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What Remains of the Cambridge Critique? Potential Conclusions and Directions for Further Research following from Recent Investigations in Capital Theory

Reply to Professor Petri's Paper "What Remains of the Cambridge Critique? On Professor Schefold's Theses"

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Abstract

The debate on capital theory is not any more on the discussions about the historical formation of neoclassical ideas in their original, most abstract form, but about the tools – certainly influenced by those ideas – which are used in teaching all over the world in applied economics. One focus still is on the macroeconomic aggregate production function, almost seventy years after Joan Robinson attacked this concept. It has turned out that reswitching is rare – once the most effective argument against the production function – and that an approximate surrogate production function can be constructed, using the approach of random matrices. This seems to weaken the critique, but a new one has emerged, which shows that the number of effective techniques on the envelope is small and that the possibilities of substitution between capital and labour are quite restricted in the relevant range of the rate of profit. This new turn in the debates on the critique of capital theory has recently come under attack by Fabio Petri of the University of Siena. The present paper constitutes the reply. It deals with the methodological difference between a fundamental critique, which was primarily directed against the logic of the pure late 19th century neoclassical theory and one attacking the applied uses of that theory in the form of the macroeconomic production function. It asks why the valid criticisms of the neoclassical conception of capital as a homogeneous factor seem to have had a lesser impact than the reswitching argument. It discusses reswitching and reverse capital deepening as relevant but, as far as basic commodities are concerned, rare phenomena. It assesses the usefulness of empirical input-output research in this area, mentions some results and concludes with a reflection on the recent 'zero-substitution' proposition.

Keywords: Capital theory, production function, reswitching, Sraffa, employment
JEL codes: B24, C62, C67, D57

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1 Introduction

My first duty is to thank Professor Petri – or may I say: my friend Fabio – for the respectful and inquisitive paper (Petri 2021b), in which he surveys much of my theoretical work in the last 15 years in an effort to discover the theoretical conception animating my research. He reacts to eight of my papers (including two written with a co-author) and touches so many issues that a book seems required to answer him. I may refer to four more recent papers (Schefold 2021a, 2021b, 2022a, 2022b) addressing some of the questions he raises in more detail. The main themes, however, which we shall take up below, are dealt with in the following four sections:

2 Method

3 The forms of capital and its supply

4 Capital reversals

5 From reswitching to the essentially unique efficient capital-output ratio

Petri's aim is clear: to defend the surplus approach and its rigorous negation of neoclassical theory. He has written important books on both these aspects of the revival of classical economic theory: his earlier book on capital theory (Petri 2004) and his monumental "Microeconomics for the Critical Mind" (Petri 2021a). My aim is to develop recent investigations into capital theory further and to assess the implications. To do so, I have to set aside prior beliefs. He speaks in his "concluding comment", p. 32-34 of the paper (Petri 2021b, henceforth quoted just with the page number), of "the reader's uncertainty about what Schefold believes" (p. 34). This, to me, is a curious phrase. The papers under discussion are theoretical. They formulate conjectures, they make assumptions and state results. Beliefs are voiced in policy papers, where we ask on the basis of limited knowledge whether we 'believe' that given objectives can be reached or that the objectives are good. I do not pretend ever to have been fully able to pursue my research without any preconceptions according to the ideal of a value-free science. One often sets out to prove a theorem that would fit the paradigm and help to identify with fellow researchers. But the essential task is to derive new results so that one has to suspend 'beliefs' about their significance, until they are firmly established. What, if an unexpected weakness of a paradigm is discovered? Such events may cause regret, but one should rather welcome them and strive to see them as opportunities to dig deeper. New propositions have been found, but conclusions are not yet certain. So, my first and most basic response is: My work on capital theory is in a state of flux and some of my answers are provisional.

2 Method

2.1 Cambridge did win – but in what sense?

To explain my position as far as is possible at present, I begin with what seems to me the most difficult point. It will reassure my critic only partially, if I agree with his statement, following Sraffa's often quoted proposition at the Corfu conference on capital, "that theory requires that notions be defined with absolute precision, and therefore either capital ... can be rigorously defined or the notion must be abandoned" (p. 8). What was meant here was *pure* 'theory', 'rigour' meant: without approximations and exceptions, 'abandoned' therefore meant: the research project that attempts to represent 'capital' as a 'single factor of production' should be abandoned within *pure* theory.

And this happened! I have never met a mainstream economist who would use neoclassical tools in his teaching and applied work who dared to deny that Cambridge (GB) had won in the capital debate. How would *I* doubt it then? The difficulty is to assess what the Cambridge victory in pure theory implies for applied economics. It is here that opinions about the debate remain divided. Similarly, it may be asked why intellectuals like Paul Samuelson were impressed, but not converted in consequence of the Cambridge debate. If one does not cling to the simplistic idea that he was a paid lackey of the bourgeoisie, one has to take seriously his opinion that Sraffa was part of – not an alternative to – a widely understood economic mainstream and therefore part of a pluralist mainstream, within which one could lean towards this or that current. Sraffa did not like this embrace, but the "mainstream" is more pluralist now than at the time of the capital debate.

Imperfections can be found in all the theories of the sciences. I remember an example in a lecture course on mechanics in theoretical physics in my second year. The crack of a whip was taken as an illustration to explain the use of a Lagrangian function. It was shown that the velocity of the end of the whipcord tends to infinity, when the whip is agitated as a coachman does, the cord being of the same thickness everywhere. However, if the thickness of the cord tends linearly to zero toward the end, this velocity gets squared. The professor derived the result with a grin. It had deficiencies: It is obvious that relativity theory excludes infinite velocities and that the stiffness of the cord was disregarded, but it explained the phenomenon: the whip cracks, because its end is faster than the velocity of sound. 'Relativity' referred to 'high' theory, 'thickness' (or was it 'stiffness'?) to mundane applied science. The use of production functions in that mixture of at least two theories, neoclassical and Keynesian, coupled with applications – what Samuelson called the 'neoclassical synthesis' – resembles more the analysis of whip-cracking than relativity theory and 'realism' presupposes simplifications like the abstraction from a measure for

the 'stiffness' of a cord – realism is not a one-to-one mapping of nature.

Sraffa himself, at the Corfu conference, spoke about a measurement of capital, in which the 'statisticians' were interested: The statisticians' measures were only approximate and provided a suitable field for work in solving 'index number problems'. What, then, is the theory that helps the statisticians to relate their measurements of capital to prices and distribution? Does the application of the measurement not involve theoretical work that is less pure, but still useful, even if it glosses over certain approximations? And such less pure theoretical work may even be indispensable in order to be realistic, when the pure theory with its strong assumptions arrives at conclusions that make no sense in the concrete economic context. The multiplier predicts a responsiveness of output to increases in investment that are not reached in reality; subsidiary elements of theory are called for to explain that. Despite the modifications of different kinds (differential savings propensities for capitalists and workers, crowding out or in, psychology), the theory of the multiplier remains important. In our case: The pure theory tells us that only one technique is in use in a country in a given state of distribution. In reality, we find many techniques coexisting in a competitive process, which we can describe at a lower level of abstraction. Pure theory tells us that, if techniques are ordered according to their appearance on the envelope of the wage curve, their capital-intensities will not necessarily diminish as the rate of profit rises, contrary to what is supposed to happen, if a neoclassical production function is given, but the majority of economists feel unconcerned – although the 'paradox' has been known for more than half a century – because they surmise that capital reversals are exceptional.

And now it has turned out that, in this, they are right, as far as reswitching and reverse capital deepening are concerned. Moreover, theoretical reasons for the rarity of these phenomena can be given. It is quite true that the reswitching-argument (to put it short – what really matters is reverse capital deepening) is not the only one that can and has been advanced against neoclassical theory, but it was regarded as of special importance, if not decisive, in the Cambridge debate. As an example, I refer to Piero Garegnani's critique of the use of neoclassical concepts in Keynes. Garegnani (2003) there starts from the problem of transferring the Keynesian saving-investment mechanism to the long period. He points out that there is no theory of the rate of interest in Keynes that would be valid in the long run. The psychological factors are not convincing, nor the argument of liquidity preference, which are both typically short run. Thus, under the disguise of the new name 'marginal efficiency of capital', the old theory of the relation between the rate of interest i and the volume of investment returns in the *General Theory*. But there are the changes in the value of physical capital K (the Wicksell effects) and the change in

physical capital in response to changes in the rate of interest, hence the desired capital stock $K(i)$ is subject to capital reversals, and Garegnani writes – his rhetoric culminating in this phrase: "There is no reason why this second kind of change should have one sign rather than another". Clearly, this statement needs to be revised, if reswitching is rare. I, for one, do not believe in a stable investment function dependent on the rate of interest, for different reasons, the most obvious being that raising interest rates may choke all planned investment, but lowering them does not automatically lead to new plans for new equipment. But this is not the point to be made. Garegnani's example is only one, prominent among many, for papers written in the aftermath of the Cambridge debate, which demonstrate the importance then attached to the demand side critique of capital in those years, as opposed to the supply side arguments now stressed by Petri. Early formulations of them have been around even longer than those of the demand side. It seems to me that Clark was aware that his notion of 'capital' was a construct, for which he sought intuitive support. On the other hand, a kind of reswitching was used as an argument by Irving Fisher against Böhm-Bawerk.

To be sure, Petri does not deny the special importance of the demand side critique, which I began to question after a first empirical investigation with Han, having believed in it before for almost forty years. Petri is convinced that reswitching remains important for three reasons: (1) because even a single instance of reswitching demonstrates the inconsistency of the neoclassical theory, (2) because he doubts the empirical evidence, since it is based on input-output systems, which he regards as inadequate for the representation of this problem of capital theory and (3) because he questions my theoretical analysis of the likelihood of reswitching.

I shall deal with (2) and (3) below. As regards (1), the door is wide open. A consistent pure neoclassical theory does not exist; reswitching is sufficient proof of that. Neoclassical theory is not able to base its versions with malleable capital on a rigorous aggregation from the data of industries, let alone from firms, in analogy to the support lent to classical thermodynamics by statistical mechanics. The big question lurking in the background here is whether pure theories in the sense of physics are at all possible in economics. Why should they exist? Only a few words about this deep and difficult question.

2.2 Sraffa and the methodology of economics

The first obstacle to the formulation of deductive theory is the freedom of the economic agent not to behave like an automaton (the maximiser of utility in neoclassical theory, the behaviour of the character mask in Marx). From this, one may perhaps abstract; Sraffa

strove to devise a theory that would be independent from subjective influences. This choice ('objectivism') limits the domain of the theory. 'Subjectivist' theory also limits the domain, in a different way, with assumptions about rationality. A theory of bounded rationality is not 'pure', for lack of precise concepts of 'boundedness'. Hence the need of the fiction of the utility maximiser in the attempt to produce 'pure' theory.

The second obstacle is historicity. To postulate that there is one true and valid economic theory is to postulate a naturalism, in which both some classical and neoclassical economists believed, and also philosophers of natural right who were influential in the catholic church. They were criticised by Marx and the historical school who thought that different theories had to be applied to different circumstances. Theories could be understood only in their context; their understanding required historical vision. If this is correct, the denial of neoclassical theory does not consist in the erection of one other theory, but in a different approach to devising theories.

This is what Sraffa did. Kant once said (Schefold 2021c) that every scientific discipline contained as much true science as it contained mathematics. He thought that mathematics (our vision of space, our understanding of numbers, our ability to erect mathematical theories on this basis) were given to us *a priori*, prior to all experience. Our apprehension of reality is conditioned by these foundations of our vision, and the concepts needed to transform our cognition into science thus had to be built from mathematics or formulated in mathematical terms. Since these were given prior to the scientific research, they provided the language to describe objects of cognition and relationships between them. They yielded a proto-science of *a priori* validity. The proto-science was for Kant a necessary truth, since he thought that we could not apprehend reality without using the mathematical form of intuition, consciously or not. Kant thus made an attempt to derive the basic principle of Newtonian physics from our vision of space, the understanding of number and measure, and the analysis of movement in space. He got surprisingly far in this endeavour in his "Metaphysische Anfangsgründe der Naturwissenschaft" (Kant 1787). However, his space was euclidean, a maximum velocity (of light) was not conceived of, and so he did get to, but not beyond Newton's axioms. There are now other geometries besides the euclidean, there are many mathematical disciplines, random processes are a possibility and so, no particular form of apprehension imposes itself as a unique and definite *a priori*. There was truth in Kant's derivation of Newton's axioms, but it was not really a necessary truth, since the assumptions (the geometry, the velocity of information) could be varied (Weizsäcker 1971). Nonetheless, mathematical concepts are available for the description of nature prior to concrete experience, and measurement of length, time, weight, mass allows to apply the concepts in experiments. Alternative mathematical theories

are available for the description of reality, and which are suitable becomes an empirical question.

But what about economics? We live within the object of investigation, the economy, and use economic terminology uncritically from day to day. Under these circumstances, neoclassical theory chooses to start from the freedom of the individual to exchange or not to exchange objects of his or her property with the aim of increasing utility in the exchange process. Marx sees the individual in the beginning of *Das Kapital* in the same position, but the individual is not any proprietor but a worker and producer, and this individual is not free to negotiate prices, for the values that determine exchange are a social given. And indeterminacy is only in the question: can the value of the product be realised – can it be sold at all?

