CENTRO DI RICERCHE E DOCUMENTAZIONE
"PIERO SRAFFA"

UNIVERSITÀ DEGLI STUDI ROMA TRE

CLASSICAL THEORY AND POLICY ANALYSIS: A ROUND TABLE

Duncan K. Foley, Pierangelo Garegnani
Massimo Pivetti, Fernando Vianello

Materiali di Discussione n. 1
2004

ARACNE

Materiali di Discussione
della Fondazione "Centro di Ricerche e Documentazione *Piero Sraffa*"

Dipartimento di Economia
via Ostiense 139 - 00154 Roma
telefono 06/57374037
e-mail: sraffa@uniroma3.it

I *Materiali di Discussione* della Fondazione "Centro di Ricerche e Documentazione Piero Sraffa" si propongono di offrire una sede di discussione tra le diverse correnti dell'economia politica contemporanea sia in campo teorico e di politica economica che nell'analisi di concrete realtà economiche.

Il Comitato Scientifico dei *Materiali di Discussione* è costituito da due membri del Consiglio della Fondazione "Centro di Ricerche e Documentazione Piero Sraffa", nonché da studiosi esterni nominati dal Consiglio.

I *Materiali di Discussione* sono depositati come opere a stampa secondo gli obblighi previsti dall'Art. 1 del D.L.L. 31.8.45 n. 660. I *Materiali* vengono inviati a studiosi, a istituzioni universitarie e a centri di ricerca interessati.

06 93781065
info@aracne-editrice.it
www.aracne-editrice.it

00173 Roma
via R. Garofalo, 133 a–b

ISBN 88-7999-740-8

I edizione: dicembre 2004

Finito di stampare nel mese di dicembre del 2004
dalla tipografia « Grafica DV di Danilo Ventura » di Roma
per conto della « Aracne editrice S.r.l. » di Roma

CENTRO DI RICERCHE E DOCUMENTAZIONE
"PIERO SRAFFA"

UNIVERSITÀ DEGLI STUDI ROMA TRE

CLASSICAL THEORY AND POLICY ANALYSIS: A ROUND TABLE

Duncan K. Foley, Pierangelo Garegnani
Massimo Pivetti, Fernando Vianello

Materiali di Discussione n. 1
2004

ARACNE

Editorial Note

In May 2001 Professor Foley, then in Rome as a guest of the "La Sapienza" University, was invited to the Centro Sraffa at the "Roma Tre" University to present a review article he had just written on a collection of essays regarding the resumption of classical theory.[1] *The result was the roundtable printed in this first number of our* Materiali di Discussione. *The interest which prompted the invitation was twofold. On the one hand, Foley's attentive examination provided an opportunity to discuss some points of the theory of distribution and, more generally, of the social-surplus-centered view which the old classical economists took of the economy, with the resulting composite method of their analysis: deductive for the effects of competition on commodity prices; and mainly inductive with respect to distribution and growth. But the Centro's interest in a discussion with Professor Foley was also a second, more specific one, consisting of his essay's central thesis according to which "the classical literature [...] offers few demonstrations of the viability of applied classical political economy as an alternative to neoclassical practice"*[2] *because it tells economists what not to do, but not "what they should do as an alternative".*[3] *This explains, Foley thought, why "despite the excellence and persuasiveness of [the "classical"] doctrinal critique", neoclassical theory continues to dominate.*

We hope that this roundtable will contribute towards making more explicit the "viability" of applications of classical political economy, testified by the wealth and variety of the "applied economics" and "policy analyses" of Adam Smith, or Ricardo, or Marx. The problem, we suggest, may rather lie in taking too much for granted the shape and methods which applied economics and policy analysis have assumed within neoclassical theory over the last decades, while also underestimating some radical changes entailed in this field by the resumption of classical theory and by its critical implications. The radical nature of those changes follows from two elements: (i) the compatibility between classical free competition and permanent labour unemployment; (ii) the relevance of the critique of neoclassical distribution in putting on a solid basis the possibility of aggregate demand deficiencies for a long period analysis, no less than for a short-period one. Once they have been solidly founded in theory, these two elements overturn the idea of a Pareto optimality of free competition. The stress of policy analysis shifts then from the allocation of resources to the growth and the distribution of the social product[4] *— and with that to a recognition of the centrality for economic policy of conflicts of interest and power within the community. If this means that policy analysis loses in the ease of the neoclassical "routine predictions" which Foley rightly recalls,*[5] *it may gain from being able to close the gap with a practice in which conflicts and power relations have long been dealt with in all but theory.*

[1] *See Foley's "Value, Distribution and Capital: A Review Essay",* Review of Political Economy *(2001, 13), here reprinted. In the delay of publication of the present issue Foley has published in the* Cambridge Journal of Economics *a second review article on the resumption of classical theory, bearing partly on questions raised at the roundtable. Some of Foley's points there are also taken up in this* Quaderno *(see the Postscript to Garegnani's intervention).*

[2] *Below p. 7.*

[3] *Below p. 12.*

[4] *Growth of social product here would be one with growth of* voluntary *leisure, if the community were to choose taking the "product" in that form.*

[5] *Below p. 7.*

Value, Distribution and Capital: A Review Essay[1]

DUNCAN K. FOLEY*

1. The World According to Garegnani

The essays in Gary Mongiovi and Fabio Petri's well-edited and stimulating collection of papers assembled to celebrate Pierangelo Garegnani's 65th birthday are devoted to explaining and defending Garegnani's formulation of the "classical" view of the history of economic thought. According to this view, which stems from Piero Sraffa's efforts to reconstruct the classical political economy of Adam Smith and David Ricardo, which was the object of Karl Marx's critique of political economy, the classical political economists had a distinctive, consistent, and correct methodological approach that has been, in Sraffa's words "submerged and forgotten" with the triumph of marginalist and neoclassical economic theory since the 1880s.

The core of this classical theory, in Garegnani's view, is the principle that competition among mobile inputs to production, labor and capital, tendentially equalize their rates of return, wages[1] and profit rates, across different sectors of production given available techniques of production. The classical political economists realized that the mobility of factors is an approximate, unorganized process, so that actual "market" prices, wages, and profit rates observed in any short time period may deviate from the "natural" prices consistent with the equalization of wages and profit rates. However, they recognized that market prices will gravitate around these "long-period equilibrium" natural prices, which constitute the true object of scientific inquiry in political economy.

In elaborating the theory of long-period equilibrium, continuing Garegnani's account, the classical political economists characteristically and rightly took the composition of output (determined by social and historical factors) and the distribution of income (as represented by the average wage or the average profit rate) as given prior to and independently of the emergence of long-period equilibrium natural prices. The factors affecting the composition of

* Department of Economics, Graduate Faculty, New School University, 65 Fifth Avenue, New York, NY 10003, USA. This paper reviews Gary Mongiovi and Fabio Petri (Eds), *Value, Distribution and Capital: Essays in honour of Pierangelo Garegnani* (London and New York: Routledge Frontiers in Political Economy, 1999). I would like to thank Will Milberg for comments on draft of this paper.

[1] In this discussion I use "wages" to mean real wages measured in wage goods.

output and the average wage or profit rate belong to a different conceptual realm from the competitive processes that enforce long–period natural prices as a center of gravitation for market prices. In particular, while market prices are determined by the movement of capital between sectors in response to market forces, the wage is determined by social forces that have little or nothing to do with the clearing of the labor market. In the classical political economists' vision, capitalist economies typically operate with a substantial and varying margin of unemployed labor, which has no generally predictable impact on the level of the wage. In turn, there is no reason to believe that the level of the wage has a systematic impact on the demand for labor.

Fidelity to these submerged and forgotten doctrines, according to Garegnani, requires the honest contemporary historian of political economy to be ever–vigilant against the tendency of neoclassical economics to read back its own doctrines into the thought of the classical political economists. In particular, suggestions that the classical political economists conceived of the composition of output as varying systematically with prices (along the lines of neoclassical demand theory) or of the wage as varying with labor unemployment, or of employment increasing with a fall in the wage, must be resolutely combatted and refuted. Alessandro Roncaglia's essay in this volume explores this conceptual cleavage between classical and marginalist conceptions of rationality and demand. Antonella Stirati's contribution criticizes the view that Ricardo shared the "wage fund" theory of McCulloch, J. S. Mill, and Senior, who thought that total wages were constrained in any period by the accumulation of wage goods, and thus that individual wages would vary inversely with employment.[2]

The classical political economists, as Garegnani sees things, were wise and farsighted in their adherence to these methodological presumptions. Their theory allowed them to see the structural skeleton of capitalist economic life through the confusing interplay of short–term fluctuations of prices and quantities, to recognize the source of capital accumulation in class divisions, and to arrive deductively at correct theorems about the impact of changes in the wage or the composition of output on the profit rate and long–period equilibrium prices. Above all, these methodological predilections protected the classical political economists from succumbing to the fallacies that arose from the attempt of marginalists and neoclassicals to unify the theories of demand, production, and distribution in a single grand synthesis based on the shallow and inadequate conception of universal market clearing. These fallacies are at the

[2] There may be some potential confusion on this point because some writers use the phrase "wage fund" to represent the view, shared by Ricardo, that the advance of wages are a part of capital, in contrast with neoclassical production functions, which include only the value of fixed capital in measuring capital input.

root of the widely held but erroneous notion that the capitalist economy is capable of self–regulation toward a full–employment state. This erroneous notion, in turn, is the source of misguided, dangerous, and damaging national and international macroeconomic policies. Massimo Pivetti's paper illustrates these perils with a discussion of the political economy of the European Monetary Union.

The scientific hubris of the marginalists, Garegnani suggests, lies in their misguided search for a formal synthesis that would explain distribution on the same conceptual basis as prices. This led the marginalists to argue that "scarcity" determines not just rents to unreproducible resources such as land (as the classical political economists had recognized) but the wage and the profit rate as well, despite the reproducibility of capital goods and the social and historical character of wage determination. Tony Aspromourgos and Peter Groenewegen explain the distinctive characteristics of the classical approach to wage determination and contrast it with neoclassical theories of the labor market. In order to fit economic reality into this Procrustean bed, marginalists and neoclassicals must dogmatically deny the existence of sustained unemployment, or relegate it to the category of "market failure", thereby ignoring one of the most important systematic manifestations of capitalist economic organization. Furthermore, the marginalists and neoclassicals have to take the indefensible position that the profit rate is a scarcity rent to capital goods, in defiance of the well–considered classical view that the reproducibility of capital goods requires a theory of the profit rate that is qualitatively different from the theory of rent. In particular, the application of rent theory to explain the returns of individual capital goods is inconsistent with the fundamental and powerful insight of the equalization of rates of profit that was the heart of classical political economy. In order to bolster the scarcity of capital theory of the profit rate, the marginalists and their neoclassical followers conjured up the phantom of a "capital" that is supposed to be a substance separate from individual capital goods but somehow embodied in them in specific quantities. The uniform profit rate is then to be interpreted as the scarcity return to this phantom substance.

Here the neoclassicals, as Garegnani sees things, like the tragic heroes of Greek drama, fell into a trap of their own construction. They were right in their belief that a market–clearing theory of distribution required the concept of capital independent of specific capital goods, but failed to recognize the logical incoherence and unsustainability of this notion of capital. Sraffa was onto this as early as the 1920s, but it was only in the 1950s and 1960s that Joan Robinson, Luigi Pasinetti, and Garegnani himself (among others), were able to bring about a decisive theoretical confrontation over these issues in the series of debates that have become known as the "Cambridge capital controversy". These debates exploded the claims of neoclassicals that the concept of capital independent of capital goods could be generalized beyond a very special and unrealistic class of production models, essentially "corn" models in which there is a single output that serves both as capital and as consumption.

However, to Garegnani's puzzlement and dismay, the neoclassicals continue to adhere to the view that input prices are a reflection of relative scarcities, and refuse to accept the conclusions implicit in the Cambridge capital controversy. Instead, they have retreated to the pure formalism of the Arrow–Debreu conception of intertemporal general equilibrium, in which each capital good at each date (and perhaps in each conceivable contingency) is separately priced in clearing markets, thus dispensing with the need for the concept of a capital substance altogether. This construction, however, springs from the same fallacious method that derailed the marginalists to begin with: it refuses to acknowledge the classical political economists' central insight that the explanations of distribution, demand and prices belong to different realms of scientific discourse. Fabio Petri's essay explores these issues in considerable depth. The early marginalists and neoclassicals at least had the good sense to retain long–period equilibrium as the object of their analysis, despite their mistaken idea that the theory of rent could explain wages and profit rates. The modern general–equilibrium neoclassicals, however, have fallen into the irremediable error of abandoning the long–period concept of natural prices altogether. The scientific consequences of this maneuver are disastrous, since it is impossible to find an operational real–world equivalent to the "equilibrium" prices of intertemporal general equilibrium. Roberto Ciccone's paper investigates and contrasts the conceptions of equilibrium prices in Smith, Ricardo and Marshall, and contrasts them with intertemporal and temporary general equilibrium theory. On the one hand, it is absurd to imagine that buyers and sellers can actually reach these prices in day–to–day trading, especially given the absence of the extensive set of dated commodity markets postulated by the general equilibrium scenario. On the other hand, the prices of intertemporal general equilibrium do not equalize rates of profit on the cost of investment goods. John Eatwell and Murray Milgate elaborate the argument that the neoclassicals dispense with the concept of capital only at the penalty of losing contact with the relevant object of study of scientific economics, long–period natural prices.

2. Neoclassical Hegemony and Classical Subalternship

Much of Garegnani's reading of the history of economic thought is persuasive and the core critical elements in its evaluation of marginalist and neoclassical economics, particularly the critique of the scarcity theory of distribution, identify fundamental and chronic weaknesses in the neoclassical research program. However, a neoclassical mainstream continues to dominate teaching and research in economics in America and, increasingly, the rest of the world. Is there another side to the story?

Ricardo's economic analysis had great success in influencing British economic policy and politics on the issue of free trade in the middle half of the 19th century. This reminds us that the market for analytical economics is fundamentally a market for policy analysis. The neoclassical mainstream does a remarkable job of supplying this market at every possible level of sophistication, from developing the abstract ideology of laissez–faire through market–oriented macroeconomic policy models to bread–and–butter econometric analyses of the impact of tax loopholes on the distribution of economic surpluses. Neoclassical economics, like a shopping mall open twenty–four hours a day, seven days a week, stands ready to meet every policy analysis need. The mall may be built on the flawed foundation of an incoherent theory of distribution, and it may in the end collapse, but in the meantime it is open for business. Foundational weakness is far from the top of the priority list of proprietors who are busy extending the range of services and building up the export market.

The ability of neoclassical economics to provide this wide–spectrum analysis is closely linked to some of the features that the classical critique demands that it give up. The classical critique sees the postulate of consumer preferences determining demand functions, for example, as unacceptable on the ground that preferences are inherently unobservable, and changes in the composition of output respond to social and historical forces that cannot be reduced to reversible demand functions. However, it is precisely the logical structure of preference and demand theory, which purports to connect observed behavior to welfare, that allows neoclassical analysis to give answers to questions about the distributional impact of tax or trade policy. It is hard to imagine a policy analysis that could avoid the question of the impact of the policy under consideration on the composition of output and the distribution of incomes. Similarly, it is hard to imagine how one could make satisfactory predictions of the outcome of policies without an analysis of the impact of the policy on the level of the wage. In committing methodological sin by putting the theories of price and distribution on the same level of abstraction, neoclassical practice gains the tremendous advantage of being able to make routine predictions about the composition of output and distribution.

Unfortunately, the classical literature, despite the excellence and persuasiveness of its doctrinal critique and researches in the history of economic thought, offers few demonstrations of the viability of applied classical political economy as an alternative to neoclassical practice. The essays in this book reflect this balance of strengths: only one paper (Massimo Pivetti's interesting, if gloomy, assessment of the political economy of European monetary union) presents empirical data about real world economies. The ongoing development of a body of applied and policy economics based on classical theoretical and methodological precepts will greatly enhance the influence and prestige of the classical cause. But there are few clues in these papers for scholars willing to undertake this formidable task. How should a graduate student of classical persuasion who wants, for example, to address the likely consequences of the "harmonization" of European tax policies for the distribution of income between and within the European nations, handle the problem of predicting changes in the wage and the composition of output? Perhaps she might do an econometric analysis to model the movement of the wage in response to a variety of historical and social factors. How different will such an analysis be from a parallel neoclassical attempt to identify a supply curve of labor? How comfortable will her classical thesis adviser be with this effort?

3. The Bath Water

The strongest and most important point that has come out of the classical critique of marginalism and neoclassical economics is its refutation of the capital scarcity theory of the rate of profit.

The notion that the profit rate can be coherently viewed as being determined by a "marginal product of capital" given by technology and input availabilities is one of the more confused and ideologically muddied chapters in the history of economics. A cost–minimizing firm facing a wage, cost of capital, and prices of capital goods determined by markets will adapt its relative use of labor and capital to those prices. The value of capital goods is a rational and appropriate measure of capital input to the firm when the prices of capital goods are determined by market forces outside the firm's control. When a cost–minimizing firm faces a range of technical methods of production that approximate a smooth continuum of capital–labor ratios, cost minimization entails setting the marginal value product of each input equal to its price. (There is, of course, vigorous debate in all schools of economic thought over how well the assumptions of a cost–minimizing firm facing market prices for inputs fits the behavior of real capitalist firms.) In this scenario, however, it is the wage and the cost of capital that determine the marginal value products of labor and capital, not the other way around.

The vision of the marginalists and their neoclassical followers was that this uncontroversial theorem of cost minimization could somehow be transformed into a theory of the wage and profit rate, and hence into a theory of distribution. Their hopes of accomplishing this transformation stemmed from their anthropomorphic vision of the economy and its markets as analogous to the allocation of scarce resources by a single decision maker with a well–defined objective function, who, at least under the assumptions of concavity of the objective function and convexity of resource constraints, can impute shadow prices (Lagrange multipliers) to the resources. This program, despite firing the imaginations of many talented economists, has never managed to disentangle the problems of aggregation of disparate preferences, treatment of time and information, definition of resource limitations, and dynamic stability of market clearing that are inherent in carrying it out to arrive at the robust, unified, and transparent account of distribution it sought. Economics owes a particular debt of gratitude (which it shows precious few signs of recognizing, to tell the truth) to Sraffa and his followers for their dogged insistence on bringing to light the ramifying incoherence of this marginalist project.

The basic point at issue in the critique of the marginalist program, as Garegnani rightly insists, is whether the theory of rent can coherently be extended from the pricing of inherently scarce unreproducible resources, such as land, to produced capital inputs and to human labor, which differs from both land and capital in entering capitalist production as the result of complex social and historical developments. One issue here is what boundary conditions it makes sense to impose on an abstract model of the economy. There is little debate that it is appropriate to represent land as being quantitatively fixed, and hence inelastically supplied, as indeed Smith and Ricardo argued. It is not easy to see how to compress the complex process by which labor reproduces itself as an input to capitalist production into a tractable mathematical boundary condition, but the notion that the wage is given to the system is surely just as plausible as the notion that the endowment of labor is given, however inadequate either formulation may be as a representation of reality.

