An unpleasant dilemma for contemporary general equilibrium theory

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1. Introduction

§1. Among orthodox economists, the opinion has established itself that the marginalist (or neoclassical) approach to prices and distribution *need not* treat the factor capital as a single *homogenous* magnitude. In the general equilibrium equations, the argument goes, this factor can be consistently specified in the way originally adopted *only* by Walras among the founders of the marginalist school, i.e. as a vector of *heterogeneous* capital goods. However, in my view this line of argument is *incomplete* and therefore misleading: it has overlooked the problem relative to the *relevance* of the theory. In other terms, what bases would make the correspondence between theory and observation possible once capital is treated along the Walrasian lines? The paper aims to provide a possible answer to this question by assessing the position on this matter of two prominent neoclassical scholars: Lucas and Hahn.

§2. In order to settle the grounds of the discussion, it may be useful first to have a quick look at the way in which the plausibility of the theory was traditionally argued in economics, by both classically and neoclassically oriented scholars. We must therefore turn to consider that *all of them* shared the view that it is clearly impossible to isolate with adequate approximation the factors that determine the *actual* (or market) prices at *any given moment*, since at each instant of time the latter are influenced by a potentially unbounded sort of accidental factors, and whose influence on the variables under examination cannot be assessed at a sufficient level of generality. But since most of these factors are bound to disappear within a very short period without leaving a significant trace in the economy, this was seen as an uninteresting theoretical problem anyway. Therefore, the

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basis to argue a correspondence between the variables determined by theory and the magnitudes that, day by day, we observe in real economies rested, traditional authors *unanimously* agreed, on the possibility to determine, not the *effective* position of actual variables at any given instant, but their *centre of gravitation* instead, namely the position (within the neoclassical approach, the "equilibrium") these variables *would tend* to realise only over sufficient time. The implication, often not clearly grasped, is that not only the equilibrium (or "normal", to use Marshall's words) prices must be *stable*; in order to allow for the compensation or correction of disequilibria, and hence for the theoretical position to emerge as the outcome of a trial-and-error process of adjustment that will generally take *considerable time* to assert itself, it is also required that its data are sufficiently *persistent* relative to the presumable speed of gravitation towards equilibrium (cf. Garegnani 2012). Until comparatively recent years, this persistence has been justified by endogenously determining the composition of the capital stock in equilibrium by means of the condition of uniformity of the rate of return over cost on capital. Now, as is probably well known, for marginalist theory, which must include the factor capital among the givens, this implies that this factor *must* be conceived as a *single factor* of production of *variable form*, measured in *value terms.*[1]

However, the value specification of capital is clearly *illegitimate*,[2] and since the 1930s neoclassical scholars have gradually shifted towards the

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1 The treatment of capital as analogous to that of labour or land also plays two additional crucial roles. First, it gives justification to the uniqueness and stability of the equilibrium, since it gives good reasons to believe, even in capital-goods producing economies, in the "correct" working of the factor substitution mechanisms, on which the *negative slope* of the factor demand curves is assumed to rest and, *ultimately*, the whole plausibility of the explanation of prices, distribution and employment in terms of an equilibrium between supply and demand forces. Second, once it is acknowledged that different techniques generally call for the employment of *different kinds* of capital goods, the "variable" form of capital justifies the *sufficient substitutability* among the different factors of production, and hence the sufficient elasticity of the factor demand curves. The latter is in turn essential to obtain plausible levels of the real wage rate and of the interest rate in equilibrium, and also to argue that, e.g. in the case of unemployment, wage reductions need not be implausibly drastic.

2 As Wicksell once put it "But it would clearly be meaningless – if not altogether inconceivable – to maintain that the amount of capital is already fixed before equilibrium between production and consumption has been achieved… a change in the relative exchange value of two commodities would give rise to a change in the value of capital… even if we conceive capital genetically, as being a certain quantity of labour and land accumulated in different years, a change in the value of commodities would also alter the conditions of their production and thus necessitate a larger or smaller change in the composition of capital" (Wicksell 1934, p. 202).

notions of intertemporal and temporary equilibrium, adopting the method of analysis on which marginalist theory stands at present. It is undeniable that these two equilibrium concepts differ from one another in several important respects, in particular in their treatment of price changes in the definition of the equilibrium. The intertemporal equilibrium as popularised by Debreu (1959) assumes the existence of complete future markets (possibly in contingent commodities), so relative future prices are determined in the initial period, simultaneously with present relative prices. The temporary-equilibrium concept presented by Hicks (1939), on the other hand, attempts to give a truly sequential structure to the evolution of the economy by relaxing the assumption of complete markets and assuming instead that for most goods only spot markets exist. Markets are therefore open at the beginning of each period, and agents take their decisions on the basis of their expectations about future relative prices, expectations that can well be mistaken. Important as these differences are, both kinds of equilibria can be considered to belong to the *same method* of analysis because, unlike the traditional versions of the theory, they *both* specify the factor capital among the givens as a set of physically heterogeneous capital goods.[3] To highlight this common feature, whose relevance is discussed immediately below, we may usually refer to these equilibrium concepts as neo-Walrasian equilibria, and to the method of analysis they belong as the neo-Walrasian method.

Within the neo-Walrasian method, the immediate contradiction of traditional neoclassical theory, namely the illegitimacy of specifying the factor capital as a value magnitude among the givens, is obviously avoided; the neo-Walrasian "way out", however, has not been without costs: first and foremost, the data relative to the factor capital in neo-Walrasian equilibria suffer from *insufficient persistence*: the vectorial endowment of capital goods is bound to change *extremely* fast during

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3 In any case, it should be noticed that it is the very inclusion of capital goods among the givens what forces the theory to deal with price-changes in the definition of the equilibrium. Given that the initial composition of the capital stock will considerably change from one period to the next along the equilibrium path, a fact that will generally entail changes in future equilibrium relative prices, this cannot be ignored by individuals when taking their economic decisions. On the other hand, the slowly changing data in the traditional versions of the theory make it legitimate to abstract from possible future price changes; it is therefore unnecessary to include the latter in the definition of equilibrium (cf. also footnote 7).

the process of equilibration, presumably with a speed of the same or higher order of magnitude as the speed with which, say, the demand for and the production of a consumption good tend to equality, a process that will generally involve trial-and-error productions on the part of firms, and hence may take considerable time to assert itself.[4] Therefore, *before* the repetitions of transactions and productions can eliminate the possible disequilibria, the data of the equilibrium itself will have changed considerably, i.e. equilibrium is *path dependent* because the effects of disequilibrium actions on the data determining it are *too relevant* to be neglected[5]; and it also becomes unclear whether it can be presumed that the system tends towards some final position.[6] Therefore, neither the intertemporal equilibrium nor the sequence of temporary equilibria can give any indication of the average trend of prices and quantities in real economies, depriving neo-Walrasian equilibria of

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4 This explains why traditional neoclassical authors were *forced* to determine the composition of capital endogenously, and hence to measure the endowment of capital as a single factor, capable of changing form: as Petri (2003, p. 390) notices, it would have been clearly *illegitimate* "to assume equilibration on the market for produced goods and not to admit the variability of the relative amounts in existence of the several capital goods".