Sraffa, in the beginning of his book, eliminates the remnants of subjectivity. He opens with a system, in which commodities are anonymously so exchanged that self-replacement is possible, but not necessarily realised. And so, with this notion of 'commodity' and of 'price' in this special function of permitting self-replacement (not 'market prices', as we are told later), he constructs his conceptual framework, his protoeconomics, creating at the same time a visualisation of the economic process, in which social categories are introduced such as profit (contrary to Marx, 'profit' precedes labour), wage, etc. The fundamental concepts of Sraffa's protoeconomics are not defined in mathematical terms as in Kant's attempt to construct a basis for Newtonian physics. Instead, the economic concepts follow from verbal definitions with visual content (the description is not merely mathematical as the description of space in mechanics), and they define each other mutually. What is meant by price, for instance, becomes clear gradually with the exposition of a specifically 'Sraffian' price theory. The historicity then is in the institutions, that are implied (no uniform rate of profit without profit maximisation, no wage without the struggle around distribution). At least three theories of distribution are alluded to, which seem loosely to form a logical as well as possibly a historical sequence: the real wage as subsistence, the direct division of the net product between wages and profits, the determination of the rate of profits by the monetary rates of interest. A pure system has been created; Sraffa leaves making the transition to real systems to the reader. To speak of a 'construction' would be an understatement, for the 'construction' refers to putting-together of given parts. The breathtaking experience, however, is to realise how 'parts' such as 'price' take a new meaning relative to everyday speech and how the reader gets involved in the creation of Sraffa's protoeconomic world.

Mathematics is already there, when the reconstruction of the world of physics is to be undertaken, Kant thought, and the consistency of mathematics would ensure that of

physics, in his case mechanics. Sraffa's conceptual framework had to be derived from the set of economic notions in everyday speech and the inconsistent approaches of received economic theorizing. The result was a piece of pure theory, suitable to criticise marginalism as pure theory, but the transition to applied economics, which must confront a more complex world, remained to be made. That this would involve working at another level of abstraction became clear for instance, when the short run was to be discussed or the concrete formation of market prices by one of many possible mechanisms, subjects which cannot be avoided, if one deals with applied economics.

With this, I have summarised what has always been my interpretation of Sraffa, since the early seventies, I think even before the term 'surplus approach' was coined (translation in Schefold 1989, Part III). I had the privilege of discussing the stylised nature of his concepts with Sraffa himself around 1969-1970. What fascinated me in particular was the bold assumption of the homogeneity of the commodities, which contrasts so much with the variety of goods traded in actual markets and which abstracts totally from the processes in which standards of quality are ensured by guilds (in antiquity, the middle ages and beyond), later by cartels and syndicates, by corporations, by the state. I have written a series of papers on the history of *Warenkunde*, in Italian *merciologia*, in the English translation of Marx the 'commercial knowledge of commodities' (Schefold 1999). The historical texts of *Warenkunde* are mainly about the changing forms of consumption goods, but, in principle, also about the forms of capital goods. Sraffa's objectivism causes him to hide behind a science-based idea of the homogeneity of commodities as use values; he speaks of gold, iron, pigs, but the definition of the standard of fineness of gold is social, and so are the qualities of wheat etc., and, of course, of labour. 'Pigs' are no doubt mentioned to remind the reader that consumption is culturally determined, iron stands for the industrial revolution, etc. Summarising these observations, one might interpret 'Production of Commodities' not only as a model with some intuitive visualisations, but as an ideal type, as a contribution not only to economics, but also to social science. The aim is to lead to an understanding of what 'capital' is in economic, social and political theory. Sraffa crowns his analysis with the insight that even if all commodities and labour are assumed to be homogeneous and measurable, 'capital' is not. The supply of 'capital' cannot be well defined. If an equilibrium can be determined, it is possible to value the capital goods by means of the equilibrium prices, but if the 'capital' is given in the form of endowments prior to the determination of equilibrium, we cannot say how much 'capital' that is before we have the equilibrium prices. The consequences of this insight are not the same for pure and applied theory (see Section 3).

2.3 The forms of capital: a neglected area of research

And here comes Fabio Petri and accuses me of disregarding the problem of the 'form' of capital! Have I not always been fascinated by the task of understanding this form? The story behind my papers, which he has read – I am grateful for that – becomes in part clear from what he recounts, but he has not really grasped the intentions of my research project, which was supposed to lead to a theory of capital encompassing also a theory of its changing forms. Only, and in this Petri is right: the form of capital has hardly been discussed in the papers he refers to – there was not much room for that. To explain my reasons, I must start from the Cambridge critique, in which the concept of capital was only narrowly conceived.

Reswitching had been important in the debate, which concerned the opposition between the classical and the neoclassical heritage. What had to be the consequences, if reswitching turned out to be rare? Or was it not so rare?

Anwar Shaikh and his school had insisted (Shaikh et al. 2020), amidst disbelief and defiant denials, that relative prices do not deviate that much from relative labour values. Separating this insight from the Marxian context, it meant that prices in terms of the standard commodity were, as functions of the rate of profit, quasi-linear, with possibly significant exceptions, and so were, also empirically, wage curves. For both, randomness seemed to be a partial, but not complete explanation (Schefold 2016a, 2019). Randomness of the matrix, of the relation of the labour vector to the matrix, of the subsystem producing the money commodity, made it possible to show, after more than one hundred years of debate about the transformation problem, that Marx's key proposition in his theory of exploitation was correct (Schefold 2016b): total profits (P) can be seen as redistributed surplus values (M), if the system is random (M is the abbreviation of Mehrwert = surplus value). Sraffa himself had an inkling of this result, however, based on the standard commodity, not on randomness, another interpretation of what Marx meant by the formation of 'averages'. I regret that Petri does not want to see the significance of the result $P = M$ (p. 2). At any rate, it seemed that Marx and the neoclassicals had something in common: capital as a substance. And it concerned the demand side of capital. If randomness was assumed, one obtained not only $P = M$, but one could also obtain the linear wage curves that had been the basis of Samuelson's surrogate production function. As Petri correctly notes, this was not a full confirmation of neoclassical theory – what Petri calls the 'supply side' of the capital problem had not been discussed, and a number of other problems were (and are) still open, for instance, the legitimacy of the random hypothesis. We shall deal with some of them later. The form of capital in neoclassical theory came up in a third application of the randomness hypothesis, after the applications to the

surrogate production function and to the transformation problem. This third application concerns the so-called 'old neoclassical equilibrium'. I tried a rational reconstruction of the old neoclassical equilibrium, which I had come to know through Garegnani and Fabio Petri himself (Schefold 2016a). The rational reconstruction involved in this case a proper grouping of the equilibrium equations and a formal existence proof. Petri seems to take my rational reconstruction as an attempt to justify neoclassical theory, but a proof of consistency does not imply that a model is valid. Moreover, there is, indeed, the big problem on the supply side: capital is given as stock of value prior to the determination of prices and what does that mean? Anathema! exclaims Petri, and he would be right, if I had accepted that notion uncritically. But I stated its problematic and said, in essence: *if* one accepts this assumption, the model has the following merits or demerits.

The reader may now have understood that I tested, inspired by the work of many others, like Anwar Shaikh, Christian Bidard, Fabio Petri himself, how the critique of capital might be developed in view of the new insights, and not just defended, after my research programme regarding the theory of joint production seemed to me closed. It has been an attempt to mediate between pure and applied economics. In between, there was a debate about intertemporal equilibrium theory, involving late papers by Piero Garegnani, among others; this debate does not have to be taken up here (Schefold 2008). The point is: Petri is right, insofar as the forms of capital play only a minor role in those of my papers he refers to, but I did contribute to two analyses of the forms of capital and value, by showing $P = M$ and by discussing the old neoclassical equilibrium.

3 The forms of capital and its supply

3.1 The forms of capital in Marx: commodities and money

Petri has identified what is – apart from the treatment of the old neoclassical equilibrium – a lacuna in the series of my papers by drawing attention to the 'supply side' problems of capital theory. How to deal with them? I propose to do it by beginning with a rational reconstruction of how they were dealt with by classical and neoclassical economists. I say classical *and* neoclassical, because Petri overlooks or, at least, does not mention the fact that the forms of value (and of capital in particular!) play an eminent role in Marx (I here neglect the less eminent role they play in Smith or Ricardo or Torrens or Mill). Was the debate about the corn model not also a dispute whether it was licit to take the physical form of capital as an expression of its value form? Marx was vehemently against doing that, as he explained in Theories of Surplus Value (MEGA^2 II 3, pp. 1271-72, Schefold

2017b, pp. 39-40). In this, the surplus approach, which I regard as an important, but partial interpretation of classical thought, is opposed to Marx.

The theory of the forms of value starts in the first chapter of *Das Kapital*, but there is no room here to consider the Marxian derivation of the forms of relative value and of the general equivalent. Through the postulate of an immanent measure of value, labour time, the substance which changes form, value, appears on the scene as a measurable phenomenon. Capital as the advance of the industrial capitalist then is more tangible than in neoclassical theory, where capital as an advance also appears in value form, but the 'substance' depends on the conventional choice of a numéraire (e.g. Walras's saving in terms of numéraire as the datum in the old neoclassical equilibrium). We recapitulate rapidly the main changes of form in *Das Kapital*, once money has been introduced: There is the circulation to realise the value of the commodity produced:

$$\text{commodity} - \text{money} - \text{commodity}$$

and the appearance of surplus value

$$\text{money} - \text{commodity} - \text{money}', \text{ with money} < \text{money}',$$

explained by the introduction of the commodity labour-power, which has the value in use of adding more value than its reproduction costs. The explanation seems to founder on the contrast between labour values and the prices of production, but, for Marx (and, in fact, if 'average' conditions hold and randomness predominates) total profits can be explained as a redistribution of surplus value, as stated above. In between the form changes introduced in the first half of the first volume of *Das Kapital* and the third volume, we encounter other form changes. The production of relative surplus value involves different forms of technical change – cooperation, division of labour, mechanisation – which I once endeavoured to represent entirely by means of transformations of Sraffa systems, but in Marx they all involve the advance of capital in monetary form, the conversion into material means of production and labour power, production and realisation in a value sum that increases due to exploitation. Production of relative surplus value occurs if the technical change reduces the amount of labour power needed to produce the commodities necessary for subsistence. Hence capital changes its form not only, insofar as it alternates between its existence as sum of money and a mass of means of production, but also the structure of production changes. The circle is enlarged in the second volume by adding the lending of monied capitalists to the industrial capitalist and the repayment of interest to the lenders at its end. The main part of the third volume adds fictitious capital to the circuit: claims to profits can be capitalised. Capital redoubles as the real thing and the discounted claims, as money had redoubled in the first volume, when money existed not only as a coin, but also as notes.

Why is it possible for Marx to describe the circuit of capital, with the form of capital changing from step to step, while the quantity of capital stays constant? And why is it possible to translate his analysis into the language of the surplus approach – the labour values being replaced by prices of production? Three conditions are essential: in the translation, long-period conditions are assumed at a given level of activity, the technique does not change and distribution is given, preferably in the form of a given rate of profit.

But these assumptions constrain the analysis. If the economy undergoes a cyclical movement, values or prices begin to change relative to each other in the cycle, as distribution changes during upswing and crisis; the clear determinations which are possible in long-period conditions become blurred. Nonetheless, Marx retains his value concept, even when the crisis leads to the collapse of firms. Capital then is 'devalued'; devalued assets are bought by financial capitalists. The precise theoretical meaning of capitals as quantities is lost, insofar as no theory for the extent of the devaluation is offered, but, in Marx, the name remains. Defenders of the surplus approach turn to a different paradigm, post-Keynesianism, when they have to discuss the cycle, which is undoubtedly an essential feature of industrial capitalism.

This change of paradigm is embarrassing for the surplus approach and for the post-Keynesians interested in the long run; attempts to integrate them have never been completely successful – at any rate, they have not led to an integration of the corresponding schools of thought. The surplus approach has *not* absorbed all that is essential in Marx's or Keynes's economics; it becomes pluralist – insofar similar to the mainstream.

3.2 The forms of capital and its supply: the non-neoclassical Wicksell effect

We therefore now start with diminished expectations, when we ask how the form of capital, the supply in particular, given Petri's challenge, can be treated in neoclassical theory – not in pure neoclassical theory – that has been rejected – but what room for a concept of capital supply is there in an approach capable of application? We move from the description of accumulation to the non-neoclassical Wicksell effect and from there to reverse capital deepening and the conditions of existence of an approximate surrogate production function both on the demand and the supply side.