Capital inputs present other problems. Their production is governed by economic calculation, but because of their durability this calculation inevitably involves dealing with time and uncertainty. Furthermore, technical change constantly alters the specific types of capital goods that are used. At the abstract theoretical level the economist is not much interested in the specificity of capital goods, but rather in the general principles that govern the production of produced inputs. The early neoclassicals, such as John Bates Clark, hoped to finesse this problem by treating the value of capital at the system level as analogous to the value of capital to the individual firm, despite the fact that the firm can reasonably be assumed to take market prices as given, while the economic system as a whole cannot. Later, John Hicks, Paul Samuelson, Lionel McKenzie, Kenneth Arrow, and Gerard Debreu elaborated various models (see Arrow and Hahn, 1971) in which it is possible to keep track of an arbitrarily large set of capital inputs to production, and to find equilibrium prices for each of them through the method of imputation. It is, however, impossible in these general models to prove any strong theorems relating the own rates of return of capital goods to the value of the capital stock, or the demand for labor to the wage. It has taken some time for this point to sink in among the neoclassical enthusiasts.

Paul Samuelson revisits this issue in his contribution to the current volume.[3] Samuelson here reports his belated recognition that there is no general monotonic relation between the interest rate and sustainable rates of social consumption as a result of pondering Sraffa's arguments, and acknowledges his earlier errors ("Yes, Homer nodded twice"). This is a rather backhanded way to compliment Sraffa's work. Samuelson frames the issue in terms of the primal problem of comparing growth paths, while Sraffa carefully confined his analysis to the dual system of prices and input prices.

The Cambridge capital debate centered initially on one aspect of this tangle of confusions, the neoclassical hope that the value of capital goods would somehow behave like a single scarce input in equilibrium. The interchanges of this debate, including an important paper by Garegnani (1970), showed unambiguously that the neoclassical construction would work in general only under assumptions on production so stringent as to amount to the assumption of a single capital good. Other work related to the Cambridge debate, for example that of Luigi Pasinetti (1974) and Stephen Marglin (1984), also underlined the other side of Sraffa's critique, the necessity of taking some distributional variable, either the wage or profit rate, as the boundary condition in production models, rather than the stocks of individual capital goods. This path clarifies the real relationship between input prices and marginal productivities (if indeed they exist), which is that input prices determine marginal productivities through the cost-minimizing choice of technology.

Neoclassical economists have a hard time keeping their minds clear on this point. They are distracted by the fact that it is possible to embed a Sraffian production system in a general equilibrium model with given stocks of inputs and calculate equilibrium prices and rates of return without any reference to the value of capital goods. They read this model as supporting the scarcity theory of profit rates, although in fact it only reproduces its own assumption of the full employment of all inputs except those that have zero prices in equilibrium. The equilibrium allocation can equally well be viewed as one in which given input prices determine cost-minimizing choices of technique that happen to be com-

[3] The presentation of Samuelson's paper, which is printed with several easily correctable errors, is a lamentable exception to the general high level of editing in this volume. For the interested reader, the second paragraph on p. 233 seems to be intended to read:

> Indeed, in the single most manageable model of the mathematical graduate seminar — in which $Q(t) = C(t) + dK/dt = F[\text{labour}(t), \text{land}(t), \text{capital}(t)] = F[L(t), A(t), K(t)] = L^{2/3}A^{1/6}K^{1/6}$ — capital can be virtually treated like any other input. For such a $F[L,A,K] \equiv F[V_1,V_2,V_3]$ with real returns $[w_1,w_2,w_3] \equiv$ [wage rate,rent rate,interest rate], marginal productivity does apply: $[w_1,w_2,w_3]$ does equal $[\partial F/\partial V_1, \partial F/\partial V_2, \partial F/\partial V_3]$ and duality theory easily deduces a converse factor-price frontier in w_1,w_2, *and* the interest rate! Singularly in this special model, r happens to behave just like the primary wage and rent rates!

Samuelson's intended meaning is clear despite the potentially confusing change in notation from L,A,K to V_1,V_2,V_3 in midstream.

patible with arbitrarily given supplies of inputs. In the absence of a compelling dynamic theory that shows how the market might find these prices, which neoclassical theory has pretty well given up hope for, Sraffa's critique carries the day.

It would have been a good thing if the Cambridge capital controversy had managed to drive a stake through the heart of the scarcity theory of the rate of profit, but it hasn't. I think this has been because the classical critique tells economists what they shouldn't do (assume full employment and market clearing in order to determine input prices) but doesn't tell them very clearly what they should do as an alternative, either at the purely theoretical level or in econometric studies.

4. The Baby

Garegnani's development of the classical critique puts great emphasis on the crucial role that the mistaken notion of a capital aggregate plays in neoclassical theory. Garegnani traces essentially all of the inadequacies of neoclassical theory to the problem of capital. In his view, the assumption of an aggregate capital is the only conceivable path to the results the neoclassicals pursue, particularly the scarcity theory of the profit rate and distribution. If an aggregate capital existed, the neoclassicals could reconstruct a downward-sloping demand schedule for labor, which would bolster their assumption of full employment; they could coherently link the demand for investment to the interest rate, which would bolster their assumption of Say's Law; and the problems of the non-uniqueness and dynamic instability of general equilibrium would be mitigated. Garegnani is not so much concerned about the income effects that complicate the dynamics of market-clearing equilibrium, even in models of pure exchange, on the grounds that these are second-order issues that could be dealt with by separating the analysis of changes in the composition of output through demand from the determination of long-period equilibrium prices. In this respect, Garegnani seems to accept more of the neoclassical program than some other critics, such as Marglin, who argue that it is wrong-headed, even granted the strong assumptions necessary to support a capital aggregate.

However, it seems to me that it is impossible for the classical tradition to eliminate the value of the capital stock and of investment as central theoretical concepts. We can surely do without the scarcity theory of distribution, but I doubt that we can do without the value of capital. This does not necessarily contradict Garegnani's position, since the value of capital can play an important role in the theories of economic distribution and growth, a fact that does not depend on its representing a capital aggregate in the sense required by neoclassical theory.

To begin with, the classical political economists regularly use the concept of the value of capital in their reasoning about distribution and growth. Smith refers frequently to "stock", meaning the value of capital, and uses the concept to frame important and fundamental classical propositions, for example, the notion that the rate of profit declines both in individual sectors and in the economy as a whole with the accumulation of stock. Ricardo phrases his account of accumulation leading to a stationary state with the conversion of surplus into rent in terms of the growth of the value of capital, and the resulting expansion of employment opportunities. Marx, who is perhaps better regarded as a critic of classical political economy than as one of its exponents, but nevertheless contributes important ideas to the classical tradition, develops his theory of the profit rate and its dynamics in terms of the value of capital, using concepts such as the "value composition of capital", the ratio of the value of flows of non-wage capital outlays to the wage bill. It is diffcult to be faithful to the reasoning of the classical political economists without allowing a central role in the theoretical story for the aggregate value of capital.

However, the issues that require economists working in the classical tradition to employ the concept of the value of capital go beyond the question of an accurate understanding of the history of economic thought to the application of classical ideas to contemporary problems of political economy. If classical economics is to develop further as a usable tool for the analysis of current economic problems, which I believe is a desirable goal, it has to link its concepts operationally to available statistics. It is impossible to find reliable and detailed statistics on the individual capital goods of most countries. Leontief's input-output statistics, which are the most widely employed attempt to sectorize and disaggregate macroeconomic data, are available only for some countries in some years, and in any case depend on theoretical assumptions that are very strong to support its system of disaggregation. The essays in this book remind us that the classical economists eschewed "subjective" explanatory factors, such as preferences, in their arguments in favor of "objective" observables, such as the actual composition of output. But the value of capital is, in fact, one of the most objective of observables available to describe the macroeconomic state and evolution of modern economies. I would argue that it is more directly observable than any facts about stocks of particular capital goods, which can be constructed only by making heroic assumptions to collect complex productive facilities into manageable categories. The fundamental issue in the study of economic growth is the relation between aggregate investment and the transformation of economic production, an issue that can be attacked quantitatively only by employing the concepts of the value of capital and investment. Furthermore, the examination of aggregate macroeconomic statistics on the value of capital reveals strong regularities and patterns that are highly suggestive of precisely the dynamics suggested by the classical political economists and Marx (see, for example, Duménil & Lévy, 1994 and Foley & Michl, 1999).

The critical demonstration that the value of capital cannot coherently be used as a capital aggregate to support the scarcity theory of distribution must not distract classical economists from formulating better theories of stability, distribution and growth in which the value of capital will have to play a major part.

5. Whig Economic History

The classical view of the history of economic thought, to which Garegnani's work has made a major contribution, has been a salutary antidote to some neoclassically-oriented scholarship which reads the classical political economists as defective precursors of neoclassical orthodoxy. Thus, for example, Smith's conception of the Invisible Hand, which in Smith is the tendency for the rate of profit on a nation's aggregate capital to be maximized by the attempts of individual capitalists to maximize the rate of profit on their own capital, is often represented as an early and incomplete version of the First Welfare Theorem of neoclassical economics, which states that, under full information and the absence of externalities, a competitive equilibrium is Pareto-efficient. The fact that Ricardo used marginal reasoning to establish his theory of differential rent is taken to indicate that he had in mind, but could not flesh out, a general equilibrium system in which the principle of rent governs all pricing and distribution. Heinz Kurz traces part of the story of the development of the concept of rent into a general concept of marginal productivity in a careful examination of the work of Friedrich Hermann and Johann Heinrich von Thünen. This paper illuminates the earliest stages of economic theorists' confusion over the applicability of the theory of rent to capital. The modern classicals do a major service in insisting on the integrity of the classical political economists' systems of thought, and their divergence from many of the dogmas of neoclassical theory, such as the insistence that unemployed resources must have a zero market price. Bertram Schefold shows the risks in reading back a theory of marginal utility into classical and pre-classical discussions of use-value in a learned essay that outlines the complexity of the development of the category of use-value from Aristotle to Savary.

However, the defense of the past against illegitimate appropriation carries with it the risk of a counter-appropriation. In their zeal to protect Smith and Ricardo from the "Whig" tendencies of neoclassical historians of economic thought (who don't seem as eager to appropriate Marx to marginalism), the modern classicals verge on another fallacy: the claim that the classical political economists had a complete, well-worked out method of inquiry into economic phenomena different from, but on the same level as, 20th-century neoclassical economics. The temptation to make this claim is the need for the modern classicals to respond to neoclassical challenges to make their method and models explicit in contemporary mathematical economic language. What the neoclassicals claim to want is the formulation of a classical alternative at the same level of mathematical specification as, say, the 20th century's mathematical reconstruction of Walras' general equilibrium theory. Garegnani and other modern classicals are aware of the pitfalls involved in trying to meet this challenge, and have attempted to respond to them with a series of subtle methodological distinctions. The process of competition among capitals that tends to bring about profit rate equalization, according to them, can be modeled at a level of mathematical explicitness comparable to the neoclassical general equilibrium analysis; but the fundamental determinants of distribution, either the wage or the profit rate, and of the composition of social output, lie in a different methodological realm, and must be explained on different principles and with different methods. There are things to be said both in favor of and against this methodological position, but it is not at all clear that it would be recognizable to Smith or Ricardo (or Marx), who seem rather to have thought they were generalizing or abstracting from real economic phenomena, not proposing models in the contemporary methodological sense at all. Thus, I doubt that Ricardo had any strong allegiance to the methodological proposition that the composition of social output was, in fact, determined at a different level from the equalization of the profit rate. On the whole, he seems to have thought that the kernel of his results simply did not depend much on the composition of social output, and therefore did not focus his attention on it. On the other hand, the modern classical position seems to underemphasize the explicit theorization of the wage rate as arising from a subsistence standard of living determined by the fertility behavior of workers in Malthus and Ricardo. This theory was probably wrong, but it depended on much the same type of reasoning in terms of tendencies and feedbacks that are at the heart of the classical account of capital mobility and profit–rate equalization.

The acuity and ambiguity of the classical political economists make their works a rich source of questions and insights for contemporary economics. Many influential and important theoretical advances of 20th century economics, including national income accounting, the theory of general equilibrium, Sraffa's reconstruction of the classical theory of profit–rate equalization, Duménil and Lévy's theory of induced technical change, and much of contemporary growth and trade theory, have their roots in a critical response to the classical political economists. Fernando Vianello's contribution to this volume, for example, explores Smith's conceptualization of social and economic accounting. Sergio Cesaratto surveys recent endogenous growth theory and finds it wanting compared to its classical roots. The modern classical school, in turn, emerged from Sraffa's critical response to the marginalist, mathematical, and statistical methodological movements in 20th century economics. Given the prestige of the classical authors, there is a natural tendency for contemporary schools of thought to enlist them as allies in contemporary debates. The classical school, having properly brought tendentious neoclassical misreadings of the classical political economists to book, will flourish best, I believe, on the merits of its own positive theoretical positions without modernizing its classical ancestors.

6. Classical Mathematical Economics

One of the most fertile areas of research spawned by Sraffa's work is the study of the static and dynamic properties of linear production systems. The problem here is that Sraffa's simplest square no–joint–production system, in which there is one process to produce each commodity, serves admirably as a counterexample to neoclassical notions of the value of capital as a substance independent of particular capital goods, but is inadequate as the foundation of a general theory of competitive prices. An adequate model has to comprehend the possibility of alternative processes for the production of commodities, a generalization that Sraffa himself undertook, and the less tractable issue of joint–production processes that produce more than one commodity. Joint production is of some importance in itself, but appears to be crucial to the general modeling of long–lived capital: in general, one–period old machines of a given type must be priced as a different commodity from new machines of that type and analyzed as joint products of the process along with final output. In the no–joint–production setting, Sraffa was able to establish certain critical results: weak sufficient conditions under which there is an interval of profit rates, including zero, at which non–negative profit–rate equalizing prices exist (taking the wage bundle as numeraire); the invariance of these prices to changes in net output; and the fact that these prices are non–decreasing functions of the profit rate in the interval of relevant profit rates. If these properties could be generalized to models with joint production, the Sraffa approach would be a strong candidate for a positive general model of pricing and capital. Without this generalization Sraffa's work still constitutes an unanswered criticism of neoclassical theories of capital, but cannot itself fill the need for a general positive theory of capital. We know, however, from counterexamples, that the crucial properties cannot be proved in a general model allowing for joint production.

The project of classical mathematical economics in the hands of such able practitioners as Bertram Schefold (represented in this volume by an essay on the history of economic thought), Neri Salvadori, Ian Steedman, and Garegnani, has become the investigation of the subtle questions of exactly what class of production models will support which of the crucial propositions, and how far the propositions can go wrong in the general joint–production case. Salvadori's essay in this volume shows that the basic propositions can be proved in the context of a "fixed–capital" model in which machines can be transferred between sectors as long as the efficiency profile of the machines is uniform. Steedman ingeniously anatomizes the dependence of prices on the profit rate in a square production system through the analytical device of "vertically integrated" production sectors.

The mathematical power and elegance of these contributions, however, leaves the basic dilemma of the modern classical research program unresolved. My own opinion (see Foley, 1985) is that the general resolution of these issues has to involve the possibility of zero prices for some commodities and the dependence of prices on the composition of net output. This approach allows for a generalization of Sraffa's fundamental results in terms of a correspondence between the profit rate and a set of prices at which employed processes have equal profit rates and processes that fall short of this profit rate are not in use. The skepticism of the modern classical school about these compromises threatens to become a stumbling block to the future development of its theoretical foundations.

7. Keynes and the Rate of Profit

Sraffa came to Cambridge under Keynes' sponsorship, and his practical knowledge of economic history and institutions, analytical perspicacity, and insight into the foundational issues of economic theory appear to have been highly prized by Keynes and his circle. This friendly encounter of two of the giants of twentieth century economics held enormous potential for breakthrough or disaster. Unfortunately Keynes and Sraffa, despite their mutual goodwill, seem to have largely talked past each other without, given the peculiarities of their temperaments, recognizing how great a gap separated their visions, and disaster has on the whole predominated. Despite his willingness to jettison Say's Law and to countenance the idea of equilibrium unemployment, which echoed Marx's idea of a reserve army of labor, Keynes persistently maintained other positions, such as the notion that the economy would always find itself on a declining marginal productivity curve of labor, and that the "long run" was of no interest, which were sharply at odds with Sraffa's critique of the marginal productivity concept and focus on long–period equilibrium prices. Nonetheless, there are signs of some tentative attempts at dialogue between Sraffa and Keynes. There exists a draft of parts of the *General Theory* in unmistakably Marxian technical language; and Keynes' arguments for the choice of a "wage–unit" for accounting and rejection of the need for indices of capital and aggregate real output suggest conversations with Sraffa. Sraffa, in turn, introduced into his *Production of Commodities by Means of Commodities* (Sraffa, 1960, p. 33) the suggestion that the distributional parameter of the system might be a profit rate determined by the complex of interest rates engineered by a central bank, which sounds like a version of the monetary theory of the rate of profit put forward by Keynes in the *Treatise* (Keynes, 1930) and *General Theory* (Keynes, 1936).

Garegnani took up this idea and his work inspired a school of macroeconomic analysis that aspires to unifying the Keynesian and Sraffian points of view. The attractions of this theoretical merger are many: Sraffian theory could provide the Keynesian tradition with the long–run theory of growth and development it lacks, while Keynesian theory could provide Sraffian economics with immediate policy relevance and analysis. Like some corporate mergers, however, this marriage has proved harder to make work in practice than it looked in prospect. Too many elements of the two methodological and theoretical traditions clash: Keynesian economics' characteristic short–run time perspective, Marshallian conceptual roots, and focus on psychological determinants of economic activity, including subjective expectations, fit ill with the long–period time perspective, Marxian conceptual roots, and rejection of "subjective" and "unobservable" explanatory factors of the Sraffian tradition. While the fundamental idea that interest rates determined by monetary policy have, in some way, to be articulated with the profit rate is an undeniably important question, the project of establishing the causal primacy of money interest rates in the chain bristles with formidable diffculties. One is the need for a coherent theory of money prices and inflation rates, which neither school has so far provided, since central bank policy influences nominal rather than real interest rates. Another is the difficulty in articulating explicitly the market forces that would translate interest rate levels into changes in the wage, which, Sraffa's theory tells us, is a necessary concomitant of a change in the profit rate.

Edward Nell investigates one path to a classical–Keynesian synthesis in which the rate of growth consistent with investment demand determines both the profit rate and savings rate. Carlo Panico's essay in this volume makes an interesting attempt to construct such an alternative by combining the Kaldor–Pasinetti theory of distributional shares determined by the exogenous growth of investment demand with the Keynes–Sraffa–Garegnani notion that the profit rate is determined by monetary policy through its influence over nominal interest rates. Panico examines the impact of state finance on the link between profit rates and growth rates through the "Cambridge equation", and its impact on Pasinetti's theorem connecting the growth rate to the highest savings propensity in an economy.

8. The Empty Chair

The concentration of the classical critique on issues in the history of thought and abstract capital theory may be one of the reasons for its failure to command more interest among even sympathetic neoclassical economists. What difference, thinks the pragmatic mainstream economist under pressure to deliver yet another paper on, say, the impact of central bank independence on economic growth using the general framework of the Solow model, does the capital theory critique make to my research issue? Even if this imaginary scholar feels some discomfort at the charge that "Whig" interpretations of the history of classical political economic thought give an unacceptably distorted picture of Ricardo's method of analysis, he or she is probably perfectly comfortable leaving that to the historians of economic thought to sort out. (Of course the same scholar may vote the next day in a Department meeting to replace the retiring historian of economic thought with an experimental economist.) As is often the case, the response of many economists on glimpsing the complexities at the heart of the theory of distribution is to lose interest in the topic in favor of research areas that promise simpler, sharper, less ambiguous, and more exciting results.

