



ARTÍCULO

Let us wake up to beauty

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Abstract The present article has three purposes. The first is to describe what was, at the time, a highly promising research program known as Disequilibrium Macroeconomics, the difficulties of developing it and the importance of its insights for understanding the nature of the Great Recession and of the possible ways out of it. Hence the interest in waking up this sleeping beauty. The second purpose is to discuss two topics that were probably at the center of the lack of success of this approach: Effective demand Failures and the Neoclassical Corridor. The deeper examination of these two ideas and the clarification of the debates around them constitute the third purpose of the article. And finally some ideas are presented in favor of using a complex systems approach to understand both the development of ideas and the nature of the economic system.

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Despertemos a la belleza

Resumen Este artículo persigue tres objetivos. El primero es describir lo que, en su día, fue un muy prometedor programa de investigación conocido como Macroeconomía del Desequilibrio, las dificultades para desarrollarlo y la importancia de sus visiones para comprender la naturaleza de la Gran Recesión y de las posibles vías de salida. De ahí el interés que tiene el despertar a esa bella durmiente. El segundo objetivo consiste en discutir dos temas que posiblemente estuvieron en el centro de la falta de éxito del programa: los Fallos de Demanda Efectiva y el Corredor neoclásico. El examen más detallado de esos dos temas y la clarificación de los debates en torno a ellos constituyen el tercer objetivo del artículo. Y finalmente se presentan algunas ideas en favor de usar un planteamiento de sistemas complejos para comprender tanto el desarrollo de las ideas como la naturaleza del sistema económico.

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1. Introduction

The present article, written especially for the 100th issue of *Cuadernos de Economía*, will try to concentrate the attention of the reader on one aspect of another piece (Urrutia, 2009) written four years ago, in the 88th issue (2009) of the same journal, and dedicated to the Great Recession under the title “Una Visión (Semi) Heterodoxa de la Crisis”. The unorthodox part of it had to do with what, at that time was known as Disequilibrium Macroeconomics and more specifically with the idea of the Neoclassical Corridor, also known as the “stability corridor”: a notion related directly to the topic of Effective Demand Failures, and indirectly to fixed prices. The idea of the corridor marks exactly the turning point of the hegemony of Disequilibrium Macroeconomics, an approach that, we may say, has its remote origins in Chapter 13 of the 2nd edition (1965) of Patinkin’s famous book of 1956; its actual development through several works of Barro, Grossman, Clower and Leijonhufvud in the late sixties and the beginning of the seventies. After an overlap with that part of Monetary Theory trying to introduce *fiat* money into the General Equilibrium Model, this approach began to ebb around 1973, to abruptly disappear at the end of the seventies while still in full swing of its popularity —marked by the generalized use in the teaching of Macro in the books by Barro and Grossman (1976) and by Malinvaud (1977). In spite of the fact that some up to date dynamic macromodels in the Keynesian tradition use fixed prices with great academic success, one can safely say that Disequilibrium Macroeconomics, and in particular the Neoclassical Corridor, are by now, real “sleeping beauties”.

The purpose of what follows is to explain what was the filter that put these beauties to sleep, describe their intellectual flaws, if any, and shed some light on what was really hidden in their beautifulness. If we wake up our senses to them, we could use these concepts as a guide to the understanding of the present crisis, to the way to be led out of it and, most important, to the nature of an economic system. It is a paper on ideas, rather than a piece on the history of economic doctrines. It is a call to take into account some intuitions coming out of complex systems (e.g., Arthur et al., 1997). These intuitions are the offspring of a clear understanding of the notion of Effective Demand Failures that might have been left behind in a not so distant past, but may be now recovered if we want to introduce yet another turn in the direction of economics.

I will proceed as follows. In the next section I will present, quite briefly, some simple ways of understanding the evolution of ideas and its application for the so-called Disequilibrium Macroeconomics, its origins, and the filter that put it to sleep. In the third section, the main part of this paper, I will make use of a very old Note of mine, never published before, in order to present the notion of the Neoclassical Corridor, both as a case study in the unpredictable evolution of economic ideas, and as an unexhausted source of intellectual inspiration, once the filter that screened it off a few decades ago was starting to fail. In the fourth section, I will try to draw on this potential to diagnose the present economic situation. The final section summarizes and offers some closing comments.

2. Evolution of ideas. From vision without technique to technique without hindsight

I learned Macroeconomics at the end of the sixties and the beginning of the seventies of the last century. The program dedicated two semesters to the topic. In the first one I read with awe a wonderful article by Hicks (“Mr. Keynes and the Classics”, 1937) and parts of two of his great books (*Value and Capital*, 1939, with its appendix dedicated to general equilibrium; and *Value and Growth*, 1965). Plus the *locus classicus* of the neoclassical synthesis represented by Patinkin’s *Money, Interest and Prices* (1965), specially the, at the time, almost esoteric chapter 13. In the second semester, I was exposed to the first writings on Disequilibrium Macroeconomics and became a real fan of an approach which did not make a sharp distinction between the not very well defined Macro and the apparently indestructible and towering General Equilibrium Model of Arrow and Debreu (1954) and followers. The challenge was indeed to build up micro-foundations to bridge the gap between Macro and Micro.