Petri and I can probably agree that one should start the analysis of a capitalist system by beginning with stationary states or steady growth under tranquil conditions. According to the stylised facts, the rate of profit stays constant, but real wages rise with productivity. Market prices may fluctuate or follow a trend, where they are charged according to mark-

ups: in either case, normal prices change, as techniques change. This change is slow and gradual, if productivity growth is due to an intensification of the labour process and more abrupt, if mechanisation takes place, and especially, if new machines are introduced; the transition to new prices, given that normal prices changed, leads to secondary effects, because the structure of costs is affected in all sectors in a basic system – at least in principle; the secondary effects are small and imperceptible in most cases in most sectors in large systems. The change of equipment will be accompanied by attempts to sell the old equipment, where that is feasible. If a firms uses gas instead of coal, it will need fewer lorries for its energy supply and can sell them. In other cases, the equipment that has become obsolete in the sector that modernises has become obsolete also in others and cannot be sold anymore or only at a loss. Petri seems to be of the opinion that old capital goods cannot be sold (p. 9). This is the generalisation of a theorist, who focuses on pure theory and represents it mentally in the economy with only a few sectors (wheat, iron, pigs, ...), but the situation is different in a large economy, where entrepreneurs with more foresight will, while preparing for the transition to a new method, also think of how to make use from the left-overs of the old. A 'shock' is one image for technical change, but gradual transition is another, and to which vision we turn will depend on the application we have in mind.

Similarly for Wicksell effects. I have proposed to analyse the non-neoclassical Wicksell effect in the context of a thought experiment, which is more concrete than what the textbook offers. Petri is probably right that such an analysis cannot be found in precisely this form in the older neoclassical literature (p. 10). The instability due to the non-neoclassical Wicksell effect becomes visible only if distribution changes, hence we must distinguish different scenarios depending on the cause of the change in distribution. In my preferred scenario, distribution changes, because there is a substantial immigration into an otherwise closed economy, so that the real market wage falls. Here is the first obstacle to the realistic (or less unrealistic) representation of the neoclassical idea: money wages are based on agreements that are not easily changed in a downward direction, and Keynes taught us to recognise that this stickiness is good for the stability of capitalism, because deflations are bad. But deflations do occur and they are not always as catastrophic and as disruptive, as the deflation was in the 1930s. So, if we swallow this step and assume that real wages fall without a large effect on activity, we have to accept the consequence that the rate of profit rises and that relative normal prices change, while, according to the thought experiment of the Wicksell process, techniques do not change, because more profitable ones are not available. Since the Wicksell effect is non-neoclassical, the rise of the rate of profit is accompanied by a revaluation of the means of production such

that the intensity of capital rises, and Petri concludes that the owners of capital get richer (p. 9–10) and that "price Wicksell effects cannot be a cause of instability of the savings-investment market"; savings simply increase in proportion to the higher wealth. The rise of the rate of interest has taken place "for some accidental reason" (he means the immigration) and so, if no other factors come in like wealth effects, nothing dramatic happens. We take up these two objections one by one, the silent revaluation of capital and the denial of the instability, beginning with the latter.

Petri forgets that some exogenous shock always is necessary to test the stability of equilibrium. If we consider an ordinary diagram of supply and demand, where the slopes of the demand and the supply curve are reversed, exchange may take place, if the price is exactly at the crossing point Q, but if it is slightly displaced, one moves away from Q, and the instability is revealed. We are in the curious situation that Petri accuses me for playing down the critique of neoclassical theory, while he himself here takes out the sting of the Cambridge critique in the presence of a capital-reversal due to a non-neoclassical Wicksell effect. By contrast, we distinguish two polar possibilities. The labour force has risen from L_1 to L_2, the capital intensity $k_1 = K_1/L_1$ has risen to k_2. First case: Unemployment persists and the desired capital stock measured in terms of normal prices rises to $K_2^1 = k_2 L_1$. Then the situation is unstable because unemployment and the pressure on wages persist. This outcome is to be expected, according to neoclassical theory and according to the Keynesian marginal efficiency of capital schedule, for the rate of profit and interest has risen. Curiously, the conclusion does not hold according to Marx, who rejects any stable interest elasticity of capital (except indirectly, if finance becomes expensive, MEGA^2 II 14, p. 569). One can conceive of production of absolute (not relative!) surplus value in this situation, animated by lower wages, the higher rate of profit and the easy disposition over labour-power, until the reserve army has been reduced to a necessary minimum. Such an outcome might be envisaged *ad hoc* also by an applied economist, of course expressed in another terminology (NAIRU, etc.). Hence the second case: Desired capital might rise to $K_2^2 = k_2 L_2 > K_2^1$.

Now to the change of the form of capital. Petri assumes that the rise of the rate of interest leads to a revaluation of the means of production as assets, so that the capitalists end up owning more wealth in the form of real capital, and their gross savings would be adequate for the gross investment involved. But in what *form* do 'savings' accrue? Suppose there is only circulating capital, to keep matters simple. The transition takes place during several periods. We have abstracted from the disturbances that are very likely to happen because of the fall of wages and the rise of the rate of interest. We reckon in terms of normal prices. The production of period t will have to be sold and

wages paid, there remains the monetary value of the circulating capital goods of period t corresponding to the rate of interest of this period, r_t, and these proceeds will be lower than the total cost of the aggregate of circulating capital goods of period $t+1$, since the demand for capital rises in the second case from period to period until K_2^2 is reached; hence credit must come in to help financing the transitions, and this finance must be forthcoming, although the rate of interest has risen.

So, we either have an instability (the first case) or we must depart drastically from neoclassical assumptions (second case). Once the level of K_2^2 has been reached, proceeds will be adequate to ensure reproduction, but finance must be obtained during the necessary transition, and the collateral consists in the replacement funds corresponding to the earlier lower capital values.

This construction of what may happen because of a non-neoclassical Wicksell effect is so precarious despite the favourable assumptions we made that one will ask why the mainstream pays very little attention to this possibility. A more substantial reason than pure ignorance could be conscious negligence, because rates of profit and interest change only in a narrow range, compared to the ranges for which non-neoclassical Wicksell effects are large. Another possibility, mentioned by Petri (p. 13) – and I quite agree with him in this – is that in an economy with large unemployment and a non-neoclassical Wicksell effect growth may pick up because of the rising rate of profit. That is the scenario we associated above with Marx. My conclusion is: While there is no room for non-neoclassical Wicksell effects in pure theory, the problem will appear as less disturbing to the mainstream economist. I believe that this conclusion holds also in other cases. The neoclassical theory of the supply of capital is less than perfect, but the deficiency can be glossed over more easily – here by introducing a dose of Keynesian realism – than on the demand side. By contrast, if there is reverse capital deepening, the consequence for neoclassical theory is immediate and dramatic, as we shall see below.

Before we come to that, we consider the form of capital. Petri reminds me that Clark illustrates his example of a transition (whale oil becomes obsolete because of petroleum; the owners of the whaling ships use their accumulated replacement funds to enter manufacturing) with the assumption that the equipment of the old technique has been used up. I have already objected that selling of equipment, perhaps with losses, may also happen, if the sector, where the method of production changes, uses capital goods, which are also used in other sectors. This means that the neoclassical author discusses the change of the form of capital in a rudimentary form: ships become money via replacement funds, money (these funds) become physical assets in manufacturing. The funds are a constraint that can be relaxed because of saving (contraction) or credit (expansion). It is Petri who

neglects these changes of form and discusses the form of capital only in relation to the neoclassical construction of capital as a 'factor' in a stable production function. This is in fact problematic.

3.3 From the Wicksell effect to reverse capital deepening

Consider now, after the non-neoclassical Wicksell effect, what reverse capital deepening implies. By and large, reverse capital deepening leads to an instability analogous to that we found in the case of the non-neoclassical Wicksell effect. Mainstream authors might therefore feel as little concerned about reverse capital deepening as they do about non-neoclassical Wicksell effects. However, the possibilities of reswitching and reverse capital deepening led to more concern in the debate, and the reason is clear: these forms of technical change question the principle of substitution (D'Ippolito 1989, p. 191).

To see it, we can ask with Böhm-Bawerk (1914): What happens, if real wages are pushed up, or the related question, which is even simpler, because it leads less directly into a controversy about inflation: What, if there is an immigration and real wages fall? Then, the practical implication of the Cambridge critique is clear at once: If the change in distribution induces the adoption of known methods that are more capital-intensive in the former case and more labour-intensive in the latter, the substitutions will be stabilising, but destabilising otherwise. This was understood by a today little known Austrian economist, Arthur Salz, who ridiculed Böhm-Bawerk's suggestion to shorten the period of production in the unemployment case by saying that the solution amounted to a return to the luddites: if there is unemployment, smash the machines! That's what shortening the period of production means! None other than Schumpeter quashed this little rebellion in the camp of the Austrian economists (Salz 1905, Schumpeter 1906). The more serious Cambridge objection was: if there is reswitching, the certainty of this recipe is gone – the theory is flawed. This conclusion has been generally accepted at the level of a debate about high theory.

Hence, in the case of applied economics, the decisive importance of the question, whether capital-reversing would be frequent. Theoretical propositions are useful in applications, if they hold most of the time, like the Keynesian multiplier, which works in practice if savings propensities are stable and a number of other conditions are fulfilled. We need not discuss other applications of the substitution principle, which are similarly affected; we concentrate on the employment question. If the cases of reverse capital deepening were about as frequent as the neoclassical case, lowering of wages in the face of unemployment would make matters worse – through installation of labour-saving equip-

ment – about as often as the opposite would happen, hence policy proposals to reduce wages and to induce the use of labour-using equipment would become worthless. No such policy problem arises in the case of the non-neoclassical Wicksell effect, since the technique is given. This insight is sufficient to explain the focus on reverse capital deepening and the comparative neglect of non-neoclassical Wicksell effects in the general discussion.

I confess that, for a while, after the results found in the paper with Han, I thought that capital-reversals were so infrequent that the essential policy recommendations derived from neoclassical theory could not be refuted by means of the arguments of capital theory. This uncertainty is visible in some of the papers mentioned by Petri and explains his uneasiness. But then came the new insight: the substitution possibilities among efficient techniques (on the envelope) are few. One could argue that there will always be *inefficient* techniques that can be adopted to employ redundant labour. That is often done in the form of preserving labour-intensive techniques. The Chinese pursue such policies quite consciously in some traditional sectors and strive at the same time to adopt the most efficient techniques in modern sectors. By contrast, to rely on the disinvestment of advanced machinery for employment purposes really amounts to luddism, as Salz pointed out. This critique – which could be shared by many who felt the same without knowing Salz – may have encouraged other measures to reduce unemployment, but the critique is specific for unemployment policy and not so obviously related to other aspects of neoclassical theory. The critique based on reverse capital deepening demonstrates that pure neoclassical theory does not work, but would be effective against the mainstream only, if it could be shown to occur sufficiently often. The insight that efficient techniques on the envelope are few, by contrast, is on the contrary effective against the neoclassical principle of substitution in general and at the same time directly relevant for mainstream policies.

3.4 Some reasons why the supply side critique has not been very effective

The discussion of the form of capital has led us to a preliminary consideration of the demand side. It will be continued in the next section, where we look at the frequency of the different kinds of switches. Now back to the supply side. I must react to Petri's correct reminder that there is a problem of circularity in taking a quantity of capital in value terms as given in the determination of an equilibrium, in which the prices for the valuation of capital are derived and where prices depend on distribution, but distribution depends on the capital supply in the neoclassical framework. Petri's critique derives from

Garegnani's (1964, 1965, p. 25) early research. He distinguished the change of the form of capital in the approaches by Clark, Böhm-Bawerk and Wicksell ("viene ... preso quale dato il 'capitale' di cui la collettività dispone ..., si ammette così che esso muti 'forma' ed acquisti la struttura fisica richiesta dalle condizioni di equilibrio.") from the approach of Walras (the investment of gross savings) and Marshall (the analysis begins with the short run). The first argument is: The definition of capital is circular.

To the objection that the definition of capital is circular in neoclassical theory one often gets the reply: circular definitions can be made rigorous by means of simultaneous equations. I concur with Petri that this is not possible in the case of the old neoclassical equilibrium.

Often, circular definitions do not present a problem, the definition of labour values is an example. If, having explained the concepts of commodity, of homogeneous labour and single product system, I say: the labour value of a commodity is the direct and indirect labour entering its production and point to the equation ($\mathbf{A}$ input-output matrix, $\mathbf{l}$ labour vector, $\mathbf{u}$ vector of labour values):

$$\mathbf{u} = \mathbf{l} + \mathbf{Au},$$

the definition is circular and given by means of simultaneous equations, but it is clear since $\mathbf{l}$ is direct and $\mathbf{Au}$ by definition indirect labour. The subsystems approach allows to transform the implicit definition into an explicit one – a rather trivial operation, from the mathematical point of view.

The same does not hold in the case of the old neoclassical equilibrium. If I may refer to my own presentation – but Petri (2004) has dealt with the subject earlier and more extensively – where "the odd peculiarity ... consists in the assumption of an arbitrary amount of capital K in terms of numéraire, given prior to the determination of prices and of its purchasing power" (Schefold 2016a, p. 29). It is also arbitrarily assumed that the system reproduces itself in constant proportions (equation 15.11, p. 20). Without these assumptions, a long-period neoclassical equilibrium with a uniform rate of profit and distribution regulated by supply and demand does not exist.