5 As to the traditional notion of capital as a single factor, while neoclassical theorists openly admits that the total quantity of capital would be gradually altered by capital accumulation, its speed of variation is sufficiently slow so as to render it legitimate to consider the total endowment of capital as unchanging when studying the process of gravitation towards equilibrium, allowing its composition to be determined endogenously. Therefore, for instance, Knight (1931, pp. 208–9) writes that the "total supply of capital is 'very large' in comparison with possible variations in it, and that the opportunity for further investment is on a similar scale with the total... Manifestly no possible variation in the amount saved in a year could make enough of a variation in the total supply... The cumulative result would be detectable after a considerable number of years".

6 To further grasp the relevance of the problem caused by the lack of persistence of the vectorial endowment of capital goods, consider an equilibrium situation and assume that, due to immigration, the real wage rate changes. This will presumably have repercussions in all sectors of the economy; it will thus take considerable time for prices to adapt to the changed costs, and for firms to adapt production so as to satisfy the new composition of final demand, which will generally change due to the changed prices of consumption goods. Along this adjustment process it can hardly be maintained that the several endowments of capital goods will not change (nor that their change is of minor importance).

being capable to have the role of a centre of gravitation of actual magnitudes.[7]

The alleged solution to the problem caused by the lack of persistence of the data consists of the studying of the stability of the equilibrium under the assumption that no economic activity actually takes place at disequilibrium prices, i.e. before equilibrium is reached. The implication is that the theoretically determined prices must be conceived to be *instantaneous* so to speak, namely directly or *immediately* identified with observable prices. However, because actual production and transaction activities will generally take place at prices that differ from those determined by the theory, these stability results are devoid of implications for the real world.

The possibility to argue the correspondence between neoclassical theory and observation on the basis of the neo-Walrasian method of analysis is also seriously impaired by a second, not less relevant, problem: when each capital good is treated along neo-Walrasian lines, there will generally be *insufficient factor substitutability*; therefore, if one assumes factor rentals to react to their respective excess demands, the equilibrium real wage can e.g. well be zero, and susceptible of drastic changes if labour supply changes only by a slight amount.[8] Clearly, these implausible results, which find no correspondence with observed facts, confirm that the neoclassical belief in the supply-and-demand explanation of income distribution is simply devoid of legitimacy.

The former shortcomings, let me insist, a *strict* consequence of the inclusion of the vectorial endowment of capital goods among the data of neo-Walrasian equilibria, strongly suggest that the latter – in any of their forms – *cannot* have the role of a position where the economy tends; and it is in view of this that the paper attempts to argue that the theory is doomed to face an *unpleasant dilemma*: once the fact is grasped that the theory *cannot* determine

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7 In the temporary-equilibrium versions, also the datum relative to expectation functions lacks the necessary persistence since the *way* in which people form their expectations is generally influenced by all sort of accidental and transitory factors that will generally change during the adjustment process. Moreover, given that expectation functions can be influenced by a wide variety of factors – an in unpredictable ways –, their inclusion among the givens creates a serious problem of *indeterminacy* into the theory. Within traditional theory, on the other hand, these problems do not arise: the persistence of the data allows neglecting the possible changes in the theoretical position itself in the definition of the equilibrium. It becomes therefore unnecessary to include exogenous expectation functions among the determinants of equilibrium (or for that sake, to assume complete markets). We shall return to the problems caused by expectations on section III (§12).

8 As noted in footnote 1 in the traditional versions of the theory this problem does not arise.

the actual path of the economy and hence that the possibility of a correspondence between theory and observation requires to assign to equilibrium its traditional role as a centre of gravitation, and thus to treat capital as a single factor of production, contemporary general equilibrium theorists must choose between Hahn's position on one hand, or Lucas's on the other. That is, either they attempt, as Frank Hahn does, to be truly consistent with their own theoretical object, an equilibrium notion that, because it includes the set of capital goods among the givens, must be forcedly *silent* on the issue of how real economies actually work; or, alternatively, they attempt, as Robert Lucas does, to endow Neo-Walrasian equilibria with explanatory-predictive value; but then, I submit, they must inescapably rely on traditional neoclassical gravitational ways of reasoning whose logic is marred by their *illegitimate* conception of capital as a single factor.

§3. The paper is structured as follows: after this introduction, the second section examines some of Lucas's contributions during the 1970s and 1980s: it is there argued that although this scholar claims to ground his work within the neo-Walrasian method of analysis (in particular, allegedly adopting the notion of intertemporal equilibrium in contingent commodities), he does not follow to its last consequences the notion of equilibrium he allegedly adopts, since this would have prevented him from saying anything about the working of actual economies. The problem is surmounted by Lucas *only* because, albeit implicitly, his ways of reasoning still *presume* the old notion of capital as a single factor. I will conclude that, Lucas's claims to the contrary notwithstanding, this author has no theory *at all*: his results regarding the actual trends of income distribution and employment are not supported by the neo-Walrasian approach to prices and distribution for the reasons addressed in §2, nor by the traditional neoclassical approach, marred by its conception of capital as a single value factor. In the third section Hahn's position on modern general equilibrium theory is discussed: on one hand, we shall see that this author is particularly clear that neo-Walrasian paths cannot have the role of a centre of gravitation of actual paths; and the section documents in this connection Hahn's critiques of Lucas and of his attempt to argue a correspondence between neo-Walrasian equilibria and observation. Therefore, with Hahn contemporary general equilibrium theory is considered *autonomously* from old reasonings, and this is what gives him the capacity to take authors like Lucas at his words and denounce the illegitimacy of their claims. However, on the other hand, I will argue that Hahn's critiques evidence an inability on the part of this author to grasp that, actually, Lucas's claims are not really justified on a presumed coincidence between actual paths and neo-Walrasian paths, but on traditional neoclassical gravitational modes of explanation which *would have been plausible* if the old notion of capital could be accepted. To put it in a nutshell: while correct, Hahn's critiques

are a *manifestation* of his lack of familiarity with those traditional ways of reasoning, and with the crucial role played in those arguments by the untenable notion of capital as a single factor of production. This inability to grasp the logic behind Lucas's arguments prevents Hahn from fully grasping the scope of his destructive critiques: for when Hahn comes to indicate what kind of equilibrium would make a correspondence between theory and observation possible, Hahn does admit that the equilibrium concept must allow the *actual* implementation of disequilibrium activities. However, he does not really understand that this *necessarily* means that the endowments of capital goods must be conceived as *endogenously* determined variables, with the further implication for the neoclassical approach that an explanation of the actual trends of prices and quantities requires this approach to be rejected, and replaced by a *different* theory of prices and distribution.

