exercise remain essentially unanswered. Schefold in S2022b, pp. 118–119 does not deny their validity, and his attempts to minimize the relevance of some of them are unconvincing.

To my first objection, that we know nothing about the true envelope of wage curves because we do not know the adoptable but not adopted methods (and goods), his reply is that “Petri does not produce any evidence that qualitatively different results would be obtained with still larger data sets”. But the larger data sets requested by my objection are impossible to obtain! A collection of reliable and sufficiently complete information on adoptable but not adopted production methods is clearly unfeasible. This is so obvious that I wonder whether the real intent of this reply is to suggest that the impossibility of knowing the true envelope should make us trust whatever we are able to obtain even if its divergence from the true envelope is unknown. But this would not be science.

On the averaging effects of aggregation, which render the ‘organic compositions of capital’ of the ‘products’ of the several sectors much more similar than those of the different goods in each sectorial ‘product’,[10] Schefold replies that in a basic system this does not cause relative prices to fluctuate less than without aggregation, because “interdependence dampens fluctuations even without aggregation” (S2022b, p. 118): a statement lacking general validity, because it requires interdependence to be everywhere *significant*, that is, changes in the price of each good must *relevantly* affect the prices of its inputs and thus reduce the possible change in its price *relative* to the prices of its inputs; which will not be the case for goods which are used by other basic goods in positive but very small amounts, so that the fluctuation of their prices has only very small effect on the price of their inputs. Schefold’s argument, very weak for basic goods, has no relevance for non-basic goods, whose price changes have no effect on the prices of their inputs: non-basics can be a cause of RCD even more easily than basic goods (not by chance the

[9] On the claims of the last sentence in this quotation, cf. below, section 7.

[10] Schefold in S2023 presents an empirical analysis based on a German 32-sectors I-O table. The enormous aggregation implicit in treating each sector as producing a homogeneous good is rendered particularly evident by the first sector’s name: *Agriculture, hunting, forestry and fishing*.

early numerical examples of reswitching and RCD concerned non-basic goods), and Schefold offers no reason for neglecting them, the only surprising argument is that capital goods are essentially basic goods — which is patently false: as noticed in P2022a, p. 103, most consumption goods and military goods are non-basic and all capital goods specific to their methods are then non-basic too (my guess would be that a majority of capital goods are non-basic).

On the doubtful legitimacy of combining 'methods' derived from different input-output (I-O) tables because a sector does not produce the same product vector in different I-O tables, Schefold replies that even if some combinations may not be legitimate, certainly the number of 'legitimate' combinations remains enormous. This is highly doubtful, because my objection applies to *all* sectors, none produces a homogeneous product, and it is impossible that the composition of the heterogeneous product of a sector be the same in different nations or years, so it requires considerable *faith* that the error is negligible to consider it legitimate to treat the same sector of two different I-O tables as producing the same good. And the assumption that all goods produced by a sector are produced by very similar methods is obviously totally unrealistic, and yet without it one has no right to assume that a sector from one I-O table, when combined with sectors from other I-O tables, continues to use the same inputs per unit of (aggregate) output, because most likely the composition of its output will have to change to adapt to the changed composition of demand for its several outputs, due to the changed 'methods' in the sectors it sells to.

Several objections remain unanswered. Let me remind readers of two of them which raise particularly clear theoretical difficulties.

First, Morgenstern's (1950) observation on the limited accuracy of economic data (P2022a, fn. 17 p. 98), which I should have listed as actually a *Tenth* objection, potentially undermines all significance of the results produced by exercises such as in Han and Schefold (2006) or Zambelli, Fredholm and Venkatachalam (2017), by rendering wage curves blurry and therefore switchpoints indeterminable. I am sorry if this implies that so many efforts were misplaced, but science cannot be built on results that lack foundations.