There is one exception, however. An equilibrium with a uniform rate of profit can be reached as a terminal state, even if endowments are given in the form of real capital goods. A formal solution has been proposed in the form of turnpike theorems in intertemporal general equilibrium with recursive utility functions: the economy starts from arbitrary endowments and tends (because it is globally stable) with shifting proportions to a terminal equilibrium, in which the standard of life corresponds to what the consumers can desire,

given technology and given that, as workers, they balance work effort against leisure, and as savers, impatience against the expectation of future consumption. All agents discount the future in the terminal state at the same rate, equal to the uniform rate of interest. The capital stock and its value then is a result, not a given (cf. my discussion of Epstein's work in Schefold (1997, p. 431)). It is remarkable that Epstein found it necessary to exclude capital reversals explicitly. Such an assumption is, despite Garegnani, not necessary, if there is only one consumer (Schefold 2008). Petri discusses the problem of foresight in intertemporal models in his paper. I concur with him that it is a problematic idea. But one must realise that modern neoclassicals have, in classical terminology, a model of gravitation of short-run prices towards a state with a uniform rate of profit, which is, of course, special because of the assumptions of perfect foresight, full employment, etc. I do not wish to discuss this further here.

Petri cannot accuse me of having neglected the supply side problem of capital, as far as the old neoclassical equilibrium is concerned. Now to the surrogate production function. Samuelson (1962) showed that, if wage curves are linear and sufficiently numerous, their envelope will be negatively sloped. If the absolute value of the slope at each r is defined to be equal to $k(r)$ and output per head y at the intercept with the ordinate to $y(r)$, one obtains a parametric representation of a production function $y = k(r)$, as demonstrated in Schefold (1989, p. 297-8). It has been objected by Salvadori and Steedman (1988) that the linear wage curves presuppose that the labour theory of value holds for each technique, if the techniques are Sraffa systems with several commodities. The intersections of the wage curves are then not switch-points, for the methods of production will be different in all industries so that the intersection will be dominated by combinations of of the methods of these techniques, and wage curves will correspond to these combinations that are not linear; hence the construction of the surrogate production function is inconsistent.

Petri has overlooked that the problematic is different in the case of random systems. They form 'families' (Schefold 2010) in that, if the distributions of the coefficients are i.i.d., this will be true for all combinations of such processes, and we may assume that these combinations are also productive. If the assumption is added that the labour vectors stand in a random relationship to the matrices as in Schefold (2013a) – these are strong assumptions, of course – one gets an approximate surrogate production function, as explained in that paper. The value of capital, K, will be equal to k, multiplied by employment, and this will be, at each rate of profit, equal to the value of the circulating capital goods measured in terms of the corresponding long-run prices. Hence one gets the famous capital 'jelly' K, discussed at length in Harcourt (1972). I concur with Petri that this does not solve the supply problem from the pure theoretical point of view. The

jelly capital is unsatisfactory not only because we are dealing with approximations, but because the supply is equal to demand only in equilibrium, while 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.

I am interested in the construction of the surrogate production function all the same, because it helps to understand the economics of the majority of my colleagues, who are neither all stupid nor so naive as to never have taken notice of the capital critique. Why do they consider the production function a useful tool, while I doubt it? They believe that the aggregation to reach jelly capital must be possible, reswitching etc. being regarded as exceptional, and they must also have an idea of how the concept of jelly capital might be used in applied economics (see e.g. the quotes from Solow in Harcourt (1972)).

What is that capital which is taken to be constant, when one wants to measure the marginal product of labour? This is a quite natural question asked by Joan Robinson about the use made of the macroeconomic production function. It is supposed to be, together with labour, the scarce mean to be put to alternative uses. It can be defined rigorously only in a one-commodity world. To extend the notion to a real economy means to introduce a fiction. James Meade called this mass 'leets' – the anagram of 'steel', in order to signal that he knew what he was doing, when he used the production function (Harcourt 1972). The procedure is impossible in pure theory, where the task is set to determine output and distribution in one period, taking as given only the techniques and the available endowment, but not the expectations formed in the past, for it is part of the conception of rationality in neoclassical theory that bygones are bygones. The problem is not so much to explain why this conception is neither rigorous nor comprehensive, but why so many intelligent economists of diverse political orientations use it all the same. The tradition of old neoclassical theory certainly plays a role. This is a partial explanation, which Petri is right to emphasize.

A complementary explanation is that mainstream economists see the economy in a sequence of transitions. The capital stock of today is inherited from yesterday, valued at yesterday's prices; it is the collateral needed to invest for tomorrow, and the prices of tomorrow are not likely to be all that much different from today. The 'supply' of capital then is the existing stock, valued at current prices. This is only by coincidence consistent with the demand for capital derived from the surrogate production function at some desired level of output and at a given distribution, that is, with the demand corresponding to a point on the isoquant. The practical problems of the measurement of the capital stock are so large, however, that they tend to conceal the theoretical inconsistency: the lack of a theory to determine the supply prior to equilibrium. But the problem disappears in a

steady state, in a golden age, as Joan Robinson would have said.

The stock represents a capacity today which constrains the possibility of construction for tomorrow. In the sequential approach, in which capital changes form times and again, as in Marx, a supply is recreated, which is adequate for the demand. The surrogate production function follows in principle from the available techniques. This construction is approximately correct, if randomness prevails. Given factor prices, the function defines a demand for jelly capital K. The supply consists in the available equipment, valued at empirical prices. A supply curve cannot be constructed, but the lack of it is unimportant in a steady state and is forgotten because of the practical measurement problems, if a new steady state or equilibrium is to be determined. The magic word then is 'capacity'.

Capacity constraints are variable, of course, as Petri emphasizes, especially from the technical point of view, but more important are the institutional conditions, on which the realisation of this technical variability depends. Their stickiness may be justified just as much as the stickiness of wages, and for similar reasons; they help to stabilise the economy. It follows for me – and this is central for our discussion – that, as far as mainstream economics is concerned, here to be represented by Samuelson, the crucial critique of the use of the production function revolves around what Petri calls the 'demand side', since pragmatic contextual justifications for taking the supply as given exist precisely because practical measurement is difficult and 'capacity' is variable for complex reasons.

Petri refers to the fact that most firms are not supply constrained in the sense that they could expand output without a significant increase of unit cost – if fixed costs are taken into account, unit costs even diminish. As Petri points out, it means that the marginal product of labour tends to rise rather than to fall in the short run. But the variability of capacity is common knowledge among applied economists and a basis for considering whether the advantage of a stimulus of demand would outweigh the disadvantages. The rising marginal product used to be discussed as Okun's law. Some economists believe that Okun's law and policies relying on it can easily be extended to the long run, but others doubt it and fear that inflationary pressures might develop. I would side with the former opinion on many occasions, but it would be naive not to recognise that the letter opinion can be supported by reference to historical precedents. A Keynesian theory of effective demand without a Keynesian theory of inflation is dangerous. The 'scarcity' of 'capital', which neoclassicals try to express in the reified form of 'K', is not only a fantasy, but results from a complex of social and economic forces. Garegnani and his school have tried to assess the potential to overcome capacity constraints, but, as far as I can judge, they are still far from taking the social forces (which they mention) and the monetary factors adequately into account. Meanwhile, the mainstream discusses the

problems in many ways. The danger came into focus that a brisk expansion of capacity may be accompanied by risky private financing, ultimately leading into a Minsky crisis, or by state financing that is not sustainable, if interest rates rise. I concur with Petri that the financial constraints should not be confused with physical scarcities. This implies that the analysis of the forms of capital should include its monetary forms.

We do not have to elaborate further that the idea of a stable normal utilisation of capacity is not a foundation of capital as a single factor of production in the sense of rigorous theory, and it is an ambiguous notion even if taken only as a constraint on production and growth, because the constraint only looks technical, while it is really institutional, and because it mixes supply and demand elements, for 'normality' often refers not only to normal supply by production with given equipment, but also to average demand conditions over time. One can try to convince economists to abandon a notion, which is so ambiguous. However, the likely response will be that some such concept was needed, and be it only as a didactic tool, while a certain elasticity could be conceded. I conclude that this supply side critique is not very effective, given the reasoning of mainstream economics and the fact that this discussion is older than the Cambridge debate. In particular, I am referring to Keynes's idea that the old (neoclassical) theory would come back in a state of full employment, which was contested soon afterwards. True progress is slow in this domain.

4 Capital reversals

4.1 Alleged and real shortcomings of input-output analysis

We now turn to Petri's section on the demand for capital. He believes that input-output tables are not "capable of answering questions on the curvature of wage curves" (p. 16). First doubts concern the data, but he does not present an empirical analysis to show that specific conclusions are invalidated, and he does not appreciate the fact that the number of data is very large so that conclusions based on averages might be valid despite the uncertainty regarding single observations. He observes that alternative techniques do not only consist of the methods used by other countries, but that firms may also have their own plans for future production – the 'books of blue-prints', as Joan Robinson would say. This is certainly true, but Petri does not even produce a scrap of evidence that qualitatively different results would be obtained with still larger data sets. He mentions that he began to object to my new approach ten years ago. So he would have had some time to produce empirical evidence for his claims. By the way (p. 17): Han and Schefold

(2006) did not find 4% cases of macroeconomically relevant reverse capital deepening, but less than two: the majority of the non-neoclassical switch-points concerned a sectoral effect: an industry employed less labour with a falling wage rate, but the intensity of capital in the economy as a whole fell nonetheless.

He criticizes the degree of aggregation, which admittedly is high relative to individual commodities. I conclude that it would be desirable to repeat the investigation at a lower level of aggregation, but I doubt that a higher variability of relative prices would be observed in a basic system. It is true that the "'organic composition' of a sector is an average of those of the single goods produced by the sector" (p. 17), but the conclusion that relative prices will depend less on distribution in more aggregated systems for this reason is not true in a *basic* system, for interdependence dampens fluctuations even without aggregation – compare my finding that Sraffa's reduction of prices to dated quantities of labour vastly exaggerates relative price movements, because it does not take this effect into account, see Schefold (2021b).

Other objections formulate correct observations, but are beside the point. For instance, the coexistence of more and less efficient firms in any industry means that the analysis is more concerned with socially necessary than with dominant techniques and is therefore closer to the classical (Marx) than the neoclassical tradition. It is true that the classification of sectors follows international conventions and does not do justice to local product variations (agriculture in cold and warm climates, in Petri's example) and he also mentions that processes are therefore "more often than not" (p. 8) not easily transferable from one country to another. Such objections have been discussed in my papers time and again. Although they are correct, I do not regard them as pertinent for the final result. To repeat the point: if only one in one hundred techniques is transferable, if there are 10 countries and 100 industries, the set of potential combinations reduces from 10^{100} to 10^{98} – still more than the number of elementary particles in the known universe. Some things certainly are transferable such as the cellphone in your pocket. What I most regret in this discussion is that no effort is made to show that the results change significantly if at least some of the deficiencies of the existing empirical analyses are removed. That would render the discussion more constructive.

Zambelli has made a valiant effort in the right direction and enriched the empirical work with his algorithm (Zambelli, Fredholm and Venkatachalam 2017). I do not concur with the interpretation of part of his results, where he attacks the neoclassical production function (Zambelli 2018), but there is no room to take discussion of his approach up here. Jakob Kalb (2022) has shown that Zambelli's findings of numerous capital reversals are largely driven by non-neoclassical Wicksell effects. Out of a total of 1213 switch-points,

only 14 display reverse capital deepening, which confirms the results of Han and Schefold (2006) – the percentage is even smaller. Zambelli does not really show what he claims to have shown. Anwar Shaikh and his school – I hope Theodore Mariolis, Lefteris Tsoulfidis and Luis Daniel Torres Gonzalez do not mind if I locate their work in this group – have produced 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, see e.g. Bienenfeld (1988), Tsoulfidis and Mariolis (2007), and Iliadi et al. (2014). Moreover, the same holds – but not to the same extent – for wage curves. It is time to at least provisionally accept those findings and to see how they might be explained.

4.2 Randomness

One candidate for an explanation of quasi-linearity is randomness. The introduction of random matrices is relatively recent even in the sciences. In economics and in the theory of capital in particular, the discussion started a little earlier than ten years ago. Many questions surrounding this notion arose, and Petri poses some of them in his paper. However, instead of contributing to the attempts of several authors to answer them, he only voices doubts. It was normal scientific procedure first to investigate what would happen, if randomness of matrices in economics corresponded to the assumptions in the most advanced mathematical treatment of the matter, the assumptions of the Goldberg-Neumann (2003) theorem. This led into new and foreign territory. Some strange phenomena now seemed closer to an explanation than before – notably the quasi-linearity of prices. But how should one deal with the zeroes in input-output tables? The Goldberg-Neumann approach admits that up to half the coefficients of the input matrix can be zeroes. Older approaches to random matrices had postulated positive matrices. Still poorly understood is the fact that the non-dominant eigenvalues of empirical input-output matrices do not all tend to zero as they should according to the theorem on random matrices – a handful of the non-dominant eigenvalues seem to be significantly different from zero, but many are close to zero and no non-dominant eigenvalue is close to the Frobenius eigenvalue.

Is randomness a matter of degree? On the one hand, empirical distributions of eigenvalues look almost as if the matrices were random. On the other hand, it is well established that empirical input-output tables do not have eigenvalues that are all approximately equal or are clustered around an average that would be significantly different from zero. Why not? Would the principle of insufficient reason, applied to the spectrum of eigenvalues, not lead one to expect just that? If Petri does not believe in a tendency of non-dominant eigenvalues to tend to zero, scientific methodology demands that he should

investigate the contrary, beginning with the simplest case: what happens, if all eigenvalues are equal and positive? The matrices then are imprimitive. I have not seen Petri analysing imprimitive matrices, but I did it fifteen years ago (Schefold, 2008). It turns out that it is then easy to construct systems with wage curves of more drastic curvature than any encountered in empirical investigations. This is an argument for randomness *e contrario*.