All these readings led to the writing of a thesis attempting to construct growth models where money played a role just as important as the idea of an exchange structure eliminating the auctioneer. Money should not be neutral, monetary policy therefore having a role to play, and the Phillips Curve was not necessarily negatively sloped. In this dissertation the, at the time, recent contributions of Clower and Leijonhufvud were a *must*, together with the first writings of H. Grossman, with or without Barro. But the task was not that easy, given that another development started almost simultaneously. Questions of expectations, aggregation of assets and aggregation of agents had far from obvious answers, and the efforts devoted to introduce money into the axiomatic approach to general equilibrium (as the one by Hahn, 1965) were not that successful. Despite the efforts made by people trying to think on temporary equilibrium, distribution effects and alternative ways of thinking about interest rates depending on the aggregation of assets (see, e.g., Gallastegui & Urrutia, 1988)

I found this intellectual climate fascinating and, in my view, the profession should have made it last longer. But it did not happen. The general equilibrium theorists continued with their task of completing the model, a task that took at least until the middle eighties and their work became the obvious reference for intellectual excellence. It was not the right moment for challenging its prominence or ask for a new model that could introduce money, take into account differences between individuals (so as to take into consideration distribution effects), contemplate the effects of alternative aggregation of assets (curiously out of the picture) and use temporary equilibrium, corresponding to the above mentioned period analysis, so as to discuss expectations meaningfully. Such task would have been formidable and we can all understand the rather opportunistic behavior of almost every macroeconomist, drifting away from static models towards dynamic ones. Abstracting from many traits of reality, these latter could “easily” introduce rational expectations. The last gasps of life of Disequilibrium Macroeconomics found their place in the two books of Barro and Grossman (1976) and Malinvaud (1977), while simultaneous-

ly the so called DSGE (Disequilibrium Stochastic General Equilibrium) models started to rule the roost.

The so called stagflation of the late seventies killed definitely the disequilibrium approach and now, after five years of recession, the DSGE model is facing the same kind of danger. It seems ironic that some of the ideas of the disequilibrium approach (*vision without technique*, as Backhouse & Boianovsky (2012) once called it) could come to macroeconomics (see, e.g., Galí, 2012, using the fix price method). It might be characterized as “technique without hindsight”. That this is the case is, at least, what I contend in this paper. And I am not alone here, if we consider the post published by Axel Leijonhufvud in VOX EU (Leijonhufvud, 2009b) (21st November, 2009) under the title “Stabilities and instabilities in the macroeconomy”. Let me quote a few significant paragraphs:

Around the turn of the century [...] macroeconomists came to a “brackish” compromise known as the New Neoclassical Synthesis. The New Keynesians adopted the dynamic stochastic general equilibrium (DSGE) framework pioneered by the New Classics while the latter accepted the market “frictions” and capital market “imperfections” long insisted upon by the former. This New Synthesis, like the Old Synthesis of fifty years ago, postulates that the economy behaves like a stable general equilibrium system whose equilibrating properties are somewhat hampered by frictions. Economists of this persuasion are now struggling to explain that what has just happened is actually logically possible. But the recent crisis will not fit. The syntheses, Old and New, I believe, are wrong. They stem from a fundamental misunderstanding of the nature of a market economy. Further technical innovations in economic modeling will not bring real progress as long as “stability-with-frictions” remains the ruling paradigm. The genuine instabilities of the modern economy have to be faced.

The economy is an adaptive dynamical system. It possesses the self-regulating, “equilibrating” properties that we usually refer to as “market mechanisms”. But these mechanisms do not always suffice to ensure the coordination of activities in the complex system. Almost forty years ago, I proposed the “corridor hypothesis”. The hypothesis suggested that the economy might show the desirable “classical” adjustment properties within some “corridor” around a hypothetical equilibrium path but that its self-regulating capabilities would be impaired in the “Keynesian” regions outside the corridor. For large displacements from equilibrium, therefore, the market system might not be able to recover unless aided by stabilization policy.

The original argument for the corridor concerned the conditions under which to expect significant deviation-amplifying multiplier effects and might not be all that persuasive by itself. It is the case, however, that all other known complex dynamic systems, whether human-made or occurring in nature, are known to have the property that their homeostatic capabilities are limited. It is extremely unlikely that the economy would be different in this regard. It is reasonable to believe, therefore, that the state-space of the system—in addi-

tion to regions with good equilibrating properties—has regions where deviation-amplifying processes have impaired these properties [...].

3. Effective Demand Failures: the neoclassical corridor

3.1. Introduction

The purpose of Leijonhufvud’s (1973) paper was to stimulate the reallocation of intellectual resources towards the difficult, important and unresolved questions gravitating around the notion of Effective Demand Failures, all of them in urgent need of theoretical modeling and empirical testing. Grossman’s comment can be understood as stating, by implication, that the questions raised by Leijonhufvud one year earlier were neither unresolved nor, at any rate, new or important. Hence they would not need any special theoretical modeling.

Here I will argue that Grossman’s misgivings were caused by the attention he paid to what I call the Clower-Leijonhufvud first thought (CL1), neglecting Leijonhufvud’s new suggestions, from now on the Clower-Leijonhufvud second thought (CL2). However, since Leijonhufvud did not clearly bring up these latter suggestions nor exploit them at that time (or later), there is room for additional analysis, taking advantage of what we have learned over the last 40 years. I will offer some (admittedly incomplete) tips for the correct modeling of those ideas.

3.2. Theory versus Analysis¹

According to Leijonhufvud, the central issue of Macro-theory is whether the market sectors of an economy behave as a “self-regulating system”, an expression that sounds understandable now that we are familiar with complex systems, but was not so at the time. The social correlate of this central issue was, and is, the “coordination of economic activities”, where full coordination has to be understood as clearance of every market, and has nothing to do with optimality of allocation. The problem is then whether there are automatic tendencies towards full coordination, and how strong they are. In Grossman’s (1974) compact statement, the central issue concerns the “nature of the market clearing process” (p. 1).