Second, neither Han and Schefold (2006) nor Zambelli, Fredholm and Venkatachalam (2017) nor Zambelli (2018) explain how they surmount the following problem, remembered by my *Third* objection and well known to scholars working with I-O tables of different nations. In order to interpret an I-O table as indicating physical technical coefficients, one chooses as physical unit for each 'good' (the product of a sector or industry, daringly assumed to be physically homogeneous) the amount worth one money unit, e.g. one dollar. But then if two I-O tables are derived from economies with different relative prices, one measures physical goods in different units in the two tables; all treatment of the two different 'methods' for the production of a 'good' derived from these tables as reflecting purely technological differences becomes illegitimate.[11]

[11] Zambelli, Fredholm, and Venkatachalam (2017) mention on p. 42 that current period values were adjusted using the prices indexes supplied by WIOD; but those price indexes correct for the effect of *inflation* (and only at broad sectoral levels, not for single products), not for different *relative* prices of products in different nations or years. Furthermore, different nations use different currencies and, when converting

So I see no reason to modify my conclusion that "the exercise in Han & Schefold (2006) appears radically misconceived" (P2022a, p. 100).

It seems clear that much more thinking and discussion should be allocated to what can be obtained from this type of exercises.

3. Further doubts on the rarity of reswitching

Continuing on the empirical evidence: it is not so clear as Schefold seems to believe, that the results of other scholars support his views. He mentions "a now large body of evidence that prices in terms of the standard commodity do not deviate as much from linearity as had been expected" (S2022b, p. 119); but Mariolis and Tsoulfidis (remembered in P2022a, p. 92 fn. 9) report that about 20% of prices change non-monotonically in the techniques derived from some I-O tables; see also Fig. 3, p. 74 in Shaikh (2022). As to Shaikh's conclusion that relative prices remain rather close to relative embodied labours, this can only concern averages: Figs. 2 and 5, pp. 73 and 76 in Shaikh (2022), show that some prices can deviate *very* considerably from labour values. Nor does one need to solve complicated matrix equations to realize it: a good that costs as much as one unit of numéraire when just produced, but must then ripen unattended for 10 years before being ready for sale, will have price 2.6 at the end of those ten years if the rate of profit is 10%, and price 6.2 if $r = 20\%$, in spite of an unchanged labour value. Techniques derived from I-O tables have difficulty with highlighting this type of effect of distribution changes on relative prices because in them all 'goods' are produced by one-period-long processes.

This last observation induces one to ask whether other aspects of the derivation of techniques from I-O tables can render DS and RCD more difficult to observe than they actually are. The issue is essentially unexplored, but probably important as the following observation suggests. Many numerical examples of DS and RCD, e.g. Garegnani (1964, pp. 45–49) or Samuelson (1966), are based on a similar difference, in the 'reduction' to dated quantities of labour, of the cost of a good produced with two different techniques: technique B has costs partly far in time from the final sale of the product, and partly very close to it; technique A has most costs at an intermediate time distance from the product sale.[12] Assume the two techniques produce DS and RCD: this can easily not emerge from

prices in one currency into prices in another, one encounters the problem of exchange rates which can depend on influences unconnected with purchasing power parity. For all the above reasons (plus Morgenstern's observation!) the 'technical' coefficients in the Han&Schefold or Zambelli calculations are not at all interpretable as reflecting physical realities with the precision required by the theoretical problem under discussion.

[12] That the techniques in many of these examples are Austrian is secondary, they can be transformed into techniques using basic goods with a change in cost behaviour as small as one likes, by rendering explicit the circulating capital goods (all specific to only one technique) produced by the technique in its succession of stages, and by modifying the initial one of the Austrian sequence of production periods by assuming that in it labour is not unassisted but rather is combined with *very small* (positive but as small as one likes) inputs of all these goods and of the 'final' good, and also utilizes a *very small* (in the same sense) quantity of corn, which is produced by corn, labour and *very small* quantities of some of the other goods.

an I-O table. Suppose indeed that, apart from corn, both techniques differ only in the use of circulating capital goods belonging to the mechanical industry, and that the final good too is a product of that industry. Different I-O tables will show only different yearly flows of value of capital goods produced by that industry and going to the same industry. There is no way that, from this, one can derive that there will be RCD as income distribution changes.