So it seems necessary to develop a theory of near random matrices and we can be happy to have an exciting new field of investigation; we should take it up, without excessive anxiety that older results might have to be modified or even overturned.

The rank of a matrix equals $n - m$, where n is the order of the matrix and m is the number of eigenvalues equal to zero. Typically, no eigenvalue of an input-output matrix is exactly equal to zero, but if m of them are small enough in modulus to be regarded as equal to zero, it means that not n, but only $n - m$ industries are really independent. What does it mean for the structure of the economy? I asked this question in several of the papers. Instead of helping to develop new aspects of the theory, Petri prefers to insist on the old. He turns to reswitching and remarks that aggregation "certainly eliminates instances of reswitching" (p. 20). The point is indeed obvious: if one aggregates down to a one-sector model, reswitching will have disappeared. He seems to regard that as an argument against me, but it is an argument against the importance ascribed to reswitching in the capital theory debate. Reswitching – here better reverse capital deepening – was and is an argument against neoclassical theory, for the phenomenon illustrates how a change in distribution can jeopardise the essential macroeconomic stability mechanism. However, if the phenomenon depends on the state of aggregation, the critique loses some force, as it then seems to depend on an arbitrary level of aggregation or disaggregation. Moreover, reswitching or reverse capital deepening always takes place in one sector so that the increase of the capital-labour ratio will normally be largest in that sector, even if it is not confined to it because of indirect price effects in basic systems. In this sense, reswitching and reverse capital deepening are sectoral phenomena, but they are invoked because of their importance in macroeconomics. This consideration confirms that reverse capital deepening is important in a large economy only if it occurs frequently and the effects are of significant size.

4.3 The probability of reswitching

Hence the importance of assessing the likelihood of reswitching, Petri's paper of 2011 lists about ten papers concerned with the issue. Two are multi-sectoral models (D'Ippolito

1989), summarised by Petri himself, and Schefold (1976). The others are concerned with two or three sector models. They have in common that they determine the economically meaningful range (say G_0) of the relevant parameters and the narrower range $G_1 \subset G_0$ of the parameters, which give rise to reswitching; the ratio $\mu(G_0)/\mu(G_1)$ then is an expression for the probability of reswitching, where μ is a measure (usually the euclidean measure of the surface). Most authors (and so does Petri in the paper under discussion) raise doubts as to the suitability of the euclidean measure – is the occurrence of reswitching in this corner not more plausible than in that other corner? – but this is a banality. The calculation of the probability for the occurrence of an event will always get more accurate, if we can use more information; the point is to capture it. Two contributions stand out in this regard: Steedman (2011), who argues that the estimates might be better, if we could treat commodities as not infinitely divisible, but this naturalism does not really help, and Salvadori (2000), who observes that the problem of the choice of the measure would be of lesser importance if the probabilities were either one or zero. To this we shall return.

Three types of models are considered: Austrian, as in the example of wine and oak chest in Sraffa (1960) and as in Samuelson's 'summing-up' in the reswitching debate, used by Hicks (1973), Laing (1991) and others, models of what Petri calls the Samuelson-Garegnani type and basic Sraffa models of low dimension. The latter need no explanation, the Austrian model is left out here, because there is no basic commodity (wine and oak chest are non-basics in Sraffa, even if he grafts their analysis on a basic system represented by its wage curve), but Samuelson's model is very peculiar – Garegnani (1970) was important as its critic, not as its creator. A technique here is given by a capital good which reproduces itself, and part of the product is used to produce a consumption good. Both sectors use labour. The consumption good is the same for all techniques, it is wheat, say, but the capital good is specific for each technique: e.g. in the first technique oxen (I), in the second horses (II), in the third tractors (III); eventually, a continuum of techniques and goods is assumed. This expresses asset-specificity as emphasized by Petri. Country (I) uses oxen, country (III) tractors, but how is substitution possible under such circumstances? It is an understatement to say that 'transitions' cannot be modelled in this case. It would be a *creatio ex nihilo* to go from (I) to (III): there must suddenly be tractors so that tractors can reproduce. Mainwaring and Steedman (2000), in a paper which is clearly important within this literature, conclude from a Sraffa model that the likelihood of reswitching is small (a few percent), while Petri (2011), in a paper that is also important, affirms for the Samuelson model that the likelihood is much higher (perhaps of the order of magnitude of 50%), and he ascribes this to the fact that here all the capital is different after a substitution. "In real economies it very seldom happens that different

methods of production of a commodity do not require different intermediate goods or machines" (Petri 2011, p. 402). He observes that such intermediate goods find no room in the Mainwaring-Steedman model and concludes that the introduction of intermediate goods makes all the difference between low and high probabilities for reswitching and reverse capital deepening.

Again he is trapped because he works with models of low dimension. According to Schumpeter (who in this expresses the classical vision), technical change is the introduction of new *combinations*, possibly to construct intermediate goods that can help to produce new final goods. The intermediate goods (machines or chemicals, say) are produced in new combinations from parts or substances available in some out of many markets in a large economy. These parts and substances are put to other uses in existing other sectors. A first framework to model such changes of technique is Sraffa's theory of fixed capital as a joint product in a many sector model. It was shown in the first mathematical formalisation of this theory in Schefold (1971) that if one takes a series of periods of production sufficiently long so that production starts with new final goods in the first period and that all intermediate goods are used up in the last, one is confronted with a single product system. Take a spade as an example: the blade is replaced every eight months, the stick every six months. If one starts with a new spade at the beginning of this year, one will work with a spade made of a new blade and a new stick after two years, and this spade after two years will, economically speaking, be a new spade, while the spade after seven months is an old spade with a new stick. Suppose that there are also wooden spades, which are more labour-intensive to make and which are used at low wage rates. As wages rise, there will be a switch from wooden spades to iron spades. It involves a change in the method of production of spades. It uses a new intermediate good, the blade, which is produced within the spade-producing industry from a metal sheet, which is an existing product of the iron industry. It had previously been sold to the chariot industry; its market now expands. New intermediate goods can thus be accommodated in the Sraffa system. We shall come back to the problem of formalising time in Section 5.1 (we transgress the single product model here by introducing a production period of two years). At the level of the final goods, however, technical change remains piecemeal. The Samuelson model cannot account for such subtleties. I am surprised that Petri praises it, while Garegnani used it for critical purposes.

4.4 Reswitching and non-basics

A broader framework than the theory of fixed capital is provided by the theory of joint production. Schefold (1989) distinguishes, besides switches of methods of production, truncation (a commodity and a process disappear from the system), dual truncation and commodity switches. Schefold (1997, pp. 291-358) discusses changes in the composition of output, overdetermination as a form of competition and underdetermination (when do overproduced and free byproducts become commodities?). So there is a framework to discuss new commodities in Sraffa – Petri himself uses it in Petri (2021a). It would be interesting to extend the theory of the probability of reswitching to joint production, but it would be analytically difficult and it might turn out to be not fruitful, since changes in the composition of output would have small effects on what one is interested in: the macroeconomic capital-labour ratio. Sraffa himself, having proposed the analysis of joint production, returned to single production in the last part of his book. Someone younger will perhaps take up this task.

Petri emphasizes the role of non-basics. I may point out that non-basics can be shown to be limit cases of basics in the mathematical treatment (Schefold 2022a). But he also has conceptual objections; let us deal with them first. He writes "The aggregation in I-O tables mixes basics and non-basics" (p. 22). This sounds as if the property of a commodity to be basic or non-basic was natural; a property of the object, i.e. independent of the analytical treatment and the context. But this is not true. If ice-cream is delivered to hotels and hotels are necessary for the administration of industry, because managers must travel, ice-cream becomes basic. By contrast, if services are regarded as a kind of consumption – some classical authors and Marx would have spoken of unproductive labour – hotels and ice-cream become non-basics. The expenditure of the managers for the hotels would have to be booked as their personal consumption and the corresponding payments by their firms as an addition to their wages. Such ambiguities are avoided by assumption in Sraffa's protoeconomics with vivid examples such as ostriches and ostrich eggs (see Schefold 2012, p. 843 for a possible background to this image). The implied consumption good consists of ostrich-feathers, worn by ladies on their hats around 1900, and no one dares to doubt that such decoration is non-basic.

Applied economics must distinguish according to the relationships that are empirically relevant. There is some awareness of the problematic in Petri, when he distinguishes "pure consumption goods" and admits that "some consumption goods are basic (electricity, petrol)" (p. 22), but the classification remains naturalist. Insofar as 'Production of Commodities' is an ideal type, it is, like all ideal types, full of counterfactuals. He forgets that the distinction in Sraffa is only in first approximation made to separate commodities

that enter as a cost into the production of others from commodities that are a cost to consumers. In second approximation, the 'demand side' also plays a role in the context of joint production and Sraffa uses the incidence of potential indirect taxation for his distinction and classes land in the case of intensive rent as a non-basic, although no production on a no-rent land appears. Petri's example leads to further differentiation in applied economics. Electricity delivered to consumers may be called non-basic, electricity delivered to firms may be called basic; the tariff is differentiated accordingly. However, electricity also is priced on the basis of other criteria, such as time of delivery and voltage. In the end, for the purposes of a theory of capital, which serves macroeconomics, one will aggregate electricity, calculate an average price for it, and this will then be looked at as a basic commodity.

We agree that paradoxes of capital are easier to generate, if one models non-basics, and a careful reading of Sraffa's 'Reduction to dated quantities of labour' will confirm it, but the conclusions for applied economics, which is concerned with capital as basics, will not be significant. Indeed, it is easy to use non-basics to construct an example that contradicts the main result of Schefold (2022a), that the probability of reswitching tends to zero, if the number of sectors tends to infinity. Recipe: take a corn-model and assume that there is in addition an infinity of pairs of non-basics analogous to wine and oak-chest in Sraffa's example. Even if the probability for getting reswitching for one of the Austrian pairs is small, the probability that reswitching occurs at least once will grow to certainty, if the number of such pairs is sufficiently large. The reader of Schefold (2022a) will easily discover which assumptions rule out such a parody of capital theory there. The example is discussed in a less provocative manner in that paper, which also discusses why the result is quite different for basics. For a preview, see Section 5.1.

Petri surmises ('concludes', p. 23) that my "claim of an extremely low likelihood of empirical occurrences of reswitching and reverse capital deepening" derives from four factors:

- My erroneous identification of empirical input-output tables with "the theoretical matrices that a rigorous discussion would need" (p. 23). Politeness prevents me from answering with the sarcasm that Sraffa used when he answered Robertson, who seemed to give precedence to inherited theory over the facts.

- My erroneous identification of input-output matrices with random matrices. Well, how does he explain that the majority of eigenvalues are small? Actual rates of profit are not uniform either, and yet we use a uniform rate of profit in our equations most of the time.

- My erroneous treatment of all commodities as basic, which "is far from true" (p.

23). – If non-basics and reswitching can so easily be made to disappear by means of aggregation, they are not really important in a critique that ultimately concerns macroeconomics.

- My treatment of "technical change at a switch" as "piecemeal, which is false"(p. 23). No, it is a mathematical truism, given my definitions. Petri is free to propose and construct another theory. I for one believe that the Samuelson model is inadequate. I have suggested how the emergence of new commodities might be incorporated in Sraffa's theory, recalling what has already been done in the theory of joint production (Section 4.3).

4.5 Random systems and their determinate counterparts

This would be the place to take up the mathematical analysis of reswitching, but Petri turns to the approximate surrogate production function, now from the demand side (the supply side was dealt with in the previous section), and we must follow suit. This construct seems to me less interesting now after the investigation with Kersting than it appeared about ten years ago, when it was conceived. The hypothetical research question was: How far does one get in the construction of a surrogate production function, if one adopts the assumption that matrices are random and the labour vector stands in that particular random relationship to the input-output matrix which is expressed in the covariance condition $cov(\mathbf{m}, \mathbf{v}) = 0$, which the reader, it is hoped, will remember – there is no room to explain it here.

The assumptions are admittedly drastic – I was myself the first, for instance, to point out that the eigenvalues do not all go to zero for actual input-output matrices. It was like an invitation to a dinner with exotic food which is not suitable as a diet for every day. Petri declines the invitation, so I must dine alone. Nonetheless, I want to explain again why I appreciate the menu.

His criticism consists in the observation: 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" (p. 24); both are positive and $\mathbf{f}$ must be the same for all techniques. He finds the postulate that $\mathbf{f}$ is the same for all techniques "breathtaking" and the construction "totally illegitimate" (p. 24). If the matrix is random in the sense of Goldberg and Neumann (2003), the distribution of the coefficients on the rows must be i.i.d. and $\mathbf{f} = \mathbf{e} = (1, \ldots, 1)$. However, Petri forgets to add what is most important: these properties must hold only *on average*, so that, for instance, many zero coefficients are possible and the actual compositions of capital are all different. Randomness of an input matrix $\bar{\mathbf{A}}$ does *not* mean that the coefficients must be "very close" to $\mathbf{A} = \mathbf{ce}$ in the sense of $|\bar{a}_{ij} - a_{ij}| < \epsilon$ for $\epsilon > 0$ and

all i, j. To make this understood, consider the random matrices $\bar{\mathbf{A}}$, for which **ce** is the determinate counterpart. This set consists of all matrices with semi-positive rows $\mathbf{a}_i$ such that the a_{ij} are i.i.d. and have mean c_i. Hence the a_{ij} can be almost anything, provided they collectively fulfil this condition. Similarly, the vector **l** can vary a great deal, given **c**, provided the covariance condition is fulfilled, see Schefold (2022a,b) for more details.