On occasion when complacent incumbents in U.S. state gubernatorial and senatorial elections have refused to meet their opponents in debate, the challengers have resorted to the dramatic tactic of debating an empty chair. Although neither labor nor capital may ultimately be scarce for the capitalist economy, the revival of classical political economy faces a definite scarcity of scholarly opponents willing to show up for debate. Frank Hahn's (1982) *Cambridge Journal of Economics* paper, in which we can hear the author almost audibly sighing with the exasperation of having to explain the elementary principles of general equilibrium theory once more to a stubbornly unreceptive audience, is pressed into service repeatedly, but not in the end very adequately, as the statement of the neoclassical position. Petri's essay anatomizes Hahn's paper to identify the crucial points at issue in the modern classical critique of the neoclassical position. The classical advocates have, in fact, very well absorbed the methods and argument of general equilibrium theory, as the papers by Fabio Petri and John Eatwell & Murray Milgate in the present volume demonstrate. (In fact, given the waning interest in general equilibrium among mainstream theorists, I suspect that the greater part of original intellectual effort in general equilibrium over the past ten years may, in fact, have been expended by its classical critics.)

Empty chairs offer little in the way of rebuttal and counter-critique, which are vital to the critical development of a theoretical position. Even when neoclassical economists direct their attention to the classical critique, it is usually with the aim of explaining to the classicals their errors in misunderstanding neoclassical doctrine, rather than constructively engaging classical theory on its own ground. The neoclassicals, of course, believe that they have solved the problems of price determination and distribution in full-information, competitive economies once and for all, so the only advice they have to offer the classicals is to abandon their quaint insistence on an outdated and obsolete method and to accept the general equilibrium framework. The resulting dialogue has done little to develop mutual understanding of participants' respective positions and much to harden them. The exponents of the classical position in this volume are unerring in their ability to identify the problems they see with neoclassical positions, but spend less energy on examining possible problems with the classical alternative, even when the two schools' views have similar structural weaknesses.

One criticism leveled at neoclassical general equilibrium theory by its non-classical critics is that it has come to depend excessively on an axiomatic deductive method of analysis of economic reality, and has thereby lost touch with the inductive, empirical side of the scientific method. The classical critique, as represented by this book of essays, avoids this issue, presumably because the classical theory, as developed here, is equally deductive and non–empirical. For example, the equalization of the rate of profit across sectors of production through competition of capitals is the theoretical core of the classical vision. This is presumably, at some level, a real tendency that should show itself in empirical data. We might expect the classical economists to be the world's experts on the measurement and empirical analysis of this process, but no trace of this work appears in this volume, nor even a reference to the empirical literature on the topic (much of it carried out in a Marxist framework).

The contributions to this volume, especially the essays of Roberto Ciccone and John Eatwell & Murray Milgate, zero in on the methodological problems with the concept of attained equilibrium price that is implicit in general equilibrium theory. The point made here, which is important and persuasive, is that it is unreasonable to imagine that markets can reach equilibrium prices instantaneously and costlessly, as neoclassical general equilibrium theory assumes. The authors argue vigorously and persuasively for the methodological superiority of the classical approach, in which market prices are acknowledged to fluctuate ceaselessly around natural long–period prices. However, the classical long–period approach involves a similar "dual" assumption about speeds of adjustment, that the composition of capital stocks will adjust rapidly and relatively costlessly to its long–period position. Petri and Ciccone discuss this assumption. Many of the same arguments that tell against the neoclassical attained equilibrium view of prices might be directed at the classical long–period composition of capital view as well.

The equalization of the rate of profit that is at the heart of Garegnani's reconstruction of classical economics depends on the conception of the economy as divided into distinct "sectors", between which capital can flow to equalize profit rates. Each of these sectors is identified with a distinctive natural price. This is a fertile and suggestive idea, but it raises serious analytical questions that the classical literature, at least as represented in this volume, has not addressed. In contemporary economies it is notoriously difficult to assign many firms to any one sector, since firms often produce a wide range of products, and the products themselves can function in many different ways. The construction of Wassily Leontief's input–output tables and the efforts of national statistical agencies to order firm data into sectoral hierarchies underline the fact that sectorization is an abstraction, and like all abstractions, raises problems in operationalization.

These observations, which I offer in the spirit of constructive criticism of the classical position, are not the only ways in which classical conceptions in Garegnani's reconstruction might be developed. I doubt, however, that the classical school will make much headway in its struggle with neoclassical error unless it broadens the range of issues it confronts beyond the history of economic thought and the abstract methodological critique of neoclassical general equilibrium theory.

TRIBUTE

This volume is a Festschrift for Pierangelo Garegnani. Garegnani's fate has been to carry on the heterodox tradition of economics in a period when it has suffered relative decline in the face of a habitually complacent, increasingly powerful, and unremittingly hostile hegemonic orthodoxy based on discredited but uncritically accepted neoclassical doctrines. The papers in this book testify to the intelligence, critical honesty, and tenacity with which Garegnani and his associates have faced this thankless duty. The futures of economics (if it has one in its present disciplinary form) and, more important, of our understanding of the deep and inward processes that shape capitalist economic development, are much brighter for their efforts.

REFERENCES

ARROW K.J. and HAHN F. (1971), *General Competitive Analysis*, San Francisco, Holden–Day.

DUMÉNIL G. and LÉVY D. (1994), *The Economics of the Profit Rate*, Brookfield, Vermont, Edward Elgar.

FOLEY D.K. (1985), "On prices of production in a general model of production", *Contributions to Political Economy*, 4, pp. 25–36.

FOLEY D.K. and MICHL T.R. (1999), *Growth and Distribution*, Cambridge, MA, Harvard University Press.

GAREGNANI P. (1970), "Heterogeneous Capital, the Production Function and the theory of distribution", *Review of Economic Studies*, 37, pp. 407–436.

HAHN F. (1982), "The neo–Ricardians", *Cambridge Journal of Economics*, 6, pp. 353–374.

KEYNES J.M. (1930), *A Treatise on Money*, London, Macmillan.

— (1936), *The General Theory of Employment, Interest and Money*, London, Macmillan

MARGLIN S. (1984), *Growth, Distribution, and Prices*, Cambridge (MA), Harvard University Press.

PASINETTI L. (1974), *Growth and Income Distribution: Essays in Economic Theory*, Cambridge, Cambridge University Press.

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

Professor Foley and Classical Policy Analysis

Pierangelo Garegnani*

1. I must first of all say how welcome are Professor Foley's well informed views and stimulating comments on what I have elsewhere called "the theoretical world of the old classical economists". I will here be concerned with three issues among those he raises in his review article, namely:

a) the structure of classical theories and the existence of an analytical "core" in them;

b) whether in the classical approach to distribution and prices the conception of capital is subject to the same critique raised against the notion of a "quantity" of capital in neoclassical theories: this will require some clarification of the critique itself;

c) the implications of the classical approach for policy analysis.

1. Surplus Principle and "Core" in the Classical Theories

2. Foley (2004) describes as follows the question of a "core" in the theories of the old classical economists:

> The process of competition among capitals that tends to bring about profit rate equalisation, can be modelled at a level of mathematical explicitness comparable to the neoclassical general equilibrium analysis, but the fundamental determinants of distribution, either the wage or the profit rate, and of the composition of social output, lie in a different methodological realm, and must be explained on different principles and with different methods" (p. 14),

and he then comments

> there are things to be said both in favour of and against this methodological position, but it is not at all clear that it would be recognizable to Smith or Ricardo (or Marx).

My answer is that the position would be as recognizable to Smith, Ricardo or Marx, as would the notion of a surplus determination of the non–wage distributive variables — their ultimate determination, that is, as the *difference* between the social product and what has been necessary for that production, in

* University of Roma Tre.

particular real wages, broadly identified with workers' subsistence. I think it has not always been made sufficiently clear that the notion of a "core" in classical theories is but an aspect of that "surplus" interpretation of them[1] which, undisputed in the case of Quesnay, is also widely if not universally acknowledged for Ricardo (often in the garb of the so called "corn model") as well as for Smith,[2] not to mention Marx who attempted to again give to the surplus determination the direct form it had in Quesnay.

Indeed, to ultimately determine the non–wage shares of the social product as the above difference is non–circular only to the extent that product and wages can there be taken as given in that determination.[3] And since no economist, least of all Smith and Ricardo, can avoid being concerned with explaining product and wages, their role as givens could only mean that those magnitudes were determined *separately* from the non–wage distributive variables and prices. This means that in those economists there was *in effect* a purely quantitative "core" of the theory where product and wages appeared as givens and where profits and/or land rents were determined as a residuum together with relative prices. And taking as data in that determination both the product, and the wages, implied taking as data the technical conditions as well, on which the labour required for that production (and, of course, the replacement of the means of production) depend. We may then say that those three sets of circumstances were "intermediate" data, separating out from the rest of the theory a "core" in which the non–wage distributive variables and the entailed relative prices could be determined.

3. But *why* these "intermediate data"? Why not go from the circumstances determining them, directly on to the non–wage distributive variables and to relative prices, as happens in neoclassical theory, where tastes and factor endowments determine outputs and wages *simultaneously*, therefore, with prices and profits?

The answer lies essentially in the kind of circumstances that were seen to determine the division of the product between wages and profits, and in the resulting distinction between two fields of analysis requiring different methods of inquiry. In one of them, free competition and the corresponding tendency to a uniform rate of profits, and uniform wages and rents, establish quantitative relations between distributive variables and between them and prices, which are simple enough to allow for deductive methods in their study.

But with respect to wages — as made clear by the explanation of them on the basis of a historically determined subsistence — and also for outputs, the purely deductive methods of the "core" could not be applied at the level of uni–

[1] See Garegnani, 2002a, pp. 243–44.

[2] On Smith as a surplus theorist cf. *e.g.* Blaug, 1987, p. 439.

[3] Cf. *e.g.* Marx's passage on the wage quoted in par. 6 below.

versality appropriate to *theory*: they could only be used for what we would today call *models*, dealing, that is, with special problems or special assumptions (think *e.g.* of Marx's treatment of outputs in his "reproduction schemes"). The attempt to fully quantify relations as complex and variable as those affecting wages and outputs would entail introducing, in the apt words of Edgeworth, "arbitrary functions representing not merely not numerical knowledge but ignorance" (Edgeworth, 1881, p. 4). And the irreversibility of most of these relations makes it difficult even to conceive of their representation as functions.

This distinction between two fields in the theory implied in effect a view of the nature of economic phenomena basically different from that of later theory. The deductive, purely quantitative relations of the "core" — which later theory attempted to extend to the whole of economics by means of demand and supply functions resulting ultimately from substitution between "productive factors" — were instead woven by the old classical economists into an analysis conducted by the more inductive methods appropriate to those "intermediate data", for whose determination institutional and historical factors played a central part. A reading of the *Wealth of Nations*, or of Marx's *Capital* makes clear what we mean.

It is in this sense that the notion of a "core" is one thing with the surplus determination of non–wage shares. It provides the answer to Jevons's criticism of Ricardo for which

> the doctrine that if wages rise profits must fall, [drawn from the equation]
> Production = Profits + Wages
> [is] radically fallacious: it involves the attempt to determine two unknowns from one equation" (Jevons, 1871, p. 269);

clearly the doctrine is not fallacious if "Production" and "Wages" can both be taken as independent variables, *i.e.* be determined separately from that "core" of the theory which the equation represents.

4. However, Foley (2004) specifies his doubts on the attribution of that "core" to the classical economists: he does so with respect to the role of wages and outputs in the analysis. On wages he writes:

> the modern Classical position seems to underemphasize the explicit theorization of the wage rate as arising from a subsistence standard of living determined by the fertility behaviour of workers in Malthus and Ricardo. This theory [...] depended on much the same type of reasoning in terms of tendencies and feedbacks that are the heart of the Classical account of capital mobility and profit–rate equalization" (p. 14, my italics).

Foley seems to think here that (a) the demand and supply of commodities to which the Classical economists appealed for the profit rate equalization of their theory of "market prices", were also broadly applied to labour and, therefore, that (b) wages could hardly be supposed to have been *data* in a "core" where prices were instead *unknowns*.

I would not disagree with part (a) of the statement, if Foley were referring to *classical* demand and supply: these however only explain the *tendency* of actual prices towards natural or normal prices which are determined *independently* of such demands and supplies (as made clear by Smith in chapter VII of the *Wealth of Nations*). The fact that broadly similar demand and supply apply to labour[4] — and here we turn to part (b) of Foley's statement — does not therefore mean that they determined the wage any more than they determined the natural prices of commodities. Foley's "tendencies and feedbacks" would contradict the treatment of wages as an "intermediate datum" only if Smith's and Ricardo's demand and supply were interpreted along the lines of *e.g.* Samuelson (1978) "Canonical classical model", of a demographic specification, that is, of the neoclassical functions determining the wages simultaneously with prices. The admission of labour unemployment — think of Ch. XXXI of Ricardo's *Principles* — should suffice to rule out any such demand and supply mechanism for Smith's or Ricardo's wages. The attribution to those authors of some version of that neoclassical mechanism is however so widespread that it may deserve a brief closer consideration here in order to make clear the sense of Smith's or Ricardo's treatment of the wage as an intermediate datum.[5]

5. In effect recognising the role of the population principle in Smith and Ricardo in no way entails the above "Canonical" interpretation of their determination of wages. Indeed *if* we were to reason in terms of neoclassical demand and supply functions, the classical population principle taken by itself, independently, that is, of the principle of factor substitution generally admitted to be inexistent in those authors, could only contribute a rigid demand and a rigid supply of labour. It would thus entail as Samuelson (1978, p. 1423) admits, zero wages when population growth has overtaken capital accumulation; or zero quasi-rents on capital goods (*i.e.* a negative net return on the capital goods' supply prices) in the opposite case or, finally, indeterminacy of distribution between wages and interest when the two rates of growth happened to balance.

That is so unless a properly elastic neoclassical labour demand is arbitrarily attributed to Smith and Ricardo for each given level of accumulated capital: without that, Samuelson's (1978) or Hicks–Hollander's (1977) interpretations would not only obviously contradict, with such zero wages or zero rentals, or indeterminacy, anything to be found in classical texts: they would also fail to explain *in logic* the basis on which their interpretation claims to rest — *i.e.*

[4] On the classical treatment of demand and supply of labour as single quantities cf. *e.g.* Garegnani (2002a, pp. 247–50).

[5] The questions sketched in this and the following paragraph are considered more fully in Garegnani (2002a).

Smith's and Ricardo's argument about wages adjusting population growth and capital accumulation to each other.

But as already indicated, the admission of permanent labour unemployment at natural or normal wages by Ricardo, Smith or Marx makes it evident that no demand–supply mechanism and elastic labour demand function, can be found in those authors. And no confusion should be caused by the wages–fund theory in the form it took *after* Ricardo. Even less should doubt be cast by Smith and Ricardo's quite different notions of "demand" and "supply" of labour which we mentioned in par. 4. As suggested by those authors' references to the "proportion" of demand to supply, *single quantities* and not *functions* are meant there for labour (*e.g.* Ricardo 1951–73, I, p. 165), no less than they are for commodities. "Demand" for labour is the broadly defined given employment possible at the stage reached by the accumulation of capital, while "supply" is, equally broadly, the amount of population — the two providing some measurement of labour unemployment or underemployment, but certainly no schedules determining the "natural" or normal wage by their intersection. Current interpretations of Smith and Ricardo's theories of wages along neoclassical lines, cannot therefore rest on the classical principle of population: they have to ultimately rest instead on the tautological presupposition of the elastic labour demand function of the neoclassical mechanism which they would wish to trace there.

6. A question arises here: if it is not the interaction between supply and a properly elastic demand function for labour, what else can trigger off the wage rises that classical authors suppose when accumulation overtakes population growth, or the wage falls they suppose for the opposite case?

The answer, I believe, lies quite simply in that "comparative strength of the competing parties" to which Marshall will later refer in order to reject it.[6] As I argued elsewhere,[7] this is what emerges in particular from those elements and passages of Smith's and Ricardo's theories of wages which, significantly enough, have long puzzled modern interpreters — elements and passages which fall instead easily into place as soon as we undo the work done by Marshall, and abandon the attempt to interpret the "demand and supply" of Smith and Ricardo in terms of the demand and supply functions whether along wages–fund, or more strictly neoclassical, lines. The relative speeds of growth of population and capital, with the resulting changes in Smith and Ricardo's "proportion of the demand to the supply of labour" — *i.e.* the varying pressure on wages of labour underemployment — can indeed be seen to affect the "comparative strength of the competing parties", and to trigger off the variations in wages and consequent population adjustments as envisaged by the classical economists, without passing at all through labour demand and supply *functions*.

[6] Marshall 1920, Appendix J.

[7] Cf. Garegnani 2002a, pp. 248–9

We may now understand better how the classical "tendencies and feedbacks" of Foley's passage above were in fact compatible with the wage treated as an "intermediate datum" in dealing with profits and prices: it was separately determined by the circumstances affecting the subsistence level on the one hand, and, on the other, by those causing sufficiently persistent divergences from that level and consisting, essentially, of the state of the balance between population and accumulation. It was in that sense that *e.g.* Marx could write:

> the foundation of modern political economy [is] the conception of the value of labour power as something fixed, as a given magnitude" (1905, p. 45).

I have discussed in some details Foley's passage on classical wages also because contrasting the classical with the neoclassical concepts of labour demand and supply illustrates well, I believe, the institutional and historical, and therefore inductive character of much of classical theory, as opposed to the deductive one of neoclassical theory.

7. But, we said, Foley (2004) specifies his doubts about a "core" in the classical theories also with respect to *outputs* treated as "intermediate data". He writes:

> Thus, I doubt that Ricardo had any strong allegiance to the methodological proposition that the composition of social outputs was, in fact, determined at a different level from the equalization of the profit rate. On the whole, he seems to have thought that the kernel of his results simply did not depend much on the composition of social outputs, and therefore did not focus his attention on it (p. 14).

What Foley writes here is, paradoxically the main argument for what I describe as the assumption of "given outputs" in Ricardo. Saying that Ricardo's essential results on distribution and prices "did not depend much on the composition of outputs" — which I take it to imply that they did not so depend *systematically*, as is the case in neoclassical theory — is the same as saying that prices and the residual distributive variable were determined separately ("at a different level", as Foley puts it) from outputs, and outputs were therefore treated as data or independent variables in that determination.

That treatment of normal outputs is on the other hand made clear by the equality of normal outputs with the respective "effectual demands", unambiguously taken as given by Smith (1776, bk. I, ch. VII) and by Ricardo. It is also more broadly evident from a perusal of Ricardo's *Essays on Profits*, or of his *Principles*, not to mention Smith's *Wealth of Nations*, or Marx's writings. It is indeed the treatment of prices *separately* from outputs, *i.e.* taking outputs as data when determining prices that forced Marshall to attribute to Ricardo the assumption of constant supply prices and then, in agriculture, where that attribution would have been obviously incorrect, the further assumption of an absolutely inelastic demand (the garb that a given output would take in neoclassical eyes).