A superficial reading of the previous statement, especially in Grossman’s compact translation, may lead one to think that the problem is not new and that it has been adequately treated by the literature on General Equilibrium (GE) and the stability of GE allocations.² However, the standard GE model (Arrow & Debreu, 1954) and most of the available exercises

1. The distinction is basic to Leijonhufvud (1973) and appears explicitly or implicitly in the following passages: p. 29 at the beginning of last paragraph; p. 29, in the last paragraph of fn.1; p.31, first complete sentence; p. 3, 4th and 5th lines; p. 35, in the last paragraph of fn. 3.

2. That stability analysis was widely off the mark, even in analytical terms (to be defined more precisely below), has been implicitly recognized by all recent contributions dealing with markets without perfect information. For a survey, see Rothschild (1973).

on the stability of GE configurations are analytical constructions.³ What Leijonhufvud is arguing for are theoretical constructions. Now, the problem of analysis is how to fit reality into it, i.e., to what extent is one particular feature of reality well represented by the analytical features of the model and, therefore, to what extent can the analytical model yield insights into that particular feature of reality. The problem of theory, on the other hand, is how to model this particular feature of reality, i.e., how to construct an analytical apparatus in terms of the theoretical features of reality. The stated distinction is not very clear but it can be illuminated by an example important for our present purposes.

Consider the particular feature of reality that we call monetary exchange. The now forty years-old contributions to the literature on the integration of monetary and value theory can be classified as analytical or theoretical according to whether they try to fit money into the standard GE model or whether they try to introduce choice-theory elements into the theory of money. The motto of analytical contributions is: “A model of the economy as satisfying as Debreu’s, but which can accommodate money” (Hahn, 1971, p. 418). The motto of theoretical constructions might very well be: “A model of the economy as satisfying as Keynes’, but which can accommodate choice-theory elements”.⁴ The illustrated distinction may just represent two different attitudes towards a problem and both approaches (analytical and theoretical) might very well converge, but in fact they have not, as far as I know. In any case Leijonhufvud’s quest for theory seemed to imply that a reallocation of intellectual resources ought to have taken place. We should have moved from GE analysis to the Theory of Coordination of Activities and from stability analysis to a Theory of Exchange capable of explaining, in economic terms, how exchange is actually carried.⁵ The Theory of Exchange would supposedly find sets of conditions for the emergence of well-defined transactions structures and the Theory of Coordination of Activities would then tell us under what conditions would a particular transactions structure lead to full coordination.⁶ It will be argued in the sequel

(and it has to be argued because it is not clear in Leijonhufvud’s original paper of 1973) that much of what he has to offer is related to the theoretical necessity of incorporating the idea of the transactions structure. This is, in my opinion, what Grossman fails to see in his comment of 1974.

3.3. CL1, its generalizations and the essence of CL2

The just described quest for theory is basic to Clower (1965) and Leijonhufvud (1968) in their reappraisal of Keynes. Starting from the theoretical idea of a monetary economy, in which there are as many spot markets as non-money commodities (n)—which are visited sequentially—and no futures markets, Clower (1967) builds an analytical partial equilibrium model without an auctioneer and without stocks. At a non-equilibrium price vector, “false” trading will take place and will lead to the “labor sales-constrained demand”, typical of the so called *dual decision hypothesis*. In a monetary economy, each market reveals only the excess demand for that commodity without revealing at the same time excess demands for all other commodities. This construction is sufficient to isolate the crucial choice-theory foundation of Keynes theory. If we now add, following Leijonhufvud (1968), that, in a monetary economy there are reasons for laborers to engage in a search behavior before adjusting wage rates, once the price vector is displaced from equilibrium by any shock, quantity adjustments take place and multiplier mechanisms are triggered immediately. This constitutes CL1 and from here on when we mention quantity adjustments we refer only to those here described.

We can now move into CL2 but, before we proceed, let us begin by noting that the dual-decision hypothesis can be generalized into a system of general “sales-constrained demands” and “purchase-constrained supplies”, together with the agents search behavior necessary to justify the fixity of prices. In Hicks (1965) fix-price method, it is analytically sound to let a *tatōnnement* in quantities run its course until it reaches what Benassy (1974) called a K-equilibrium, which is obviously Pareto inefficient.⁷ According to *period analysis* (Grandmont & Laroque, 1976), at this point, prices are allowed to adjust in response to certain pressures, taking into account the interaction among markets implicit in the definition of effective or sales-constrained demands (Grossman, 1971).⁸

Now we can move to CL2. One aspect of CL2 (from now on, CL2A) is the removal of a theoretically unsound feature

3. The 1962 work by Hahn and Negishi on the stability of a non-*tatōnnement* stability mechanism could be considered as theoretical in the sense that it incorporates the theoretical feature of lack of recontracting. See also Arrow and Hahn (1971, ch. 13).

4. One of the first examples of the first approach is Hahn (1971). Clower’s (1965, 1967 and 1971) and Leijonhufvud’s (1968) works are the original examples of the alternative approach.

5. For a terse explanation, see Hirshleifer (1973).

6. The distinction between analysis and theory is not very different from Hahn’s (1971) distinction between what he calls, without taxonomic purposes, abstract and realistic models, and it should replace the obsolete distinction between Micro-theory and Macro-theory. On the other hand, the distinction under discussion has nothing to do with the usual ones between unrealism and realism, or irrelevancy and relevancy, since those terms are contingent on the problem at hand. Take Ostroy’s (1973) model of a sequence of simultaneous bilateral (between two individuals) trades. It is a theoretical model, in the precise sense that it incorporates a specific transactions structure. The model is quite relevant for the understanding of the existence and nature of monetary exchange. However, the specific transactions structure assumed seems unrealistic to me, abstracting from whether it is more realistic than other transactions structures implicitly used by other models. On the other hand, take the standard G.E. Debreu (1959) model as the pa-

radigm of analytical models. It is certainly quite irrelevant to the understanding of how society transacts. However, it might be a realistic representation of a future, and perhaps Utopian, transactions structure (see Kurz & Wilson, 1974).