Petri should compare the construct of the approximate aggregate production function not only with reality but also with the usual neoclassical rationalisation of the production function by means of one-good economies or Samuelson's 1962 model. Compared to that standard, the approach is more sophisticated and, in my eyes, less abstruse than I had once thought, while Petri finds it unpalatable, because even the suggestion that production might have a random character does not appeal to him. It is curious that he, as an expert of pure theory, should not look at the approximate surrogate production function in a different perspective. It had been thought that the surrogate production function existed only in one-commodity worlds. It has now been shown that it also exists, if the systems are random, in pure theory. This discovery remains, even if the assumptions are considered as unrealistic (Schefold) or very unrealistic (Petri), and even if a new restriction on the possibility of substitution has now been found in Kersting and Schefold (2021).

The admittedly provocative assumption that the compositions of capital are – apart from possibly large perturbations – the same in all industries would be confirmed empirically, if all non-dominant eigenvalues were almost equal to zero, for that would mean that the matrix would – apart from perturbations – be of rank one. In actual fact, most non-dominant eigenvalues are small, but a few are not, which suggests that the structure of actual economies, as represented by input-output analysis, can be explained as the superposition of a few fundamental compositions of capital, disturbed by randomness. Petri's observation that not all eigenvalues tend to zero does not reveal an absurdity, but leads to an exciting problem for future research. The great Einstein disliked quantum mechanics, which he had helped to create, and to which he dedicated much more time than to relativity theory, according to his own estimate, but he would insist: 'Gott würfelt nicht' ('God does not play dice'). One hundred years later, quantum mechanics is the empirically best established theory of physics.

Petri formulates at the end of this section: "Anyway, ... the possibility to construct an approximate aggregate production function is not sufficient at all for the validity of the traditional marginalist picture..." True, and I never pretended anything else, if 'traditional' is his word for the 'pure' theory, but to conclude the debate like this is an expression of Petri's reluctance to incorporate new insights in his *Weltbild* (image of the world/ framework of ideas), which, like the ancient Persian religion, seems to know only the Good and the Bad.

5 From the rarity of reswitching to the essentially unique efficient capital-output ratio

5.1 More on reswitching: the mathematical analysis

I limit myself to only a small number of comments on Petri's remarks about the probability of reswitchung, since I have written a new paper about the matter, with results that go beyond those affirmed in 2016 (Schefold 2022a). I shall soon invite my critic to react to the five theorems now obtained, by which I hope to have explained the rarity of reswitching and reverse capital deepening, which I regard as an empirical fact, now supported by new empirical material, as announced above. These empirical results again make use of input-output tables, against which Petri has expressed his reservations. But the theory as such is independent from this underpinning.

The Appendix of the paper of 2016 proposed to analyse reswitching by the same method I already used in 1976 to prove that reswitching was not based on fluke cases but had a positive probability for any given regular system. Petri summarises the method for two-sector models. The probability for reswitching is supposed to be captured by comparing the sets of conceivable (semi-positive) methods of production, that have a switch with methods actually employed in one (to simplify: the first) sector at one rate of profit with the set of methods of production that would lead to a switch at another rate of profit. The intersection of the sets, relative to the first set, is a measure of the probability that actual techniques have a switch at the first rate of profit and a reswitch at the second. This probability will be zero because the intersection of the sets (which are simplices) will be of lower dimension than the sets themselves, but if the rate of profit of the second switch is varied, the intersection will cover a region of the first set, in the case of regular systems, but not in the case of the labour theory of value when relative prices are constant. So, in the regular case, the probability is certain to be positive. So much for the paper of 1976.

Since the investigation with Han, which had been undertaken to prove that reswitching was frequent, had led to the contrary result, and since examples for reswitching could be constructed for two and three sector models, I tried to prove that the probability, as I had defined it, would tend to zero for larger and larger matrices. The basic idea was simple. The set of potential reswitch-points could be shifted within the first simplex so that it fitted a transformed set, contained in a smaller simplex within the first, the edges i of which would be shorter than those of the first by a factor γ_i, $0 < \gamma_i \leq 1$. The probability as defined would then at most be equal to the ratio $\gamma_1 \cdot \ldots \cdot \gamma_n$ of the volume of the smaller

simplex, divided by that of the first, and this product would tend to zero with $n \to \infty$, if an infinity of the γ_i was smaller than one, and this I *assumed* explicitly (Schefold 2016, pp. 51-52), justifying the procedure with intuitive arguments. I called it a 'boundedness condition' in Schefold (2017a, p. 183, p. 187).

Petri challenges the result. Could not the γ_i tend to one? Well, of course they could! Otherwise, the boundedness condition would not have been needed. The paper gave theoretical reasons for the observed rarity of reswitching by providing a proof, given boundedness, and by arguing that boundedness was plausible, but it did not characterise a general set of systems, for which the reswitching probability would necessarily tend to zero.

Rigorously speaking, this is still the situation in principle. However, the new paper (Schefold 2022a), soon available for discussion, contains more: A geometrical analysis of the sets that have to be measured in order to obtain the probability, and arguments from the theory of prices to show how the sets move in function of the rate of profit; then five theorems are offered, each formulating sufficient conditions under which the probability of reswitching becomes small, as n increases. The sufficient conditions, taken together, cover a broad range of cases so that I feel that the rarity of reswitching has been explained for large systems quite comprehensively. We illustrate what can be proved as briefly as possible, beginning with the formulation of the problem that had been used in Schefold (1976), in the usual notation. $(\mathbf{A}, \mathbf{l})$ is a regular system, productive and basic; $\hat{\mathbf{p}}$ are prices in terms of the wage rate. $M(r) = \{(\mathbf{a}_0, l_0) \geq 0 \,|\, (1+r)\mathbf{a}_0\hat{\mathbf{p}} + l_0 = \hat{p}_1\}$ is the simplex of all potential methods of production $(\mathbf{a}_0, l_0)$, which are as profitable as the actual method of production $(\mathbf{a}_1, l_1)$ at rate of profit r. $M(r_1) \cap M(r_2)$ is the set of methods that have a switch with $(\mathbf{a}_1, l_1)$ both at r_1 and r_2. Assume $r_1 = 0$ for simplicity. $M^\star = \bigcup_{0<r<R} M(0) \cap M(r)$ is the set of methods that engender a switch at $r = 0$ and at some $r > 0$; it was shown in Schefold (1976) that $M^\star$ contains an open neighbourhood, hence the probability of reswitching, defined as $\mu(M^\star)/\mu(M(0))$, μ the euclidean measure, is not zero, if the system is regular. $M(r)$ is spanned by its $n+1$ vertices $z_i\mathbf{e}_i$, $\mathbf{e}_i$ unit vectors; $i = 1, \ldots, n+1$; and $z_i(r)$ are the coordinates of the vertices in dimension i. The definition of $M(r)$ yields:

$$z_1(r) = \frac{1}{1+r},\ z_i(r) = \frac{\hat{p}_1}{(1+r)\hat{p}_i},\ z_{n+1} = \hat{p}_1.$$

Now consider $M(0)$. Its vertices are denoted by $P_i = z_i(0)\mathbf{e}_i$. The vector pointing from P_{n+1} to P_i is denoted by $\mathbf{f}_i; i = 1, \ldots, n$. P_{n+1} is the tip of simplex $M(0)$; the simplex B, spanned by $\mathbf{f}_1, \ldots, \mathbf{f}_n$, is its basis. The edges of $M(0)$ are line segments represented

by the $\mathbf{f}_i$. We draw each of them in the hyperplane with the dimensions i and $n+1$. The edges of $M(r)$; $0 < r \leq R$; can be drawn in the same hyperplanes (see Figure 1) with the coordinates x_i, $i = 1, \ldots, n$, and with y for coordinate $n+1$. The intersections

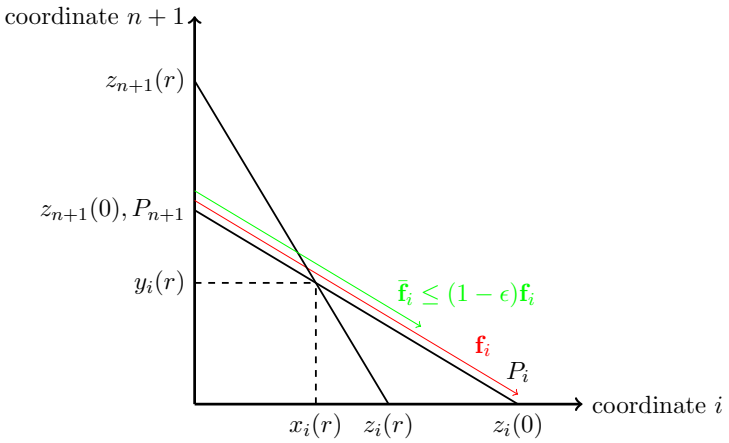

Figure 1: The edges of $M(0)$ and $M(r)$ in the hyperplane with coordinates i and $n+1$ and their intersection in this hyperplane as a function of r.

of the edges with coordinates i and $n+1$ are at $z_i(r)$ and $z_{n+1}(r)$, and the intersection of the edges of $M(0)$ and of the edge of $M(r)$ in plane $i, n+1$ is denoted by $x_i(r)$ and $y_i(r)$. The $x_i(r)$ stand therefore for points on the coordinate axis i. These coordinate axes are different for different i, while the y_i are all on coordinate $n+1$; $i = 1, \ldots, n$. The intersections of the edges follow from the linear equations

$$\frac{x_i(r)}{z_i(0)} + \frac{y_i(r)}{z_{n+1}(0)} = 1,$$

$$\frac{x_i(r)}{z_i(r)} + \frac{y_i(r)}{z_{n+1}(r)} = 1.$$

Inserting $z_i(r) = \frac{\hat{p}_1}{(1+r)\hat{p}_i}$ and $z_{n+1} = \hat{p}_1$, we get equations with prices as parameters

$$x_i(r)\hat{p}_i(0) + y_i(r) = \hat{p}_1(0),$$

$$x_1(r)(1+r)\hat{p}_i(r) + y_i(r) = \hat{p}_1(r).$$

These equations can also be derived directly from the definitions of $M(0)$ and $M(r)$. They

yield the solutions

$$x_i(r) = \frac{\hat{p}_1(r) - \hat{p}_1(0)}{(1+r)\hat{p}_1(r) - \hat{p}_1(0)},$$

$$y_i(r) = \frac{(1+r)\hat{p}_i(r)\hat{p}_1(0) - \hat{p}_i(0)\hat{p}_1(r)}{(1+r)\hat{p}_1(r) - \hat{p}_1(0)}.$$

As r moves from 0 to R, the intersections $(x_i(r), y_i(r))$ move on edge i of $M(0)$ and form one connected (because of continuity) line stretch f_i which will, however, move out of the positive orthant $\mathbb{R}^{n+1}_+$, if y_i turns negative (x_i will remain positive).

To get a first result, we suppose that the y_i stay positive and, what is more, we suppose (boundedness condition) that there is $\epsilon > 0$ such that the f_i are contained in a vector $\bar{\mathbf{f}}_i = (1-\epsilon)\mathbf{f}_i$ (see Figure 1). This is a new variant of the boundedness condition. One can show that the set of reswitch-points $M^\star$ is contained in the convex hull $M^{\star\star}$ of the f_i, and $M^{\star\star}$ is contained in the simplex $M^{\star\star\star}$ spanned by the $\bar{\mathbf{f}}_i$. It is clear that $\mu(M^{\star\star\star}) = (1-\epsilon)^n \mu(M^\star(0))$. This means that the probability of reswitching tends to zero with $n \to \infty$, given this new boundedness condition.

The boundedness condition is fulfilled, if each y_i stays above a certain threshold for $0 < r \leq R$, and, as a precondition, y_i is positive if and only if

$$(1+r)\frac{\hat{p}_i(r)}{\hat{p}_i(0)} > \frac{\hat{p}_1(r)}{\hat{p}_1(0)}.$$

This condition is fulfilled, if prices remain close to labour values. It will also be fulfilled if the first sector has a low capital intensity relative to the others. But it is, of course, quite possible that some y_i turn negative in the general case. This means that there will be a set of reswitch-points $B^\star$ in the basis B of simplex $M(0)$ and in its neighbourhood. It turns out that the probability of reswitching depends for large systems on $\mu(B^\star)$, since, as n increases, the number of vertices of B increases so that it is the mass of the reswitch-points in those vertices, close to B, which matters, while reswitch-points higher above B belong to vertex $n+1$; their mass tends, relatively speaking, to zero. Schefold (2022a) discusses conditions under which $\mu(B^\star)$ remains small relative to $\mu(B)$ so that it continues to be true that the likelihood of reswitching tends to zero.