The conclusion seems clear that the 1.3% of switches causing RCD, found by Han and Schefold, and the similar percentage found, we are told, by an unpublished reconsideration of Zambelli (2018) due to Jacob Kalb, prove little on the actual frequency of occurrence of RCD; it seems extremely likely that this frequency is higher, possibly much higher, because I-O tables make it impossible to notice many cases of cost structures easily susceptible of causing RCD.

Anyway the relative frequency of switches associated with RCD on the envelope is positive even according to these highly questionable attempts to empirically assess it. If on the basis of the above considerations one takes, say, 4% as a more realistic measure of this average frequency than 1.3%, this means that cases are certainly possible in which the frequency of occurrence of RCD on the envelope is greater, even considerably greater; the probability of such cases will be low but positive, and therefore such cases are highly likely to have occurred a number of times in the history of capitalism, given the thousands of different savings-investment markets corresponding to different capitalist economies in different years since the early 1800s. In such cases the savings-investment market cannot be presumed to have worked as pre-Keynesian theory assumed, even conceding a given employment of labour: the savings-demand function (i.e. the investment function) would have had ample sections in which it was not a smoothly decreasing and rather elastic function of the interest rate. And yet investment and employment and the rate of interest were determined in these cases too. So as noted by Ciccone (1996, p. 42: Engl. transl. in P2011, fn. 25 p. 415) one is induced to look for an alternative approach capable of explaining what determines investment and the rate of interest in these cases too. And an alternative approach *is* available. There is no reason why one should stop advancing these considerations and repeating them as many times as necessary to defeat deaf ears.

4. On the *a priori* probability of reswitching

Therefore Schefold's appeal to empirical evidence is unconvincing. There remains his argument in S2016, S2018, and S2023 (and in Kalb and Schefold 2024) that the *a priori* probability of reswitching tends to zero as the dimension of the basic-goods matrix increases.

Schefold takes here the same approach as in earlier studies (including one by me, P2011): assume there is a switch of techniques on the envelope of wage curves of the given economy at a certain rate of profit r^*; assuming 'piecemeal' switches (more on this later), what changes as r passes from a little less to a little more than r^* is the direct method for the production of one commodity, call it commodity 1; take one of the two alternative methods for its production as given and ask what the probability is that the second method be such that the two wage curves intersect again at another admissible rate of profit. Consider as *a priori* possible all 'second methods' represented by vectors of technical coefficients of inputs and labour which, at the prices and wage associated with r^*, are equiprofitable with the first method: a continuum of vectors. Let S(r^*) be the set of all these mathematically admissible 'second methods'. Call R(r^*) the subset of S(r^*) consisting of methods each one of which yields a second switch with the first method at some other admissible r. The probability of reswitching (given a switch at r^*) is defined as the ratio of the volume (the Lebesgue measure) of R(r^*) to the volume of S(r^*).

This amounts to assuming that there is some kind of random mechanism which chooses the vector of technical coefficients of the second method among all those which are mathematically admissible given the condition that they must generate a switch with the first method at r^*; assuming a uniform distribution of the vectors in S(r^*), the ratio R(r^*)/S(r^*) is considered to measure the probability that the randomly chosen vector in S(r^*) is also in R(r^*). A detailed graphical illustration of S and R for the case of two-commodities (plus labour) economies is available in the mentioned papers by Schefold and also, more concisely, in P2021b.

I am not aware of published discussions of the assumptions implicit in such an approach apart from Salvadori (2000) and Bidard and Carter (2000). I accepted the approach in P2011 although noting that it suffered from the problem pointed out by Salvadori, because it was clear that D'Ippolito's (1987) conclusion did not hold even neglecting that problem, so it seemed unnecessary to go deeper into the legitimacy of the approach. But it is opportune now to take a step back and ask what one is really doing when proceeding in this way. My hints on this issue in P2022a were too quick, I try now to be clearer.