7. See Barro and Grossman (1971), Benassy (1975), and Grossman (1971). At the so-called K-equilibrium, markets are cleared, but people are not doing what they would like to do at current prices. This runs counter to the standard definition of equilibrium. This explains why the work referred to in the text is generally associated with disequilibrium analysis.

8. The dual decision hypothesis can also be modified for *ad hoc* purposes. Urrutia (1978) forces Clower’s idea into a sequential structure of non-overlapping markets in order to analyze the coexistence of inflation and unemployment in a growing monetary economy.

of CL1, namely the absence of stocks: “modern economies maintain, in normal times, an enormous, and elaborate system of physical and financial buffer stocks”, asserts Leijonhufvud (1973, p. 38). An easy, although roundabout, way of capturing the theoretical necessity of incorporating the existence of buffer stocks is to reflect upon two of the different roles *realized sales* play in CL1, or *realized transactions* in its generalizations or modifications.⁹

First, under Say’s Principle, and in a world without stocks, realized sales are equal to current income and the latter is the relevant constraint on purchases. If this constraint is binding, we say we have aggregate demand deficiency. In the real stock-flow world, however, current income may exceed realized sales by an amount depending on the volume of buffer stocks—which include the stock of trade credit sustainable by the volume of physical and monetary stocks.

Second, only in a monetary economy without stocks, with markets visited sequentially, and with money being the only counterpart of any transaction, the relevant constraint on the demand signals a household may emit in one market are its realized sales in other markets. If this constraint is binding, we say that effective demand has failed.

I want to focus on a communication failure, rather than on a deficiency of aggregate demand. In a transactions structure with $n(n-1)/2$ trading posts (direct barter), in a sequence of simultaneous bilateral trades or in a system of a single “market” with an auctioneer, realized sales do not effectively constrain the emission of demand signals. In a monetary economy with buffer stocks, demand signals are not totally limited by actual or expected realized sales in other markets.

Including stocks and trade credit is, as we have just seen, one aspect of CL2. We can detect another important feature (let us call it CL2B) by wondering whether the distinction between aggregate demand deficiencies and Effective Demand Failures is indeed meaningful. It would be so if, and only if, i) aggregate demand is not deficient, but there are Effective Demand Failures; and/or ii) there are no Effective Demand Failures, but aggregate demand is deficient. It is fairly clear that neither i) nor ii) hold in a monetary economy as the one visualized by CL1, even if stocks and trade credit are appended to it. For the distinction between aggregate demand deficiency and effective demand failure to make some sense, we need to connect it to something other than the existence of buffer stocks; for instance, the distinction between alternative transaction structures or trading arrangements.

Under a direct barter arrangement (Veendorp, 1971), we assume that GE allocations exist and yet might be impossible to reach (deficiency); and if this is the case, they will not be reached (failure). Under a sequence of simultaneous bilateral trades without money (Ostroy, 1973), we also assume the existence of GE allocations, and they can be reached (not deficiency), but will not be reached (failure). Adding money and a monetary authority to this latter trading arrangement, GE allocations will be reached if they exist. With a single “market” and an auctioneer (Walrasian transactions structure), GE allocations will be reached if they exist.

These examples suggest that the reason why certain trading arrangements do not lead to communication failures is that, under these arrangements, individuals are not bound by pure flow constraints, but can generate trade credit. Hence it is quite safe to state that the essence of CL2 is an appeal to the explicit consideration of a) the possibility of “fooling” the budget constraint by means of the generation of trade credit to an extent supposedly dependent on the volume of accumulated stocks (CL2A); and of b) the underlying trading arrangement (CL2B).

These two aspects of CL2 are logically separable. The latter refers to how traders meet regardless of whether money is used or not. It is about transactions structure proper and, in this respect, it is very relevant whether we have direct barter, a sequence of simultaneous bilateral trades, or a Walrasian transactions structure. Nowadays, we easily recognize a problem of networks here. The first aspect of CL2 is whether, given the transactions structure, the conventional budget constraint can be “fooled”. In this regard, the existence of money and/or trade credit is the crucial question. Although logically separable, these two aspects are linked by the fact that the volume of stocks is probably not independent of the transactions structure.¹⁰

Whether or not Leijonhufvud had in mind both aspects is a moot question. Certainly, the explicit wording of Leijonhufvud in 1973 focuses primarily on CL2A. But if the little logical exercise performed here is correct, we may interpret him as implying both CL2A and CL2B. Indeed, if some of the assertions of Grossman (1974) are misunderstood, they cannot be properly interpreted without taking into account CL2B. I will vindicate Leijonhufvud’s claims in a discussion of the two points raised by Grossman (1974).

3.4. Significance of the monetization of exchange

My comments will be short, since this point is actually related to CL1. Grossman agrees with Leijonhufvud, and with everybody else for that matter, in the following point: the kind of exchange structure visualized by CL1 is crucial for the theory of effective demands as opposed to notional demands. Grossman, however, argues that Leijonhufvud’s argument is unconvincing because “in his barter example,

9. There is a third role of realized sales, the one they play as expected income. In generalized versions of the dual decision hypothesis, the aim of which is to prove the existence of a K-equilibrium as a fixed point of recursive process (Benassy, 1975), it is assumed that expected transactions at t are equal to perceived constraints on transactions at $t-1$. When the aim of the generalizations of the dual decision hypothesis is not the proof of existence, as it is in the case of Barro and Grossman (1971) and Grossman (1971), the recursive process can be collapsed into an instantaneous *tatōnement* in quantities allowing for expected transactions to be equal to currently perceived constraints. In both cases, perceived constraints are intimately related to realized transactions, and a crucial point is its specific relationship.