2. Lucas on equilibrium and capital

§4. Lucas's work during the 1970s can be interpreted as an attempt to firmly ground Friedman's contributions on the Phillips Curve and on the trade cycle on neo-Walrasian general equilibrium tenets. In his seminal contribution, *Expectations and the Neutrality of Money* (1972), Lucas replaces Friedman's hypothesis of adaptive expectations with the assumption of rational expectations (henceforth RATEX) first suggested by Muth (1961), namely that on average agents know the correct distribution of future equilibrium variables[9]; Lucas therefore introduces the "new" (1972, p. 104) notion of RATEX equilibrium. As he further argues in Lucas (1980), this equilibrium concept is allegedly grounded on the notion of "contingent-claim equilibrium", "originally proposed by Arrow and Debreu" (1980, p. 707).[10] Their innovation, he continues, consists of explicitly incorporating uncertainty into general equilibrium theory "by indexing goods both by the date on which they are to be exchanged and by the (perhaps stochastically selected) 'state of nature' contingent on which the exchange is to occur" (1980, p. 707). Lucas goes on to the claim that there are two alternative ways to interpret this equilibrium concept. One way is to assume the existence of complete future markets in contingent commodities as e.g. in Debreu (1959). However, he notes, "with prices determined in advance, the issue of price expectations does not arise"

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9 As Muth (1961, p. 316) argued: individual's subjective expectations "tend to be distributed, for the same information set, about the prediction of the theory (or the 'objective' probability distributions of outcomes)".

10 Cf. also in Lucas and Sargent (1981, p. 305), where the authors argue that the concept of equilibrium used by RATEX theorists "stemmed mainly from work by Arrow (1964) and Debreu (1959)".

(1980, p. 707). The other interpretation, the argument goes, the one Lucas himself adopts in order to include expectation functions into the analysis in an explicit manner and hence to give a truly sequential structure to the economy, is to assume a sequence of temporary equilibria with RATEX.[11] The basis to argue that both interpretations are formally equivalent is precisely that the specific hypothesis about agents' expectations entailed by RATEX implies that the average expected path is the *perfect foresight* path (cf. Rodano 1984, section I) which, as is well known, is formally equivalent to the intertemporal equilibrium in contingent commodities.[12]

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11 In Lucas's words: "One way to interpret a 'contingent-claim' equilibrium is as a description of an economy in which all state-contingent prices are determined in advance, in the clearing of a single grand futures market… Alternatively, one may… think of a contingent-claim equilibrium as being determined via a sequence of 'spot' markets, in which current prices are set given certain expectations about future prices" (Lucas 1980, p. 707). Furthermore, he assumes RATEX as "a principle to reconcile the price distributions implied by the market equilibrium with the distributions used by agents to form their own views of the future" (1980, p. 707). We may incidentally note that on the basis of this same citation Rodano (1984, p. 43) too has maintained that "Lucas' idea [is] that an A–D [Arrow–Debreu] equilibrium can also be interpreted as being determined via a sequence of 'spot' markets, in current prices are set given certain expectations about future prices", whereas Lucas notes, these "certain expectations" are RATEX. On this basis Rodano (1984, p. 25) argues that "Lucas and his followers maintain that the necessary and correct micro foundations [of macroeconomics] are to be found in modern General Equilibrium Theory".

12 For the equivalence between the perfect-foresight path and the intertemporal path in contingent commodities, cf. e.g. Radner (1982, pp. 940–42) and Rodano (1984, section III). Although not essential for our purposes, which at this stage of the exposition only attempt to establish Lucas's alleged adoption of the notion of contingent-claim equilibrium, and more generally, of the neo-Walrasian method, as his so-called micro foundation, we may incidentally note that, beyond the formal aspect of the problem, Radner (1982, p. 942) and Rodano (1984, p. 42) have disputed that a perfect-foresight path – or in a stochastic environment the RATEX path – can be considered equivalent to the intertemporal path in contingent commodities. The reason is that the assumption of correct price expectations is contradictory with the assumption that the equilibrium *must* be found by the market (cf. also Petri 2009, pp. 14–15). More recently, the alleged equivalence between the perfect-foresight path and the intertemporal equilibrium path has been also convincingly questioned by Mandler (2002) and Fratini and Levrero (2011). We may also add that while Lucas occasionally argues that the assumption of RATEX implies that "agents are assumed to know the pertinent objective probability distributions" and that "This hypothesis is imposed by way of adhering to the tenets of equilibrium theory" (Lucas and Sargent 1981, p. 307), when one admits the possibility of contingent markets, modern general equilibrium theory does not require agents to have correct, common expectations about probabilities of future states; it can deal with any beliefs of individual agents about probabilities of future states of nature.

§5. As is perhaps well known, Lucas's explanation of the trade cycle heavily rests on the assumption that individuals have access to *imperfect information,* and hence they are unable to disentangle between monetary and real shocks, with the implication that the former can well have temporary effects on output and employment, despite agents have RATEX and base their production and investment decisions on the "real" characteristics of the economy.[13] However, this is essentially irrelevant now. For our purposes, it is enough to note that unlike traditional neoclassical scholars (or for that matter, unlike Friedman[14]), Lucas does not attempt to explain economic fluctuations in output and employment as disequilibrium *deviations* with respect to a "norm" that the economy tends to realise after a process of learning (error correction) on the part of agents has taken place. In Lucas's model agents *never* learn because the density of the "shocks" is known, they do not commit any error even in the very short run[15]; and the recourse to the famed *auctioneer*, who, by preventing disequilibrium activities from actually taking place, is argued to operate "so rapidly that he is not noticed" (Lucas 1980, p. 711), implies that even in a neo-Walrasian setting that includes physically heterogeneous capital goods among the givens –and whose quantities and forms in disequilibrium can be very quickly altered if so decided[16] – agents' optimal decisions can be made mutually consistent *instantaneously.* In short, the business cycle, represented by the sequence of

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13 Cf. De Vroey (2001) for a detailed analysis of Lucas's (1972) business-cycle model.

14 As is probably well known, Friedman (1977) argues that the trade-off between labour unemployment and inflation depicted by the Phillips Curve can only hold in the short-run, since any attempt of the monetary authorities to decrease the unemployment rate by increasing the money supply will sooner or later make workers – who are assumed to have adaptive expectations – realize that the price level will eventually rise proportionally and, therefore, they will inevitably end up raising their demand for higher money wages so as to leave their real wage unaltered. There is a "normal" level of employment where the economy tends over sufficient time, and that is determined by the forces of supply and demand; hence, Friedman argues, a persistent level of inflation caused by monetary expansion will ultimately cause "perceptions [to] adjust to reality" and therefore "Ultimately, employment will be back at the level that prevailed before the assumed unanticipated acceleration in aggregate nominal demand" (Friedman 1977, p. 14).

15 As Lucas and Sargent (1981, p. 307) explain, the errors agents commit "are unavoidable given their limited information".

16 Lucas extends the 1972 paper to model the production of capital goods and hence considers investment decisions in Lucas (1975; see §7).

RATEX equilibria, is now conceived as an "equilibrium cycle": prices and quantities, Lucas (1980, p. 709) forcefully declares, "are taken to be always in equilibrium".

The implication that one draws from these considerations is that the traditional distinction between *market* (disequilibrium) and *normal* levels of output and employment, and the claim that, over several market rounds and by trial and error, the average of the former will tend to coincide with the latter, are simply erased from economic analysis. However, the alleged *instantaneousness (*or *immediateness*) of the theoretical variables appears to be hardly defensible: if for no other reason, because of the potentially unlimited number of accidental factors that are bound to affect the economy at any given moment, whose action clearly prevents the theory from determining the actually observed prices and quantities at *each* instant of time. On this basis, the claim that the economy follows an RATEX equilibrium emerges as an unjustified statement, or as Hahn would argue (see §12), as a simple "axiom".