Salvadori (2000, p. 356) criticizes the paper by Maiwaring and Steedman (2000), which adopts essentially the same approach, by noting that a uniform probability distribution means vastly different things depending on how one chooses the elements describing the alternative techniques, for example direct or vertically integrated technical coefficients, and on what one assumes about their 'density' in S and in R, for example by stretching or compressing the axes on which one measures the elements of the vectors: this alters the shapes of R and S and thus also R/S: "by an appropriate one-to-one transformation the 'probability' can either be made to get very close to zero or to get very close to unity". He adds (p. 357) that for this reason a uniform probability distribution "cannot be justified simply in terms of the argument that since there is no information on it, the equi-probable distribution is the most appealing one": the same argument could be advanced for other ways to choose and measure the magnitudes defining the available production methods and hence their 'density' in a continuum. Bidard and Carter (2000) in their turn state that "one may imagine other geometric representations (S′, R′) for which the ratio L(R′)/L(S′) would be different, and therefore the above probability has no intrinsic meaning. More precisely, the nature of the problem itself requires a definition of its probabilistic structure at the very beginning" (p. 358).

These observations appear unassailable but they stop at noticing the arbitrariness of the proposed measure of probabilities. One can go deeper by asking, is something missing whose absence causes this arbitrariness, not present in other determinations of probability? Upon reflection the answer seems clear: what is missing is what Bidard and Carter ask for, the "definition", that is the precise characterization and justification, of the "probabilistic structure" of the problem.[13] Indeed one cannot start discussing the probability of an event without a previous careful description of the randomness that authorizes talking of a probability and specifying it quantitatively. If one obtains from observation the distribution of a characteristic, e.g. the distribution of the weights of sparrows in a given area and year, and if one can argue that the elements contributing to the weight of each single sparrow depend on a multitude of largely accidental events, then one can derive the probability that the weight of a sample consisting of one sparrow falls within a certain range. If one does not have an observed distribution of weights of sparrows, but this weight can be argued to depend in a precise way on a certain number of influences, for each one of which one has a probability distribution, then one can derive the distribution of weights. One must start either from an observed distribution, or from a distribution derivable from some 'mechanism' throwing out the values of the several instances of a characteristic with given probabilities. In either case, one must have reasons to postulate a mechanism producing with a specified randomness the values of a characteristic, a mechanism which may be well specified or simply emerging from the empirical observation of a distribution presumably due to random influences. Without this, it seems illegitimate even to talk of probabilities.[14]

Schefold's approach does not comply with this need. It simply *assumes* that a randomness exists, in the birth of production methods, which allows us to consider the vectors of these methods as random outcomes. But one cannot assume things out of the blue. The assumptions must be plausible, supported by arguments. What allows us to talk of randomness here? Certainly the engineers working day and night to improve the performance of racing motorbikes would laugh at any suggestion that the solutions they find to produce faster motorbikes were as equally probable as any other vector in the continuum of a priori abstractly conceivable permissible vectors representing improvements relative to the starting point, or that if not the uniform distribution, still some probability distribution existed.

So there is a deeply unpersuasive element at the very start of Schefold's effort to consider the available production methods as random results of a mechanism generating numerical probabilities that a method has one or another characteristic. It seems really difficult to imagine the known methods for the production of a good as resulting from

[13] However, Bidard and Carter proceed to propose a measurement of probabilities which appears to fall under their own (and Salvadori's) criticism, it depends on how the alternative techniques are described.

[14] At least, of *objective* probabilities which is what is relevant here, since what is at issue is the nature of the world and not personal opinions.

random processes.[15] But then the logical basis itself to talk of probabilities is missing.

It seems then illegitimate to talk of a probability that the alternative method to produce good 1 that generates a switch with the first method is in the set of methods yielding double switching. The premises to talk of such a probability are missing. Simulations are then illegitimate too.