8. A word of clarification may be useful at this point on a further question raised by Foley, but also in different form by other authors, in connection with the classical "core". Foley (2004) writes:

> Smith or Ricardo (or Marx) [...] seem rather to have thought they were generalizing or abstracting from real economic phenomena, not proposing models in the contemporary methodological sense at all (p. 14).

Yes, I would entirely agree, if Foley means that those authors were attempting the universal explanations of a *theory* and not exploring the logical implications of admittedly special assumptions as is done in today's *models* (cf. par. 3 above). But, in so doing, they would instinctively strive for consistency — and, operating as they did, within a science which had begun generating systematic treatments of the economy since at least Quesnay's *Tableau Economique* — their work would, we are legitimised to think, precipitate into something like a system. The question of a "core" in classical theory is not, that is, question of a *model* in the contemporary sense: it is a question of a *theory*, in which the authors do adapt instinctively to a reality in which the forces of competition, acting for the uniformity of prices and rates of remuneration, and reflected in the "core", are seen to operate within a framework where broader institutional and historical forces impose a different method of analysis (par. 3 above; cf. also Garegnani, 2002b, p. 397).

Of course, classical authors needed not have been fully aware of the terms of their system, no alternative analytical structure being there to impose on them recognition of their own. And it is to that lack of awareness that Foley (2004) seems to refer for doubting

> the claim that the Classical political economists had a complete well-worked out method of inquiry into economic phenomena different from, but on the same level as, 20th century neoclassical economics" (p. 13).

But the fact that Latin writers may have been only partly conscious of the rules they followed in expressing themselves, does not prevent modern grammarians from tracing such rules in their writings, no less than they can do for modern writers.

2. The Classical Approach and the "Quantity of Capital"

9. Let me now come to our second subject: capital. Professor Foley (2004) writes:

> it seems to me that it is impossible for the Classical tradition to eliminate the value of the capital stock and of investment as central theoretical concepts" (p. 11).

I agree: it clearly is impossible to explain distribution, prices, growth, without referring to produced means of production: however, the different analytical structure puts on its measurement in the classical theories conditions that are different from those for neoclassical theories, and which can be satisfied unlike what happens for the latter.

In order to deal however briefly with the issue we need first to specify better what is the question of "capital" we are referring to. Just because it provides parameters for the determination of prices (*e.g.* by expressing the technical conditions of production) capital has *ultimately* to enter that determination as a magnitude, or a set of magnitudes, which are independent of prices, and this is true both for classical and neoclassical theories. And when that independent measurement is achieved, the "value of the capital" of Foley's passage is no problem, since it can be obtained in the same way as the value of any other set of products.

The central point here is that, after the attempts of Jevons, Böhm Bawerk, Wicksell and others at defining an "average period of production" independent of distribution and prices, it has had to be recognised that an independent measurement cannot be effected by means of a *single magnitude*: it can only be effected in terms of a set of magnitudes, be they the physical quantities of the several capital goods of Walras or Sraffa, or the dated quantities of labour of Wicksell or Dmitrieff.

Now, the measurement of capital in terms of a set of magnitudes raises no logical difficulties in the classical theories. The independent measurement of capital is only needed there for expressing the technical conditions of production and a set of quantities can therefore be quite satisfactory. This is shown by the physical quantities of the several means of production of Sraffa's equations or, by the set of absolute periods of production of Dmitrieff and others. A measurement in terms of a set of magnitudes does not however do in neoclassical theory which ultimately needs the single magnitude for the reasons we shall now briefly recall.

10. As Foley well knows, the question of capital in neoclassical theory is both complex and controversial. It also is of basic, if not always transparent, importance. Thus, to Foley's "pragmatic mainstream scholar" asking what difference does the capital critique make in his research "on central banks and economic growth" for which he is using the Solow model (Foley, 2004, p. 18), my answer would be that it may ultimately make the whole difference as to whether growth is controlled, as Solow supposes, by the saving decisions of the individuals taken jointly with population growth and technical progress or, *e.g.*, it is the latter circumstances (some or all) that may be controlled by growth, in turn controlled by aggregate demand. Briefly, the controversy on capital turns on whether or not neoclassical analysis provides us with a valid theory of the economy.

The point from which we must start in order to understand the dependence of neoclassical analysis on "capital" as a single magnitude is that, for savers, heterogeneous capital goods are perfect reciprocal substitutes, in proportion to their values, as providers of the single commodity 'future income'. Unlike the demand for consumption goods, that for capital goods from savers results from preferences that are non–specific to the particular capital goods, and are only specific with respect to *values of aggregates of them*.[8] Hunger can be satisfied by corn and not by coal, but the demand for future income by savers can be satisfied by looms as much as by tractors or any of the other thousands of capital goods, whichever provides the highest return on its value. As Walras lucidly put it, savings are demand for the single commodity "future perpetual income" whose price is the reciprocal of the rate of interest (Walras, 1954, 274 ff.). And of course in neoclassical theory individual decisions about that particular commodity play a role in determining prices and outputs no less than decisions about the other commodities, so that its quantity, *i. e.* the quantity of capital, will have to appear in the system like the quantity of any other commodity.

It was this perfect substitutability of the capital goods for wealth holders, and the resulting "quantity of capital", that provided the basis of the attempt, at the end of the 19th century, by the main marginalist stream, to extend to the distribution between wages and profits, the classical principle of rent generalised to any number of "factors" (cf. Garegnani 1970, p. 407). Indeed, had it been question there of taking as factors the individual capital goods, the possibility of varying their proportions to labour and to each other, in analogy with the classical proportions of land to labour, would have been almost nil — and almost nil would have been the substitutability between factors, on which that neoclassical attempt relied. Alternative production processes differ by the *kind* of capital goods employed rather than by their *proportions* with labour and between themselves. The idea of a variability of factor proportions, and factor substitutability, rested therefore on taking the different kinds of capital goods as quantities of the same single factor "capital",[9] the single commodity "future income" demanded by savers.

No less importantly the same fluidity of the capital endowment taken as a single magnitude allowed for its physical composition adjusting to the equilibrium techniques and outputs, and thus to the condition of a uniform effective rate of return on the capital goods' supply prices[10] — as necessary for equilibrium prices as are uniform wages or rents for each quality of labour and land.

[8] Cf. Garegnani 2000, parr. 12, pp. 33–4.

[9] Cf. n. 12 below.

[10] The qualification of the rate of return as 'effective' is intended to take care of the case in which changes in relative prices over time are considered in the definition of the equilibrium and, accordingly, what is "effectively" a *uniform* rate of interest is expressed by *different* own commodity rates. (On the confusion which has frequently marred the capital controversy between, on the one hand, the divergence of the *effective* rates of return mentioned in the text, due to an arbitrary initial physical composition of the capital endowment and, on the other hand, the divergence of *own* commodity rates due merely to changes of relative prices over time, see Garegnani 2003, Appendix II, examining it in Hahn 1982).

Indeed these are the conditions which since Adam Smith's "natural prices" have been held necessary in order to determine (under competition) "normal prices" whose persistence would warrant a sufficient correspondence with *observable* prices.[11]

Thanks to the notion of capital as a single 'factor', but only thanks to it, the way appeared thus to be open for replacing with the desired generalisation of classical rent the historical and social factors (subsistence and population), to which the early classical economists had referred the distribution between wages and profits. Thus, in particular an inverse relation would have had to hold between rate of interest and quantity of capital employed with given quantities of other factors, analogous to that between (intensive) rent and proportion of land to labour. This would have ensured with regard to the forces originating from the production system, a uniqueness and stability of the distribution analogous to the classical one between rents and wages plus profits.

11. This dependence of neoclassical, but not of classical, analysis on capital as a single magnitude explains, on the other hand, why its abandonment, comparatively innocuous within the classical approach, was likely to entail a deep change in neoclassical theory and in its significance.

That abandonment in mainstream neoclassicism occurred as a result of the first phase of the capital controversies of the 1960's and early 1970's. The way to it had in fact been paved by Hicks in his *Value and Capital* (1939), when he came to realise the impossibility of proceeding with the conception of capital as the single quantity on which he had based his *Theory of Wages* (1932). The only conceivable alternative to that conception within neoclassical theory was however that used by Walras for his general equilibrium system, where each capital good constituted a separate factor: and the latter was the conception Hicks adopted. That conception is in fact the same which, we noted, hardly allows for the substitutability between 'factors' on which the theory rests, a circumstance which may well suffice to explain why that conception of capital despite its greater definiteness had failed to enter the neoclassical mainstream in the near three quarters of a century since Walras had advanced it. On that circumstance Hicks had to turn a blind eye in his revival of Walras's theory of capital.[12]

[11] Once the tendency of actual to normal prices is preliminarily established, the persistence of the latter allows for a repetition of transactions sufficient to let the normal price emerge as some average of the actual prices (on the question cf. Garegnani, 1976, pp. 26–9; also 2002b, section III.)

[12] It is interesting to note how Hicks, whose *Value and Capital* (1939) was to implant the Walrasian conception of capital in the neoclassical mainstream, would write in (1932b) that "[Walras's and Pareto's] theories of capital [...] are the last part of their work which one can consider as final, or accept without the most careful consideration [...]. For it is surely significant that Walras's elaborate theory of capitalisation does not reappear in the later work of Pareto, while there is nothing very substantial to take its place" (Hicks, 1932b, p. 297). Further, and more

But above all Hicks had to cope with the logical inconsistency of Walras, whose capital endowment as a vector was incompatible as we also saw with the condition of the uniform effective rate of return on the capital goods' supply prices, which Walras, like all his contemporaries and successors, had introduced in the system.[13] And Hicks coped with that inconsistency by simply dropping the condition and, with that, the notion of a persistent normal price allowing for a correspondence between theoretical and observable variables (par. 10). That notion, central to previous neoclassical authors was replaced with that of temporary or intertemporal equilibria dependent on future prices leaving a choice only between the Scylla of inexistent complete future markets and the Charybdis of indeterminate subjective expectations. From this incidentally and from the indefinite number of Walrasian factors replacing the traditional trinity, there also came the formalism Foley (2004, p. 6) rightly laments.

It is not surprising therefore that this overturning of the then accepted notions of capital and equilibrium exerted little influence, at first, in England and, in particular, in Cambridge still at the centre of economic theorising with the Keyneses, Pigous, Robertsons, Shoves. As we claimed above, it was only when the early phase of the capital controversy had swept away the traditional versions based on the "quantity of capital" and had left no other choice, that the Hicksian reformulation could become dominant — in the forms it had assumed in the meantime at the hands of Samuelson and other mathematical economists.

12. It can perhaps be now better realized how the abandonment of the notion of a single quantity of capital was in fact a momentous change in neoclassicism — a change which left the theory deeply different from what it originally was intended to be, marking what we may call a "Hicksian divide" in its evolution.[14]

The toll which the potential explanatory capacity of the theory has had to pay for this change may however appear now to have been paid in vain. As a cor-

specifically, in his *Theory of wages* (1932a) Hicks contrasted the marginal product of labour he was using then — obtained by keeping constant the "quantity of co-operating capital [but not its] form" (*ibid.*, p. 20) — with the corresponding "short period marginal product" obtainable on a Walrasian basis, where on the contrary, capital could *not* change its physical "form": he then curtly dismissed the latter because "it is very doubtful that this marginal product [...] can be given any precise meaning that is capable of useful application" (*ibid.*, p. 20–21). Although little noticed Hicks's about turn on a matter as basic as that, is I believe, a dramatic symptom of the depth of the crisis which neoclassical theory has in fact undergone because of the inconsistency of its original notion of capital. It also bears a striking witness to the ultimate causes of the "Hicksian divide" in neoclassical theory to be presently pointed out in the text (cf. Garegnani, 1976, p. 34 ff.).

[13] On Walras's inconsistency and the need to conceive the capital endowment as a single magnitude in order to satisfy that condition of uniformity of returns cf. Garegnani 1976, p. 34.

[14] It is indeed for the period following Hicks's *Value and Capital* that some authors have noticed a "Formalist Revolution", whose origin they see essentially in that book, though they seem in some difficulty in tracing its causes (cf *e.g.* Hutchison, 2000; Blaug, 1999).

rect understanding of the neoclassical problem of capital with its roots in the homogeneity of capital goods for savers, might well have suggested, the Hicksian reformulation has not eliminated the ultimate dependence of neoclassical theory on the notion of capital as a single magnitude: as we said, savers decisions, which, of course affect the system, are taken in terms of that 'quantity' which must therefore be present somewhere in neoclassical demand and supply system like that of any good demanded.

As is now beginning to emerge, that dependence, removed at the immediately visible level of the capital endowment, remains at the less transparent but more fundamental level of the savings–investment process. There, the inexistence of a consistent measurement of capital independent of prices entails, *e.g.*, the same consequences of multiplicity and instability of the equilibria and, more generally, of implausible results which had been pointed out in the traditional context of normal prices.[15]

3. The Classical Approach and Economic Policy

13. We may pass now to the third and last of the issues I listed at the beginning: the classical approach and policy analysis. Professor Foley writes that while the criticism of neoclassical theory identifies "fundamental weaknesses" in neoclassical theory, yet "a neoclassical mainstream continues to dominate teaching and research in economics in America and increasingly in the rest of the world". This continuance and increase in neoclassical domination might perhaps need some qualifications after what we saw in par. 12,[16] but we are here interested in Foley's explanation of that. He writes (Foley, 2004, p. 7):

> the market for analytical economics is fundamentally a market for policy analysis (...) [and] in committing methodological sin by putting the theories of price and distribution on the same level of abstraction, neoclassical practice gains the tremendous advantage of being able to make routine predictions about the composition of output and distribution"

and, continues, "despite the excellence and persuasiveness of its doctrinal critique and researches in the history of economic thought", little work has been done in the ambit of the classical resumption in the direction of "applied Classical political economy as an alternative to neoclassical practice" (p. 7).

It is undoubtedly true that, for the reasons we shall presently see, much work connected with the resumption of classical analysis has been on theory and, partly, on history of thought. We should not however risk missing what has

[15] Cf. *e.g.* Garegnani, 2000, parr 30–31, and 1970, pp. 425–6. Cf. also the Postscript below.
[16] Cf. n. 19 below.

already been done about policy, and generally applied work, by taking work in those fields as a near synonym of what it is for contemporary neoclassicists. The complexity of the economic world might indeed justify some *a priori* scepticism about the validity of any theory where "models" make, as Foley puts it, for "routine predictions".

In fact the contributions to policy analysis from the classical side have, I think, not been inconsiderable already, at least in laying what I believe is a firm basis for it. We may leave aside for the moment the role for policy and applied analysis which classical theory may be playing simply as an antidote to neoclassical straightjackets. But the implications for policy analysis should not have escaped notice when it was made clear how the critique of the neoclassical theory of the division of the product between wages and interest and the resumption of classical theory, entailed the relevance of aggregate demand for long run growth, and not only for short period fluctuations in activity.[17] The point is of basic importance in itself, but when solidly established in all its implications, it largely overturns dominant policy analysis.

14. Before coming to that we may however consider first how justified it is to say that the focus of the classical revival has *excessively* been on theory, in particular on its "core", and on the history of thought. I would argue that to the extent to which the focus has in effect been there, that has been only appropriate.

It is indeed only reasonable to suppose that when a theory is new to the profession as the classical one in fact is, its impact on policy analysis generally follows, and does not precede, some initial understanding and acceptance of the theory.[18] It was accordingly natural (when the choice was not predetermined as it often was by the need to answer criticism or dissipate misunderstandings) that scholars working for the resumption of the classical approach should concentrate first their efforts in directions which were susceptible of widening its understanding and acceptance in the profession.[19] And these directions quite

[17] Those implications were argued *e.g.* by the present writer in (1962) (see 1978–79, Part I, 335n), with reference to the question of "structural" labour unemployment in Italy. Work has been similarly done in the meantime on wages and growth: see *e.g.* Ciccone, 2003; Eatwell and Milgate (Eds), 1983; Garegnani, 1992; Palumbo, 1996; Stirati, 2001, section V; Trezzini, 1995; etc.

[18] Thus, the applied work that followed upon Keynes's *General Theory* would clearly not have occurred, without some broad acceptance of the critique of orthodox theory and of the developments contained in that book.

[19] Foley (2004, p. 18) refers to "the dramatic tactic of debating an empty chair" used in American politics, and to how "the revival of classical political economy faces a definite scarcity of scholarly opponents willing to show up for debate". That chair might perhaps appear less empty when we remember the agitation which followed the publication of the New Palgrave Dictionary (Eatwell, Milgate, Newman, 1987), or recall Arrow (1991), Samuelson (1987a), (1987b), (1990), (1998), (1999), (2000), or Hahn and Petri (Eds) (2003), etc. and recall also the debate on the interpretation of the Classical economists launched in the 1970's by, essentially, Hicks–Hollander (1977)

naturally were the three of the critique of the dominant theory, the investigation of the structure of the classical approach and of its implications (necessary also for clarification among the scholars directly involved in its resumption), and, last but not least, the establishment of the continuity with the work already done by the classical authors, from Smith to Ricardo and Marx. Not to give priority to these directions of work would have been accepting a subordinate role for an approach whose solidity had been tested through the authors just mentioned and was felt to have all titles to replace a deeply flawed dominant approach.

Work in those three directions entailed on the other hand focussing on the specific "surplus" features of the theory, and thus on its "core" as most rigorously and systematically represented in Sraffa (1960)[20] — a focus which, be it said incidentally, had to do with "following" a gifted scholar, just as much as serious scientific work always does.

15. As we return now to the relevance of the classical revival for policy analysis, I would confine myself here to mentioning what I see as the two basic implications of the classical approach for economic policy: one regards content, the other method.

As for content, I would provocatively unify what I see as the main implications of the classical approach, under the heading of the "principle of the under-

and Samuelson (1978), and which appears to have recently intensified with the contributions by Blaug (1999), Peach (1993), (1999), and the numerous publications of Samuel Hollander. But what interests us here is how some missing distinctions may explain an overemphasis on that "empty chair". In the years after the war, as a result also of temporary circumstances, the issue of an alternative to neoclassical theory occupied central stage in a way it does not at the moment. However only few elements of that Classical revival which is the object of Professor Foley's review, were present then, and even those few were little understood, as can perhaps be more easily seen now, by re-reading works like *e.g.* Bliss (1975) or Hahn (1982). The alternative to neoclassicism which was present in the debate, and was widely confused with that advanced by Sraffa, was in fact the quite different post-Keynesian approach advanced, perhaps prematurely, by Robinson, Kaldor and others. The same was partly true with respect to their critique of neoclassicism which, though it enforced the abandonement of the traditional version of the theory, it also allowed for resistance to the criticism on the "post-Hicksian" basis, by targeting the quite dispensable construct of the aggregate production function. It might indeed be argued that so far as the Classical revival is concerned the "chair" is getting progressively *less* empty.