§6. But is it really the case that Lucas envisages the theoretical variables as immediately observable? The author sometimes justifies his claim on the grounds that "Any model that is well enough articulated to give clear answers to the questions we put to it will necessarily be artificial, abstract, patently 'unreal'" (Lucas 1980, p. 696). A good model, as Lucas declares, "will not be exactly more real than a poor one, but will provide better imitations [of reality]" (1980, p. 697).[17] However, as I discuss immediately below, I believe that the claim that the economy follows an RATEX-equilibrium path can be justified on more plausible bases than this rather instrumental argument.[18]

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17 In a subsequent contribution, Lucas (1988) also attempts to defend himself by arguing that, if the notion of RATEX equilibrium is extended over the infinite future, the economy would eventually reach a position of steady growth. Moreover, Lucas goes on to argue that it is this final equilibrium, and not necessarily the rational equilibrium path itself, the position the actual economy will be gravitating around: "What of economies that begin off the balanced path – surely the normal case? Cass showed – and this is exactly why the balanced path is interesting to us – that for any initial capital $K(0) > 0$, the optimal capital-consumption path $(K(t), c(t))$ will converge to the balanced path asymptotically. That is, the balanced path will be a good approximation to any actual path 'most' of the time" (Lucas 1988, p. 11). However, this claim seems to be devoid of justification: actual economies are far from being in a position of steady growth, and before the economy can reach this position, the data that determine the steady growth equilibrium path will have changed considerably.

18 However, as we shall see below, this justification is devoid of legitimacy because it presupposes the traditional notion of capital as a single factor.

Indeed, it is not often noticed that in some of his writings Lucas *does* admit that the actual economy cannot be conceived as being always in RATEX equilibrium; the latter, he argues, is only a position that the economy will tend to realise only after sufficient time. Consider for instance what Lucas writes in *Equilibrium Search and Unemployment* (1974) with respect to the assumption of RATEX:

> The economic interpretation of this assumption of rational expectations is that agents have operated for some time in a situation like the current one and have therefore built up experience about the probability distribution which affects them. For this to have meaning, these distributions must remain stable through time. (Lucas 1974, p. 190)

And then he observes that the probability distributions of the variables under examination

> are learned by processing observed frequencies in some sensible fashion... which has the property that the 'true' distributions become 'known' after enough time has passed. (Lucas 1974, p. 204)

As these passages clearly show, Lucas openly admits that the hypothesis of correct expectations in equilibrium presupposes a learning process on the part of agents that will take sufficient time to unfold itself and that will generally involve the *actual* implementation of disequilibrium activities. Accordingly, essentially unchanging data are needed if the assertion that individuals will eventually gain the necessary experience about the conditions ruling on the market is to be plausibly justified.[19]

The view of equilibrium as the outcome of an adjustment process that operates only over sufficiently long periods also arises in a subsequent and little-noticed article[20] *Adaptive Behaviour and Economic Theory* (1986). In that

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19 At this juncture we may note, albeit largely implicitly, that individuals can only come to know the correct probability distribution after some adjustment process is implied by Lucas's (1980, p. 711) resort to an auctioneer-guided tâtonnement to justify how an RATEX equilibrium comes about. In fact, if the assumption of RATEX were to be taken at *its words*, individuals would correctly forecast equilibrium prices, and the respective quantities produced and sold in the market; no need would therefore arise to actually *find* the equilibrium values, as it is implied in the auctioneer-guided tâtonnement (on this point, cf. also Petri 2009, pp. 14–15; also Rodano 1984, p. 42; Radner 1982, p. 942).

20 *Cf.* however De Vroey (1998), Kirman (2003) and Vercelli (1991).

work Lucas (1986, pp. 411–13) documents Vernon Smith's (1962) contribution in which, through a series of experiments with a group of individuals, this latter author attempts to show how equilibrium in a single market is to be reached by trial and error and experimentation after several market rounds. Relying on Smith's positive results, Lucas asserts that the auctioneer-guided tâtonnement is, in fact, a dispensable assumption, since Smith' setting

> shifted the task of adaptation from the auctioneer to the same agents whose preferences determine the equilibrium, and permitted trades to be consummated whenever mutually agreeable, just as they are in actual free markets. (Lucas 1986, p. 413)

Actually, Smith's results, approvingly quoted by Lucas to defend the supply-and-demand explanation of prices and distribution in "actual free markets", are not surprising: as Lucas (1986, p. 412) himself notes by quoting Smith, in Smith's set-up individuals are faced with the "same conditions of supply and demand prevailing initially in each period". However, then Lucas is not authorised to extend these positive results to a neo-Walrasian framework (the alleged micro foundation of the RATEX approach), because the constancy of the datum relative to the vectorial endowment of capital goods cannot be plausibly justified within *that* method. In other words, Lucas must accomplish an impossible task: he must reconcile the fact that actual economies *generally* involve disequilibrium activities with his so-called micro foundation, the contingent-claim-equilibrium concept that, to avoid the path dependency of the equilibrium caused by the lack of persistence of the given vectorial endowment of capital goods, is condemned to rule out false productions, investments and trading.

§7. It may be useful to recall now that *no need* arises within the traditional neoclassical method to rely on a fictitious auctioneer to justify how equilibrium is to come about: granted the notion of capital as a single factor, this magnitude has the sufficient persistence so that, if stability can be assumed, the process of equilibration can be legitimately considered as involving actual, i.e. disequilibrium, activities (cf. §2). In view of this remark it is worth wondering whether the contingent-claim equilibrium "originally proposed by Arrow and Debreu" actually is the so-called micro foundation of the RATEX approach, as Lucas himself declares, or not. In other words, has the traditional value conception of capital actually disappeared from Lucas's analysis? As I argue below, the answer appears to be negative. Consider, for instance, Lucas's article *An Equilibrium Model of the*

Business Cycle (1975)[21] (cf. also Lucas 1988). There Lucas develops a one-sector model where the consumption-capital good is produced by labour and itself under a well-behaved, constant-returns-to-scale production function (Lucas 1975, p. 1115).

The point I wish to make is that the model developed in the 1975 paper is, essentially, a Ramsey (or Solow) model[22]; and, as notably noticed by Petri (2004, p. 326), in that kind of models the single capital good has "the same role as the single "capital" of traditional long-period equilibria [since] is taken to be a summary index of the heterogeneous capital endowment of the economy that remains unchanged (if net savings are zero) when the composition of the capital endowment in the real economy changes due to changes in relative factor prices". From this perspective it is readily seen that each RATEX equilibrium that is determined in the 1975 model presents, essentially, the same features as a traditional neoclassical (long-period) equilibrium[23]; in particular, the sufficient persistence of its data plausibly authorises one to argue that under a process of trial and error individuals can eventually come to learn the equilibrium prices, and the respective quantities that must be brought to the market. Therefore, it can be reasonably argued, at a first level of approximation, that the auctioneer is a valid simplifying device that however can be dispensed with: in any event, if uniqueness and

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21 As noted in footnote 16, in the 1975 article Lucas extends the 1972 paper to a framework where the production of and the demand for capital goods (investment) is explicitly considered.