10. One of the implications of the fact that CL2A and CL2B are logically separable is that, contrary to what was thought under CL1, money is not necessarily related to any particular transactions structure. The following terminology will be used: transactions structure or trading arrangements refer to CL2B, while exchange structure refers to CL2A and CL2B.

the economy has only two goods (labor and commodities) and one market, obviously with a single market, market interactions, including those which generate effective demands, are non-existent. In his monetary example, the economy has three goods (labor, commodities and money) and two markets. However it is not clear from his example that the fact that the third good serves as media of exchange is crucial.”

It is difficult to admit that Leijonhufvud’s argument is unconvincing, since he is just explaining Clower’s paradigm, which lies at the very heart of CL1. From this paradigm, it became clear that an exchange structure consisting of markets visited sequentially and money as the unique medium of exchange, could generate effective demands different from notional ones.

In any case the model put forward by Grossman “in order to bring out the significance of the monetization of exchange in a more effective and transparent way”, is interesting – as it is his 1971 paper, his joint work with Barro (Barro & Grossman, 1971) and the unpublished paper he cites from Benassy of this year 1974 and which becomes Benassy (1975) when published. As I already mentioned, these are powerful generalizations of CL1. They can be considered, in fact, as the analytical completion of CL1 Leijonhufvud is asking for in order to explain why the monetary system considered does not home in towards a full coordinated state.

However, using properly understood CL2, the examples of Leijonhufvud (1973) can be interpreted in an alternative way, which I take to be more attuned to his purposes. In the barter example there are two “markets”, and every good is a medium of exchange. The transaction structure can then be interpreted as a Walrasian one that clearly creates no coordination failures. On the other hand, in the monetary example there are two markets visited sequentially and a unique medium of exchange. The transaction structure is the one associated with CL1 and, without stocks and trade credit, it will create coordination failures.

The interesting point, in my view, is not whether the number of markets is sufficient to generate market interactions, but rather which kind of transaction structure is assumed to hold and what kind of network the agents form. Even in the barter example, with only one “market” and two goods, there are possible alternative transaction structures: how transactors are going to meet, and according to which rules are they going to transact, if the transactions structure is, e.g., one of bilateral trading?

Alternative transaction structures are associated with different patterns of information flows and, presumably, with different behaviors of stocks. This is what Leijonhufvud points out rather clearly. In particular, he is inquiring into the possible effects the introduction of stocks and trade credit in any transaction structures might have on coordination failures, and specifically on the transactions structure implicit in CL1. To this now I turn.

3.5. Relation between price and quantity adjustments

According to Leijonhufvud, the introduction of buffer stocks into the exchange structure visualized in CL1 generates a Neoclassical Corridor fully surrounded by a Keynesian zone. Within the corridor any deviation from a fully coordinated

path generates deviation-counteracting feedback mechanisms, which are stronger the larger the displacement. Outside the corridor any deviation from a fully coordinated path triggers deviation-amplifying feed-back mechanisms due to Effective Demand Failures.¹¹

It is obvious that, under CL1 and the exchange structure compatible with it, the width of the corridor is literally zero and, therefore, any deviation from a fully coordinated path will immediately generate quantity adjustments. The reasons for that are, once again, the exchange structure visualized in CL1, and the search procedure consistent with this exchange structure.

The gist of CL2 is that, under the exchange structure of CL1 supplemented with buffer stocks and trade credit, the width of the corridor can be positive. Within the corridor we have price adjustments. Outside the corridor we have quantity adjustments which, through a *tatōnnement* in quantities, will lead to a K-equilibrium. Prices will supposedly adjust later, in response to the appropriate pressures as discussed above. Since Leijonhufvud abstracts from this latter type of price adjustment, we may interpret him as considering price and quantity adjustments always as alternatives, in spite of his explicit wording.

Grossman claims that this is flatly wrong. He is not denying that, for a given displacement from the fully coordinated path, the smaller the coefficient of price adjustment λ ($0 < \lambda < \infty$) the stronger are the pressures which produce quantity adjustments.¹² What he is asserting is that, for a given λ , the greater the displacement the greater are the pressures for quantity adjustments, but the greater are also the pressures for price adjustments. He thinks, furthermore, that this deprives the corridor of its analytical basis.

The two issues are however separable. Even assuming that price adjustments would take place after a displacement, and in the *ad hoc* fashion of stability analysis,¹³ CL2 implies that, for a given λ , the greater the displacement the more likely are quantity adjustments. This makes the corridor an analytically meaningful notion. To see this, consider the classical analytical problem proposed by Grossman in his 1974 comment. For a given λ , the greater is the decline in the money supply, M , the greater is the price adjustment per period and the greater are the pressures for quantity adjustments.

In this respect Grossman is correct. Under CL2, however, individuals are not bound by pure flow constraints, but hold stocks of every commodity and can generate trade credit up to a limit presumably dependent on the volume of stocks they hold. Therefore, the volume of stocks determines the number of periods over which individuals can still make

11. His wording is, of course, quite careful, and he never rules out the possibility of both kinds of adjustment taking place simultaneously (Leijonhufvud, 1973, p. 32), but he does not make any use of this implicit qualification.

12. Although Grossman devotes some space to showing that this is so, this has nothing to do with the issue at stake. See footnote 1 in p. 37 of Leijonhufvud (1973).

13. The two assumptions are not equivalent, since even if it is true that the price adjustment as a proportion of the excess demand created by the displacement is the assumed behavior of the auctioneer, it is not necessarily the behavior of a price setter in disequilibrium. See footnote 2.

their notional demands effective and, therefore, allow only for price adjustments.