[20] Work on the "core" as such would thus not seem to imply 'aversion to risk' (Vianello, 2004 p. 62) any more than, say, looking for a lion in its den, rather in the forest where it sometimes strolls. With regard to Ginzburg's criticism, also reported there, about my confining Marx's argument on the transitory nature of capitalism to the inverse relation between wage and rate of profits (Ginzburg 2000, p. 140), I may note that the "antagonistic relation between wages and profits" I mentioned in the interview there referred to was not meant to be simply the analytical inverse relation (which, as Ginzburg knows well, is present also in neoclassical theory where it has generally coexisted with an harmonious views of capitalism). The 'antagonistic' character of the relation comes largely from the "out of core" considerations which were in fact specified in that same volume (*e.g.* Garegnani, 1981, p.64).

utilisation of productive resources in a market economy". That "principle" directly descends from the fact that there is nothing in classical economic theory to ensure that a market economy left to itself would tend to fully utilise labour and the other available resources. (Unlike in neoclassical theory where Say's law is one with the theory of distribution and prices, Ricardo's adherence to that "law" was essentially the result of a failure to distinguish between decisions to save and to invest, and it did not entail for him any tendency to the full employment of labour as made clear by his chapter "On Machinery").[21]

It has accordingly been argued that the full utilisation of productive capacity can be seen to occur only under special circumstances and for limited periods of time. Indeed there are reasons, which at times naked observation imposes in various forms even upon orthodox analysis, for believing that some underutilisation of resources, in particular unemployment of labour, is *systematically* required for the stability of a market economy. But what should above all be noted is that our "principle of underutilised resources", derives most of its strength not from the underutilisation of capacity which can be observed in the economy at any given time: it derives it from the compound–interest–rate–like effect which pertains to the missed potential increases of productive capacity entailed in the observable underutilisation of resources over the past. Thus, over a period of time of some length, observable unused capacity grossly understates the real underutilisation of resources in the economy, and the missed opportunities of growth due to lack of aggregate effectual demand.[22]

Now, the implications of the principle of the underutilisation of resources in a market economy are clearly fundamental for problems of policy. The Pareto optimality of a competitive economy vanishes: classical theory has no difficulty in recognising the presence of involuntary labour unemployment by which outputs could be increased, especially when we recognise the possibility of lost potential savings. It is thus recognized as altogether normal to have positions of the economy where from a strictly technical point of view, economic welfare in the sense of disposal of goods, could increase for the many, without decreasing for any of the few.

There vanishes, in particular, the notion that the distribution of the product between classes realised by competitive markets is necessary for the full employment of productive resources and therefore for a maximisation of the social product. The competitive prices of commodities loose the attribute at expressing the relative scarcity of the resources required for their production,

[21] See par. 4 above. On the sense of Ricardo's adherence to Say's law see Garegnani, 1978, I, pp. 338–40. On the way in which competitive wages were seen as altogether compatible with labour unemployment by classical and pre–classical economists see Stirati (1994), Levrero (2003).

[22] *E.g.* Garegnani–Palumbo (1998). Cf also Palumbo (1994) for further consideration of how that fits with the researches of economic historians and dissatisfaction of many of them with orthodox economic theory.

and were instead seen to quite simply reflect the particular way in which a competitive market left to itself tends to distribute the social surplus, or a part of it, in proportion to capital. That breaks the magic circle within which policy analysis tends to be confined by the neoclassical preoccupation with distorting the Pareto–optimal allocation of resources allegedly effected by competitive prices. The circle, that is, is broken open for analysing the use of economic policy in order to influence the distribution of income and the growth of the economy. The focus of economic policy shifts from the *allocation* of productive resources to their *growth* and to the distribution of the resulting product.

16. The second basic implication for policy analysis, we said, concerns method. Using Foley's own words, we could say that there will be less space for "routine predictions" about the effects of policy, whether from theoretical analysis, or econometric models: the fact that the analysis would largely have to be carried out outside the "core" will see to that. Most importantly, there will be less illusion that policy may be left to technicians who will steer an exact course in some objectively specifiable "collective" interest. There will instead be more awareness that most policy decisions will generally favour some groups and damage others, and will no less generally face relations of power. Even a policy of fuller utilisation of resources, which might seem to be in the obvious interest of the whole community because it increases the amount of goods of which the latter disposes, will meet and has in fact met obstacles which, if we could cast aside the glasses of neoclassical theory, the naked eye would reveal to lie in how that policy would affect the relative power of the classes and groups concerned.

POSTSCRIPT

A. During the delay in bringing out the above round–table, Professor Foley has published the review article (Foley, 2003) of a further collective book on 'classical' matters, namely Kurz's *Critical Essays on Piero Sraffa's Legacy in Economics* (2000), and what I have said in the round table above may be of use for clarifying my position concerning some additional points Professor Foley raises in the second review.

Thus the "Hicksian divide" I mention in par. 12 is indeed the same dividing line which Foley describes as between a "neoclassical parable", treating capital as a single magnitude, and "contemporary neo–Walrasian theory" (2003, p. 229). I share, of course, his critical stance towards both those positions, but a basic disagreement is present when he writes that

> neo–Walrasian general equilibrium theory is similar to Sraffa's construction in that neither suggests a systematic relationship between the equilibrium rate of

> interest and the value of the capital stock per worker [and the two theories] are thus incompatible with the neoclassical 'parable' of a uniform capital "substance" (Foley, 2003, p. 229).

As I recalled in my text (par. 12 and 21) and have argued at length in (2000), post–Hicksian, neo–Walrasian theory appears to depend on the "capital substance" and the corresponding "systematic relationship" mentioned by Foley, no less than the pre–Hicksian "neoclassical parable" did — although of course the dependence is now less evident because unlike the "parable", neo–Walrasian theory avoids referring to an initial capital endowment given in terms of such a "substance".

B. The above reference to my (2000), published in the second book reviewed, may also help to clarify my purpose there, which Foley understands to be

> reconstructing virtual (out of equilibrium) supply and demand functions for saving and investment and showing that these schedules imply unplausible out of [...] equilibrium dynamics. But there are many ways to reconstruct out of equilibrium behaviour schedules in general equilibrium models, [...]. Garegnani's approach is ingenious but no more compelling than any others.

My aim in (2000) was not however to advance a particular analysis of the stability of neoclassical general equilibria: it was to show that, once the production of capital goods is properly introduced, the resulting overall properties of the equilibria are in so sharp a contrast with observation, as to throw into doubt the theory leading to them (2000, p. 392). And the properties in question regard the multiplicity of the equilibria and the zero levels of key prices in them, no less than their instability.

As for the latter property (largely implied already in the multiplicity of the equilibria) I surely did not deny that there are "many ways to reconstruct out–of–equilibrium schedules" besides the ones I introduced: on the contrary, the multiplicity of possible hypotheses on the matter was explicitly stressed. The point was rather that, given the implications of reverse capital deepening which those schedules are meant to bring out, only acrobatic hypotheses about out–of–equilibrium behaviour would allow concluding for the stability of the system. As I wrote there:

> our critical aim strengthens the legitimacy of assuming that the dominant out–of–equilibrium movement will be along the [savings supply and investment demand] schedules [since] if instability were to result under that assumption, it would be all the more plausible when obstacles to the adjustments to equilibrium are also considered [i.e. under different hypotheses about out–of–equilibrium behaviour] in the connected markets which the schedules assume to be kept in equilibrium" (2000, p. 427, our italics).

C. On the other hand, when Professor Foley continues the passage above by saying:

> the internal development of general equilibrium theory by its own adherents has shown its sharp limitations. The Debreu–Sonnenschein–Mantel theorems, which show that any excess demand functions that satisfy continuity and Walras's law can be supported by some appropriately defined Arrow–Debreu economy underline the fact that the Arrow–Debreu theory is extremely weak in explanation power,

it certainly is interesting and revealing that proponents of the theory should themselves recognise that the latter is "extremely weak in explanation power" (but, what else ought a theory be strong on?). However the "Debreu–Sonnenschein–Mantel theorems" only apply under conditions of pure exchange (or of production *without* capital), and therefore at their root lies nothing more than the income effects already considered in more transparent, if less general, ways by Marshall, Walras, Wicksell, etc. These effects were then argued to be compatible with a more precise definition of the substitution principle in consumption, and were generally taken to be no more worrisome for the theory than a backward rising labour supply is — which may well explain why today, too, despite the nominal radicality of those conclusions, demand and supply functions remain at the centre of the mainstream. Now, the difficulties pointed out in my (2000) differ in that they originate from a quite different source, *i.e.* capital and, I have argued, do throw into question the very principle of substitution between "productive factors" on which the theory was founded and still ultimately rests.

D. Foley's undue confinement of the problem of capital to what he describes as "the neoclassical parable" is, I believe, a drastic underestimate of the implications of the problem for the dominant theory in all its version (par. 10 in my text). It is an underestimate which emerges again when Foley wonders with Burmeister (2000, p. 394), about the fact that the

> victory of the Cambridge UK side in the Capital controversy has left no mark on the reality of neoclassical economic research method.

This overlooks the deep change which Foley himself stresses from "the neoclassical parable" to "contemporary neo–Walrasian theory" — where "the parable" is of course, that of Marshall, Jevons, Pigou, etc and, essentially, also that of Walras and Pareto (my par. 11), *i.e.* of all pre–"Hicksian–divide" neoclassicism. That momentous change was conceived, it is true, before the "Cambridge controversies", but it could achieve dominance, I submitted (my par.11), only *after* the controversy had swept away the traditional versions of the theory or "parable" which had allowed people to think that neoclassical demand–and–supply, *i.e.*

factor substitutability, *had* "explanation power", and constituted therefore an acceptable theory. That seems to me a substantial mark which the Cambridge UK side has already left on present day neoclassical theory.

E. Foley however reports two "tests" proposed by Burmeister for whether "a serious consideration of the issues of heterogeneous capital [...] can be given by modern economists", namely first the possibility of identifying "interesting economic questions whose correct answers require a model with heterogeneous capital goods", and second, to "find ways to give such models empirical and policy content" (Burmeister, 2000, p. 313). As for the first, I would refer readers to *e.g.* my observations on Foley's "pragmatic mainstream scholar" in my par. 10 above (*e.g.*, reswitching of techniques and reverse capital deepening are possible only with multiple capital goods: do Burmeister and Foley seriously doubt that such phenomena raise "interesting economic questions"?). As for the second test, and the policy content of the question of capital, I would simply refer readers to my Section III.

REFERENCES

ARROW K.J. (1991), "Ricardo's Work as Viewed by Later Economists", in *Journal of the History of Economic Though*, 13, Spring 1991, pp. 70–77.

BLAUG M. (1987), "Classical Economics", in J. Eatwell, M. Milgate and P. Newman (1987).

— (1999), "The Formalist Revolution or What Happened to Orthodox Economics after World War II", in Backhouse R. E., Creedy J. (Eds), *From Classical Economics to the Theory of the Firm. Essays in Honour of D.P. O'Brien*, Cheltenham, Edward Elgar.

BLISS C.J. (1975), *Capital Theory and the Distribution of Income*, Amsterdam, North–Holland.

BURMEISTER E. (2000), "The Capital Theory Controversy", in Kurz H.D. (2000).

CICCONE R. (2003), *Debito pubblico, domanda aggregata e accumulazione*, Roma, Aracne.

EDGEWORTH F. ([1881] 1967), *Mathematical Psychics*, New York, Kelley.

EATWELL J. and MILGATE M. (1983) (Eds), *Keynes's Economics and the Theory of Value and Distribution*, London and New York: Duckworth & Oxford University Press.

EATWELL J., MILGATE M., NEWMAN P. (1987) (Eds), *The New Palgrave: A Dictionary of Economics*, London and Basingstoke, Macmillan.

FOLEY D. K. (2003), "Sraffa's Legacy", *Cambridge Journal of Economics*, 27, pp. 225–38.

— (2004), "Value, Distribution and Capital: A Review Essay", in the present

Materiale di discussione, first published in the *Review of Political Economy* 13, 3, pp. 365–81.

GAREGNANI P. (1962), *Il problema della domanda effettiva nello sviluppo economico italiano*, Roma, SVIMEZ.

— (1970), "Heterogeneous Capital, the Production Function and the Theory of Distribution", in *The Review of Economic Studies*, 37 (3), pp. 407–36.

— (1976), "On a Change in the Notion of Equilibrium in Recent Work on Value: a Comment on Samuelson", in Brown M., Sato K. and Zarembka P. (Eds), *Essays in Modern Capital Theory*, North Holland.

— (1978), 'Notes on Consumption, Investment and Effective Demand', Part I and Part II, in *Cambridge Journal of Economics*, 2, pp. 335–53, and 3, pp. 63–82, respectively.

— (1981), *Marx e gli economisti classici*, Torino, Einaudi.

— (1992), "Some Notes for an Analysis of Accumulation", in E. Nell (Ed.), *Beyond the Steady State*, London, MacMillan.

— (2000), "Savings, Investment and Capital in a System of General Intertemporal Equilibrium", in H.D. Kurz, *Critical Essays on Piero Sraffa's Legacy in Economics*, Cambridge, Cambridge University Press.

— (2002a), "Misunderstanding Classical Economics? A Reply to Blaug", *History of Political Economy* 34:1, pp. 241–54.

— (2002b), "Sraffa's Price Equations: a Stationary Economy or 'Normal Position'?", in Boehm S., Gehrke C., Kurz H.D., Sturn R. (Eds), *Is There Progress in Economics?*, Edward Elgar, Cheltenham.

— (2003), "Savings, Investment and Capital in a System of General Intertemporal Equilibrium", in Hahn F., Petri F. (Eds), *General Equilibrium: Problems and Prospects*, Routledge, London.

GAREGNANI P. and PALUMBO A. (1998), "Accumulation of Capital", in H.D. Kurz, N. Salvadori (Eds), *The Elgar Companion to Classical Economics*, Cheltenham, Elgar.

GINZBURG A. (2000), "Sraffa e l'analisi sociale: alcune note metodologiche", in Pivetti M. (Ed.), *Piero Sraffa. Contributi per una biografia intellettuale*, Roma, Carocci.

HAHN F. H. (1982), "The Neo–Ricardians", *Cambridge Journal of Economics*, 6, December, pp. 353–374.

HAHN F. H. and PETRI F. (2003) (Eds), *General Equilibrium: Problems and Prospects*, London, Routledge.

HICKS J. R. ([1932a] 1964), *The Theory of Wages*, London, Macmillan; 2nd edn., New York, St Martin's Press.

— (1932b), "A Reply", in *Economica*, August, pp. 297–330.

— (1939), *Value and Capital*, Oxford, Clarendon Press.

HICKS J. R. and HOLLANDER S., (1977), "Mr. Ricardo and the Moderns", *Quarterly Journal of Economics*, vol. 9 (3), pp. 351–369.

HUTCHINSON T. (2000), *On the Methodology of Economics and the Formalist Revolution*, Cheltenham, Edward Elgar.

JEVONS W.S. (1871), *The Theory of Political Economy*, Macmillan, London

KURZ H. D. (2000) (Ed.), *Critical Essays on Piero Sraffa's Legacy in Economics*, Cambridge, Cambridge University Press.

LEVRERO E.S. (2003), "Some Notes on Wages and Competition in the Labour Market", in Ciccone R., Gehrke C. and Mongiovi G. (Eds), *Sraffa and Modern Economics*, forthcoming, London, Routledge.

MARSHALL, A. ([1920] 1949), *Principles of Economics*, London, Macmillan.

MARX K. ([1905] 1969), *Theories of Surplus Value*, 3 voll., London 1969–74, Lawrence & Wishart.

MONGIOVI G., PETRI F. (1999) (Eds), *Value Distribution and Capital*, London, Routledge.

O' BRIEN D. P. (1975), *The Classical Economists*, Oxford, Clarendon Press.

PALUMBO A. (1994), "Alcune osservazioni sulle teorie economiche della crescita e la storia economica: il ruolo della domanda aggregata", Università di Roma "La Sapienza", Dipartimento di Economia Pubblica, *Materiali di Discussione*, n. 26.

— (1996), "Notes on Investment, Saving and Growth", *Contributions to Political Economy*, XV, pp. 105–115.

PEACH T. (1993), *Interpreting Ricardo*, Cambridge, Cambridge University Press.

— (1999), "Surplus to Requirements: Kurz and Salvadori's The Elgar Companion to Classical Economics", *Journal of the History of Economic Thought*, 21, 4, pp. 449–67.

RICARDO D. (1951–73), *The Work and Correspondence of David Ricardo*, edited by Piero Sraffa, vol. I–XI, Cambridge, Cambridge University Press.

SAMUELSON P.A. (1978), "The Canonical Classical Model of Political Economy", *Journal of Economic Literature*, vol. 16, December, pp. 1415–34.

— (1987a), "Sraffian Economics", in J. Eatwell, M. Milgate and P. Newman (Eds), *The New Palgrave: A Dictionary of Economics*, London and Basingstoke, Macmillan.

— (1987b), "Out of the Closet: a Program for the Whig History of Economic Science", in *History of Economic Society Bulletin*, 9, 1.

— (1990), "Revisionist Findings on Sraffa", in Bharadwaj K. and Schefold B. (Eds), *Essays on Piero Sraffa*, London, Routledge.

— (1998), "Report Card on Sraffa at 100", in *The European Journal of the History of Economic Thought, Autumn.*

— (1999), "The Special Thing I Learnt from Sraffa", in Mongiovi G. and Petri F., (1999).

— (2000) "Sraffa's Hits and Misses", in Kurz H.D., (2000).

SMITH A. ([1776] 1950), *The Wealth of Nations*, 2 voll., London, Dent & Sons.

SOLOW R.M. (1956), "A Contribution to the Theory of Economic Growth", *Quarterly Journal of Economics*,70, pp. 65–94.

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

STIRATI A. (1994), *The Theory of Wages in Classical Economics*, Cheltenham, Elgar.

— (2001), "Inflation, Unemployment and Hysteresis: an Alternative View", *Review of Political Economy*, vol. 13, n.4, 2001, pp. 427–51.

TREZZINI A. (1995), "Capacity Utilisation in the Long Run and the Autonomous Components of Aggregate Demand", *Contributions to Political Economy*, XIV, pp. 33–36.

VIANELLO F. (2004), "Reviewing a Review", in the present *Materiale di Discussione*.

WALRAS L. (1954), *Elements of Pure Economics*, Jaffè edition, London, Allen&Unwin.

Distribution, Inflation and Policy Analysis

Massimo Pivetti*

I should like to comment on three points touched upon by Duncan Foley in his review essay of *Value, Distribution and Capital: Essays in Honour of Pierangelo Garegnani*, edited by Gary Mongiovi and Fabio Petri. One concerns "the need for a coherent theory of money prices and inflation rates" in the project for establishing the causal primacy of money interest rates in the determination of distribution, and, connected with this, the question of real vs. nominal interest as well as that of "the difficulty in articulating explicitly the market forces that would translate interest rate levels into changes in the wage" (Foley, 2004, p. 17). Another point concerns the "interesting attempt" (*ibid.*, p. 17), made by Panico in his contribution to the volume in honour of Garegnani, to combine profit rate determination by the monetary policy, through its influence over interest rates, with the Kaldor–Pasinetti theory of distribution, once state finance is introduced within the latter. Finally, my comments will concern the point made by Foley that the Classical school "offers few demonstrations of the viability of applied Classical political economy as an alternative to neoclassical practice", and that the development of a body of applied and policy economics based on Classical theoretical precepts would "greatly enhance the influence and prestige of the Classical cause" (Foley, 2004, pp. 7–8).