22 In the 1975 contribution, Lucas does not model the behaviour of the household sector explicitly. However, following the standard practice of Ramsey kind of models, he suggests that consumers' demands can be derived from a representative consumer's well-behaved intertemporal-utility maximising problem. The assumption that the household sector behaves *as if* there was a single consumer who maximizes an infinite-horizon utility function is also found in e.g. Lucas (1988).

23 In this connection a point that seems to have been little noticed is worth stressing (cf. however Boianovsky 1998): it is Ramsey (1928, p. 556) himself, who, in his seminal contribution on the theory of economic growth, accepts that the very long run or secular equilibrium is useless to explain actual economic conditions since it may "never be reached". The problem is surmounted by also determining an "equilibrium in the meantime", i.e. at each point in the process of capital accumulation, on the basis of what he calls a "temporary" capital supply curve, which is simply the traditional *given* endowment of value capital, with the implication that the resulting equilibrium is a traditional long-period equilibrium.

stability can be plausibly assumed (cf. footnote 24), after sufficient time the economy will gravitate around the full-employment growth path determined by theory.[24] These considerations authorise one to conclude that, far from being a simplifying assumption, the hypothesis of one capital good, which, since in real economies capital is heterogeneous, amounts to assuming that one can treat capital as a value factor, in other words the traditional conception of capital, emerges as a *distinctive* feature of Lucas's construction, and hence the plausibility of his results stands or falls with the conception of capital as the traditional homogeneous magnitude.

Interestingly enough, it is Lucas (1988) himself who eventually admits that this is how he reasons (i.e. *as if* things continued to work in the ways justified by the old conception of capital). "We can, after all", Lucas accepts in a lecture delivered at the University of Cambridge, "no more directly measure a society's holdings of physical capital than we can its human capital" (1988, p. 35). However, "Physical capital", so Lucas continues,

> is best viewed as a force, not directly observable, that we postulate in order to account in a unified way for certain things we can observe: that goods are produced that yield no immediate benefit to consumers, that the production of these goods enhances labor productivity in future periods, and so on. The fact that the postulates of both human and physical capital have many observable implications outside the contexts of aggregate models is important in specific, quantitative ways, in addition to simply giving aggregative theorists a sense of having 'microeconomic foundations'. (Lucas 1988, pp. 35–6)

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24 We may note in this connection that, conveniently enough, the same hypothesis of a single capital good that justifies the persistence of the data of *each* RATEX equilibrium, and hence makes the identification of the latter with a traditional neoclassical equilibrium possible, also allows Lucas (1975, p. 1116) to derive well-behaved and sufficiently elastic factor demand curves for capital and labour. Indeed, the one-capital good model is sufficiently restrictive to exclude the possibility of reswitching and reverse capital deepening; phenomena that, as notably argued in Garegnani (1970), *reveal* that the adjustment processes traditionally envisaged by neoclassical authors to justify the tendency towards the position determined by the theory may not work in the expected direction, not only jeopardising the uniqueness of the equilibrium, but also its stability, both essential features to assign to equilibrium its traditional role as a centre of gravitation. Things would not be entirely different in this respect if, as Lucas occasionally does (cf. footnote 16), one were to argue that the centre of gravitation of the economy is a position of steady growth: as Schefold (2005) has recently pointed out in this connection (cf. also Burmeister 1980, pp. 124–26; Mc Kenzie 1986, p. 1337), the stability of the steady growth equilibrium would *still* require the absence of reswitching of techniques, a result that can be assured only if capital could be conceived as a single factor analogous to labour or land.

The passage clearly suggests that Lucas still envisages the several capital goods in a very traditional way, i.e. as embodiments of a single factor of variable form; capital is seen as a "force" whose increased quantity (no doubt, a result of past savings) allegedly raises the marginal productivity of labour, not a guaranteed result unless more savings means in some sense more capital in spite of the changed capital goods. This faith that things actually work in the ways justified by the old conception of capital (deprived of foundations after the Cambridge Controversies in capital theory during the 1960s) explains why the author believes that the traditional neoclassical results regarding the trends in income distribution and employment, easily derived from those "aggregative" models in which capital, as argued, is essentially treated as a single factor, are also valid in the general heterogeneous-capital context. Unfortunately, Lucas appears to forget, or to simply ignore, that "outside the context of aggregate models" these traditional neoclassical results are not generally valid.

§9. To sum up, Lucas accepts that real economies are *not* all the time in perfect RATEX equilibrium; and yet, he strongly believes that the path traced by RATEX-equilibrium prices and quantities indicates actual paths with sufficient approximation; however, given his admission that a process of learning that may occur after "enough time has passed" in a situation of unchanged distributions is necessary for RATEX to be reached, the actual path is implicitly admitted to be potentially rather different in the very short period and even in the short period from the RATEX-equilibrium path, so the neo-Walrasian method can have a correspondence with observation, and can be also used to derive policy prescriptions, only because there are trial-and-error processes of adjustment going on in the economy that ensure that long-period trends essentially coincide with the ones predicted by his one-good models. In short, despite his claims to the contrary notwithstanding, Lucas's arguments are fully traditional, as he himself eventually admits: he reasons *as if* things continued in fact working in the ways justified by the old conception of capital, the single factor measured in value terms. The main conclusion that comes out from this discussion is that Lucas has *no theory at all*: neither are his results supported by modern general equilibrium theory, unable to deal with *actual* disequilibria, nor can his claims be justified by traditional neoclassical theory, since the need to specify the endowment of capital in value terms makes equilibrium indeterminable.

3. Hahn on neo-Walrasian equilibrium

§10. While Hahn (1984[1982a], p. 114) openly admits that the neo-Walrasian method of analysis developed since the 1930s, and in particular

the notion of intertemporal equilibrium perfected by Arrow and Debreu in the 1950s, "is a major intellectual achievement" (cf. also Hahn 1973a, 1973b), he also believes that this equilibrium *only* is an *abstract* solution *concept* that characterises a purely *hypothetical* situation in which agents' plans are mutually compatible and hence implementable; therefore, Hahn goes on to affirm, this construction can be only useful as a *benchmark* against which theoretical propositions regarding the behaviour of actual economies can be assessed (Hahn 1973a, 1973b, 1984[1982b]).[25] Hahn is therefore harshly critical with those scholars who, like Lucas, have attempted to justify the possibility of a correspondence between RATEX theory and observation allegedly relying on the notion of equilibrium developed by Arrow and Debreu as their micro foundation.