Now, the larger the displacement the smaller this number of periods and, therefore, the larger the probability that eventually effective demands diverge from their notional counterparts. Consequently, the more likely quantity adjustments are. Notice that my argument does not imply that price and quantity adjustments are necessarily alternative. It is only when the displacement from a fully coordinated path is very substantial that we are allowed to use Hicks' fix-price method. For it is only in such a case that Effective Demand Failures will eventually occur. Given the exchange structure considered, a search procedure consistent with it will set in justifying the consideration of prices as fixed.

It is thus seen that CL2 has a (say) methodological implication working against the method of analysis used by CL1 and its generalizations. In particular, the work of Grossman, Benassy and Grandmont and Laroque, referred above as generalizations of CL1, seem to be relevant only for fixed-price vectors that differ widely from the equilibrium one, due to the substantial size of the original displacement. In consequence these pieces of analysis can hardly be considered relevant to explain or describe the workings of the system under the small displacements characterizing normal times. In my view, it also follows that Veendorp's result on local stability (1975) is not relevant to the problem.

Having said all this, I find quite paradoxical Grossman's confessed impossibility of imagining "conditions under which some prices may show no tendency to change although desires to sell and to buy do not coincide in the respective markets". If these conditions are not apparent to him, he should not have used the fix-price method (Barro & Grossman 1971; Grossman, 1971). Of course, it was a legitimate use, because the exchange structure of CL1, together with the search behavior consistent with it, provides an obvious example of the conditions under consideration. In fact, Leijonhufvud devoted his famous 1968 book to the examination of these very conditions.¹⁴

Finally, in my argument vindicating the notion of the corridor, I only made use of the first aspect of CL2. However, we also need CL2B to understand that all "prices might be at their 'right' (GE) levels, but amounts transacted differ persistently from the desired rates of sale and purchase in some markets". It is certainly true that, under certain transaction structures, GE configurations will not be reached even if prices are at their right levels (Ostroy, 1973; Veendorp, 1970). This is not true, however, in the exchange structure of CL1 Leijonhufvud is referring to at this point. Therefore, Grossman is justified in objecting to the validity of the statement.

3.6. Difficulties in the modeling of the new ideas

In order to describe and explain the short-run workings of a market system in normal times, we need a formal model which allows simultaneously for both price and quantity ad-

justments. This crucial implication of my previous argument has not been achieved by the analytical generalizations of CL1.

I submit that if (and when) it is ever accomplished, it will have to take into account CL2 as explained above. Incorporating CL2 into such a model might be difficult. A very basic difficulty is that we cannot hope (without making use of not very popular ideas about complex systems) to fully integrate the Theory of Exchange and the Theory of Coordination of Activities. This impossibility is reflected in the usual procedure of specifying a transactions technology set —actually, just a way of skirting the question of which is, in fact, the structure of the transactions structure (*pace*, e.g., Hahn, 1971).¹⁵ It is difficult to imagine how the passage of time in the model will generate successive transaction structures, each of them making for a different theoretical model of Coordination of Activities. Someone may guess that incorporating technological progress into the transaction technology set of the previous footnote could solve the difficulty. It does not, because the idea of technological progress is an analytical construction which covers our theoretical ignorance: why at a certain moment a new method of transactions becomes available? Why does society know it may be more efficient without trying it? Why should society try it? What we can hope to have is a Theory of Exchange which isolates conditions for the emergence of a particular transaction or exchange structure. Be it the economic fact of transaction costs, or the political fact of a monetary authority, these conditions have to be incorporated into the model so as to have a Theory of the Coordination of Activities under the particular transaction or exchange structure consistent with these conditions.¹⁶

Even if all this had been accomplished for, say, the monetary exchange structure visualized in CL1, we still face the problem of how to model the budget constraint. The value of purchases at current prices can exceed the amount of outside money the individual holds by an amount limited by the size of the individual's stocks of money and other physical goods. The model has to explain not only that, under the exchange structure considered, people will hold stocks, but also how the size of these stocks is determined and what amount of trade credit will this particular size support.

Even assuming that this can be done in a meaningful way, a host of other problems appear. Suppose M declines. In the first day of the recursive process each individual forms his notional demands taking into account the new kind of budget constraint at the old equilibrium price vector. Each individual visits each market sequentially. Some individuals will be rationed in, say, the first market visited. Each individual will now form his/her effective demands taking into account the new kind of budget constraint at the old equilibrium price vector and the fact that s(he) has been rationed

14. It goes without saying that much of the book has to be revised in accordance with CL2. CL2 is indeed very sweeping. Cf. footnote 3 in pages 34-35 of Leijonhufvud (1973).

15. This is reminiscent of the usual practice in GE analysis of specifying a social production set not derived from the individual production sets. The analogy would be totally correct only if it were the case that we did not know how individual producers do what they do.

16. Note the care with which Hahn (1971 and later in Hahn, 1989) strives to incorporate into the transaction set what he thinks are conditions for the existence of money.

in the first market visited; and this for any market and all individuals.

But now we are not allowed to let this *tatônnement* in quantities run its course until a K-equilibrium is reached, because, at the end of the day, somebody, according to some policy, is going to partially adjust the prices. We can safely assume that some agent with a temporary monopoly power in disequilibrium modifies prices as part of a rational search procedure, but this search procedure cannot be described—as it was in the case of quantity adjustments—without price adjustments. Whatever story we devise to describe it, it has to be consistent with the transaction structure envisioned.