1. Both the explanation of money prices and of the process by which policy-determined changes in interest rates translate into changes in the real wage emerge quite clearly from my monetary explanation of distribution (see Pivetti 1991). Money interest is there viewed as an autonomous determinant of normal money production costs. Given the rate of interest to be earned on long-term riskless financial assets, and given the money wage, which is the direct outcome of wage bargaining, the price level can be determined in a system of price equations *à la* Sraffa (in which however both the wage rate and commodity prices are expressed in money proper), together with distribution of income between profits and wages. In a closed economy and for any given situation of technique, there is a price level that depends on the money wage and on the money rate of interest, with the latter acting as the regulator of the ratio of the price level to the money wage. This ratio is thus seen as the con-

* University of Rome "La Sapienza".

necting link between the rate of interest and the rate of profit: by the competition among firms within each industry, a persistent change in the rate of interest causes a change in the same direction in the level of prices in relation to the level of money wages, thereby generating a corresponding change in the rate of profits and an inverse change in the real wage. Monetary policy and wage bargaining come out of this analysis as the main channels through which class relations act in determining distribution. And the level of the real wage prevailing in any given situation is viewed as the *final result* of the whole process by which distribution of income between workers and capitalists is actually derived.

Now, the question of inflation and that of real vs. nominal interest has been dealt with within this theory of distribution (see Pivetti 1990, with the attached comments and replies; Pivetti 1991, ch. 6; Stirati 2001, esp. pp. 430–9). For the sake of further clarification, let me briefly return to these questions.

In the face of increases in the price level, competition among firms within each industry causes the rate of profit to adapt not to the nominal but to the real rate of interest (the interest rate net of inflation), as it is the latter which represents the real opportunity cost of any capital (be it borrowed or not) invested in production. Thus, with a constant nominal interest rate, the higher the rate of inflation the lower the real rate both of interest and of profit. Assuming therefore an increase in money wages, in order for the real profitability of capital to remain unaffected, nominal interest must be adjusted upward — taking, however, into account that any such adjustment in the nominal rates of interest affects in its turn prices and hence real interest. In my contributions I have tried to show that, in the face of any change in money wages, a nominal interest always exists at such a level that the prices resulting from the calculation at that nominal rate would keep the real rate at a desired or target level. So that by manipulating nominal interest it is always possible in principle — albeit at the cost of an accelerating inflationary process — to leave distribution unaffected in the face of any increase in money wages or of any other initial agent of price increases.

My point thus is that also in an inflationary context one can still argue for the "primacy" of nominal interest, in the sense that normal distribution is still primarily governed by monetary policy *through* the nominal interest rate. To the extent to which monetary policy makers possess the power to establish the level of the nominal rates of interest, they can influence the distribution of income accordingly — i.e. keep the profitability of capital at a desired or target level. Of course, non–distributional targets — such as debt management, balance of payments or exchange rate targets — may also strongly influence, in this or that concrete situation, policy decisions concerning interest rates. Given one or other of these targets, the monetary authorities might well decide, for example, to keep nominal interest rates unchanged in the face of increases in money wages. It would then be this very decision that would cause both the real rate of interest and of profit to fall and the real wage rate to rise.[1]

2. This monetary theory of distribution and the Robinson–Kaldor–Pasinetti approach — the so-called post-Keynesian or Cambridge theory of distribution — belong to two entirely different worlds and one would be hard put to try to combine them. In the Cambridge theory of profit–rate determination normal distribution plays the role of accommodating savings to investment decisions (bizarrly regarded as independent of income distribution) and money interest must therefore be viewed as a subordinate phenomenon, ultimately beyond the reach of policy. Thus Joan Robinson has explicitly criticized as "unnatural" the view of the rate of interest as an independently determined monetary phenomenon that governs the rate of profit: "Over the long run", she wrote (nonchalantly reversing Keynes' point of view), "the interest that rentiers can exact is dominated by the profits that entrepreneurs can earn, not the other way round" (Robinson 1979, p. xxii). As to Pasinetti, he has stressed that the theory of rate-of-profit determination through the money rate of interest and Kaldor's rate-of-profit determination through the rate of growth "are *alternative*" (Pasinetti 1990, p. 462, italics in the original), so that they cannot both hold true.

The introduction of state finance within the Cambridge theory does not substantially alter the picture as to the incompatibility of the two views on income distribution. If the monetary theory of distribution holds true, so that the rate of profit is actually determined by the rate of interest, and moves parallel with it, then the rate of growth of the economy will have no role to play in the determination of normal distribution; it will merely determine, in the context of the steady growth conditions assumed by the Cambridge theory, the government primary surplus or deficit necessary to the equilibrium between saving and investment. And the normal rate of profit would still be independent of the rate of growth if, given investment decisions, one were instead to look at the primary deficit as the endogenous policy variable through which full-employment growth is maintained. The case in which the rate of growth and the money rate of interest *appear* to contribute to profit–rate determination is that of steady growth in which also the primary surplus or deficit is

[1] It has been observed that this argument could be made to stand on its head: "One could equally argue that corresponding to the exogenously given nominal rate of interest there is always a rate of increase in money wages that would produce enough inflation to reduce the real rate of interest and hence profits enough to allow the workers to obtain their desired real wage" (Serrano 1993, p.122). Though in principle it is true that the behaviour of money wages might take the lead in the determination of real interest (situations in which the workers are strong enough throughout the economy to obtain their desired real wage cannot be ruled out *a priori*), in actual fact it is generally much easier — both technically and politically — for the monetary authorities to establish the course of nominal interest rates than for workers that of money wages.

taken as given. With a given primary deficit, for example, the higher the rate of interest the higher also the overall government deficit, and hence the higher, given the propensities to save, the rate of profit necessary for the equilibrium between saving and investment. The problem here, though, is that if in the assumed steady growth conditions the exogenously given rate of growth is lower than the exogenously determined money rate of interest, then primary surpluses, or sufficiently contained primary deficits, might easily imply, when combined with sufficiently low public debt to income ratios, that the rate of profit necessary for the equilibrium between saving and investment should be *lower* than the rate of interest.

But then why assume steady growth conditions in the first place? In the far from steady growth conditions in which we live, savings adapt to investment decisions through changes in output levels and in the long run also through changes in available capacity, so that no need normally arises for changes in distribution in the face, for example, of a higher rate of accumulation. What ultimately makes the two views on income distribution alternative, is that they stem from two entirely different explanations as to what normally determines the level of prices in relation to the level of money wages. In the post–Keynesian theory of distribution changes in the price level in relation to money wages are determined by changes in aggregate demand, whilst according to the monetary theory of distribution they are determined by lasting changes in the money rate of interest, with the latter variable viewed merely as an exogenously determined component of normal money production costs.

3. The point raised by Foley on classical political economy and policy analysis is extremely pertinent. It can be said that the scanty and occasional attention paid by most modern classical political economists to applied and policy issues has become especially disturbing over the last 20 years, when it has been concomitant with an increasing influence of neoliberalism and an astonishingly widespread adoption of neo–liberal remedies. One can hardly expect many young economists at the beginning of their career, who often feel somewhat uncomfortable with the current state of the discipline and the dominant neoclassical practice, to be durably attracted by the alternative way of theoretical reasoning if the majority of its representatives keep silent on the main issues that happen to occupy the centre of the policy debate — be they financial liberalization or central bank independence; the Maastricht Treaty or the European Stability Pact and the pursuit of primary surpluses; the rush on privatisations or that on an ever–increasing flexibility of the labour market. Moreover, if these and other such issues had been dealt with regularly over the last 20 years on the basis of modern classical theoretical precepts, the market ideology that has so deeply permeated also the whole of the European Left would have found at least some barrier to its progress.

That said, I think however that a greater or lesser influence and prestige of the classical cause is primarily a matter of the overall cultural climate, which is in turn essentially the result of concrete historical conditions and of the overall policy stance dictated by them. True, Keynes thought that the success of his ideas and the acceptance of the prescriptions based on them was merely a matter of time: the time necessary for convincing "the expert and the ordinary man" that he was right (cf. Keynes 1934, p. 35). But the very fact that the success of "Keynesism" has been *temporary* conflicts with this conviction of Keynes'; and one can actually maintain that, to a great extent, both the so–called "Keynesian revolution" and the "theoretical restoration" of the 1980s and 1990s were the effect, rather than the cause, of deep changes in the policy objectives of the major capitalist countries. This standpoint permits one to understand why the lack of applied and policy analysis within the classical approach was much less of a problem, with respect to the question of its influence and prestige, during the 30 years following World War II (that is, during the last 30 years of Sraffa's active life as a scholar). Concrete reality was then such as to dictate an unorthodox policy stance to the governments of the major capitalist nations, thereby also allotting substantial room for the influence of unorthodox theoretical views. The concrete reality I am referring to was the necessity for advanced capitalism to prove itself capable of overcoming its chief historical shortcomings (unemployment, poverty, and huge disparities in the distribution of wealth and income) which made it vulnerable to Communism, especially in Western Europe, in a situation in which the Soviet Union had won the war and the alternative social system was displaying great capabilities in many fields. In sum, my point here is that the current lack of headway made by the classical school in its struggle with neoclassical views should be seen primarily as just one of the cultural effects of a situation of 'unchallenged' capitalism, whose most concrete outcome to date has been the deeply anti–social and increasingly authoritarian environment we currently live in.

REFERENCES

FOLEY D.K. (2004), "Value, Distribution and Capital: a Review Essay", in the present *Materiali di Discussione*, first published in the *Review of Political Economy*, vol. 13, 3, pp. 365–81.

KEYNES J.M. [1934], *Letter to the Editor of* The New Statesman and Nation, 24 November 1934, in *The Collected Writings of John Maynard Keynes*, edited by D. Moggridge, Vol. XXVIII, *Social Political and Literary Writings*, Macmillan & Cambridge University Press, London, 1982.

MONGIOVI G. and PETRI F. (1999) (Eds), *Value, Distribution and Capital: Essays in Honour of Pierangelo Garegnani*, Routledge, London and New York.

Reviewing a Review

Fernando Vianello*

1. Duncan Foley's review (Foley, 2004) of Pierangelo Garegnani's festschrift (Mongiovi and Petri, eds., 1999) goes far beyond what is commonly meant by a review. What it does is, in fact, to provide a comprehensive critical account of the Classical–Marxian–Sraffian 'surplus approach' to political economy, as clarified, developed and canonized by Garegnani in more than forty years of unremitting theoretical activity. All adherents to the surplus approach (among whom I reckon myself) should be grateful to Foley for his contribution. For to know what an intelligent, learned and honest scholar is able to read in their position allows them to observe themselves, as it were, from outside, and may be of invaluable help in making them aware of deficiencies and obscurities in what they maintain; this being so not only when the criticism hits the nail on the head, but even when it appears based on a misunderstanding. For misunderstandings may often have causes which deserve close examination.

Garegnani's reconstruction of the the Classical–Marxian–Sraffian paradigm is described by Foley as characterized by three basic tenets: (a) the centres of gravitation, towards which the economy is led by the action of 'free competition' are not, in general, full–employment positions ('capitalist economies typically operate with a substantial and varying margin of unemployed labour'; Foley, 2004, p. 4); (b) no inverse relationship can be established between the demand for the 'factors of production' and their respective rates of remuneration ('there is no reason to believe that the level of the wage has a systematic impact on the demand for labour'; *ibid.*, p. 4); (c) 'The factors affecting the composition of output and the average wage or profit rate belong to a different conceptual realm from the competitive processes that enforce long–period natural prices as a centre of gravitation for market prices' (*ibid.*, pp. 3–4). While tenets (a) and (b) are actually traceable back to the classical economists and Marx, Foley contends, this is not so with tenet (c) ('I doubt that Ricardo had any strong allegiance to the methodological proposition that the composition of social output was, in fact, determined at a different level from the equalization of the profit rate"; *ibid.*, p. 14). Moreover, as we shall presently see, he regards tenet (c) as a serious stumbling block in the way of comprehensive economic analysis, as also of cultural hegemony.

* University of Rome "La Sapienza". My thanks go to Andrea Ginzburg, whose suggestions and criticism I have largely drawn on, and to Duncan Foley, whose comments on a previous version of this paper led me to make extensive changes in Section 2.

What Foley refers to when speaking of two 'different conceptual realms' is Garegnani's distinction between the 'core' of the surplus theories and the part of the analysis that lies outside it. Let us consider briefly this distinction. The 'core' — or what 'might in fact be described as constituting the "theory of value" such as we find it in the surplus theories' (Garegnani, 1984, p. 297, note 13) — contains the *necessary quantitative relationships* that allow us to determine the rate of profits and the natural, or (as I shall say) normal prices corresponding to any value assigned to the wage — the quantities produced and the technical conditions of production being taken as given. The 'core' of the surplus theories, Garegnani writes,

> is isolated from the rest of the analysis because the wage, the social product and the technical conditions of production appear there as already determined. (Garegnani, 1984, p. 296)

The wage, the quantities produced and the technical conditions of production are, in other words, studied (together with many other things) *in a different part of the theory*, where analysis is conducted at a lower level of abstraction, with factual observation playing an all–important role, and the purely deductive reasoning typical of the 'core' giving way to such a mix of inductive and deductive reasoning as the particular problem under examination may require.

As Garegnani observes, the classical economists' separation of the analysis into distinct logical stages

> contrasts sharply with what we find in the later marginalist theories. In these, the determination of the wage is in fact inseparable from, and symmetrical to, that of the other shares of the product. Moreover, the demand–and–supply mechanism used in that determination implies that... distribution, outputs, and relative values are all determined simultaneously taking as data the tastes of the consumers, the endowments of 'factors of production' and the technical conditions of production. The determination of these three sets of data is then seen as falling largely outside the domain of economics. (Garegnani, 1984, p. 297)

This amounts to saying that the entire neoclassical theory is made up of relationships as definite and necessary as those in the 'core' of the surplus theories — and established, like the latter, through deductive reasoning. It is this feature of the neoclassical theory that is responsible for its formal elegance.[1]

[1] In connection with the criticism of the concept of 'capital' employed in the neoclassical theory of income distribution, Foley reminds us that Smith 'refers frequently to "stock", meaning the value of capital' and that Ricardo 'phrases his account of accumulation... in terms of the growth of the value of capital'. Moreover, he warns us that if 'Classical economics' has to become 'a usable tool for the analysis of current economic problems... it has to link its concepts operationally to available statistics' (Foley, 2004, p. 12). So far, so good. The question arises, however, of whom Foley has in mind as considering the statistical measurement of capital as taboo. In the proceedings of the Corfu Conference on the Theory of Capital we read: '*Mr. Sraffa* thought one should

As soon, however, as we abandon purely deductive reasoning, the need to conduct the analysis at a lower level of abstraction becomes apparent. For the level and composition of output, the methods of production and the wage are all subject to manifold influences, of different and variable intensity, which may combine with each other, or counterbalance each other, in various ways and to various extents. As a result, a change in a variable can lead to different results on account of differences in the circumstances giving rise to it and the sequence of events it belongs to. Thus, a rise in the wage will cause a rise in demand and production of consumer goods, which in turn may encourage investment. But the associated fall in the rate of profits may act in the opposite direction of discouraging investment. The final outcome will be different according to the relative weight of these two factors. Marx calls attention to both, describing their unpeaceful coexistence as a 'contradiction':

> Contradiction in the capitalist mode of production: the labourers as buyers of commodities are important for the market. But as sellers of their own commodity — the labour power — capitalist society tends to keep them down to the minimum price. (Marx, 1885, p. 320, n. 32)

Though a remedy for overproduction, Marx contends, the rise in the wage may pave the way to a different kind of crisis.

Ricardo's famous statement about 'quantity' vs. 'proportion' comes here to the point:

> Political Economy you think is an enquiry into the nature and causes of wealth — I think it should be called an enquiry into the laws which determine the division of the produce of industry amongst the classes who concur in its formation. No law can be laid down respecting quantity, but a tolerably correct one can be laid down respecting proportion. Every day I am more satisfied that the former enquiry is vain and delusive, and the latter only the true objects of science. (Ricardo to Malthus, 9 October 1820; italics added)

Though Keynes (1936, p. 4, note 2) refers to the above passage as proving Ricardo's lack of interest 'in the *amount* of the national dividend, as distinct from its distribution', what Ricardo is actually saying is that *the search for laws* regulating the level of output is 'vain and delusive': hence the necessity of dealing with 'quantity' at a lower level of abstraction than with 'proportion' (namely, with the determination of the rate of profits as the proportion of the surplus to the capital employed).

emphasize the distinction between two types of measurement. First, there was the one in which the statisticians are interested. Second, there was measurement in theory. The statisticians' measures were only approximate and provided a suitable field for work in solving index number problems. The theoretical measures required absolute precision' (Lutz and Hague, (Eds), 1961, p. 307).

What in the above passage is called 'Political Economy' is, indeed, the 'core' of the surplus theories, and the subject matter of little more than *chapter I* of Ricardo's *Principles*. But the border between the 'core' and the remaining part of the analysis is there only to be crossed; and, indeed, Ricardo does cross it continuously. Thus, he deals with the accumulation of capital, and the resulting extension of cultivation to inferior qualities of land — which in turn (taken together with the improvements in husbandry) causes a change in the method of production of corn relevant to the determination of the rate of profits. The purpose of the whole exercise is, of course, to account for the changes in the real–world rate of profits (and to provide guidance for trade policy). But in order to achieve this result Ricardo needs a *theory of value*. It is in discussing the problem of value (made difficult by the interdependence between the normal prices and the rate of profits) that he takes the quantities produced and the technical conditions of production as given.

Marx, to take another example, endeavours to explain both how the rate of profits is affected by technical change and how the introduction of new methods of production is fostered by a fall in the rate of profits consequent upon a rise in the wage. But he organizes this part of his analysis around a 'core' (the redistribution of the surplus–value among the different trades) where, once again, the quantities produced, the wage and the technical conditions of production are taken as given.

Consider now *Production of Commodities by Means of Commodities*. Almost *the whole* of this (very short) book is devoted to the problems tackled by Ricardo in chapter I of his *Principles* and by Marx in a few chapters of book III of *Capital*, namely to the relationships which can be established between the wage, the rate of profits and the normal prices. It is only for the purpose of studying these relationships that Sraffa, like Ricardo and Marx, takes the volume and the composition of output, as also the technical conditions of production, as given, and assumes the wage (or, from a certain point on, the rate of profits: see Sraffa, 1960, p. 33) as an independent variable.