This criticism renders less important to criticize other debatable aspects of the approach. One aspect, however, deserves mention: the absurdity of assuming as *a priori* possible *all* mathematically admissible vectors of technical coefficients for the 'second method' that satisfy the condition of a switch at r^*. In this way one absurdly admits as perfectly possible not only nonsensical production processes that produce metals without any raw material input, bread without any flour input, ice cubes without any water input, and so on, but also direct production methods requiring a *strictly positive* vector of technical coefficients, that is, methods that need positive quantities of *all* basic goods the economy produces — and in Schefold's approach the probability that a method *not* be like that is zero! How can one trust an approach based on such unreal assumptions?

But restrictions on the *a priori* possible methods aimed at excluding too unlikely cases would inevitably be arbitrary, questionable and potentially inexhaustible.

A possible alternative is to look for plausible considerations on what kinds of alternative methods for the production of a good one can expect to exist. This is virgin territory; I discuss one possible case, certainly many other ones can be imagined. A frequent alternative to a certain production method is another method that requires the previous development of a better instrument for the production of a desired characteristic of the final good: for example, a more costly alloy in the engine of a motorbike that allows reaching higher temperatures without the engine melting down. This idea of further anticipated costs that improve efficiency is, in embryo, the idea that Böhm-Bawerk tried to develop into a theory of income distribution by combining it with a given supply of capital (subsistence fund) and the average period of production; but this combination is not obligatory. Production by using the previously prepared instrument requires higher costs some time before the final good comes out, so the alternative method can be more convenient or not, depending on the rate of profit. If the new instrument also requires some extra cost close to the completion of the production process, a frequent case because of needs for maintenance, one has the time distribution of costs that easily produces DS, utilized in many numerical examples as recalled in section 3. Alternative methods with such a structure of costs no doubt are far from rare; it is even possible to imagine that they are the most common type of new alternatives to a given method, since a natural way to look for improvements in the production process of a good is by looking for better ways to produce some of its inputs. As noted, the presence of such alternatives is not easy to observe because they require more disaggregation than I-O tables are able to supply. But one can be certain that they are there, and they must induce us to expect DS to be not very rare.

[15] Nor is the real frequency of techniques producing RCD empirically obtainable, for the reasons discussed.

5. Small effect on the aggregates?

In connection with the conclusions of the previous section I wish to comment on Schefold's persuasion that if RCD is not very frequent then one can neglect its possibility because "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" (S2010, p. 122; also S2013, pp. 1167–1168). The persuasion is repeated in Kalb and Schefold (2024).

But there is no guarantee that "each single small change of methods of production in different industries can only exert a small effect on the aggregates". Schefold reasons here as if the assumption he makes, that at a switch the production method changes in only one industry, implied that the dimensions of other industries are little affected. But it is not so. A change in method in one industry can imply nearly any change in the vector of quantities produced by the other industries. In P2022a I mentioned the possibility that a change in distribution makes it convenient to shift trains from coal-powered to electricity-powered, certainly a relevant change (and even more relevant changes can be conceived). The example was presented as a non-piecemeal one,[16] entailing the activation of industries previously not active at all; but further reflection has made me realize that what I really wanted to point out was the possibility of a switch inducing very large changes in the vector of quantities produced and hence in the average K/L ratio, and that for this to happen there is no need that the switch be non-piecemeal (although a non-piecemeal switch entailing the birth of new industries makes the possibility much more evident, because it entails the appearance, in the vector of produced goods, of possibly many goods previously not produced at all). It suffices that the change in method relevantly alters the dimensions of the industries supplying inputs to the industry whose method has changed, and that the latter industry is a large one — not overly unlikely possibilities.