The basis for Hahn's criticism is, first, that for an RATEX-equilibrium path to be endowed with an explanatory-predictive role, it is necessary that the position determined by the theory is unique. However, on the grounds of the negative results on uniqueness reached during the 1970s, Hahn (1984, p. 4; cf. also 1984[1982b], p. 313, 1990, p. 237) notes that "it is only very rarely the case that one has any reason to claim that equilibrium is unique". Moreover, the indeterminacies caused by multiple equilibria, as Hahn (1987, 1990) remarks, are particularly pervasive within the RATEX approach: the assumption of RATEX to surmount the lack of complete markets may cause "self-fulfilling" or "sunspot" equilibria to emerge, with the implication that

> the economy evolves the way in which it does because expectations are what they are and not for any 'real reason'. (Hahn 1987, p. 584)

where by "any real reason", Hahn means the data of neo-Walrasian equilibria relative to factor endowments, preferences and technology.[26]

In Hahn's view, the surmounting of the indeterminacies caused by self-fulfilling beliefs needs a reasonable *mechanism* that, by explaining how individuals manage to *coordinate* their expectations, can be used to plausibly justify which of the possible equilibrium paths is actually followed by the economy. However, these mechanisms, the argument continues, are

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25 The claim that intertemporal equilibrium can be used as a benchmark will not be discussed here. For our purposes it is enough to note that this role of the notion of intertemporal equilibrium presumes that this equilibrium concept cannot indicate actual paths with sufficient approximation.

26 *Cf.* e.g. Cass and Shell (1983), in particular the appendix of that article for an example of the possibility of a self-fulfilling (sunspot) equilibrium that is *not* a randomization of the "real" equilibria.

missing in RATEX literature: the problem is surmounted only because the economy is assumed to rely on a fictitious agent, the so-called "Central Planner", who performs the task of coordinating individuals' believes. "It seems to me", the author therefore concludes,

> that this is not so much sidestepping the problems as turning one's back on them. (Hahn 1990, p. 238)

§11. Before turning to assess the scope and limits of Hahn's previous objection, let us first consider the second critique raised against Lucas and the RATEX school, which essentially turns round their claim that the "economy is always in equilibrium". The objection here is not that much that this assertion presupposes that neo-Walrasian equilibria are stable while, however, "only very special assumptions seem to ensure this happy outcome" (Hahn 1984[1982a], p. 125).[27] Rather, Hahn will remark that even when stability can be demonstrated, "the speed of convergence may *be very slow*...[hence] even if final clearing of all markets were achieved, it would represent an equilibrium constrained by the debris of the actual groping process" (Hahn 1970, pp. 2–4; cf. also 1982a, 1982b, 1984, 2008), namely under the actual implementation of disequilibrium adjustments neo-Walrasian equilibria would be generally path dependent, as we noticed in §2. The surmounting of this problem by the recourse to an auctioneer-guided tâtonnement "is very strained" (1970, p. 6), Hahn continues, since it implies that no disequilibrium activities are actually allowed to take place, although "we know at full well that actual binding decisions are in fact made at all prices" (1970, p. 3). While in the 1970 contribution Hahn limits himself to argue that "it is doubtful" (1970, p. 3) that the tâtonnement process is an "appropriate simplification" (1970, p. 3) for stability analysis, in Hahn (1982a, 1982b) the author strengthens his position further and reaches a very similar conclusion as the one we arrived at in the introduction (§2). "It is obvious", he asserts, that the auctioneer-tâtonnement

> is incapable of providing a satisfactory answer to the stability question in most actual economies. (Hahn 1982a, 1982b, p. 746; see also 2008, p. 274)

Unlike us however, Hahn does not draw from these considerations the conclusion that the recourse to the auctioneer *already* presupposes an immediate correspondence between the theoretical and the empirical variables. This conclusion is reached on a slightly different basis: he notes

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27 In Hahn (1970, 1982a), there is a thorough assessment of the problems of stability in modern general equilibrium theory.

that the RATEX school is not really worried about the possible cases of instability because it simply *assumes the problem away* by imposing "the axiom that the economy is at every instant in competitive equilibrium" (Hahn 1984, p. 4; cf. also Hahn 1970, p. 6, 1990, p. 241). "To that extent", Hahn declares, "we seem to be prepared to live on faith" (Hahn 1970, p. 6).

§12. If we now turn to assess the scopes of the critiques raised by Hahn, it would seem at first sight that they are well justified. Hahn takes Lucas at *his words* and correctly points out the illegitimacy of his reasonings: given that Lucas openly declares that this approach is built on the basis of the neo-Walrasian-equilibrium method, he has no right to assume that the economy will follow a unique equilibrium path as it is the case in the Solow–Ramsey models he has recourse to. However, even granting the uniqueness of this path, the problems of stability and, above all, of persistence that invade neo-Walrasian equilibrium theory in any of their forms will a fortiori affect RATEX equilibria too. The implication is that the claim that the economy follows a well-determined RATEX path has no solid justification behind: it becomes, as Hahn declares, an "axiom" that implies living "on faith". One can therefore hardly disagree with the author when he writes that "It is a triumph of wishful thinking that a number of economists... take... rational expectations, as descriptively satisfactory" (Hahn 1992, p. 5).

However, on the basis of the remarks made in section II (§6 and §7), I would like to make the following point, which it not often noticed and seems to have escaped Hahn's attention too: the claim that the RATEX path is a good approximation of the actual path of the economy was *not really* justified on the basis of the notion of equilibrium "originally proposed by Arrow and Debreu", as Lucas would have made us to believe. Albeit usually implicitly, it was justified on a basis which would have been much plausible, *if* old neoclassical modes of explanation (hence the traditional notion of capital) could be accepted.

On these grounds, it seems that the critiques raised by Hahn to the RATEX school lose much of their force. Indeed, let us start by considering Hahn's second objection: we have noticed that Lucas *does not* actually claim that the economy is *always* in RATEX equilibrium; on the contrary, he admits that individuals are likely to make mistakes in their forecasts, and accepts that they adapt their behaviour by learning and experimentation under adjustments that may take sufficient time to assert themselves; we have further seen that behind his recourse to the auctioneer there is in fact a presumption that adjustments are stable and time-consuming relative to an economy whose data actually change relatively slowly, and hence there emerges a faith in the connected traditional neoclassical adjustment

mechanisms of substitution, hence in the traditional notion of capital in value terms.

If we now turn to consider Hahn's first critique, namely that self-fulfilling expectations may cause the indeterminacy of the equilibrium path, note that the old conceptions of equilibrium and of capital also remove – or at least considerably weaken – the relevance of this objection too. If as we have suggested, each RATEX equilibrium is interpreted as a traditional neoclassical equilibrium, its slowly changing data allow individuals' expectations – that may depend on purely transitory and accidental factors – to be revised and corrected in the light of experience. In other terms, the persistence of the data justifies the treatment of expectations as *endogenous* variables, rather than as determinants of the equilibrium; no need therefore arises to rely on a fictitious "central planner" to justify how individuals' beliefs are coordinated, as Hahn seems to believe. The process of coordination of individuals' expectations would be one and the same thing as the process of convergence towards the equilibrium determined by the more persistence data, *if* that convergence can be assumed (cf. Garegnani 2012, p. 1427; Petri 2004, p. 43).[28,29]

In sum, the point I want to make is that granting the notion of capital as a single factor that, incidentally, as Hahn (1982b, p. 370) *does consider* to

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28 This however does not mean denying the influence of individuals' expectations in economic variables. If necessary, this influence can be examined at a second level of approximation while studying the oscillations around the equilibrium path.