2. As it will be recalled, in Foley's account of Garegnani's 'core' – 'non–core' distinction 'the factors affecting the composition of output and the average wage or profit rate' are placed in 'a different conceptual realm from the competitive processes that enforce long–period natural prices as a centre of gravitation for market prices'. This amounts to state that the analysis of the gravitation processes belongs in the 'core' of surplus theories, together with the necessary quantitative relationships between the wage, the rate of profits and the normal prices. As Ricardo puts it, however, 'no general rule can be laid down for the variation of [market] prices in proportion to [the] quantity [brought to market]' (Ricardo, 1822, p. 220). Much, he warns us, will depend on a factor so resistant to general rules as 'the opinions formed on the probability of the future supply being adequate or otherwise to the future demand' (*ibid.*, p. 220). The only 'general rules' market prices obey concern, in fact, the *direction* in which they diverge from normal prices and the *tendency* of this divergence to be eliminated by the inflow and outflow of capitals. This is all that is required in order to make a general theory of normal prices meaningful (as it accounts for what can be described as the *basic trend* of market prices) and necessary (as market prices cannot be accounted for but in terms of their divergence from normal prices). But the *degree* of divergence of market from normal prices and the related vicissitudes of the competitive processes will be different from one case to another, and cannot therefore be accounted for in the 'core' of the theory. This is why, in discussing the laws which regulate the value of commodities, Ricardo leaves market prices out of the picture:

> having fully acknowledged the temporary effects which, in particular employments of capital, may be produced on the prices of commodities... by accidental causes... we will leave them entirely out of our consideration, whilst we are treating of the laws which regulate natural prices... In speaking then of the exchangeable value of commodities, or the power of purchasing possessed by any one commodity, I mean always the power which it would possess, if not disturbed by any temporary or accidental cause, and which is its natural price. (Ricardo, 1821, pp. 91–2)

(Compare Sraffa's statement that his analysis 'contains no reference to market prices'; Sraffa, 1960, p. 9.)

Foley's misrepresentation of the distinction between the 'core' of the surplus theories and what lies outside it leads him to reject the reading of the classical position based on the above distinction, on the ground that

> the... theorization of the wage rate as arising from a subsistence standard of living determined by the fertility behavior of workers in Malthus and Ricardo... depended on much the same type of reasoning in terms of tendencies and feedbacks that are at the heart of the classical account of capital mobility and profit-rate equalization. (Foley, 2004, p. 14)

The circumstance that 'the same type of reasoning' is employed in two parts of the surplus theories *both lying outside the 'core'*,[2] it may be observed, can hard-

[2] The reasons why the analysis of the gravitation processes lies outside the 'core' have been stated in the preceding part of this Section. As for the determination of the wage, two points should be paid attention to. The first is that the classical economists and Marx were interested in *the actual composition of the wage-basket*, and that such a subject — which the neoclassical economists, interested as they are in nothing else than the *form* of the functions of demand for and supply of labour, regard as lying outside the scope of their inquiry — can by no means be discussed at the same level of abstraction as the determination of the rate of profits corresponding to any given wage (for any given set of quantities and of methods of production). The second point is that, whatever role Smith and Ricardo may have accorded to mechanistic considerations related

ly provide an argument against the possibility of tracing back the 'core' – 'non core' distinction to the classical economists.

But Foley misrepresents Garegnani's distinction also in a second way. 'It is hard to imagine', he writes, that the analysis of the consequences of a particular tax or trade policy

> could avoid the question of the impact of the policy under consideration on the composition of output and the distribution of incomes. Similarly, it is hard to imagine how one could make satisfactory predictions of the outcome of policies without an analysis of the impact of the policy on the level of the wage. In committing methodological sin by putting the theories of price and distribution on the same level of abstraction, neoclassical practice gains the tremendous advantage of being able to make routine predictions about the composition of output and distribution. (Foley, 2004, p. 7; italics added)

As this passage makes clear, Foley thinks that to take the (level and the) composition of output and the wage as given *for the purpose of determining the rate of profits and normal prices* prevents one from considering the factors (such as a tax or trade policy) that may cause a change in the above variables. But why should this be so? Ricardo did make predictions about the impact of trade policy on the level and the composition of output. Marx did make predictions about the impact of the level of economic activity on the size of the 'industrial reserve army' and of the latter on the level of real wages. Yet they did not try to establish between the variables involved in the above predictions the same sort of necessary quantitative relationships as they established between the wage, the rate of profits and normal prices for a given set of quantities produced and methods of production. It is this difference that Garegnani's distinction between the purely logical relationships forming the 'core' of the surplus theories and the remaining part of the analysis endeavours to capture.

A comment by Marx on the treatment of the wage as a given magnitude by the Physiocrats may help to clarify the issue:

> therefore, the foundation of modern political economy, whose business is the analysis of capitalist production, is the conception of the value of labour–power as something fixed, as a given magnitude — as indeed it is in practice in each particular case... The minimum of wages therefore correctly forms the pivotal point of Physiocratic doctrine... If moreover they made the mistake of conceiving this minimum as an unchangeable magnitude — which in their view is determined entirely by nature and not by the stage of historical development, which is itself a magnitude subject to fluctuations — this in no way affects the abstract correctness of their conclusions. (Marx, 1861–63, vol. I, p. 45)

to the growth in the demand for labour and the accompanying changes in population, they made the actual composition of the wage-basket to depend — as Ricardo, by far the more mechanical-minded of the two authors, put it — 'on the habits and customs of the people' (Ricardo, 1821, p. 97).

In Marx's opinion, then, to take the wage as given (for the purpose of determining the non-wage shares in the social product) is 'the foundation of modern political economy'. At the same time, he is adamant that the wage is liable to change, and that its changes can and must be studied (as, indeed, he himself does in book I, chapter 23 of *Capital*).

Not seeing this, Foley treats the question of whether the wage should or should not be taken as given not as a *methodological* question (the answer to which is that the wage should be taken as given for some purposes, but not for others), but as a *factual* question: whether the 'notion that the wage is given' is to be ascribed a higher or lower degree of 'plausibility'. His answer to *this* question is that

> the notion that the wage is given to the system is surely just as plausible as the notion that the endowment of labor is given, however inadequate either formulation may be as a representation of reality. (Foley, 2004, p. 9)

The question of 'plausibility' is far from irrelevant. Both Smith and Ricardo hold that a tax on wages or on the necessaries consumed by the workers causes the money wage to rise rather than the real wage to fall (see WN, V.ii and Ricardo, 1821, chs. IX and XVI). And this may indeed be regarded as 'plausible' in some historical situations, and as 'implausible' in others. However, to take the wage as given *for the purpose of determining the rate of profits* (or to take the rate of profits as given *for the purpose of determining the wage*, if we follow Sraffa in treating the former, rather than the latter, as an independent variable) does not derive its justification from its being 'plausible', but from the circumstance that the determinants of the wage (or of the rate of profits) cannot be analysed at the same level of abstraction as the relationships forming the 'core' of the theory. (As for 'the notion that the quantity of labour is given', referred to in the above passage, it is obviously 'implausible' whenever labour supply is thought to adjust to labour demand through changes in population or — more realistically — through attraction of 'discouraged' workers into the labour market, immigration, etc.)

3. In the 1857 *Introduction* (which remained unpublished) to his *Zur Kritik der Politischen Oekonomie* Marx describes the method of the classical economists as based on historically and socially determined abstractions, as opposed to the generic abstractions of later economists such as J. S. Mill or Carey.[3] The latter

[3] As pointed out by Ginzburg (2000), 'the object of polemics in the '57 *Introduction* are not the classical economists, whose analytic method of determined abstractions is in fact positively appraised... but the exponents of the method of generic abstractions: above all, the post-classical economists who came in with the demise of Ricardo's school, namely J. S. Mill and Carey'.

abstractions are based on a definition of the phenomenon to be analysed (e. g. 'exchange' or 'production') which captures those characteristics of it which are common to the different situations in which it can be observed. The former on leaving aside a great number of aspects and connections of the situation considered, in order to isolate the crucial ones — the division of labour in its relationships with the extent of the market and the productive powers of labour, the extension of cultivation to inferior qualities of land and its influence on the rate of profits, etc. — and to allow them to exert their influence, as it were, *in vacuo*. As far as Ricardo is concerned, Schumpeter sees things in much the same way (though he condemns what Marx extols):

> The comprehensive vision of the universal interdependence of all the elements of the economic system that haunted Thünen probably never cost Ricardo as much as an hour's sleep. His interest was in the clear–cut result of direct, practical significance. In order to get this he cut the general system into pieces, bundled up large parts of it as possible, and put them in cold storage — so that as many things as possible should be frozen and 'given'. He then piled one simplifying assumption upon another until, having really settled everything by these assumptions, he set up simple one–way relations so that, in the end, the desired results emerged almost as tautologies. For example, a famous Ricardian theory is that profits 'depend upon' the price of wheat... Profits could not possibly depend on anything else, since everything else is 'given', that is, frozen. It is an excellent theory that can never be refuted and lacks nothing but sense. The habit of applying results of this character to the solution of practical problems we shall call the Ricardian Vice. (Schumpeter, 1954, pp. 472–73; see also p. 668)

It may be useful to compare the above passage with the following one, taken from Marx's 1857 *Introduction*:

> It seems to be correct to begin with the real and the concrete... with e.g. the population, which is the foundation and the subject of the entire social act of production. However, on closer examination this proves false. The population is an abstraction if I leave out, for example, the classes of which it is composed. These classes in turn are an empty phrase if I am not familiar with the elements on which they rest. E.g. wage labour, capital, etc. These latter in turn presuppose exchange, division of labour, prices, etc... Thus, if I were to begin with the population, this would mean a chaotic conception of the whole, and I would then, by means of further determinations, move analytically towards ever more simple concepts, from the imagined concrete towards ever thinner abstractions until I

Although 'criticism of the classical economists for having mistaken historical for natural forms can also be found in this text', this criticism has to do not with 'limitations of their analytic method as such', but only with 'limitations in their philosophy'. This being true, in particular, of the 'Robinsonades' of Smith and Ricardo, which are seen by Marx 'as idealized prefigurations of a nascent "free competition" society, and thus viewed with indulgence' (p. 119).

> have arrived at the simplest determinations. From here the journey would have to be retraced until I have finally arrived at the population again, but this time not as a chaotic conception of the whole, but as a rich totality of many determinations and relations... The concrete is concrete because it is the concentration of many determinants, hence unity of the diverse. It appears in the process of thinking, therefore, as a process of concentration, as a result, not as a point of departure, even if it is the point of departure in reality and hence also the point of departure for observation and conception. (Marx, 1859, pp. 99–100)

Schumpeter describes as 'cut[ting] the general system into pieces, bundl[ing] up large parts of it as possible, and put[ting] them in cold storage' what is conceived by Marx as 'mov[ing] analytically towards ever more simple concepts, from the imagined concrete towards ever thinner abstractions'. He praises as a 'comprehensive vision of the universal interdependence of all the elements of the economic system' what Marx brands as 'a chaotic conception of the whole', and brands as the Ricardian Vice what Marx praises as 'the scientifically correct method' (Marx, 1859, p. 101). And yet, their views of the Ricardian method are strikingly similar. In Schumpeter's opinion Keynes, too, is guilty of the Ricardian Vice:

> Speaking of Lord Keynes's theory, Professor Leontiev has called this procedure Implicit reasoning. The similarity between the aims and methods of those two eminent men, Keynes and Ricardo, is indeed striking, though it will not impress those who look primarily to the advice a writer tenders. Of course, there is a world between Keynes and Ricardo in this respect, and Keynes's views on economic policy bear much more resemblance to those of Malthus. But I am speaking of Ricardo's and Keynes's methods of securing the clear-cut result. On this point they were brothers in spirit. (Schumpeter, 1954, p. 473, note 3; see also Leontief, 1937, particularly pp. 69–71)

In chapter 18 of the *General Theory* — devoted, as he says, to 'gather together the treads of my argument' — Keynes states 'which elements in the economic system we usually take as given, which are the independent variables of our system and which are the dependent variables' (Keynes, 1936, p. 245). The elements taken as given include the existing quantity of labour and equipment, the tastes of the consumers and the disutility of labour. As for the independent and the dependent variables, Keynes writes:

> Our independent variables are, in the first instance, the propensity to consume, the schedule of the marginal efficiency of capital and the rate of interest, though, as we have already seen, these are capable of further analysis. Our dependent variables are the volume of employment and the national income (or national dividend) in wage-units'. (Keynes, 1936, p. 245; italics added)

As pointed out by Pasinetti (1974, p. 44), the rate of interest is placed by

Keynes at the beginning of a causal chain leading from it (through the schedule of the marginal efficiency of capital) to the volume of investment and from the latter (through the marginal propensity to consume) to the level of output. The reason for this is obviously not that the 'independent variables' are held to be insensitive to changes in the other variables of the system. As Keynes warns us, the 'further analysis' of which the rate of interest (like the other independent variables) is capable must necessarily take into account the circumstance that a rise in the level of employment will tend to increase the demand for money in three different ways; for (a) 'the value of output will rise when employment increases even if the wage–unit and prices (in terms of the wage–unit) are unchanged', (b) 'the wage–unit itself will tend to rise as employment improves', and (c) 'the increase in output will be accompanied by a rise of prices (in terms of the wage–unit) owing to increasing cost in the short period'. Keynes's opinion is, however, that *in spite of these (and other) 'repercussions'* — and of the fact that the rate of interest, the schedule of the marginal efficiency of capital and the marginal propensity to consume are 'liable to change without much warning, and sometimes substantially' — 'these seem to be the factors which is useful and convenient to isolate' (Keynes, 1936, pp. 248–49), not being connected by necessary quantitative relationships either with each other or with other variables considered in the analysis. (In particular, according to Keynes, and contrary to what is commonly maintained, the rate of interest is not connected by a definite relationship to the real output and the price level — the extent to which it will be influenced by a change in these magnitudes not being independent of the behaviour of the Monetary Authorities and of the way in which market expectations react both to this behaviour and to the original change in real output and/or the price level. See Bonifati and Vianello, 1998, p. 95.)

Between the Classical–Marxian–Sraffian tradition and the Keynesian approach there are, thus, much closer methodological affinities than between the latter and the long chains of deductive reasoning[4] of the marginalist theory. This appears to escape Foley, who writes:

> Too many elements of the two methodological and theoretical traditions clash: Keynesian economics' characteristic short–run time perspective, Marshallian conceptual roots, and focus on psychological determinants of economic activity, including subjective expectations, fit ill with the long period time perspective, Marxian conceptual roots, and rejection of 'subjective' and 'unobservable' explanatory factors of the Sraffian tradition. (Foley, 2004, p. 17)

Let me dwell briefly on two of the elements mentioned in the above passage: the role accorded by Keynes to subjective explanatory factors and his confinement to short–period analysis. When we consider such subjective elements as

[4] See Marshall's 'short chains of reasoning', as referred to in Garegnani (1984), p. 297, note 14.

the tastes of the consumers and the disutility of labour — which in the *General Theory* no less than in the pre– and post–Keynesian orthodoxy are made responsible for the form of the functions of demand for goods and of supply of labour — one cannot but agree with Foley that nothing could, indeed, be more alien to the surplus approach than these typical generic abstractions. If, however, we turn to the expectations referred to by Keynes in his masterly analysis of the financial and money markets (as also, e.g., in that of the impact of deflationary expectations on the marginal efficiency of capital), I cannot, indeed, see any reason why these all–important aspects of real–world economies should be left out of consideration in surplus–approach theorizing. That this is also Piero Sraffa's view of the matter is made apparent by his enthusiastic comment on the passage of the *General Theory* in which Keynes argues that the rate of interest is 'a highly conventional, rather than a highly psychological, phenomenon' ('È così che si fa una teoria' — this is how a theory should be made — he annotated on page 203 of his copy of the book; see Ranchetti, 1998, p. 43.)

As for Keynes's time perspective, Foley does not confine himself to the (indisputable) observation that the *General Theory* is mainly concerned with the factors determining the level of employment in the short run (a strategic choice which is likely to have been dictated, like that of placing the treatment of the labour market at the very beginning of the book, by the purpose of answering Pigou's short–run centred *Theory of Unemployment*), but goes so far as to state that 'Keynes persistently maintained… that the "long run" was of no interest' (Foley, 2004, p. 16). This widely shared opinion appears to have been originated by a much quoted (and little understood) passage from *A Tract on Monetary Reform*, stating that 'in the long run we are all dead'. The reading of the *entire* passage,

> In the long run we are all dead. Economists set themselves too easy, too useless a task if in tempestuous seasons they can only tell us that when the storm is long past the ocean is flat again, (Keynes, 1923, p. 65)

makes it clear, however, that Keynes is not implying that the long run is unimportant, but that the orthodox economists' lack of interest in the short run (i. e. in the span of time in which the disruptive effects of wage–and–price deflation manifest themselves) is utterly unjustified. Keynes's own interest in the long run implications of the principle of effective demand is apparent, e.g., when, in chapter 24 of the *General Theory*, he states that

> inasmuch as an increase in the habitual propensity to consume will in general (i.e. except in conditions of full employment) serve to increase at the same time the inducement to invest, the inference commonly drawn is the exact opposite of the truth. Thus our argument leads towards the conclusion that in contemporary conditions the growth of wealth, so far from being dependent on the abstinence of the rich, as is commonly supposed, is more likely to be impeded by it. One of

the chief social justifications of great inequality of wealth is, therefore, removed. (Keynes, 1936, p. 373)

The suggested extension of the principle of effective demand from the short to the long run has been taken up within the surplus approach — this being, in fact, one of the very few 'non-core' areas which has been systematically explored. In particular, Garegnani (1979) has shown that the above extension can find no sound foundation until the neoclassical demand curve for capital and the resulting investment function have been got rid of.

4. In order to understand the roots of Foley's dissatisfaction with the distinction between the 'core' of the surplus theories and the remaining part of the analysis, attention should, in my opinion, be directed not so much to the above distinction as such (which, as I endeavoured to argue, is well-grounded and, indeed, indispensable), as to the impression we may have given — to him as well as to many others — that what we saw as really important was more the 'core' in itself than its capacity to provide a guide to the analysis of the real world. Undoubtedly, this impression originated partly in criticism of the neoclassical theory (which is concerned with such a 'core' subject as the change in normal prices consequent upon a change in the rate of profits) and defence of Sraffa's interpretation of Ricardo (which concerns the 'core' of the latter's theory) having occupied for a long time — and, indeed, continuing to occupy — the centre stage. Two more factors, I shall however argue, contributed to it.

The first may be called risk aversion. As has been observed, the 'risk of going astray... is inherent to the method of determined abstractions' (Ginzburg, 2000, p.120). Travelling the vast territory lying outside the 'core' of the surplus theories is, indeed, a rather risky business, as nobody tells us in advance in which direction to move, and either we have no maps other than those we ourselves make along the way, or the maps made by previous travellers may prove wrong (whether because of original errors or of changes intervening in between). This is, of course, the natural condition of anybody engaged in genuine scientific research. But while in other fields the above condition is felt by the scientific community as natural and inescapable, in economics we are confronted with the cultural hegemony of a theory which promises a perfectly safe, long deductive journey from a few very general postulates to the explanation of almost every aspect of economic life. (This is not to imply that neoclassical authors do not do genuine scientific research. Far from that. What I maintain is that they do not do it thanks to the guidance provided by the neoclassical theory, but usually proceed the other way round, rationalizing in terms of — and thus making compatible with — the neoclassical theory results that have been reached independently of it.) This may help to explain why we have been so reluctant to abandon the safe precinct of the 'core' — as also so unduly respectful of the boundaries (unknown to Smith and Marx) between 'economics' and such disciplines as anthropology, sociology or history (which, in turn, have been to a greater or lesser extent influenced by the neoclassical theory).