If we now look at the following day of the recursive process, I do not think it will be reasonable to let individuals form their effective demands taking into account, besides the new kind of budget constraint at the new non-equilibrium price vector, expectations of future transactions based on passed actual transactions. And this because yesterday's prices are not today's prices, and the information provided by the latter has already been incorporated in the budget constraint. Thus, there seems to be no justification for the use of static expectations as in the generalizations of CL1. Even worse, there seems to be no basis for any kind of rational expectations.

For instance, individuals might think that the new price vector is the equilibrium one and will accordingly express their notional demands which take into account the new kind of budget constraint and nothing else. Since it has been assumed that the new price vector is a non-equilibrium one, some individual will be rationed in, say, the first market visited. A new set of effective demands will be formed as in the first day of the recursive process. At the end of the second day some agent will move the price according to the rational search procedure. The day after, individuals might very well be at a loss as how to form their effective demands. No surprise then that we (economists) feel at a loss concerning the modeling of their behavior.

3.7. Some conclusions

The difficulties reported here are challenges, but they do not constitute an impossibility theorem. Assume then that they have been overcome, and that the resulting recursive process can be shown to have a fixed point we call a P-Q equilibrium.

What are the likely implications of this new choice-theory apparatus for those macro-constructions which, like the consumption function (Clower, 1965) or the accelerator (Grossman, 1972), had received such a nice choice-theory foundation under CL1? My optimistic conjecture is that, what was true under CL1 will still be true because quantity adjustments continue to occur, but that the new choice-theoretical foundation will yield more specific implications. This is the main reason underlying Leijonhufvud's urge for formal modeling.

We could finally inquire into the efficiency properties of the PQ-equilibrium. If it is identical with GE, it will be Pareto-efficient. If it is not identical with GE, the efficiency of the PQ-equilibrium could be analyzed according to the criterion proposed by Arrow and Hahn (1971), and also used by Benassy (1975) in his analysis of K-equilibria. I think, howe-

ver, we should take into consideration the transaction structure, because the alternative efficiency criterion refers to a bilateral barter trading arrangement. We could try to develop alternative efficiency criteria, suited to the particular transaction structure considered. What CL2 suggests is that it is perhaps time to lay down the importance of static efficiency criteria and to devise new concepts relating to the coordinative performance of the economic system.

Since I have shown that, for a given λ , the larger is the width of the corridor the greater is the volume of stocks, it follows that the greater the stocks the less likely is the occurrence of unpleasant unemployment situations following a shock of random magnitude. That is, the greater the volume of stocks the greater the "safety" of the system.¹⁷

Now, as I have already said, the volume of stocks is probably dependent on the transactions structure. Therefore if appears as if we could begin the analysis of the relative "safety" of alternative transaction structures.

I hope the importance and far-reaching implications (even, or specially, for macro-theory) of CL2 are now clear. We should perhaps talk less about the micro-foundations of Macro-theory and more about the macro-stimulus to Microtheory. This was appropriate to say in the 1970s and still is forty years later.

4. Recessions under the forgotten lens

In order to round up this paper it is worth demonstrating that the ideas underlying the development of the notion of Effective Demand Failures are useful for the understanding of the different crises observed. The focus will be now be put on the two most important ones in the last forty years. The 35 year-old oil crisis and the Great Recession we are still in, at least in Europe.

4.1. The oil crisis

This is the appropriate place to remember the oil crisis of 1973/74 and 1979/80 and the feeling of bafflement about what was the cause of the increase in the price of oil and what was to be done. Together with M.C. Gallastegui, I wrote a paper in *Economies et Sociétés* in 1983 (Gallastegui & Urrutia, 1983), the spirit of which was indeed tinted by ideas of Disequilibrium Macroeconomics. To begin with we wanted to use the idea of the fixity of prices. As we already knew—and at the time had not been yet forgotten—, after any shock it is possible that the prices do not equilibrate the situation immediately allowing for quantity adjustments leading to the workings of multipliers and accelerators. We did justify this possibility on the basis of the Neoclassical Corridor or, as it is now known, stability corridor. We did focus on the width of this corridor and the determining factors of this width. The size of Monetary and Fiscal policies, relative to the magnitude of the shock, were indeed the main determining factors. Restrictive

17. A great volume of stocks is normally taken as a sign of lack of efficiency, and here it is argued that it provides "safety". The two things are not contradictory and suggest that these might be a trade-off between efficiency and "safety."

macro-policies lead to a narrower corridor. After the OPEC-engineered shock, it was rather obvious that we were poorer with a smaller productive capacity and our potential growth was indeed smaller.

At the time, we submitted the suggestion that among the determining factors of the width of the corridor, we had to introduce the notion of Credit Limit, a notion not at all standard at the time. While this limit was not reached, the corridor's width would be large enough. But the situation changes immediately once we approach this limit and quantity adjustments begin to be felt. We did mention that it was likely that the monetary and fiscal policies might not be generous enough because their size was calibrated according to the old growth potential, but we did not however push forward the argument about the limit of credit depending on the level of leverage of the banking system—something we have now learned too late. The difference with the present situation is precisely that the banking system is now indeed exhausted and incapable of giving more credit. The robustness of the financial system in general is then crucial. One of the main fiascos of the Spanish economy in the last years has been the mis-information about this robustness.

4.2. The Great Recession

Let us now turn of the notion of the Corridor that I think is appropriate to understand what has come to be known as the Great Recession and discuss the ways out of it. This notion, as we have seen, is not easy to grasp in all its richness with the tools of neoclassical economics, which is why it had so little success (Laidler, 2008). However this “sleeping beauty” has been visited by various princes (as, e.g. Kehoe & Levine, 2006, in the context of debt constrained markets), and the notion of the corridor has been used in a way which turns out to be useful now. In 2007 Dohtani, Inaba and Osaka wrote a paper on neoclassical growth in which the notion is used in terms more recognizable now. They complicate the standard model by introducing a consumption function depending on permanent income. This turns out to be sufficient for the model to show the same and unique stationary state shown by the standard model, but there is now a stability corridor around the growth path. Within the corridor, the neoclassical vision is validated because any path converges to the unique equilibrium; outside the corridor, every path diverges from the neoclassical long-run equilibrium.