29 We may also note that the value notion of capital also considerably weakens the scope of a third objection occasionally raised by Hahn (1990, p. 237) to the RATEX school: Hahn argues that once complete future markets are accepted not to exist, 'self-fulfilling' beliefs, or possible expectational mistakes, can well cause Pareto inefficient outcomes. While a neoclassical author who, like Lucas, accepts that agents are likely to make mistakes would not disagree with Hahn on this issue, the traditional notions of equilibrium and of capital would give her good reasons to dismiss the relevance of this problem too: granted the notion of capital, the persistence of the data and the correct working of the factor substitution mechanisms would ensure that over sufficient time the action of the supply and demand forces would eventually assert itself fully, hence the possible inefficiencies (apart from those not surmountable by the market, e.g. externalities, etc.) will be gradually corrected in the light of experience, exerting a relatively minor negative effect on growth. Moreover, precisely because the tendency towards the full employment of resources would be constantly at work, this scholar would be on strong grounds to object to Hahn's claim that the absence of complete future markets "leaves *vast scope* for Government intervention" (1990, p. 246): she would argue, granted the tendency towards the full employment of resources, that the scope for Government intervention would actually be quite narrow.

have some "heuristic" value,[30] the conclusions traditionally reached by neoclassical theory and now defended by Lucas acquire plausibility. We see here why the conception of capital as a single factor (and hence in value terms) is not a "simplification" or a "parable" as Hahn (1982b, p. 370) has occasionally argued, but is a necessary requisite for the plausibility of the entire supply and demand approach.

Therefore, while RATEX scholars do have faith in the explanatory properties of general equilibrium theory, it is not because they think that the economy is constantly in equilibrium, but rather because they believe that the path traced by the sequence of RATEX equilibria is what the economy approximately follows because of the traditional neoclassical gravitational mechanisms. Therefore, they still have faith in those *same* persistent forces and mechanisms – whose plausibility stands or falls with the notion of capital as a single factor – as those used by the founders of the marginalist approach to argue that the economy would follow on average a full-employment growth path (with factors earning their marginal products). Now, it is true that e.g. Lucas seldom admits that this is how he reasons, so Hahn is justified to some extent in accusing him – taking his statements at face value – of "being prepared to live on faith"; but Hahn never seems to realise what faith this author relies on, and what is wrong with that faith: its implicit reliance on the indefensible notion of capital as a single value magnitude. All this seems to explain why Hahn finds it so difficult to believe, a "mystery" in his own words (Hahn 1984[1982b], pp. 308–9), that RATEX paths, allegedly grounded on modern general equilibrium theory, can still be argued to indicate actual paths with sufficient approximation.[31]

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30 In his well-known article "*The neo-Ricardians*" (1982b), Hahn (1982b, p. 370) "doubts" that one-capital good models are actually "useless" (1982b, p. 370), and then he adds, "we use simple models (e.g. macroeconomics) to gain insights of a certain kind. Simplification is never without cost and the cost is sometimes loss of rigour. It remains to be shown that the cost is too high in this instance". Therefore, even Hahn seems to hesitate to give up the picture of economic growth derivable from one-goods models. This may explain why in his own work (cf. Hahn and Solow 1995, chapter 6), Hahn himself has had recourse to a standard neoclassical production function with labour and capital, the single factor, as arguments to model the behaviour of firms. This may also explain why Hahn has accepted in general terms the negative relationship between the rate of interest and investment decisions (cf. e.g. Hahn 1992, p. 11)

31 Incidentally, the appendix to this paper shows that Lucas and his school are not the only ones to attribute to modern general equilibrium theory an explanatory role by introducing traditional neoclassical reasonings that presume the old notion of capital as a single factor. This same attitude is also present in general equilibrium specialists like Arrow.

§13. Hahn's inability to see the hidden (albeit illegitimate) defensive belt behind Lucas's claims is not actually surprising, considering that he does not seem to grasp the central role played by the value notion of capital to make a correspondence between neoclassical theory and observation possible. To confirm this, it is sufficient to examine the following short statement by Hahn: "Why", the author wonders,

> do people balk only at aggregation of machines and not of people? (Hahn 1975, p. 364)[32]

But the issue of "aggregation of machines" is considered by the critics of neoclassical theory to be more problematic than that of "people" simply because the entire plausibility of the neoclassical approach crumbles once the illegitimacy to treat, under sufficiently general conditions, the factor capital as a single factor, is accepted, and hence the factor capital is specified as a vector of physical capital goods. Besides the problem of persistence,[33] we noticed in the introduction that when each capital good is considered as an independent factor, there will generally be insufficient factor substitutability, with the implication that, for instance, an extremely low level of wages, probably zero, can emerge in equilibrium (or possibly so high a wage as to reduce the other gross rentals to zero). Clearly, the implausibility of this result reveals the implausibility of the neo-Walrasian method itself. As Hahn himself acknowledges in his 1971 work with Arrow, the equilibrium position should be economically "sensible". "By sensible", they explain,

> of course, we can mean all sorts of things. Certainly, though, we should not be much interested in an equilibrium with a zero real wage. (Arrow and Hahn 1971, pp. 354–55)

4. Concluding remarks

§14. The overall argument of this paper can be summarised as follows. The blind alley in which the neoclassical approach currently finds itself seems to be the outcome of the impossibility of this approach to satisfactorily

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32 Cf. Petri (1999) and Garegnani (2003, appendix II) to find other statements by Hahn that further confirm the inability on the part of this author to understand the importance of the notion of capital as a single factor for neoclassical theory.

33 Unlike the composition of a given vectorial endowment of capital goods, which can be *very quickly* altered, the different kinds of labour (and land) need not be aggregated because their endowments can be safely assumed to change *slowly* relative to the speed of adjustment of disequilibrium prices towards their equilibrium values.

include capital goods within the supply-and-demand explanation of prices and distribution. Moreover, this has pushed neoclassical authors to face the *unpleasant dilemma* of having to choose between endowing general equilibrium theory with an explanatory role marred by its illegitimate, albeit implicit, notion of capital as a *single* scalar measured in value terms; or alternatively, to consistently treat each capital good as a *distinct* factor of production, and accordingly to renounce to use general equilibrium theory to account for the trends in actual economies. As seen, Lucas and the RATEX school have adopted the first position, while the second stance has been endorsed notably by Hahn. This scholar, moreover, has shown the capacity to take authors like Lucas at their words and to show that, if modern general equilibrium theory is taken seriously, it cannot be the micro foundation of Lucas's reasoning. However, this is only half of the story: while correct, Hahn's critiques also evidence a clear inability on the part of this author to understand that, in fact, the *real* justification of Lucas's arguments are traditional gravitational modes of explanation, which would have been plausible had not they been marred by their implicit treatment of capital as a single factor of production, measured in value terms.