We can here return to Salvadori's (2000) point (Section 4.3 above): Contrary to Petri's objection and to Steedman's doubts, it does not really matter whether one takes the euclidean or some related measure, if the assertion is that the probability of reswitching tends to zero. One could argue in the present case that potential techniques near the basis of $M(0)$ should be excluded or regarded as improbable because they all have in common that the labour input is zero or very small. If we ascribe a low probability to these

methods, it may mean that $\mu(M^{\star\star\star})$ must be multiplied by some factor α, $0 < \alpha < 1$, and $\mu(M(0))$ by β, $0 < \beta < 1$, hence the probability by α/β, but it remains zero.

Petri (p. 26-27) actually refers to sectors; he writes: "A fundamental assumption of the argument is that all alternative methods represented by points in $M(r_1)$ are equally probable ... 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", the use of commodities that cannot be direct inputs to bread production must be assigned probability zero.

Sure, this would be correct, if we were specially interested in a bread-producing economy, but since we are interested in the economy as a whole, we assume that such effects cancel on average, and such assumptions are commonly made. If a comparison is made between the life expectancy in two countries, one calculates from the mortality rates of the corresponding populations and does not take into account that some people smoke, others have been diagnosed with cancer, so that their life expectancy will differ from the average. These life expectancies for individuals and groups are relevant for the people concerned, for insurances and for special policies like printing warnings on packages of cigarettes. They are in general not used, if one assesses the sustainability of pension schemes of countries. Similarly, we here estimate the likelihood of reswitching, where it is a question of macroeconomics capable of application, by using the information provided by input-output tables and do not go down to the level of bakeries.

Another criticism: "The argument is also based on excluding 'non-piecemeal' switches which introduce new capital goods" (p. 27).

I already partially answered the objection regarding new goods in the context of the Samuelson model, where I asserted that intermediate goods could be accommodated in Sraffa (Section 4.3). Petri refers specifically to machines, these "give more space to compound interest to produce inversion of the direction of change as r rises, making reswitching easier". Machines are analysed in Sraffa's (1960) book within the part on joint production. I admitted in Section 4.4. that I really should have to pass, if Petri asked me to reproduce my analysis within a general joint production framework, but machines can be accommodated – for brevity, we take the case of constant efficiency. If this capital good is the n-the input in the first sector, and a_{1n} machines are used, they enter the cost of production in the 'centre' of the fixed capital system (Schefold 1971) not with a_{1n}, but multiplied with the coefficient $r(1+r)^{T-1}/[(1+r)^T - 1]$, which tends to $1/T$ for $r = 0$, since T is the life-time of the machine, and the coefficient expresses the need to write off $1/T$ of the initial value of the machine, as long as there is no financial charge, because $r = 0$. Petri is right that higher powers of $1+r$ therefore enter the price equation

with the introduction of machinery, but this is not directly relevant according to the new paper on the rarity of reswitching. What matters there is the relative number of so-called 'empty vertices' and 'transgressions'. The transgressions are cases where the y_i considered above turn negative and neighbourhoods of vertices at the basis B of $M(0)$ are occupied by reswitch-points, while the neighbourhoods of 'empty vertices' contain no reswitch-points. It is argued that the empty vertices predominate, if reswitching is examined for 'average industries'. This will still be the case, if there is an average distribution of fixed capital, hence the probability for reswitching will still tend to go to zero for large systems. The details of such an extension of the model remain to be worked out.

5.2 Limits to substitution

Petri's subsequent comments on my paper with Götz Kersting are not yet based on the published version (Kersting and Schefold 2021), but on the prepublication in a working paper (Kersting and Schefold 2020). In particular, they do not refer to the alternative assumptions made about the joint probability distribution of the wage curves, which make some difference for the results, nor does he discuss the empirical findings and the numerical experiments. The early version of the Kersting-Schefold paper did not yet point out that the proof that the expected number of wage curves on the envelope at most equals $\ln s$ (ln: natural logarithm, s: total number of wage curves – a formula first proved in Schefold 2013b) can be extended from linear wage curves (as in Samuelson) to non-linear ones, if reswitching is sufficiently rare (Kersting and Schefold 2021, p. 523) – a somewhat ironic finding: the new turn in the critique of capital is strengthened by abandoning an important element of the old critique.

Even more important than the number of wage curves on the envelope is the fact that the efficient techniques in the relevant range of the rate of profit will not only be few but have similar capital-labour ratios. This is easiest to show for a uniform distribution of the maximum wage rates and rates of profit, but it is also true, if their distributions are normal and are positively rather than negatively correlated. Petri himself seems to argue that they are positively correlated (p. 30: "if n is great..."). The intuitive argument follows from the stylised argument in Figure 2, the analytical proof is in Kersting and Schefold (2021, pp. 515-517). Suppose that there are s wage curves, they are linear, the maximum wage rates and the corresponding maximum rates of profit are distributed with uniform distributions between zero and upper bounds $\bar{w}$ and $\bar{R}$ respectively, and that the distributions are not correlated. As one moves down the maximum wage rate, starting from $\bar{w}$, it is likely that one soon encounters a wage curve of type w_1 in Figure 2 with

which a large maximum rate of profit R_1, not much smaller than $\bar{R}$, is associated, and the distance δ_w between $\bar{w}$ and $y_1 = w_1(0)$ and similarly $\delta_R = \bar{R} - R_1$ will tend to zero, as s tends to infinity. We assume that the composition of net output is given and that it is used as the numéraire. Hence the maximum wage for any technique at $r = 0$ equals output per head y and R is equal to the output-capital ratio, hence $R = Y/K = y/k$, where k is the capital-labour ratio, equal to the absolute value of the slope of the wage curve. There will be a large number of wage curves of type w_2 in Figure 2 that 'originate' between $\bar{w}$ and w_1, with maximum rates of profit between zero and R_1, and similarly many wage curves of type w_3 that 'arrive' between R_1 and $\bar{R}$ and that 'originate' between zero and w_1. Finally, there are the wage curves of type w_4 that are entirely dominated by w_1.

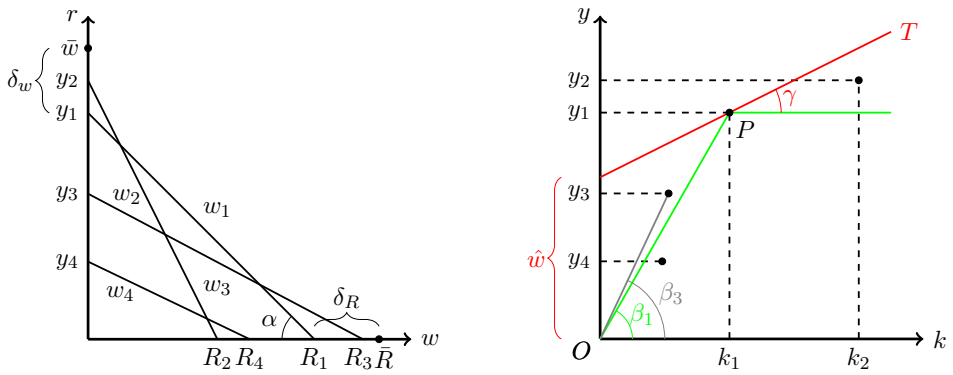

Figure 2: Wage curves and the corresponding points in the k-y-diagram ('production function'). The kink in the production function means that distribution will tend to be indeterminate between $0 < W/Y < 1$, where $k_1 = K_1/L_1 = \tan\alpha$, $R_1 = Y_1/K_1 = y_1/k_1 = \tan\beta_1$, etc. Note $\delta_R > \tan\beta_3 - \tan\beta_1$. If δ_w and δ_R tend to zero, the 'production function' tends to OP and then tends to continue horizontally for k rising to k_2 and possibly beyond. As s increases and δ_w and δ_R diminish, wage curves of type 2 and 3 move towards the production function and the space below gets filled with inefficient techniques. Distribution according to neoclassical theory would be determined by the slope of a tangent T to the production function, but is underdetermined. The slope of T would be equal to the derivative of the production function, $\tan\gamma$ would be equal to the rate of profit and $\hat{w}$ to the wage rate, if the production function was smooth, but the kink of the 'production function' is here in the limit such that $\tan\gamma$ can be anywhere between $y_3/k_3 = R_3$ and $(y_2 - y_1)/(k_2 - k_1)$, with R_1 tending to R_3 and y_1 to y_2, while no tendency can be postulated for y_3 and R_2.

To each wage curve, there corresponds a point (k, y) in the k-y-diagram (right-hand

side of Figure 2). The outer boundary of the convex hull of these points is Samuelson's pseudo-production function. He assumed implicitly that the maximum wage rates and the maximum rates of profit are largely inversely related, but if they are not, distribution can remain completely undetermined, as is shown in the legend to Figure 2.

The assumption of a uniform distribution between given bounds is only a simplification, going to the other extreme from Samuelson's implicit assumption of a largely inverse correlation between maximum rates of wages and profit. The empirical investigation in Kersting and Schefold (2021) shows that the assumption of a normal distribution with a moderate positive correlation gives the best fit. The result then is essentially the same: only a small number of wage curves appear on the envelope and the corresponding capital-labour ratios are almost the same.

All this will have to be discussed, when Petri will have studied the published version and if he chooses to comment on it. Meanwhile, I want to react to one claim: Petri wants to show that the smallness of the number of wage curves on the envelope can be deduced from technical progress alone, without using the "doubtful assumption of randomness" (p. 31). The argument has some appeal and its simplicity would make it didactically useful, but I regard it as circular. Moreover, it comes as a surprise at the end of Petri's paper, since reswitching and reverse capital deepening cannot be frequent, if the number of efficient techniques is small.

We both agree that new techniques are adopted because firms choose methods that minimise costs and lead to extra-profits, given the prevailing rate of profit. We also accept the stylised fact that the rate of profit is stable. Hence the real wage must rise, as it actually has done for maybe 150 years in the old industrialised countries. Petri now says that 'productivity' has risen, too, i.e. the "vertical intercept of wage curves in terms of a representative consumption-basket". But this is not a logical necessity, it is an observation. Logically, the wage curve could show a slope declining over time towards zero near the actual rate of profit, or the maximum rate of profit (the – horrible dictu – 'productivity of capital') could rise and net profit per head could fall, while wages per head – the wage rate – rises. Since the argument that net output per head rises has been introduced by Petri by means of a historical argument, he now feels compelled to add an analytical one and says that if net output per head and the real wage were not to rise simultaneously, "the wage curves would have to be very non-linear", a nice admission that even Petri prefers wage curves which are something like quasi-linear, and an assumption which begs the question (see p. 31).

Logically, it is possible that we have wage curves which substantially deviate from linearity, for instance in the form of two wage curves that have even more than two switches in common, and it is possible that this constellation does not change, as the

real wage rises at a constant rate of profit. All that we have to assume is that the most common and simplest form of progress prevails: a uniform rise of the productivity of labour because, what Marx calls the intensity of labour, rises; formally because all components of the labour vector diminish uniformly, while the input-output matrix and the alternative method generating the switches stay the same, as far as the inputs of commodities are concerned – only labour per unit of output shrinks. As time goes on, the two intersecting wage curves turn around the maximum rate of profit (to avoid a slight complication, assume that there is a switch also at the maximum rate of profit), and the multiple switches will occur at higher and higher wage rates, while the rates of profit, where they occur, remain the same, and so the hierarchy of the techniques defined by the actual rate of profit somewhere in-between will stay the same (see Figure 3). It means that we cannot explain why the number of efficient techniques is small without the 'doubtful assumption' of randomness.

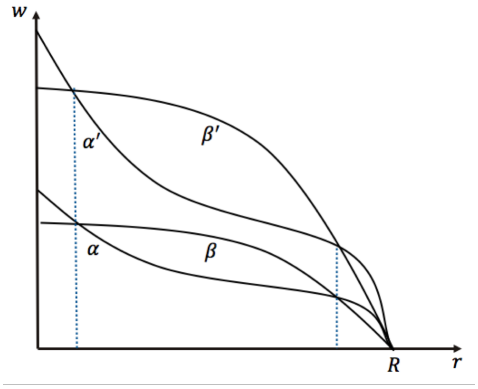

Figure 3: The archetypical form of technical progress, intensification of labour, does *not* eliminate reswitching, for $\mathbf{p} = (1 + r)\mathbf{Ap} + w\mathbf{l}$, with $\mathbf{l} \to \alpha\mathbf{l}$, $0 < \alpha < 1$, means that wage curves α and β turn upward around the maximum rate of profit, they become wage curves α' and β', and switch-points can then remain at the same rates of profit.

I apologise for this pedantic exercise and I return to my plea to think historically, as Marx did. Petri has here introduced a historical argument to get to an explanation why the number of wage curves on the envelope is surprisingly small. He has not been historical enough. Marx pioneered in his theory of relative surplus value a kind of historical morphology of different forms of technical progress (cooperation, division of labour, mechanization) and added to that the saving of raw material. Each of these forms modifies the wage curve, but gives no reason to expect strong curvatures. Of course, these Marxian forms need modernisation. If we could meet on this ground, we should not be so distant after all.