My insistence in P2022a, p. 102 on non-piecemeal switches, and also on non-basics, was motivated by a persuasion that Schefold's assumption of all goods basic and common to all techniques reduces the possible phenomena associated with switches of technique in a way that tends to decrease the likelihood of reswitching. What I have said on

[16] Switches of technique due to changes in income distribution are called 'piecemeal' by Schefold when at a switchpoint only one industry changes production method. A non-piecemeal switch from technique A to technique B is one where the adoption of a different cheaper method in, say, industry one entails the use of capital goods not utilized at all in technique A, so new industries are activated and possibly some industries (producing capital goods no longer required in technique B) disappear; so the matrix of technical coefficients changes in more than one industrial vector of coefficients, its dimension (the number of industries) generally also changes. Schefold assumes that all techniques use the same goods, all basic, and then it is a theorem that switches of technique are piecemeal; I would tend to believe — in accordance with Bharadwaj (1970) — that the more frequent case is that two different methods to produce a good differ in some specific capital goods, so that the switch from one to the other method is non-piecemeal, entailing the activation of some new industry. I apologise for having wrongly stated in P2022a, p. 102, because of a misreading, that Zambelli, Fredholm, and Venkatachalam (2017), p. 41, assumed all switches to be piecemeal: in fact they quoted Bharadwaj correctly and agreed with her. Sorry.

nonbasics in section 2, and the above example of a switch from coal-powered to electricity-powered trains, appear to me sufficient to confirm me in that persuasion. But I did not stress that any result achievable with non-piecemeal switches that activate previously non-active industries, or with non-basic goods, can be replicated essentially unchanged by assuming that these industries are actually basic industries but barely so, because their outputs are basic but used in very small quantities by other industries.[17] However it can be difficult, when assuming that all goods are basic, to remember that one can still obtain practically all possibilities that intuition associates with non-basics or with non-piecemeal switches (Schefold's persuasion criticised in this section is perhaps proof of this difficulty), so my insistence on these possibilities was not useless.

6. Rare does not mean negligible

So DS and RCD may not be very frequent, but certainly do happen; a measurement of their probability appears impossible because the premises authorizing the use of probabilities are missing, but there is no way of excluding the possibility that sometimes the value of capital per unit of labour can behave in ways incompatible with traditional neoclassical theory (even neglecting the Kersting-Schefold 2021 argument of too few techniques on the envelope); which sends us back to the considerations at the end of section 3. A theory cannot neglect cases contradicting its premises only because these cases are not frequent. It must be capable of explaining them as in fact not in contradiction with its premises and forecasts, otherwise even a single case can show the need for a total rehauling and adoption of a different theory — a not infrequent event in physics.

Thus for example Marshall was far from dismissing Giffen goods because of their low frequency (which he conceded); he disputed in correspondence Edgeworth's dismissal of the possibility as too unlikely (Stigler 1947), and in the *Principles* he wrote that "such cases are rare; when they are met with, each must be treated on its own merits." (Marshall 1920, p. 132). Marshall did not explain why the necessary treatment of each case "on its own merits" did not reveal the need for a fundamental revision of the entire theory of value, but it is not difficult to understand the reason: the problem concerned the uniqueness and stability of the partial equilibrium of the market for a single consumption good; for a Giffen good the demand curve can have an upward-sloping portion, but will anyway be ultimately decreasing, implying in Garegnani's words: "the well known anomalies of the demand for inferior goods [...] did not call into question the general supply-and-demand analysis of prices only because it could be plausibly argued that: (a) should those anomalies give rise to a multiplicity of equilibria, the equilibrium position with the highest price would be stable, while that with the lowest price would, in all likelihood, be stable too; and (b) if the latter equilibrium were unstable, the rest of the economic system would not be affected since all we would have is that once the price has fallen below the

[17] The same holds for Austrian techniques as pointed out in fn. 9.

level of that equilibrium the commodity would not be produced due to a lack of demand willing to pay the supply price" (Garegnani 1970, p. 426, fn. 2). So even in the market for a Giffen good supply and demand could be argued to bring to equilibrium, and although there could be two locally stable equilibria in that market, the resulting indeterminacy of the economy-wide general equilibrium could be neglected as very minor, owing to the little size of that market relative to the entire economy.