I wish to conclude by suggesting that the failure to understand the logic behind these traditional neoclassical modes of explanation may explain why Hahn has not followed to its bitter end the implications of his destructive critiques to Lucas's kind of arguments: for when Hahn comes to indicate what kind of equilibrium would have the explanatory, positive role that is wrongly attributed to modern general equilibrium theory, one finds in him a very interesting admission. In a passage of *History and Economic Theory* (1991), Hahn writes:

> equilibria which cannot be reached from historically given initial conditions by an acceptable process of learning should, I contend, be ruled out. What that means is that the equilibrium definition *should include* the requirement of *reachability*… All this may be summed up by saying that economic theory should deal with equilibria which are *stable* under some *acceptable* process. (Hahn 1991, pp. 70–71, emphasis added)

However, from these considerations one may expect Hahn to draw the negative conclusion, given that any "acceptable process" of adjustment must allow for the implementation of *actual*, i.e. disequilibrium, activities and therefore the position to which the adjustment converges cannot be defined and considered "stable" on the basis of a given vector of capital endowments known before the adjustment has been completed (i.e. the several endowments of capital goods must be *endogenously* determined variables), that the neo-Walrasian treatment of capital is simply incompatible

with the "requirement of reachability". In view of the fact that in neoclassical theory the treatment of the capital goods as endogenous variables requires the value specification of the capital endowment of the economy, which is unacceptable, Hahn would therefore have to admit that a positive theory of prices and distributions requires the neoclassical approach and its supply-and-demand logic to be rejected, and replaced by an *alternative* theory. On the contrary, this author has been always reluctant to leave that logic behind,[34] as if, deeply inside, Hahn himself shared Lucas' (and also Arrow's, see the Appendix) *faith* in the explanatory power of the neoclassical approach.

Acknowledgements

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34 Hahn (1984, p. 10) has, for instance, argued that Arrow–Debreu theory "gives us the best base camp for sallies", namely it *must* be the starting point of economic theorising. This may explain why even Hahn has eventually fallen in the temptation to endow modern general equilibrium theory with an explanatory role. Consider, for instance, the following sentence by Hahn: "The crudest empirical observations will convince one that there is no unique rate of profit to be observed in the economy. Do we conclude from that that competition is functioning badly? Answer: No. Consult any general equilibrium text" (Hahn 1975, p. 361). Note then how Hahn is here declaring that general equilibrium theory would have a positive role, namely it could explain why actually observed (own) rates of profits are not necessarily uniform in the different businesses, without this fact implying the absence of free competition.

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Appendix: Arrow and the neowalrasian method

§15. We have seen in the main text that the real reason why Lucas attributes to RATEX equilibria an explanatory-predictive role is that he relies on traditional neoclassical reasonings, despite allegedly adopting the equilibrium notion developed by Arrow and Debreu as his micro foundation. This appendix shows that this same attitude can be found in some of the writings of general equilibrium *specialists* like Kenneth Arrow. It will therefore further contribute to unravel what Hahn (1984[1982b], pp. 308–09), being unfamiliar with those traditional reasonings, can only see as a "mystery", namely "how it came about that the Arrow–Debreu model came to be taken descriptively". Let us consider Arrow's (1974) contribution. On one hand, he openly admits (1974, p. 253) that "The balancing of supply and demand is far from perfect", and hence "by recurring periods" labour unemployment and the existence of idle capacity are likely to emerge, and he adds, "by very considerable magnitudes". But "when all due allowances are made", he continues,

> the coherence of individual economic decisions is remarkable. As incomes rise and demands shift, for example, from food to clothing and housing, the labor force and productive facilities follow suit. Similarly... As technology improves exogenously, through innovations, the labor made redundant does not become permanently unemployed but finds its place in the economy... On the other hand, a growing accumulation of instruments of production raises real wages and in turn induces a rise in the prices of labor-intensive commodities relative to those which use little labor. All these phenomena show that by and large and in the long view of history, the economic system adjusts with a considerable degree of smoothness and indeed of rationality to changes in the fundamental facts within which it operates. (Arrow 1974, pp. 253–54)

Note, first, how in this passages the equilibrium, which no doubt Arrow identifies with intertemporal equilibrium, *does play* an explanatory role. Second and perhaps more surprisingly, note that, in order to explain how the "remarkable coherence of individual economic decisions" actually takes place, Arrow envisages adjustments that take sufficient time to exert their effects, and he explains the tendency towards equilibrium in the factor markets by having recourse to the direct and indirect factor substitution mechanisms in terms of labour and "instruments", the latter clearly seen as embodiments of quantities of a single factor since he speaks of "accumulation of instruments" for what on the contrary generally represents a nearly total change in the types of capital goods existing. He speaks of this accumulation of "instruments" as raising the marginal product of labour, and he speaks of labour intensity of products as if ascertainable through the simple ratio of labour to "instruments" and independent of prices. The avoidance of the word "capital" (probably due to the fact that at that time the results of the Cambridge controversies had already taken root in a

considerable part of the economics theorists) is not enough to hide the fact that the logic of the argument implies the possibility of treating the different capital goods as representing embodiments of a single factor capital of variable "form", whose ratio to labour can be treated as ascertainable independently of income distribution, and whose increase allows Arrow to speak of "accumulation". In other words, the argument assumes the traditional conception of capital, which is also what authorises the admission of actual, time-consuming, adjustments tending to definite results. Like Lucas then, Arrow still believes in the old neoclassical adjustment mechanisms based on capital the single factor, and it is this belief that makes him attribute to general equilibrium a positive, descriptive role, showing in all this less clarity than Hahn on the difficulties of justifying those mechanisms on the basis of the kind of general equilibrium he has formalised.[35]

To conclude, also consider what Arrow writes when he attempts to describe the policy implications of the Second Welfare Theorem. He says:

> General competitive equilibrium above all teaches the extent to which a social allocation of resources can be achieved by independent private decisions coordinated through the market. We are assured indeed that not only can an allocation be achieved, but the result will be Pareto efficient. But...there is nothing in the process which guarantees that the distribution be just... If we want to rely on the virtues of the market but also to achieve a more just distribution, the theory suggests the strategy of changing the initial distribution rather than interfering with the allocation process at some later stage. (Arrow 1974, p. 269)

Clearly there is here the presumption that the spontaneous action of the market forces of supply and demand will allow the equilibrium corresponding to the new distribution of endowments to be established and, moreover, that this more "just" equilibrium will only depend on the new configuration of the data, that is to say, that it will be independent of the details of disequilibrium productions and transactions. This is, in Arrow's view, what "the theory suggests".

Abstract

By examining the contributions of two prominent contemporary neoclassical economists, i.e. Lucas and Hahn, the article attempts to shed light on the problematic relationship between neoclassical theory and

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35 In this respect, Petri (2004, chapter 5, appendix 5.A.2) has noticed that in his discussion of the problem of price taking (Arrow 1959), Arrow in fact does not seem to be very clear that traditional ways of reasoning are illegitimate when applied to modern versions of general equilibrium theory.

observation. It is argued that this approach must face the unpleasant *dilemma* of having to choose between endowing general equilibrium theory with an explanatory role that is marred by its illegitimate notion of capital as a single factor of variable form (Lucas); or alternatively, to consistently treat each capital good as a distinct factor of production, with the bitter implication that the theory must simply renounce to have a correspondence with observation (Hahn).

Keywords

Lucas, Hahn, neo-Walrasian theory, centre of gravitation, capital theory