Let us now see how this, now awake, beauty may help to understand what is happening during the Great Recession. We are used to recognizing the failures of effective demand when an unemployed worker cannot make his demand effective because he lacks income. We also know that a savings decision is not automatically translated into an effective demand of future goods. And there is an additional way of showing a failure of effective demand. This happens when the financial system is in such a disarray that makes it impossible for entrepreneurs to credibly show their present demand of factors of production due to the impossibility of supplying future goods now. In a 2004 interview, conducted by Snowdon, Leijonhufvud says quite expressively that his third way of effective demand failure makes it impossible to credibly say the following: “I have this investment pro-

ject that will pay off in the future and I want to trade that prospect for the factors of production today necessary to produce those future goods”. And he continues: “And that’s where we end up if the financial system is totally clogged up with bad loans. That has been, and still is, the Japanese situation”. If the problem was the conventional Keynesian one (of consumers being cash-constrained), then there is a rationale for public works. But that was never the Japanese problem. Their problem was that they did not move directly to clean up the banking system after the collapse of the real estate and stock market bubble.

This was the situation of Japan twenty years ago, and it might not be mere chance that some of the notions of Disequilibrium Macroeconomics have been preserved in Japan. That situation is not very different from what it is currently happening in the Great Recession, except that the construction crisis is now happening in the USA. In situations of this nature, it is not enough to increase the public spending. For this move to be effective, it is necessary to have a clean financial system capable to serve as a conveyor belt of this public spending. But the financial system is crowded with bad loans. If the government and the banking system are in collusion, the situation looks indeed rather dark indeed. That happened in the USA with Lehman brothers and in the UK with Northern Rock, and also with the apparently sound situation of banks in Spain.

Quite recently, Leijonhufvud (2009a) concluded in a VOX EU column (no ordinary recession) that:

Fiscal stimulus will not have much effect as long as the financial system is deleveraging. Even if that problem were to be more or less solved, the government deficit would have to offset both the decline in industry investment and the rise in household saving—a gap that is rising as the recession deepens. Here, too, the public is skeptical and prone to conclude that a program that only slows or stops the decline but fails to “jump start” the economy must have been a waste of tax payers’ money. The most effective composition of such a program is also a problem.

5. Final comments

I hope I have made clear that the dynamics of any intellectual field is complex and, as such, it may generate unexpected patterns of behavior, not necessarily optimal. This is what has happened in economics in the last forty years. We have witnessed new ways of thinking in macro beyond the DSGE models, even though a new paradigm has not yet emerged. There is a large variety of approaches shaking the foundations of mainstream economics. From Behavioral Economics (unifying the results in Experimental Economics, Experimental Psychology and Neuronomics) doubts are cast on the canonical notion of rationality. Previous objections came from bounded rationality, network analysis and the dynamics of complex systems: See Kirman (2011). And now also from those new Black Swan ideas about uncertainty that are so challenging and humiliating to the forecasters. I want to emphasize now that a particular, albeit rudimentary, complex system had been here for forty years. It even had its day of glory, soon forgotten: this is the Disequilibrium approach

to macroeconomics and, more specifically, the idea of corridor explored above.

Had we not forgotten this clever idea, we would have considered alternatives to understanding the great Recession in terms of DSGE models stressing rational expectations (in order to make room for the inefficacy of conventional policy measures). We could have understood four years ago that the system was working outside the Neoclassical Corridor, and move to a way of thinking that made room for complex systems and their possible unexpected results.

Had we reasoned along these lines, we would have immediately realized that the more leveraged the productive system is, the narrower is the Neoclassical Corridor. We could have come to avoid completely useless policies taking into account the size of both the disequilibria and the parameters of the leverage, as well as the value losses in the balance sheets. Then we could have produced a correct diagnosis and the need to globally coordinate efforts. We might have been even able to explain that to socialize the losses of the financial system was better than to let it fall, because in this latter case the corridor would have become a simple line without any width.

But as I have stressed from the beginning, this paper does not only aim at waking up a sleeping beauty. I also want to illustrate the so-called path dependency in the social construction of ideas. And here Leijonhufvud has played a double role. To begin with, he was the author capable of really departing from the general equilibrium framework, demanding new micro-foundations that—as I think I have shown—could have avoided quite a heavy burden of erroneous policies. But he was also an early whistle blower on the dangers of not leaving the treaded path and not turning towards, what we could now call, *Econophysics*.

Regarding the first issue, it is still notable that Leijonhufvud has been ignored, that Barro, in a private conversation with the author of this article, explicitly rejects the disequilibrium approach and even wishes he had not written the joint book with Grossman. Even more remarkable, Google returns very few hits if you type “Neoclassical Corridor”. Backhouse and Boianovsky (2012) provide some elements of an explanation. I only wanted to illustrate the path dependency here in the History of economic ideas emphasized by Brian Arthur (1994) and Arthur et al. (1997).

As a pioneer of *Econophysics*, Leijonhufvud is also an excellent case study for historians, since he tried to organize the profession around computational economics, an alternative way to tackle problems that were difficult to model, but were easy to simulate. In 1991 he organized the Center for Computable Economics in UCLA that he directed up to 1997. His approach was not very much appreciated among his colleagues, and he moved to the University de Trento in the Computable and Experimental Economics Laboratory (CEEL) where he could continue his involvement in this branch of *Econophysics*.

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