6 Conclusion

Two basic insights remain after this mutual critique: (1) Reswitching exists, but it tends to disappear in large models. (2) The efficient techniques are few and the capital-labour ratio is largely independent of distribution. The critique based on reswitching and reverse capital deepening (1) amounts to a kind of agnosticism: one does not know what happens to the capital-labour ratio or the capital-output ratio if distribution changes, e.g. because the rate of profit moves up and down during a cycle. By contrast, the new critique (2) offers a positive result: the capital-labour ratio of the efficient technique(s) essentially is given. The new critique is to be preferred.

At any rate, the new critique stands a better chance of being heard today, after the transformation of the mainstream, which we discussed in Section 2. If one thinks historically, one must accept that the impact of a theory depends on the environment, in which it is published. Sraffa told me more than once how much he admired Keynes for his ability to conceive of his new ideas and to write them down quickly, so as to reach his public at the right time. I am convinced that Sraffa's book would have made a much bigger impact, had it been published in the 'years of high theory', as Shackle (1967) put it, say in 1930. Thirty years later, it still had an impact. But how many would have reacted to it, had it been published posthumously sixty years later in 1990 or today? Shackle (1967, p. 5) wrote of marginalism: "In its arresting beauty and completeness this theory seemed to need no corroborative evidence from observations". Sraffa's critique was addressed to theory in this pure form, if the interpretation attempted in Section 2 above is correct. Samuelson, the main representative of the neoclassicals in the debate, was Schumpeter's pupil, and "Schumpeter's methodology in *Wesen* (transl. as Schumpeter 2017 – BS) can best be interpreted as instrumentalism, that is, the view that theories are not descriptions but instruments for deriving useful results and are neither true nor false". Schumpeter would call that pragmatism – for instance, he regarded the subjective theory of value as more practical than labour values (Shionoya 1997, pp. 91, 96, 113).

In this perspective, Samuelson believed in the possibility of a synthesis of neoclassical and Keynesian thought. This was instrumentalism, but it was also a kind of pluralism *avant la lettre*. Samuelson is still representative for the *de facto* pluralism of the mainstream. Meanwhile, pluralism has become radical – or radicals have turned pluralist: "The demand for pluralism is not a demand for new theory, but a demand for a wider vision, from which new theories (plural) may emerge, applicable to different parts of social life" (Skidelsky 2020, p. 6).

If pluralism is representative for the frame of mind of the modern economist, main-

stream or radical, modern economics is oriented towards applied theory, with its multiplicity of models, its contextual approach, its striving for empirical validation (using the external methodology of econometrics) and its orientation towards the concrete economic policies, while pure theory used to rely on one model, which was ahistorical, stressing elegance and consistency according to an internal logic, and only remotely was it associated with a broad political programme. Accordingly, the old critique addressed the belief in pure theory, the new the use of what has remained of long-run analysis for the underpinning of macroeconomics, of the relation between growth and distribution and for the understanding of development. Like Petri, I admire pure theory – we shall always need it to clarify our ideas – but I have lost a former conviction that there must be one unifying approach to economics. The historicity of economic phenomena, the complexity of the system, as some would now say in the language of evolutionary theory, demands flexibility, which is not the same as simplification. It is relatively easy to see that the existence of reswitching or the impossibility to rigorously treat capital as a single factor of production contradict pure neoclassical theory. It is more difficult to derive a methodology for theories that approximate a reality that is shifting and to take a new turn, when that is needed. 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, the Cambridge and the monetary theory of distribution (for instance Petri 2021a, chapter 13). Capital theory, discussed at a similar level of abstraction, leads to the approximate surrogate production function. 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 is ruled out, while history decides which of the other alternative theories of distribution and employment applies. What could favour the surplus principle more than this result?

References

Bienenfeld, M. (1988): Regularity in price changes as an effect of changes in distribution. *Cambridge Journal of Economics*, 12(2), 247-255.

Böhm-Bawerk, E. von (1914): *Macht oder ökonomisches Gesetz?* Reprint 1975 (With an Introduction by H.-H. Barnickel.). Darmstadt: WBG.

D'Ippolito, G. (1989): Delimitazione dell'area dei casi di comportamento perverso del capitale in un punto di mutamento della tecnica. In *Aspetti controversi della teoria del valore*. Ed. by Luigi Pasinetti. Bologna: il Muliono, pp. 191-198.

Garegnani, P. (1964, 1965): Note su consumi, investimenti e domanda effettiva. Offprint from *Economia Internazionale*, 17(4), 18(4), Genova.

Garegnani, P. (1970): Heterogeneous capital, the production function and the theory of distribution. *Review of Economic Studies*, 37(3), pp. 407-436.

Garegnani, P. (2003): Capital and effective demand [first published 1988], reprinted in *The Legacy of Piero Sraffa*. Ed. by Heinz D. Kurz and Neri Salvadori. Cheltenham: Elgar, pp. 157-216.

Goldberg, G. and Neumann, M. (2003): Distribution of subdominant eigenvalues of matrices with random rows. *Society for Industrial and Applied Mathematics Journal on Matrix Analysis and Application*, 24(3), pp. 747-761.

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), pp. 737-765.

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

Hicks, J. R. (1973): *Capital and Time: A Neo-Austrian Theory*. Oxford: Oxford University Press.

Iliadi, F., Mariolis, T., Soklis, G., and Tsoulfidis, L. (2014): Bienenfeld's approximation of production prices and eigenvalue distribution: Further evidence from five European economies. *Contributions to Political Economy*, 33(1), pp. 35-54.

Kalb, J. (2022): Reverse capital deepening remains rare. A comment on Zambelli's 'The aggregate production function is NOT neoclassical'. *Unpublished Working Paper*.

Kant, I. (1787): *Metaphysische Anfangsgründe der Naturwissenschaft*. Riga: J.F. Hartknoch, Zweyte Auflage.

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.

Kersting, G., and Schefold, B. (2021): Best techniques leave little room for substitution. A new critique of the production function. *Structural Change and Economic Dynamics*, 58, pp. 509-533.

Laing, N. F. (1991): The likelihood of capital-reversing and double-switching. *Bulletin of Economic Research*, 43(2), pp. 179-188.

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

MEGA^2: Marx/ Engels Gesamtausgabe.

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

Petri, F. (2011): On the likelihood and relevance of reswitching and reverse capital deepening. In *Keynes, Sraffa and the Criticism of Neoclassical Theory: Essays in Honour of Heinz Kurz*. Ed. by Neri Salvadori and Christian Gehrke. London: Routledge, pp. 380-418.

Petri, F. (2021a): *Microeconomics for the Critical Mind. Mainstream and Heterodox Analyses*. Cham: Springer.

Petri, F. (2021b): What remains of the Cambridge critique? On Professor Schefold's theses. *Centro Sraffa Working Papers*, No. 50.

Salvadori, N., and Steedman, I. (1988): No reswitching? No switching! *Cambridge Journal of Economics*, 12(4), pp. 481-486.

Salvadori, N. (2000): Comment on Mainwaring and Steedman. In *Critical Essays on Piero Sraffa's Legacy in Economics*. Ed. by Heinz D. Kurz. Cambridge: Cambridge University Press, pp. 354-357.

Salz, A. (1905): *Beiträge zur Geschichte und Kritik der Lohnfondstheorie*. Stuttgart and Berlin: J.G. Cotta.

Samuelson, P. (1962): Parable and realism in capital theory: The surrogate production function. *Review of Economic Studies*, 29(3), pp. 193-206.

Schefold, B. (1971): Piero Sraffas Theorie der Kuppelproduktion, des fixen Kapitals und der Rente (Mr. Sraffa on Joint Production). Private Print. Online as: `https://www.wiwi.uni-frankfurt.de/fileadmin/user_upload/dateien_abteilungen/abt_ewf/Economic_Theory/publications/Theorie_der_Kuppelproduktion_Bertram_Schefold_1971.pdf`.

Schefold, B. (1976): Relative prices as a function of the rate of profit: a mathematical note. *Zeitschrift für Nationalökonomie/Journal of Economics*, (H. 1/2), pp. 21-48.

Schefold, B. (1989): *Mr. Sraffa on Joint Production and Other Essays*. London: Unwin Hyman.

Schefold, B. (1997): *Normal Prices, Technical Change and Accumulation*. London: Macmillan.

Schefold, B. (1999): Use value and the 'commercial knowledge of commodities': reflections on Aristotle, Savary and the Classics. In: *Value, Distribution and Capital: Essays in Honour of Pierangelo Garegnani*. Ed. by Gary Mongiovi and Fabio Petri. London: Routledge, pp. 122-144.

Schefold, B. (2008): Savings, investment and capital in a system of general intertemporal equilibrium – An extended comment on Garegnani with a note on Parrinello. In *Sraffa or an Alternative Economics*. Ed. by Guglielmo Chiodi and Leonardo Ditta. London: Palgrave Macmillan, pp. 127-186.

Schefold, B. (2010): Families of strongly curved and of nearly linear wages curves: a contribution to the debate about the surrogate production function. In *Economic Theory and Economic Thought: Essays in Honour of Ian Steedman*. Ed. by John Vint, J. Stanley Metcalfe, Heinz D. Kurz, Neri Salvadori and Paul Samuelson. London: Routledge, pp. 135-155.

Schefold, B. (2012): The economy in Jewish history: New perspectives on the interrelationship between ethnicity and economic life. *The European Journal of the History of Economic Thought*, 19(5), pp. 839-844.

Schefold, B. (2013a): Approximate surrogate production functions. *Cambridge Journal of Economics*, 37(5), pp. 1161-1184.

Schefold, B. (2013b): Only a few techniques matter! On the number of curves on the wage frontier. In *Sraffa and the reconstruction of economic theory: Volume one*. Ed. by Enrico Sergio Levrero, Antonella Palumbo and Antonella Stirati. London: Macmillan, London, pp. 46-69.

Schefold, B. (2016a): Marx, the production function and the old neoclassical equilibrium: Workable under the same assumptions? With an appendix on the likelihood of reswitching and of Wicksell effects. *Centro Sraffa Working Papers*, No. 19.

Schefold, B. (2016b): Profits equal surplus value on average and the significance of this result for the Marxian theory of accumulation: Being a new contribution to Engels' Prize Essay Competition, based on random matrices and on manuscripts recently published in the MEGA for the first time. *Cambridge Journal of Economics*, 40(1), pp. 165-199.

Schefold, B. (2017a): The improbability of reswitching, the certainty of Wicksell-effects and the poverty of production functions: The Cambridge critique of capital transformed. In *Research in the History of Economic Thought and Methodology: Including a Symposium on New Directions in Sraffa Scholarship*. Ed. by Luca Fiorito, Scott Scheall and Carlos Eduardo Suprinyak. Bingley: Emerald Publishing Limited, pp. 171-194.

Schefold, B. (2017b): *Great Economic Thinkers from the Classicals to the Moderns*. New York: Routledge.

Schefold, B. (2019): The transformation of values into prices on the basis of random systems revisited. *Evolutionary and Institutional Economics Review*, 16(2), pp. 261-302.

Schefold, B. (2021a): Transformations of the Cambridge critique. *The Indian Economic Journal*, 69(2), pp. 241-254.

Schefold, B. (2021b): Sraffa's 'reduction' of the prices of basics. *Structural Change and Economic Dynamics*, 59, pp. 575-581.

Schefold, B. (2021c): Sraffa's Protoeconomics – A comment on Ajit Sinha's 'Interpreting the nature of Sraffa's equations: A critique of Garegnani's interpretation'. In *A Reflection on Sraffa's Revolution in Economic Theory*. Ed. by Ajit Sinha. Cham: Pelgrave Macmillan, pp. 29-40.

Schefold, B. (2022a): The rarity of reswitching explained. *Unpublished Working Paper*.

Schefold, B. (2022b): From reswitching to zero substitution. *Unpublished Working Paper*.

Schumpeter, J.A. (1906): [Review of A. Salz] Beiträge zur Geschichte und Kritik der Lohnfondstheorie. *Zeitschrift für Volkswirtschaft, Sozialpolitik und Verwaltung*, 15, pp. 97-98.

Schumpeter, J. A. (2017): *The Nature and Essence of Economic Theory* [*Wesen und Hauptinhalt*]. London: Routledge.

Shackle, G.L.S. (1967): *The Years of High Theory*. Cambridge: Cambridge University Press.

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

Shionoya, Y. (1997): *Schumpeter and the Idea of Social Science*. Cambridge: Cambridge University Press.

Skidelsky, R. (2020): *What's Wrong with Economics? A Primer for the Perplexed*. New Haven and London: Yale University Press.

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

Steedman, I. (2011): Indivisible inputs and the probability of reswitching. In *Keynes, Sraffa and the Criticism of Neoclassical Theory: Essays in Honour of Heinz Kurz*. Ed. by Neri Salvadori and Christian Gehrke. London: Routledge, pp. 382-388.

Tsoulfidis, L., and Mariolis, T. (2007): Labour values, prices of production and the effects of income distribution: evidence from the Greek economy. *Economic Systems Research*, 19(4), pp. 425-437.

Weizsäcker, C.F. von (1971): *Die Einheit der Natur. Studien*. München: Hanser.

Zambelli, S., Fredholm, T., and Venkatachalam, R. (2017): Robust measurement of national technological progress. *Structural Change and Economic Dynamics*, 42, pp. 38-55.

Zambelli, S. (2018): The aggregate production function is NOT neoclassical. *Cambridge Journal of Economics*, 42(2), pp. 383-426.

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