To the extent to which an analogy can be made between low frequency of Giffen goods and a supposed low frequency of double switching and RCD — that is, if for the sake of argument the instability the latter might cause is discussed while neglecting the logically prior problem with determining the supply of capital —,[18] the implication is that a supply-and-demand analysis of income distribution must predict realistic outcomes also for the cases of upward-sloping demand curves for capital or for labour, independently of how frequently these can occur if they *can* occur. But for such cases the theory predicts instability and enormous jumps of income distribution, something never observed.

7. There is no factor of production 'capital'

Anyway discussion of the likelihood of DS and RCD should not make one forget what was pointed out in P2022a, pp. 96–97 and P2022b, pp. 1039–1041, and was implicit in the second point of agreement between Schefold and myself recalled in section 1: the argument that DS and RCD question the stability of supply-and-demand equilibria of factor markets is an 'even conceding' or 'overkill' argument, which for the sake of argument neglects the logically prior problem that capital does not satisfy the conditions required for treating it as a factor of production. As recalled in section 1, the logical need to consider the 'quantity of capital' as proportional to the value of capital goods suffices to render it impossible to treat that value as a datum for the determination of equilibrium; and an entity, whose magnitude cannot be treated as given because it depends on what it should contribute to determine i.e. prices, does not qualify as a factor of production.

Any residual hope that some 'technical' measure of capital intensity might be found, that would confirm that a decrease of the 'price' of capital (the rate of profit) induces an increase of its use relative to labour and thus an increase of output per unit of labour, is destroyed by the possibility that the dependence of the value of capital goods on distribution may cause reswitching, which implies that a decrease of the 'price' of capital can induce a *decrease* of output per unit of labour, a clear contradiction of the 'principle of substitution'. It is not by chance that Sraffa noted that what makes reswitching possible, namely the possibility of inversion of the direction of change of relative prices as the rate

[18] I am accepting here the traditional limitation of the analysis of the problems raised by Giffen goods to a partial-equilibrium framework where income distribution and other prices are assumed given, so no logical problem arises about the derivation of the supply curve and the demand curve for the Giffen good.

of profit increases,[19] “cannot be reconciled with *any* notion of capital as a measurable quantity independent of distribution and prices” (Sraffa 1960, p. 38). So there is no way to explain the changes in output per unit of labour induced by changes in distribution as due to the induced changes in the employment of a factor of production. Conclusion: there is no factor of production ‘capital’.

This is by itself decisive. Its implications are not always fully grasped. The simple *possibility* of reswitching destroys the right to treat capital goods as elements of a single factor of production; this destroys any residual hope that it might be possible to speak of a given endowment of this imaginary factor;[20] this makes it impossible to construct the neoclassical decreasing labour demand curve, since it is impossible to specify the quantity of capital kept constant as one varies the real wage. Without the labour demand curve there is no supply-and-demand mechanism causing wage changes to push toward full employment. The consequent inability to argue for a spontaneous tendency toward the full employment of labour makes it illegitimate to treat labour as fully employed, but this undermines the derivation of the investment function from the function that associates to each rate of profit the aggregate normal value of the capital goods demanded to produce the presumable full-employment net output, see P2022a, fn. 16 p. 98, and P2015. So one must turn elsewhere for a theory of investment, and for a theory of wages and of the effects of changes in wages on employment.[21]

This argument holds independently of the frequency of reswitching; a convincing proof of rarity of reswitching would not rehabilitate the neoclassical approach; what is missing is the logical requirements for the consistency of that approach. This eliminates the possible rehabilitation of neoclassical analyses which, as noticed at the beginning of section 2, the most recent considerations by Schefold on the rarity of reswitching ambiguously do not exclude.

So it seems necessary to turn to non-neoclassical theories of income distribution and of output and growth. Mainstream macro and growth models and explanations relying on capital treated as a single factor and on capital-labour substitution must be relegated to the dustbin.

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[19] This inversion is known to happen even with matrices derived from I-O tables, as remembered in section 3.

[20] Thus the frequent distinction between supply-side and demand-side problems of the traditional marginalist conception of capital as a single factor should not make one forget that the ‘demand-side’ problem highlighted by reswitching, the non-validity of the ‘principle of substitution’, has the supply-side implication that there is no factor of production ‘capital’, rendering nonsensical the idea of a given endowment of this non-existent factor.