The second factor at work has been a certain amount of ambiguity as to the role accorded to the 'core' of surplus theories. The 'core' contains what Marx calls 'the inner connection [*Zusammenhang*] of bourgeois relations of production' (Marx, 1867, pp. 174–75, note 34), whether it takes the form of the wage–profit relationship (as with Ricardo and Sraffa) or that of the production and redistribution of the surplus–value (as with Marx). If what I have maintained in this paper is correct, the 'inner connection' is something around which to organize our knowledge of the real world, this being its only purpose and justification. Thus, to appreciate the actual consequences of a rise in the real wage, one must look to such 'non–core' factors as 'technical change, institutional innovations and, more generally, the capacity of the system under study to respond and adjust' (Ginzburg, 2000, p. 140). There is, however, a way of interpreting the 'inner connection' which is susceptible of

> turning an 'open' system into a 'closed' one. This happens whenever the defensive needs for reassurance prevail over the requirements of research, and what should have been only a basis for initial orientation is given the (metaphysical) role of capturing the 'essence' of the system. The beginning of analysis becomes, then, its point of arrival (ibid., p. 120). If research takes off from the relations within the 'core' to venture beyond, seeking to integrate those inductive and deductive components of the analysis that the problem under consideration evidently demands, then the results should prove interesting and fruitful. But if, however, a sort of 'reassurance' against apologetics for the status quo is sought, then we shall find ourselves continuously drawn back into the 'core' in order to reassert... the validity of the 'antagonistic' relationship between rate of profit and wage. (ibid., p. 140)

When Mark Blaug refers to the 'utter indifference of Sraffian interpreters to the opening three chapters of the *Wealth of Nations* on the division of labour' (Blaug, 1999, p. 220) as an example of 'an amazingly narrow interpretation which omits some of the most exciting and indeed fruitful elements in the thinking of the classical authors' (Blaug, 1999, p. 215), Garegnani is perfectly right both in asking him to state who are the 'Sraffian interpreters' he is talking about, and in describing as a 'paradox' (as far as his own interpretation is concerned) the circumstance that

> an interpretation pointing to the role which the classical economists gave to institutional and historical factors in the division of the product between classes, and in the process of capital accumulation is being criticized as an 'amazingly narrow' one... [which ignores] their broader social and political interests (Garegnani, 2002, p. 1).

But we should also ask ourselves where such a misunderstanding originates and whether we can disclaim any responsibility for having involuntarily encouraged it.

REFERENCES

BLAUG M. (1999), "Misunderstanding Classical Economics: The Sraffian Interpretation of the Surplus Approach", in *History of Political Economy*, vol. 31, 2, pp. 213–36.

BONIFATI G. and VIANELLO F. (1998), *Il saggio dell'interesse come fenomeno monetario e il saggio di rendimento del capitale impiegato nella produzione*, in De Vecchi and Marcuzzo (1998), pp. 93–118.

DE VECCHI N. and MARCUZZO, M. C. (1998) (Eds), *A cinquant'anni da Keynes. Teorie dell'occupazione, interesse e crescita*, Edizioni Unicopli, Milano.

FOLEY D. K. (2004), "Value, Distribution and Capital: a Review Essay", in the present *Materiali di Discussione*, first published in the *Review of Political Economy*, vol. 13, 3, pp. 365–81.

GAREGNANI P. (1979), "Notes on Consumption, Investment and Effective Demand", Part I and Part II, in *Cambridge Journal of Economics*, 2, pp. 335–53, and 3, pp. 63–82, respectively.

— (1984), 'Value and Distribution in the Classical Economists and Marx', in *Oxford Economic Papers*, vol. 36, June, pp. 291–325.

— (2002), *Misunderstanding Classical Economics? A Reply*, mimeo.

GINZBURG A. (2000), *Sraffa e l'analisi sociale: alcune note metodologiche*, in Pivetti (Ed.) (2000), pp. 109–41.

LEONTIEF W. (1937), "Implicit Theorizing: A Methodological Criticism of the Neo-Cambridge School", in Leontief (1977), pp. 58–71.

— (1977), *Essays in Economics. Theories and Theorizing*, Basil Blackwell, Oxford, vol. I.

KEYNES J. M. (1923), *A Tract on Monetary Reform*, in Keynes (1971–1989), vol. IV.

— (1936), *The General Theory of Employment, Interest and Money*, in Keynes (1971–1989), vol. VII.

— (1971–1989), *The Collected Writings of John Maynard Keynes*, Macmillan, London & Basingstoke.

LUTZ F. A. and HAGUE D.C. (1961) (Eds), *The Theory of Capital. Proceedings of a Conference held by the International Economic Association*, Macmillan, London.

MARX K. (1857), *Introduction*, in *Grundrisse. Foundations of the Critique of Political Economy (Rough Draft)*, Penguin Books, Harmondsworth, pp. 81–111.

— (1861–63), *Theories of Surplus-Value*, Progress Publishers, Moscow.

— (1867), *Capital. A Critique of Political Economy*, vol. 1, Penguin Books, Harmondsworth, 1976.

MONGIOVI G. and PETRI F. (1999)(Eds), *Value, Distribution and Capital. Essays in Honour of Pierangelo Garegnani*, Routledge, London and NewYork.

PASINETTI L.L. (1974), *Growth and Income Distribution. Essays in Economic Theory*, CUP, Cambridge.

PIVETTI (2000) (Ed.), *Piero Sraffa. Contributi per una biografia intellettuale*, Carocci, Roma.

RANCHETTI F. (1998), *Sraffa e Keynes: note per una critica della teoria keynesiana dell'interesse e della moneta*, in De Vecchi and Marcuzzo (1998).

RICARDO D. (1821), *On the Principles of Political Economy and Taxation*, third edition, in Ricardo (1951–73), vol. I.

— (1822), *On Protection to Agriculture*, in Ricardo (1951–73), vol. IV.

— (1951–73), *The Works and Correspondence of David Ricardo*, edited by Piero Sraffa with the collaboration of M. H. Dobb, CUP, Cambridge.

SCHUMPETER J. A. (1954), *History of Economic Analysis*, edited from the manuscript by E. Boody Schumpeter, Oxford University Press, New York; George Allen & Unwin, London.

SMITH A. (1976), *An Inquiry into the Nature and Causes of the Wealth of Nations*; general editors R. H. Campbell and A. S. Skinner, textual editor W. B. Todd, Clarendon Press, Oxford. The Glasgow Edition of the Works and Correspondence of Adam Smith.

SRAFFA P. (1960), *Production of Commodities by Means of Commodities. Prelude to a Critique of Economic Theory*, Cambridge University Press, Cambridge.

Response to Garegnani, Pivetti, and Vianello

DUNCAN K. FOLEY

1. Friendly Debate

It is an unusual privilege to have three distinguished scholars take the time to make thoughtful and responsive comments on a paper. This is particularly welcome, since my aim in writing *Value, Distribution and Capital: A Review Essay* (Foley, 2001) was to take the stance of a friendly critic of the contemporary Sraffian/Classical school of economics and to provoke this kind of interchange.

Clearly Garegnani, Pivetti, Vianello and I agree on many points, probably more points and more important ones than those on which we disagree. We agree particularly in our disappointment with the wide acceptance of neoclassical economic theory as the basis for scientific investigation of political economic questions, despite fundamental flaws in its theory of distribution and capital. What disagreements we may have seem to me to concern the constructive question of how to develop a compelling alternative to neoclassical methods of analysis without compromising the scientific insights bequeathed to us by the Classical political economists and Piero Sraffa.

I take the comments of my interlocutors to indicate general agreement with one of my central points, the need to develop the Sraffian/Classical point of view as a research tool for questions of current policy importance. I hope the readers of my original review essay and of this paper will not take my observation that neoclassical economics is particularly well–adapted to provide answers to policy questions (usually, in fact, pretty much the same answers, which can be predicted even before the research has been undertaken) as an endorsement of the scientific value of the neoclassical approach to policy research: in my opinion the misleading conclusions of these studies often reflect the specific flaws in its theoretical foundations identified by Sraffian/Classical critiques.

There are, in fact, a number of pressing policy problems in which the Sraffian/Classical point of view has a distinct advantage over neoclassical approaches precisely because of its long period theory of competition and focus on the surplus approach to understanding capital accumulation and capitalist economic development. The analysis of the impact of various pension systems on growth and distribution, the study of technical change and its impact on environmental quality, and the long–term effects of regional economic integration on competition and profitability strike me as good examples of such problems.

Garegnani makes the important point in his comments that Sraffian/Classical policy analysis sets itself particularly high standards of scientific integrity because it acknowledges the historical and contingent character of political economic policy and the inescapable importance of conflicting power relations among classes in understanding these questions. These high stakes may contribute to the "risk aversion" Vianello shrewdly identifies as one of the characteristics of the Sraffian/Classical school. It may very well be, for example, that in venturing outside the "core" to address important issues of policy analysis, Sraffian/Classical economists will find themselves disagreeing more often than they are accustomed to in the discussion of pure theory and the history of economic thought. I don't see anything alarming in this prospect. In fact, vigorous policy debate within a school can effectively bring its understanding of issues to the attention of the public and policy makers.

With this preliminary discussion of the main point of agreement as an introduction, let me turn to responding to some of the more controversial points raised in my essay and the comments.

2. The "Core"

Both Garegnani and Vianello return to the issue of the "core" and its significance in pure theory and in the history of thought. I would like to try to clarify my position on these points.

First, let us look at the issue of pure theory and method, since that is critical to ongoing research work. My point is that the Sraffa's discoveries concerning the mathematical structure of models of sectoral competition, and the emergence of prices of production and an average profit rate, important as they are in substance, are even more important as paradigmatic examples of a distinctive method and way of looking at political economic problems, and that this method can be applied successfully to other problems in political economy. My discomfort with the language of the "core" arises from a worry that it will deter researchers from attacking these problems in the same spirit that Sraffa attacked the problem of prices of production. I suspect that my interlocutors tend to hear this position of mine as pointing toward the kind of "unified" theory of distribution and allocation that is the hallmark of neoclassical economics. This is not my aim at all. Perhaps the difficulty here arises from a presumption on the part of my interlocutors that any attempt to attack problems outside the "core" with the same analytical tools Sraffa deployed will inevitably lead back to a version of neoclassical economics, that is, a theory that forces complex political economic phenomena into a Procrustean bed and distorts real historical determinations. I don't think so. The structure of neoclassical economics as a theory of tastes, technology, and resources determining economic outcomes inherently prevents it from addressing problems like the endogeneity of population and technology which are hallmarks of the Classical approach, but lie outside the "core" of the Sraffian project.

Second, let me try to clarify my views on the history of thought, and particularly the interpretation of Smith, Ricardo, and Marx. There is no doubt that Ricardo did have a theory of value, the profit rate, and prices of production which took the wage as given and reasoned in one-directional terms about the impact of changes in wages on profits and prices of production. In this sense Ricardo can be regarded as having developed a version of what we now regard as the "core". Marx, in his discussion of prices of production and competition develops his own version (whether it is superior to Ricardo's or not is still a matter of controversy) of this theory.

But it still strikes me as problematic to elevate these observations to a claim that Ricardo or Marx gave a privileged ontological or epistemological position to this particular chain of reasoning. Garegnani makes the reasonable claim that from a logical point of view Ricardo had to clear up the impact of changes in distribution on values and prices as a preliminary to discussing accumulation and other important issues. But even the critical logical position of the theory of competition and prices of production in Ricardo's argument doesn't seem to me to be decisive evidence for its being different in method or significance from other aspects of Ricardo's argument, for example, the theory of subsistence wages, or of accumulation, or of the stationary state, or of induced technical change in the chapter on Machinery. This point seems even stronger for the case of Marx, who does not even mention the theory of prices of production in the whole first Volume of Capital, except perhaps for a single notoriously obscure paragraph warning the reader not to confuse values and prices.

In my reading of both Ricardo and Marx, they used the same methods of reasoning and observation to reach their conclusions about many other issues as they did to attack the problem of competition and the emergence of prices of production.

Now it may be that there is a qualitative difference between the "core" theory of competition and prices of production and all the other insights of political economy. I don't think so, but even if this turned out to be true, I don't think there is convincing evidence that Ricardo or Marx thought so, either.

3. Keynes and Sraffa

Pivetti, who has done as much as anyone in the world to develop the monetary closure of the Sraffian system (following up Sraffa's suggestion that the interest rate set by the central bank might provide the determination of the profit rate necessary to determine distribution), puts forward the case for the consistency and relevance of this theory. This is surely a useful reminder (to me, at least) that the Sraffian/Classical school has not neglected policy analysis (as is Pivetti's ongoing research and commentary on European monetary policy and institutions).

Pivetti's discussion argues persuasively that theoretically one can view the nominal interest rate as determining the average markup of prices over wage costs, and thus regulating the real wage indirectly. This conclusion also holds on paths of steady money wage increase. In good Keynesian fashion this closure requires the assumption of an exogenously given level or path of money wages, and once this assumption is granted, the system is well-determined. These observations are an indispensable and useful first step toward developing a synthesis of the Sraffian and Keynesian points of view. But they leave a number of questions unresolved, unfortunately questions that are often at the center of real-world debates over monetary and macroeconomic policy. To begin with, what determines the rate of growth of money wages? There is a widespread belief, supported by certain historical episodes and some statistical evidence, that the rate of increase of money wages is sensitive to the rate of unemployment and hence to the rate of growth of output, which is also influenced in the short run by credit availability (and perhaps by the level of interest rates). The monetary closure of Sraffa's theory has to address these potential linkages in some fashion or other. Another strand of Classical/Marxian/Keynesian thought would argue that monetary policy is critical for distribution through another channel: by raising interest very sharply, as happened in 1979, for example, the central bank can induce a sharp increase in unemployment, and a fall in real wages as a result of a decline in the rate of growth of money wages. This mechanism is not as well-adapted to long run steady state analysis as Pivetti's closure, but it also seems to have a kernel of political economic realism at its core.

The point here is that the political economic debate in advanced capitalist countries would benefit from as vigorous a development of a Sraffian/Classical analysis of central bank policy as is possible. The discussion of monetary policy is monopolized by bromides like the notion that "price stability" is the only valid goal of monetary policy (which is demonstrably untrue both as a matter of political realism and as a matter of theoretical modeling) or the "Taylor rule" of central bank behavior. Central banks, as Keynesian and Sraffian/Classical theory both suggest, are much more a part of the politics and power struggles of contemporary capitalism than these formulations suggest.

Garegnani rightly emphasizes another key point common to the Sraffian and Keynesian schools, the assumption that capitalist economies normally operate with a substantial margin of unemployed labor and capital. As he says, this assumption completely undercuts the opportunity cost reasoning that is the foundation of neoclassical welfare-based policy analysis, since if there are in fact unemployed resources commanding a non-zero price in an economy opportunity cost is undefined. This line of thinking suggests an important research topic for Sraffian/Classical (and Keynesian) economists, which is to understand better how markets function to allocate resources in the presence of unemployment. This is a particularly opportune area for creative critical thinking, because neoclassical economics is rendered incapable of addressing it without betraying its own conceptual foundations. (The neoclassical school is not unaware of this problem, as the proliferation of models based on efficiency wages and other informational imperfections that attempt to reconcile the existence of unemployed resources at non-zero prices with market equilibrium attests.) The development of a better-articulated and more realistic theory of the allocation of resources in capitalist economies would also provide firmer foundations for Sraffian/Classical policy analysis.

4. Methodology

I would like to register publicly here my strong agreement with the methodological perspective so clearly enunciated in Vianello's comment, which he bases on Marx's brilliant discussion of abstraction in the (long unpublished) Introduction to his *Contribution to the Critique of Political Economy*. Marx here comes as close as anyone has to describing in explicit terms the methodological secret behind the analytical power of the Classical political economy approach.

It is precisely this vision of method that makes me uncomfortable with some claims advanced concerning the methodologically privileged position of the "core" in the Sraffian/Classical tradition. It is easy enough to locate the theory of prices of production and the emergence of an average profit rate in the layering of abstractions Marx describes in this essay. In fact, the theory of prices of production is an excellent example of an intermediary stage in this type of reasoning, at which certain concrete elements of capitalist economic reality (such as competition among capitals) have been explicitly introduced and others (such as monopoly and the intervention of the State) have not been. This leads me to view these "core" results as a central and indispensable part of the analysis, but not methodologically isolated from other equally important parts, such as the theory of wages, of induced technical change, and of population.

I also agree with Vianello that Keynes largely shares this methodological practice, even though he never, even in his most explicit discussions of methodology, formulates this view of abstraction explicitly. (It is hard for me to imagine a thinker formed in the British rational/empiricist tradition who could formulate the equivalent of Marx's view of abstraction.) I have always taken this as evidence that every deep and penetrating insight in political economy rests ultimately on the methodology of abstraction identified in Marx's text. Keynes had to break through (as he does say explicitly) the fog of Marshallian interdependence of determinations to arrive at his own epoch-making discoveries. This is an important point in favor of the possible eventual synthesis of the Keynesian and Sraffian perspectives, and I take Vianello's remarks as an important reminder in this respect. Nonetheless, this methodological compatibility cannot by itself resolve all of the substantive differences between the Keynesian and Sraffian points of view I pointed out in my original review essay.

5. "Risk aversion" and Political Economy

Let me close this brief response by commenting further on another point Vianello raises, the "risk aversion" experienced by the Sraffian/Classical tradition in exploring questions outside the "core". Vianello properly points to some of the roots of this feeling, including the existential insecurity inherent in any genuinely original scientific research. I would add to Vianello's diagnosis two other issues.

One is that this type of research faces not just the "cultural hegemony" of mainstream economics, but fierce ideological pressure. Once one ventures outside the Sraffian "core" (which is itself highly contested territory, since it addresses directly the theory of distribution), one enters a treacherous field of political struggle. It is highly possible in this kind of work to find plausible logical and analytical paths leading to unexpected and devastating conclusions. Under these circumstances it is only prudent to move cautiously and tentatively, though I would argue that it is essential to move.

A second source of risk aversion in the Sraffian/Classical tradition seems to flow from Sraffa's own scientific personality, the quirks of which have left their mark (as is common in history with outstanding innovative figures) on the practice of his followers. Sraffa held himself to a very demanding standard of intellectual rigor and depth. He maintained a fiercely independent and uncompromising critical stance toward the work of other scholars. It is not easy for ordinary mortals, conscious of their own limitations and in awe (probably increasing awe as they grow older and understand better just what Sraffa actually managed to accomplish) of Sraffa's achievement to feel confident in pushing into new problems without even a hint in Sraffa's own work for guidance.

My own instinct in this case is to depend more on the collective strength of scholarly and scientific debate than on individual genius for guidance. The best way for ordinary mortals to move a research program forward is to make mistakes, and correct the mistakes of others as best we can. This collective process is our best hope for making advances and recognizing blind alleys and subtle pitfalls in reasoning. The achievement of a Sraffa can inspire us to avoid sloppy thinking, but it would be a pity if it were to deter us from new investigations.

REFERENCES

FOLEY DUNCAN K. (2001), "Value, Distribution and Capital: A Review Essay", in *Review of Political Economy*, 13(3), pp. 65–381.

GAREGNANI PIERANGELO (2004), *Professor Foley and Classical Policy Analysis.*

PIVETTI MASSIMO (2004), *Distribution, Inflation and Policy Analysis.*

VIANELLO FERNANDO (2004), *Reviewing a Review.*

ISBN 88-7999-740-8