[21] Also, besides the problems with the investment function, the idea of a savings-investment market where the interest rate acts as the price bringing toward equilibrium is fraught with difficulties concerning the determination of the savings function: savings do not exist before income is produced, and income is not produced before demand — i.e. ultimately investment and the other components of autonomous demand — stimulates production. This is a further reason to view the argument that RCD questions the stability of the long-period neoclassical general equilibrium as an ‘even conceding’ argument. On the problems of attempts to argue the stability of the savings-investment market in a Marshallian short period, see P2022a, p. 92, P2022b, pp. 1041–44.

This makes it evident that reswitching and RCD are far from being only 'of didactic interest',[22] they have very concrete implications: they require abandoning all arguments based on capital treated as a factor, and on capital-labour substitution. Insistence on these implications appears necessary in order to get them understood and accepted by the many economists who have absorbed decades of reasonings based on a given capital endowment and on capital-labour substitution and will not find it easy to get rid of such habits of thought and to admit the conclusions imposed by the above argument, even when ready to concede that there are insurmountable problems with the 'quantity of capital'. (Schefold too ought to make an effort to stop talking of 'capital intensity' and of insufficient substitutability between capital and labour — as if it made sense to talk of economy-wide substitution between labour and a non-existent factor). And of course it is indispensable to carefully explain the above argument to students, if we want them to learn to be rigorous in their theoretical reasonings, and capable of rejecting inconsistent theories when becoming economists.

Of course the implications of the argument require, in order to be fully grasped, that one becomes familiar with the differences entailed — relative to neoclassical analyses — by the natural alternative, a classical-Keynesian approach that stresses social conventions and conflict among social groups with different interests as indispensable determinants of income distribution, and autonomous demand and the multiplier as indispensable to the explanation of employment and growth. Not teaching at all this alternative must be recognized for what it is — an anti-scientific attempt to defend the dominance of neoclassical explanations by wilful maintenance of ignorance and refusal to accept debate. (On this issue I see no disagreement between myself and Schefold).

One final observation is opportune on the relevance of the frequency of reswitching for applied analyses. Schefold seems to think that one should simply assume that reswitching does not happen.[23] But nothing relevant for applied analyses hinges on making or not such an assumption, *once one is clear that even making it would not legitimize a return to analyses based on capital-labour substitution*. That reswitching *can* happen is sufficient to oblige one to turn to a different theory of income distribution, employment and growth. Once one turns to the different theory that naturally suggests itself, the classical-Keynesian one just hinted at, the frequency of RCD becomes irrelevant because of the flexibility of production in response to demand variations remembered at the end of section 1, which destroys the neoclassical idea of the 'quantity of capital' as a constraint on output and employment that obliges K/L to decrease if one wants an increase of employment. The disappearance of that constraint is well illustrated — if I can be allowed to quote myself — by the following observation based on that flexibility: "(apart from political reactions) there generally is no incompatibility between more employment and higher wages; all that is required is that the higher wages be accompanied by a stimulus

[22] See the end of the first paragraph of section 2.

[23] See again above, the first paragraph of section 2. Schefold's observation, quoted at the end of that paragraph, that "[reswitching's] empirical relevance for the analysis of real world situations has never been shown" is dealt with by what I proceed to argue in the text.

to aggregate demand. This will be so even when it were the case that a higher wage implied a shift to more value-capital-intensive techniques and therefore required more savings:[24] the increase in savings will be brought about by the increase in aggregate output" (P2011, p. 416). The adaptability of the quantity of services of capital goods to output needs, in the short period through variations in the degree of capacity utilization, in the longer period through investment altering capacity itself, renders the presence or absence of RCD generally unperceivable, and fundamentally irrelevant for the explanation of employment and growth.

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[24] (Clarification added in 2025) That is, implied a greater K/L which required net investment and hence more savings in order to be reached.

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